Early impacts of Mandatory Work Activity

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Acknowledgements

Thanks to independent experts, the National Institute of Economic and Social Research (NIESR), who performed a brief peer review of the analysis, focusing in particular on the methodology. NIESR concluded that the methodology was sound, although the nature of the selection process for programme referrals means that it is very difficult to identify truly comparable individuals who were not referred. As a consequence, it is possible that benefit impacts are underestimated. However, subject to this, the key conclusions - that MWA had a small and transitory impact on benefit receipt, and no impact on employment - appear reasonable. NIESR emphasised that the analysis was preliminary, and made a number of recommendations for future work to develop and extend the analysis. NIESR's review was confined to the methodology; NIESR did not have access to the actual data used and did not attempt to replicate the results.

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1 Introduction

Mandatory Work Activity (MWA) was launched in May 2011 to provide extra support to a small number of Jobseeker's Allowance (JSA) claimants who need extra help to develop the disciplines and behaviours associated with employment. This was at a time when a number of other employment programmes for jobseekers had begun or were about to begin, some voluntary - such as work experience (January 2011) and New Enterprise Allowance (April 2011), other mandatory – such as Skills Conditionality (August 2011). Most notable and by far the largest was the launch of the Work Programme in June 2011.

MWA is delivered by contracted providers who source placements that offer participants a chance to develop fundamental work-related disciplines and behaviours, and also deliver a benefit to the local community. Referrals to the scheme are at the discretion of Jobcentre Plus advisers. Guidance to advisers indicates that a claimant suitable for referral to MWA is one who is lacking or failing to demonstrate the disciplines and behaviours needed to seek out and secure employment.

Once a claimant is referred to Mandatory Work Activity, participation is mandatory and sanctions apply if a claimant fails to participate without good cause. MWA placements last for 4 weeks and for 30 hours a week (unless there are agreed restrictions on the Jobseeker's Agreement in which case the number of hours is reduced). Claimants undertaking a MWA placement continue to receive their benefit and are required to continue to sign for their benefit each fortnight and be available for and actively seek employment during their participation period.

Mandatory Work Activity statistics[1] indicated that from May 2011 to November 2011, there were 24,010 referrals to MWA and that for the same period there were 1,630 sanctions[2] for failing to participate on MWA.

This report is a quantitative analysis of MWA, providing estimates of:

1. the net impact of MWA on the likelihood of referrals being in receipt of an out of work benefit[3] or training allowance[4] during the first 21 weeks following referral;
2. the net impact of MWA on the likelihood of referrals being in employment during the first 21 weeks following referral; and

[1] http://statistics.dwp.gov.uk/asd/asd1/pwp/pwp_gbw_feb12.pdf
[2] http://83.244.183.180/sanction/sanction/LIVE/tabtool.html
[3] Jobseeker's Allowance, Income Support, Incapacity Benefit or Employment and Support Allowance
[4] A training allowance is a maintenance allowance paid instead of Jobseeker's Allowance, out of public funds to people taking part in a course of training or instruction provided by the Department or under arrangements made with a partner.

3. the net impact of MWA on the likelihood of referrals being in payment of an out of work benefit (adjusting benefit outcome to exclude claimants whose benefit had been sanctioned), or training allowance during the first 21 weeks following referral.

The first two outcome measures are standard for this type of evaluation study. The third measure, though unusual, is helpful for our assessment because while an individual is in receipt of a sanction it is still technically possible to be in receipt of JSA but not receiving any payment. Therefore the third measure adjusts the outcome, and thus impact measure, to consider only individuals who are in payment of JSA; in other words, the outcome does not include individuals who are currently on JSA but in receipt of a sanction.

The main analysis focuses on the first cohort of referrals from the beginning of the programme in May 2011 up to and including July 2011. The cohort was chosen to give a reasonable tracking time allowed by the data for a sizeable number of referrals. In addition to a number of sub-group analyses, any signs in changes to the results since the beginning of the programme are explored by examining the impacts over 13 weeks on a later cohort, referred between August and September.

Also some tentative estimates of the impacts separately on those who actually *started* a placement, and on those who were referred but did not start, are also presented.

The remainder of this chapter provides context for the study. Section 1.1 describes the rationale for the present analysis. Section 1.2 introduces the policy background and the overall design of the programme. Section 1.3 describes the overall participation on the programme.

1.1 Rationale for the present analysis

The challenge faced when evaluating employment programmes such as MWA is that we can never be certain what would have happened to claimants if they had *not* been referred to the programme, particularly when the referral is as a result of adviser discretion. Unless we know what would have happened to these referrals if they had not been referred, we cannot say whether the programme actually made any difference to their labour market prospects.

Therefore, the aim of the impact analysis is to compare the observed labour market outcomes of MWA referrals with an estimate of their ‘counterfactual’ outcomes (the labour market outcomes which would have occurred in an ‘alternative world’ if they had not been referred).

Our methodology draws heavily and builds on the approach used by Ainsworth and Marlow (2011). We use Propensity Score Matching (PSM) to construct a suitable comparison group of individuals who were not referred (denoted non-referrals from herein) who most closely resemble the group of

referrals under analysis. We then use the labour market outcomes of the matched non-referral group as an estimate of the counterfactual outcomes of the MWA referrals and compare these with the observed referral outcomes. We use a rich data set comprising the individual characteristics of referrals and non-referrals to carefully construct the matched comparison group of non-referrals.

Advisers have broad discretion in selection, which will in part depend on a number of subjective judgements. Therefore the task to construct a comparison group that mimics the selection decision is not trivial and poses a significant challenge. Not accounting for this selection decision accurately will mean that estimates for the counterfactual and thus impacts will not be robust. However, we believe that we have included sufficient variables to control for the factors believed to affect selection, and set out our reasoning in section 3, but acknowledge that, as in all such matching approaches, we cannot rule out the possibility that there were additional, unobserved factors which we have been unable to account for.

An alternative methodological approach is to use duration analysis to exploit the timing of each event: i.e. referral – start – referral to sanction and adverse decision. Such an approach has been used for example by Arni et al (2009) to examine the effectiveness of unemployment sanctions on benefit. Although we did not have time to explore this approach in this study it might be worth investigating in future work.

1.2 Policy Background and Design

MWA is available to JSA claimants as part of a range of support available through the Jobcentre Plus Offer, which intends to provide claimants with personalised, responsive support needed to find them employment.

The Jobcentre Plus Offer is available from day one of a JSA claim until a claimant either leaves benefit or enters the Work Programme. JSA claimants are usually referred to the Work Programme after 52 weeks of claiming JSA, with 18- to 24-year-olds being referred from 39 weeks and more disadvantaged groups from week 13. Once participating on the Work Programme, a JSA claimant does not receive the vast majority of support available via the Jobcentre Plus Offer, only continuing to ‘sign’ at short Fortnightly Jobsearch Review (FJR) meetings with Jobcentre Plus advisers.

MWA is exclusively for JSA claimants that are being helped by Jobcentre Plus advisers as part of the Jobcentre Plus Offer. Once a JSA claimant becomes a Work Programme participant, MWA is no longer available for them to be referred to. MWA is targeted at JSA claimants aged 18 or over who are judged to be lacking or failing to demonstrate the disciplines and behaviours needed to seek out and gain employment.

The aims of MWA are to:

- re-enforce the responsibilities associated with claiming JSA;
- provide jobseekers with the disciplines associated with employment while at the same time enabling them to make a contribution to the local community;
- increase jobsearch activity and engagement with other back to work support from jobseekers who participate; and
- increase off-flow rates from JSA, as part of the wider support available through Jobcentre Plus.

Responsibility for identifying and referring suitable claimants to MWA lies with Jobcentre Plus advisers in conjunction with their Advisory Team Manager. Thereafter, contracted providers are responsible for the delivery of the provision.

MWA placements last for four weeks and are usually for 30 hours a week, allowing claimants time to look for work while still providing them with the labour market disciplines associated with work. Some claimants may agree with their Jobcentre Plus adviser that they are only seeking work for a smaller number of hours. These claimants are referred to Mandatory Work Activity that, with time to look for work, is equivalent to the same number of hours.

MWA is delivered by contracted providers, with one Prime Provider in each of 12 Contract Package Areas (CPAs). While a Prime Provider in a CPA might deliver placements via sub-contracting arrangements, all referrals to MWA within each CPA are to the Prime Provider who holds the contract. This means there is no competition between Prime Providers in a CPA. Providers are responsible for sourcing the placements, informing a claimant of the details of their placement, arranging for their start and ensuring their continued participation in the placement. The Department does not specify what the placement should be, but does expect that every placement will offer people the opportunity to gain fundamental work disciplines, as well as being of benefit to local communities. It is also expected that placements are additional to any existing or expected vacancies the host organisation might have.

Providers are also responsible for raising a compliance doubt where a claimant has failed to participate in a placement. They are also responsible for reasonable travel, childcare and additional support costs while the claimant is undertaking a placement. Providers complete a reference for every claimant who completes their MWA showing what they have done and the progress they have made so that prospective employers will have an applicant that can demonstrate that they possess work-related disciplines.

In each CPA there is a strict annual limit on the number of claimants the provider can take on to MWA provision. Therefore, overlapping Jobcentre Plus Districts within a CPA must work together to manage referral numbers.

Referrals would not normally be made for claimants who have been claiming JSA for less than 13 weeks. However, Jobcentre Plus advisers have the flexibility to refer these claimants where they judge it to be appropriate. Since places are limited, the guidance to advisers stresses the importance of deploying provision appropriately. Examples include:

- where the primary barrier to a claimant finding work is a lack of focus and discipline on their part, MWA has the potential to help them. But where that lack of focus has an underlying cause such as significant disability or a low level of basic skills then the programme would only be appropriate if it were deployed as a step within a structured approach designed to address the claimant's multiple barriers;
- if a lack of work experience is proving to be a barrier to finding work for an otherwise well-focused claimant, advisers should address this through other Jobcentre Plus Offer measures;
- if a claimant has recently received a labour market-related sanction/disallowance and MWA would be beneficial to helping the claimant to develop disciplines; and
- if the claimant is due to commence Work Programme provision within the next few weeks, advisers should consider seriously whether a referral is the best use of resource.

The intention is that a referral should never come as a surprise to a claimant. The referral should also not be used as a threat and must be seen to be fair and reasonable; there will be some explanation as to the reasons for the referral and an overview of the provision and mandatory aspect. The adviser should also explain that the final decision on a claimant's suitability is made in consultation with an Advisory Team Manager.

Claimants undertaking a MWA placement remain on benefit and, therefore, are required to continue to 'sign' for their benefit at FJRs and be available for, and actively seek, employment during the period of their participation.

There are a number of ways in which an individual can be referred again if they sign off JSA and then return; the guidance is that a re-referral should be made when either:

- a claimant ceases to claim JSA after starting their MWA placement but before they complete the four weeks, and the new referral date is less than 14 days after the provision end date; or
- a claimant leaves provision early but continues claiming JSA, and the new referral date is within two weeks of the date they left the placement.

Otherwise all subsequent referrals (for claimants who have not started or where more than two weeks have passed) are made as further 'initial' referrals; if a claimant ceases to claim JSA between point of referral and start date of MWA placement the Advisory Team at the Jobcentre must consider if it is appropriate to make a subsequent MWA referral.

Once a claimant is referred to MWA, a fixed term sanction penalty can be imposed if the claimant has failed to participate without good cause, but continues to claim benefit. A first failure to participate results in a reduction - or loss - of JSA for a period of 13 weeks, while further failures result in a sanction of 26 weeks. When a claimant is referred for a sanction for non-attendance, the imposition will depend on whether they attend or not and whether good cause for not attending can be stated to the Decision Maker. If the Decision Maker decides to impose a sanction after the referral, an award of JSA can be made under the hardship rules[5].

It is difficult to compare MWA with past programmes, least of all because MWA is operating in a different economic climate and also due to the short run of data available to us. However there are parallels with the Project Work pilots in 1996-97 and more recently the Intensive Activity Period (IAP) pilots in 2004-07.

Project Work was a programme aiming to help people aged 18-50 who had been unemployed for two or more years to leave the register and get work. Claimants in a number of districts were offered 13 weeks of optional help with job search and employers were given a wage incentive. If they had not left benefit by 13 weeks they were required to perform mandatory work experience for a further 13 weeks. The evaluation (Bryson et al. 1998) found that claimants left benefits at the point before the transition to being mandated onto work experience, but there was little change in their likelihood of entering employment.

The IAP randomised control trial pilots examined the impact of mandating JSA claimants aged over 50 years old to participate in the IAP phase of the New Deal 25 plus (ND25+). IAP provided assistance such as training opportunities and work placements. Prior to the study participation had always been voluntary for this age group. Claimants participating in the pilot were older and at a later point in their claim than MWA referrals on average. However, since the aim for MWA is to target jobseekers that lack the disciplines for effective work search and so are likely to be at risk of becoming long term unemployed, the group is likely to be more similar than the age and length of unemployment would suggest.

The evaluation of IAP (Dorsett et al. 2008) showed that around 10% of claimants left benefit before starting on IAP who would not have done if it had been voluntary. The overall impact of the programme was to reduce time spent on JSA by 49 days. The impact of IAP was also in the typical range of off benefit impacts of other employment programmes - for example, the impact of New Deal for Young People (NDYP) was around 64 days.

There is also international evidence on the size of the impact of similar mandatory programmes which shows similar results. For example, Rosholm and Svarer (2004) showed that claimants were deterred by mandatory activity

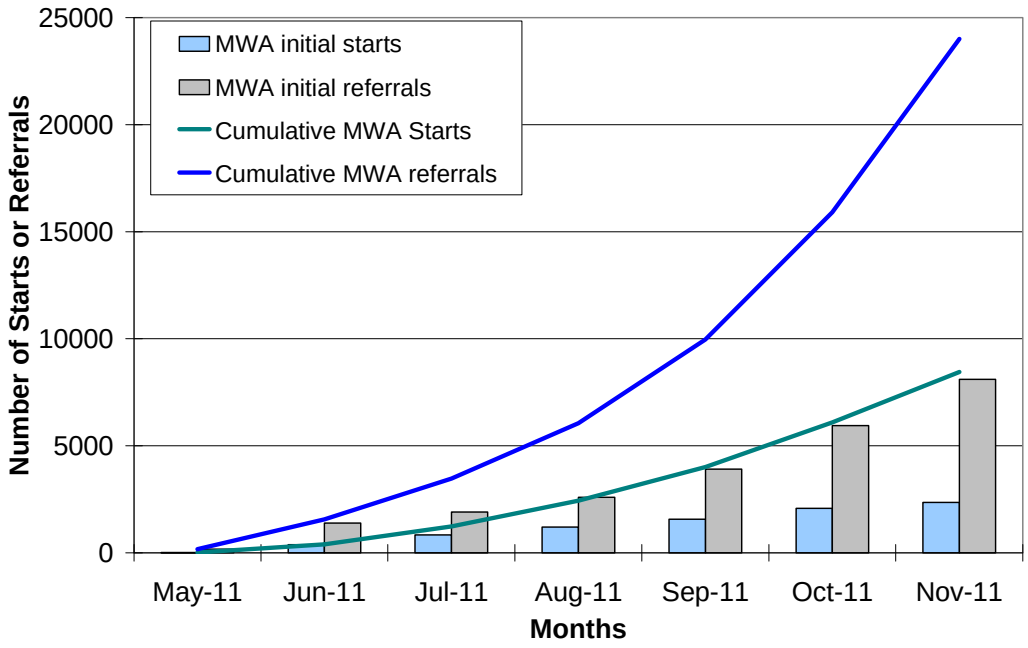
[5] JSA hardship is an award of Income Based JSA made at a reduced rate to give a minimum level of financial support to certain individuals, in certain prescribed circumstances, who do not qualify for JSA under normal rules and would suffer hardship if JSA were not paid.

in Denmark by an average of three weeks. Black et al (2003), drawing on evidence from the United States, showed a mean deterrence of about two weeks.

1.3 Referrals and Starts to MWA

In the period from May 2011 up to and including November 2011, there were 24,010 referrals[6] (and 1,040 re-referrals) and 8,610 starts to MWA placements. Figure 1.1 shows that activity began quite slowly with just under 200 referrals in May 2011 increasing to over 8,000 referrals and over 2,000 starts by November 2011. In the same period there were 1,630 sanctions for failing to participate on MWA.

Figure 1.1: Monthly and cumulative referrals and starts to MWA



Source: Pre-Work Programme and Get Britain Working Official Statistics, Feb 2012

Table 1.1 shows the spread of referrals to MWA across regions in Great Britain. Since referrals are selected at adviser discretion, it is difficult to define

[6] As defined in the official statistics: a MWA referral is defined as the date that the adviser inputted the referral onto Jobcentre Plus' Labour Market System; a MWA start is defined as the date the claimant attended their initial interview with the placement provider - this was input by the provider into the Provider Referrals and Payments system (PRaP) which then updated Jobcentre Plus' Labour Market System.

an eligible population to compare against this distribution. Given that referrals are generally selected from claimants with long benefit histories and durations, we have chosen the proportion of JSA claimants who passed through six months' duration (six month threshold flows) during May-November 2011 to give a rough sense of the spread of the potential pool of referrals. The data suggests that the geographical spread of referrals is reasonably commensurate with the spread of JSA claimants.

Table 1.1*: *MWA referrals by region

Region	***MWA referrals (May-Nov 2011)***	***Six month JSA threshold flows (May-Nov 2011)***
Central England	23%	21%
London and the Home Counties	26%	22%
North East	14%	17%
North West	11%	12%
Scotland	9%	9%
Southern England	10%	12%
Wales	5%	5%
Unknown	1%	1%
Great Britain	***24,010***	***576,420***

Source: Pre-Work Programme and Get Britain Working Official Statistics Official Statistics, Feb 2012 and National Benefits Database.

Table 1.2, which shows the age distribution, indicates that MWA referrals are younger than claimants passing through 6 months' JSA duration.

Table 1.2*: *MWA referrals by age

Age	***MWA referrals (May-Nov 2011)***	***6 month JSA threshold flows (May-Nov 2011)***
18-24	41%	29%
25+	59%	71%
All	**24,010**	**576,420**

Source: Pre-Work Programme and Get Britain Working Official Statistics Official Statistics, Feb 2012 and National Benefits Database.

2 Data and Sample Definition

This section outlines the data and sample definition used in the evaluation. Section 2.1 describes the method of drawing samples from which we construct groups of referrals and non-referrals for comparison. Section 2.2 describes the variables used in the evaluation. Finally, Section 2.3 compares the referral and non-referral samples.

2.1 Sample definitions

This section describes the referral and non-referral samples selected for the main impact analysis (Section 4.2 describes a number of sensitivity tests which use alternative referral and non-referral samples).

2.1.1 Defining the MWA referral sample

The aim of this paper is to estimate the impacts of a referral to the MWA programme. A MWA referral is defined as the date that a Jobcentre Plus adviser inputted the date of a referral onto Jobcentre Plus' Labour Market System.

The main analysis was performed on a sample that were referred between May 2011 and up to and including July 2011 and were receiving JSA in the week they were referred. The cohort was chosen to provide a reasonable tracking time (21 weeks of outcomes) allowed by the data for a sizeable number of referrals.

All referrals meeting the following criteria were included:

- the referral must be claiming Jobseeker's Allowance (JSA) in the week of being referred to their MWA placement; and
- the referral must be the earliest referral that an individual had between May and up to and including July 2011.

The resulting referral sample size was 3,190. Appendix 1 shows in more detail how this sample of 3,190 referrals was selected from the total of 3,380 who were referred to MWA during the cohort period.

2.1.2 Defining the non-referral sample

In defining the non-referral sample, the aim is to select non-referrals who can best represent what would have happened to MWA referrals if they had not been referred to MWA.

To compare the outcomes of referrals and non-referrals over a time period such that non-referrals can represent what would have happened to MWA referrals if they had not been referred, pseudo referral dates[7] were assigned to each non-referral. The pseudo referral dates for non-referrals were subsequently treated as equivalent to the actual referral date for referrals.

The non-referral sample was drawn from DWP administrative data sets. All non-referrals meeting the following conditions were included:

- The non-referral must be claiming JSA in the week of their assigned pseudo referral date;
- Claimants must not have been referred to MWA between May 2011 and July 2011[8]; and
- The pseudo referral date must be between May 2011 and July 2011.

Before including these conditions the number of non-referrals was 2,980,000. After applying them the resulting sample size was 611,230. Due to the size of the dataset and following sensitivity tests that showed that there was very little gain to running analyses on the whole sample, analyses were run using 20% samples of the non-referral sample; this meant that the final sample size was 124,740.

Section 3 describes how suitable 'matched' groups of referrals and non-referrals were selected from these samples and compared to estimate the impacts of MWA on employment and benefit receipt. This selection was carried out using Propensity Score Matching.

2.2 Data sources and variables

The evaluation was carried out using administrative data derived from two main sources:

1) *DWP administrative databases*, which provide details of spells on DWP benefits, characteristics of DWP customers (drawn from Jobcentre Plus' Labour Market System which relies on inputs from advisers), sanctions and spells on employment programmes, including referrals to MWA and the Work Programme; and

2) *Her Majesty's Revenue and Customs (HMRC) Tax System*, which provides details of spells in employment.

[7] The pseudo referrals were generated using the same methodology employed by Ainsworth and Marlow (2011) in their assessment of the European Social Fund. The method outlined in Appendix 3 aligns the non-referrals and referrals to two time dimensions: calendar time and length of time on benefit so that the distribution of monthly MWA referrals mirrors the distribution of monthly pseudo referrals.

[8] A sensitivity analysis was performed by also excluding individuals who were referred to MWA during the 21 weeks of tracking after the end of the cohort on the 31st July, but this made very little difference to the results.

As in most evaluations it would have also been desirable to draw on data on educational attainment. However data on qualifications is not routinely collected by Jobcentre Plus advisers.

It is widely recognised that there are both advantages and disadvantages to using administrative data compared with, for example, survey data. We outline below some of the broad differences between these two methods:

- administrative data allows for a much larger sample size (close to the population) than survey data;

- survey data tends to suffer from non-response;

- administrative data can also suffer from omissions and errors – notably, there are substantial flaws in the HMRC employment data, as set out in section 2.2.2 below;

- administrative data allows variables and outcomes to be tracked over a longer period than survey data, which generally offers only a snapshot in time; however

- administrative data is limited to a pre-defined set of variables, while survey data can provide a richer data set tailored to a specific research question.

While survey data could provide additional variables with which to control for referral characteristics (as found by, for example, Dolton and Smith, 2011), the present study uses purely administrative data for the following reasons:

- the larger sample size allows us to explore the sensitivity and heterogeneity of the estimated impacts with regard to using different referral and non-referral groups (see Section 4.2); and

- the costs and time involved in undertaking fieldwork to collect survey data are high. Administrative data is readily available on DWP systems.

2.2.1 Description of Variables

Table 2.1 outlines the variables used for controlling selection to referral to MWA. The methodology is described in Section 3. Appendix 5 outlines the method used for constructing the benefit and employment history variables and explains the advantage of the approach over alternative methods.

Table 2.1: Variables and values used for controlling for selection

Variable	Type	Values
Gender	Categorical	*Male; Female.*
Age	Numerical - continuous	*18-59 years old.*
Disability[9]	Categorical	*Not disabled; Disabled; Unknown.*
Ethnicity	Categorical	*White; Black; Asian; Mixed; Chinese; Other; Unknown.*
Sought occupation	Categorical	26 broad categories: e.g. *"Administrative"; "Health Professionals"; "Sales Occupations".*
Marital	Categorical	*Single; Married; Widowed; Divorced; Separated; Cohabiting; Unknown.*
Lone Parent[10]	Categorical	*Lone Parent; Not a Lone Parent* (at any time within 2 years prior to start date/pseudo referral date).
Jobcentre Plus District	Categorical	48 Jobcentre Plus Districts in Great Britain and *Unknown.*
Low Qualified	Categorical	*No; Yes; Unknown.*
Local Authority labour market[11] characteristics: - Employment rate; - Unemployment rate; - Economic inactivity rate; - Average pay; - Job density; - Vacancy density;	Numerical - continuous	Employment, unemployment and economic inactivity rates range between 0 and 1. Average pay, job density and vacancy density can take any positive value.
Benefit[12] history	Categorical	104 binary variables – representing each of the 104 weeks prior to MWA referral date/pseudo referral date. Values are: *in receipt of benefit; not in receipt of benefit.*
Number of benefit claims	Numerical - integer	Number of benefit claims made in the previous 104 weeks.
Employment history	Categorical	104 binary variables – representing each of the 104 weeks prior to MWA referral date/pseudo referral date. Values are: *in work; not in work.*

[9] Since disability is set by a Jobcentre Plus adviser based on claimant self -disclosure, this variable is not a systematic identification of disability as defined by the Disability Discrimination Act (DDA).
[10] 'Lone parent' defined by marital status information to infer partner status and HMRC Child Benefit data to infer parental status.
[11] Source - Labour Force Survey: rates, averages and densities are calculated by dividing by the working age population.
[12] 'Benefit' is defined as any of four out of work benefits (Jobseeker's Allowance, Incapacity Benefit, Employment and Support Allowance, Income Support) or training allowance. Other benefits are not included in the benefit history variables or outcomes.

MWA referral/pseudo referral month	Categorical	Months from May 2011 up to and including July 2011 are given distinct values.
Benefit start month[13]	Categorical	All months up to May 2011 are given distinct values for the benefit spell prior to MWA referral /pseudo referral.
Other programme participation. (See Appendix 2 for a full list of programmes included.)	Numerical - integer/Binary	Number of days spent on each DWP programme in two years prior to MWA referral/pseudo referral. Programmes for which a reliable end date is not held are binary coded to reflect a start on the other programme or no start.
Sanction and Disallowance history	Numerical - integer	Number and type of sanctions/disentitlements in two years prior to MWA referral/pseudo referral. Three types: *"Fixed", "Variable", "Disentitlement"*.

Table 2.2 shows the additional variables that we monitor following the referral/pseudo referral date.

Table 2.2: Variables monitored following the referral/pseudo referral date

Variable	**Type**	**Values**
Benefit[14] outcomes	Categorical	21 binary variables – representing each of the 21 weeks following the MWA referral date/pseudo referral date. Values are: *in receipt of benefit; not in receipt of benefit.*
Employment outcomes	Categorical	21 binary variables – representing each of the 21 weeks following the MWA referral date/pseudo referral date. Values are: *in work; not in work.*
Number of benefit claims	Numerical - Integer	Number of benefit claims in the 21 weeks following referral.
Other programme participation. (See Appendix 4 for a full list of programmes included.)	Numerical – continuous/Binary	Number of days spent on each DWP programme in the 21 weeks following the MWA referral/pseudo referral. Programmes for which a reliable end date is not held are binary coded to reflect a start on the other programme or no start.
Sanctions and Disallowances	Numerical	Number and type of sanctions/disentitlements in the 30 weeks following the MWA referral/pseudo referral. 4 types: *"Fixed", "Variable", "Disentitlement"*, *"MWA sanction".*

[13] Benefit start and end dates refer to the benefit spell leading up to the MWA referral.
[14] 'Benefit' is defined as any of four out of work benefits (Jobseeker's Allowance, Incapacity Benefit, Employment and Support Allowance, Income Support) or training allowance. Other benefits are not included in the benefit history variables or outcomes.

2.2.2 Data quality issues

Employment data

The employment data used in this analysis comes from Her Majesty's Revenue and Customs (HMRC) data systems. In most circumstances, employers are obliged to notify HMRC when an employee starts or ends a spell of employment. Employment history and outcomes of individuals were derived using the recorded start and end dates of these notified employment spells. However, there are a number of documented issues with the quality of this data.[15] These are briefly described below:

1. Employment spells are only recorded when a tax form is submitted. Some employment spells, such as those corresponding to self employment and individuals not earning higher than the PAYE threshold, are therefore not recorded;

2. If HMRC do not know the date on which an employment spell started, they assign a start date of the 6th April in the year that they become aware of the employment spell. This may not be the actual year in which the spell began. A similar process occurs when HMRC do not know the date on which an employment spell ended. In this case they assign an end date of the 5th April; and

3. A small number of records contain other known errors, such as missing start dates or missing end dates.

Table 2.3 shows the proportion of employment spells with a potential issue recorded for non-referrals and MWA referrals. It shows that a high proportion of spells had 5th April end dates and 6th April start dates. Also there were more MWA referral employment spells with 5th April end dates and 6th April start dates than non-referrals.

Table 2.3: Proportion of Employment Spells with a potential issue

	Non-referrals	MWA referrals
5th April end date	17.0%	19.0%
Start after end date	0.9%	0.8%
6th April start date	20.6%	23.2%
0/1 day claim	3.7%	4.1%
Missing start date	0.1%	0.0%
Total Employment spells	**1,303,180**	**4,060**

As in other evaluation studies, such as Beale et al. (2008) we have followed advice to mitigate the problem of all dates with errors, by randomly assigning start and end dates within the assigned tax year for records in which they are unknown.

[15] http://research.dwp.gov.uk/asd/asd5/rports2007-2008/rrep432.pdf

We acknowledge that our estimates of the impact of MWA on employment rely on imperfect data. However, they still provide an important estimate of how effectively MWA impacts on the future employment prospects of referrals, if we assume that there is no systematic bias between recording of employment for referrals and non-referrals. Of course this assumption may not hold if, for example, individuals who are referred to MWA placements are more likely to go into low-paid part time jobs so that employers on average are less likely to record employment spells. This could drive the employment impacts downwards because instances of employment are less frequently recorded for those individuals amongst referrals than non-referrals. On the other hand it is possible that referrals are more likely to be employed by larger employers which are more likely to record employment spells, thus driving the employment impacts upwards. At this time we do not have information about the type of employer that employs individuals who have been referred to MWA.

DWP administrative data

On the whole, we believe that the recording of out of work benefit and training allowance spells to be reasonably accurate. However, we should note that with the exception of JSA spells, there is some inaccuracy in some of the imputed start and end dates of spells and that some very short spells are omitted. Nonetheless we believe the estimates for the impact of MWA on benefit receipt to be more reliable than employment impacts.

We also believe that the sanctions database provides an accurate measure of when an individual receives a sanction. Although the sanction decision date is well populated, as described in Appendix 2, significant proportions of sanctions have missing start dates and end dates, making it difficult to estimate duration of any particular sanction.

Characteristics data from the Jobcentre Plus Labour Market System contains a number of missing values because advisers do not routinely fill in all of the fields during client interviews or client does not disclose the information. This is particularly the case for variables identifying ethnicity, disability and low qualified. In the case of variables with missing values, ‘unknown’ is treated as a valid category for controlling for referral characteristics.

There are no statistically significant differences in the proportions of missing or unknown values for the referral and non-referral samples, with the *marital* variable reporting 1% and *occupational choice* reporting 2% levels of missing data in both groups. The proportion of missing data for ethnicity is small (5% in the treatment and 6% in the comparison group), however the proportion of missing data with the ‘low qualified’ marker is high (72% in the treatment and 73% in the comparison group). All other variables report less than 1% missing or unknown values.

2.3 Comparing referrals and (unmatched) non-referrals

This section compares the basic characteristics of individuals in our main MWA referral and (unmatched) non-referral samples.

Table 2.2 lists summary statistics detailing personal and demographic characteristics, benefit receipt, sanction records, and participation on DWP employment programmes other than MWA. The table includes only a number of summary characteristics – for a full list of variables included in the analysis, refer back to Table 2.1.

In terms of demographics, referrals were slightly different to non-referrals. Referrals were younger (mean age was 32 years old) than non referrals (mean age was 35 years old) and there was a higher proportion of men amongst referrals (77%) compared to non-referrals (68%). Also there was a higher proportion of ethnic minorities amongst referrals (30%) than non-referrals (23%). In addition, there was a much higher proportion of single people amongst referrals (82%) compared to non-referrals (69%).

Although durations on JSA were similar, referrals spent more of the previous two years on benefits/JSA (75/66 weeks) than non-referrals (64/55 weeks) and less time in employment (21 weeks compared to 37 weeks). The fact that there is minimal difference between the mean number of weeks spent on benefits and JSA between referrals and non-referrals indicates that individuals' time on other benefits (ESA/IB/IS) is similar across groups. Also referrals were likely to have had more benefit spells in the last two years than non-referrals.

Furthermore, referrals were almost three times more likely to receive a sanction in the two years prior to the referral and after the referral compared to non-referrals. Referrals spent more of the past two years on other DWP programmes (30 weeks) than the non-referral group (25 weeks). Also a higher proportion of those in the referral group had a Work Programme referral following a MWA referral (32%) compared to the non-referral group (25%).

Overall before controlling for differences, these statistics suggest that MWA referrals were less engaged with the labour market than non-referrals.

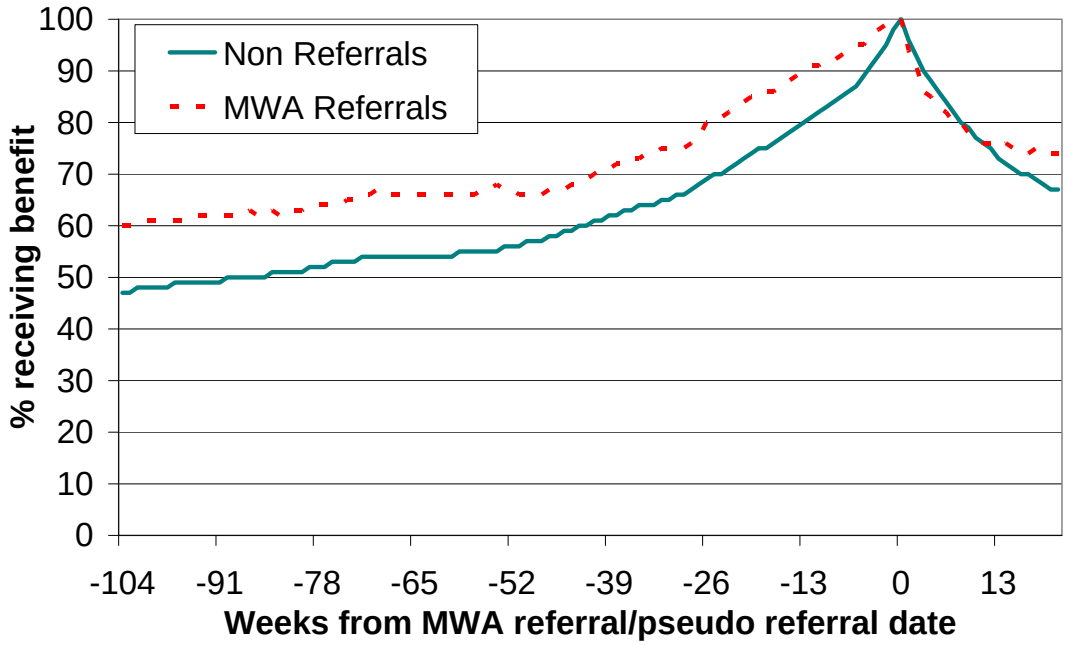
Table 2.4: Characteristics of the referral and (unmatched) non-referral samples

	MWA referrals	Non referrals
Observations	3,190	611,230
	Personal/ Demographic Characteristics	
Age (mean years)	32*	35*
Male(%)	77*	68*
Disabled(%)	24*	23*
Ethnic Minority (%)	30*	23*
Low Qualified (%)	19*	21*
Lone Parent (%)	5*	8*
Average Local Authority Unemployment Level (%)	9	9
Single (%)	82*	69*
Married (%)	6*	12*
	Benefit Receipt	
Weeks of past two years on benefit (mean weeks)	75*	64*
Weeks of past six months on benefit (mean weeks)	23*	21*
Weeks of past year two years spent receiving JSA (mean weeks)	66*	55*
Weeks of past two years spent in employment (mean weeks)	21*	37*
JSA duration at point of referral (mean weeks)	35	36
JSA duration more than six months (%)	47*	49*
Benefit spells over past two years (mean)	3.4*	2.8*
	DWP Programme Participation	
Weeks of the past two years spent on other DWP programmes (mean weeks)	30*	25*
Work Programme referral in 21 weeks since MWA referral(%)	32*	26*
	Benefit Sanctions	
Sanctions received in the two years prior to MWA referral (mean)	1.04*	0.42*
Fixed sanctions received in the two years prior to MWA referral (mean)	0.55*	0.23*
Variable sanctions received in the two years prior to MWA referral (mean)	0.11*	0.05*
Disallowances received in the two years prior to MWA referral (mean)	0.36*	0.14*
Sanctions and disentitlements received in the 30 weeks after referral (mean)	0.53*	0.17*

* Significant difference at 95% confidence interval between referrals and non-referrals

Figure 2.1 compares the benefit[16] receipt rates of the MWA referrals and non referrals each week before and after the MWA referral/pseudo referral date. The benefit rates shown are for the complete sample in each case, i.e. before any attempt is made to select groups of referrals and non-referrals with similar characteristics. Differences between the referrals and non-referrals are therefore not attributable to impacts of MWA, but rather a combination of MWA impacts and differences in the characteristics of those who were referred and those who were not**.**

Figure 2.1: Benefit receipt rate among referrals and (unmatched) non-referrals



Two years before the MWA referral/pseudo referral date, the proportion on benefit was lower amongst the non-referral group (47%) compared with the referral group (60%). This is the case right through to the MWA referral/pseudo referral date. These observations confirm that the MWA referral group tended to be further away from the labour market than those in the non-referral group.

Over the first 10 weeks following the referral/pseudo referral date, the proportion of referrals receiving benefits was lower than the proportion of non-referrals. Between 10 and 21 weeks after the referral/pseudo referral date, the proportion of the referral group in receipt of benefit decreased from 77% to 74% while the non-referral group experienced a sharper decline of benefit receipt between week 10 and 21, from 77% to 67%.

[16] 'Benefit' is defined as any of four out of work benefits (JSA, IB, ESA, IS) or training allowance.

The rest of this section compares the characteristics of the cohort of 1,710 (54%) referrals that started, the 1,480 (46%) MWA referrals that did not start (non-starters), and the (unmatched) non-referral sample.

Table 2.5: Characteristics of the referral and (unmatched) non-referrals samples for starts and non-starters

	MWA referrals (Starts)	MWA referrals (Non Starts)	Non referrals
Observations	1,710	1,480	611,230
	Personal/ Demographic Characteristics		
Age (mean years)	33*	31*	35
Male(%)	74*	80*	68
Disabled(%)	26*	22*	23
Ethnic Minority (%)	26*	34*	23
Low Qualified (%)	21*	17*	21
Lone Parent (%)	5	5	8
Average Local Authority Unemployment Level (%)	8	9	9
Single (%)	84	82	69
Married (%)	6	6	12
	Benefit Receipt		
Weeks of past two years on benefit (mean weeks)	76*	73*	64
Weeks of past six months on benefit (mean weeks)	24	23	21
Weeks of past year two years spent receiving JSA (mean weeks)	64*	66*	55
Weeks of past two years spent in employment (mean weeks)	22	21	37
JSA duration at point of referral (mean weeks)	34	35	36
JSA duration more than six months (%)	44*	47*	49
Benefit spells over past two years (mean)	3.1*	3.8*	2.8
	DWP Programme Participation		
Weeks of the past two years spent on other DWP programmes (mean weeks)	26*	30*	25
Work Programme referral in 21 weeks since MWA referral(%)	40*	23*	26
	Benefit Sanctions		
Sanctions received in the two years prior to MWA referral (mean)	0.86*	1.25*	0.42
Fixed sanctions received in the two years prior to MWA referral (mean)	0.44*	0.69*	0.23
Variable sanctions received in the two years prior to MWA referral (mean)	0.13*	0.1*	0.05
Disallowances received in the two years prior to MWA referral (mean)	0.29*	0.44*	0.14
Sanctions and disentitlements received in the 30 weeks after referral (mean)	0.36*	0.73*	0.17

* Significant difference at 95% confidence interval between referrals and non-referrals

In terms of demographics, MWA starts and non-starters were slightly different. Starts were slightly older (mean age was 33 years old) than non-starters

(mean age was 31 years old) and there was a lower proportion of men amongst starts (74%) compared to non-starters (80%). Also there was a lower proportion of ethnic minorities amongst starts (26%) compared to non-starters (34%), as well as a slightly higher proportion of starts who were single (84%) compared to non-starters (82%).

Although starts spent slightly more time on benefit (76 weeks) than non-starters (73 weeks) they spent slightly less time on JSA (64 weeks) than non-starters (66 weeks), which means that starts spent more time on other benefits than non-starters. Starts had fewer benefit claims in the previous two years than non-starters but a similar amount of time in employment.

Although non-starters spent less time on benefit, surprisingly, non-starters were much more likely (45%) than starts to receive a sanction in the two years prior to the referral. Equally, non-starters were 50% more likely than starts to receive a sanction during the weeks following referral.

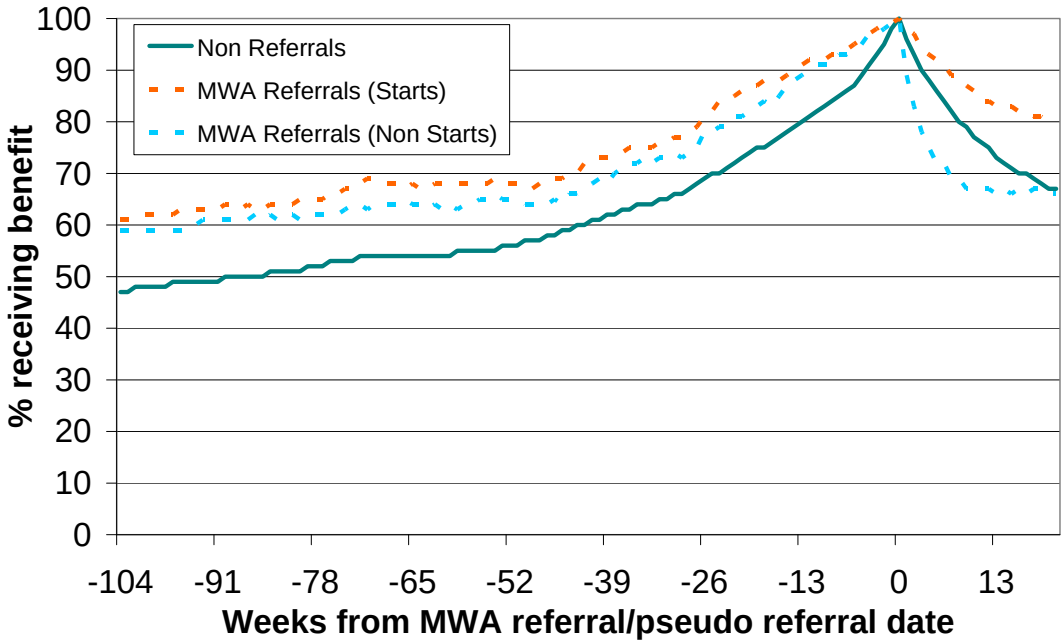
Also, non-starters spent more of the past two years on other DWP programmes (30 weeks) than starts (26 weeks); also a higher proportion of starts had a Work Programme referral following a MWA referral (40%) compared to non-starters (23%).

Figure 2.2 compares the benefit receipt rates of MWA starters, non-starters and non referrals each week before and after the MWA referral/pseudo referral date.

Two years before the MWA referral/pseudo referral date, the proportion on benefit was slightly lower for non-starters (59%) compared to starts (61%). This is the case right through to the MWA referral/pseudo referral date. Overall, these observations suggest that MWA starts tended to be slightly further away from the labour market than non-starters.

Over the first 10 weeks following the referral/pseudo referral date, there was a rapid decline in the proportion of non-starters receiving benefits compared to starters; although we have not controlled for characteristics this suggests that a deterrent effect was present. Between 10 and 21 weeks after the start of the MWA placement, the proportion of non-starters in receipt of benefit levels at about 67%. This compares to a flatter decline in the proportion receiving benefits to just 80% by week 21 for starts.

Figure 2.2: Benefit receipt rate for MWA referrals (Starts and Non-Starters) and (non-matched) non-referrals



The outcome measures used so far do not consider the effect of sanctions. Figure 2.3 shows the proportion of MWA referrals in receipt of benefit compared to the proportion in payment after adjusting using the methodology detailed in Appendix 3, so that if an individual is in receipt of JSA and a sanction they are considered to be not in payment of JSA. Figure 2.4 shows the same outcome measures for non-referrals. The data indicates that (before controlling for differences) the adjustment for sanctions has a relatively large effect (for some weeks it difference was seven percentage points) for MWA referrals compared to non-referrals for which the effect is never more than one percentage point.

Figure 2.3: Proportion of MWA referrals in receipt of benefit and in payment of benefit

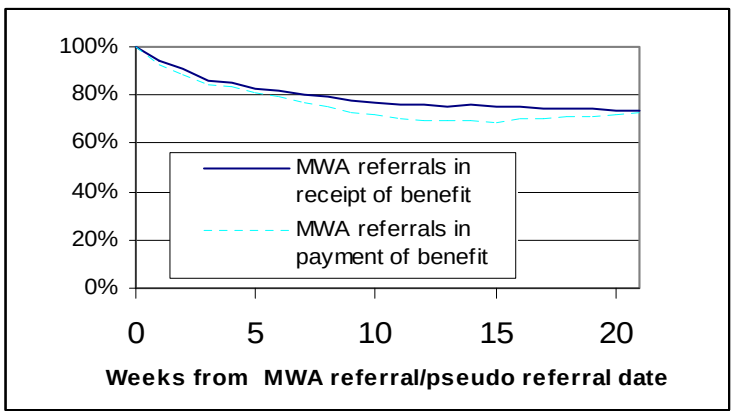
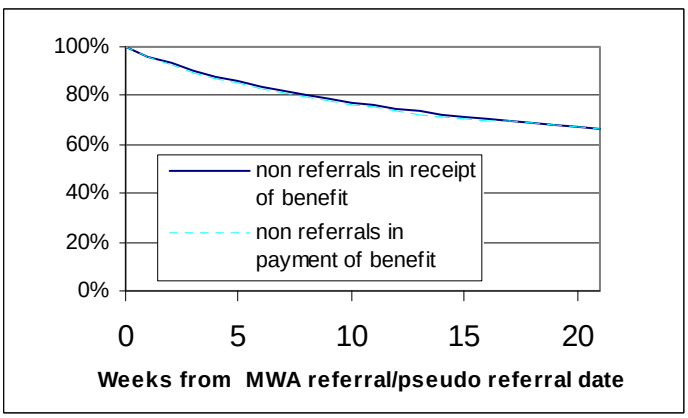


Figure 2.4: Proportion of non-referrals (unmatched) in receipt of benefit and in payment of benefit



2.4 Cohort Analysis of Referrals

This section describes what happened to the May to July cohort of referrals by 21 weeks following the referral date. In addition to showing the proportion of starts and non-starters on and off benefit, Figure 2.3 shows whether individuals were referred for a sanction, referred to the Work Programme or had a new referral/re-referral. Note that for clarity we have shown only the main referral 'pathways'; for example we have not shown on the diagram the small number of individuals who started, were on benefit at 21 weeks and had a new/re-referral.

Of the 3,190 referrals, 54% started and 46% did not start their MWA placement within 21 weeks. Overall starts took on average 17 days from referral to start, with 80% of starts taking less than 3 weeks.

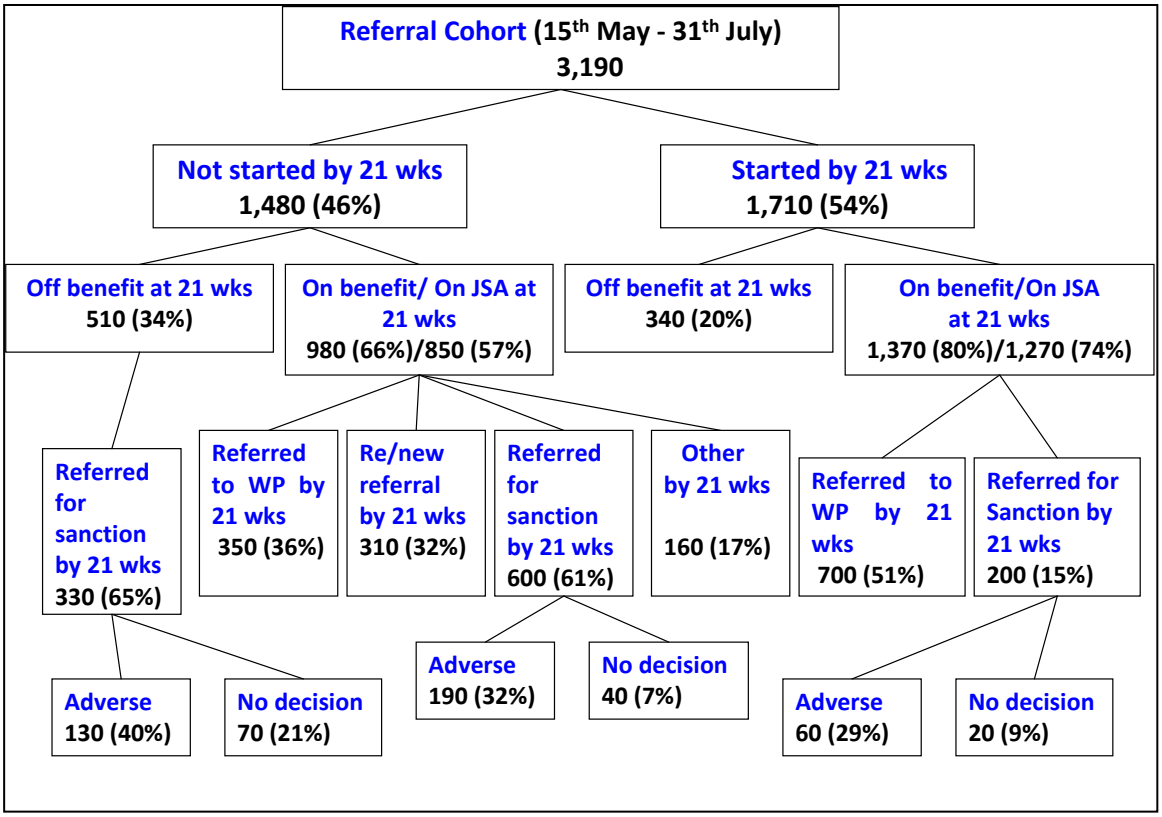
Overall, starters were more likely to be on benefit 21 weeks after referral (20% of starters and 34% of non-starters). Not shown on the diagram is that 64% of non-starters broke their JSA claim and 57% of those returned within 21 weeks compared to 36% of starters breaking their claim with 54% of them returning to benefit.This indicates that non-starters were more likely to finish their original claim and return to benefit within 21 weeks of an MWA referral than starters. It is also worth noting that the proportion of non-starters on benefits other than JSA at week 21 was higher (66%-57%=9%) than for starters (80%-74%=6%), indicating that non-starters were more likely to return to other benefits, such as ESA, than starters.

Many of the individuals who did not start a MWA placement were referred for a sanction. Of the 510 who were off benefit by week 21, 65% were referred and 40% of these received an adverse decision[17]. Of the 980 still on benefit, 600 (61%) were referred for a sanction of which 32% incurred an adverse decision. 36% were referred to the Work Programme, 32% had a new referral/re-referral; there was no evidence of future activity for 17% of this group. Note that the sum of these figures do not add up to 100% because some individuals were referred to more than one activity – in particular 21% of non-starters were referred for a sanction and the Work Programme within the 21 week period.

Of the 1,370 who started and were on benefit, 15% were referred for a sanction with nearly 30% of these incurring an adverse decision and a high proportion (51%) were referred to the Work Programme within 21 weeks. Overall, of those who got referred to the Work Programme, a third were referred to the Work Programme 9 weeks following their referral – the average length of time between MWA referral and Work Progamme referral was about 12 weeks.

[17] 21% received no decision which means that the decision was either cancelled or reconsidered

Figure 2.5: 'Snap Shot' at 21 weeks following MWA referral[18]



The next section describes the methodology used to select the comparison group of non-referrals who are similar to MWA referrals with regard to demographic characteristics, labour market history, sanction history and previous programme participation. This enables us to attempt to isolate the effect of the MWA referral on the labour market prospects of individuals.

[18] Figures are rounded to the nearest 10 so boxes might not add up. Also there are overlaps between referral to sanction, Work Programme and new/re-referral.

3 Methodology

This section outlines the methodology used to estimate the impacts of MWA on the labour market prospects of referrals. The impact estimates are specifically for the *average effect of treatment on the treated, ATT (*in this case, the 'treatment' is being referred to MWA.)

Section 3.1 describes the Conditional Independence Assumption, which forms the foundation of impact evaluations of this type. Section 3.2 describes the Propensity Score Matching (PSM) methodology used to control for selection bias and construct a suitable counterfactual for the MWA referral group. Finally, Section 3.3 describes the method used to measure the labour market outcomes of referrals and non-referrals in our samples, and of using these outcomes to estimate the net impacts of the MWA programme.

3.1 Conditional Independence Assumption

The aim is to estimate the average effect of being referred onto MWA. We can not use a simple comparison between the benefit and employment outcomes of referrals with those of non-referrals as this could be biased if there are systematic differences between these groups which may be related to the labour market outcomes of interest (see Table 2.2). However, if we can control for all characteristics that influence selection onto the programme, then the outcome that would result in the absence of treatment is the same in both cases (Bryson, et al. 2002). Our identifying assumption is that conditional on the variables we have observed (as defined in Table 2.1), the counterfactual outcome is independent of referral. This is known as the 'Conditional Independence Assumption' (CIA). It enables us to infer the counterfactual outcomes for MWA referrals, and therefore to attribute any differences between carefully matched referral and non-referral groups to the effect of MWA. We control for characteristics using a Propensity Score Matching methodology, as described in Section 3.2.

The remainder of this section discusses how the individual level data (as described in Chapter 2) allows us to try to control for the difference in typical characteristics between those who are referred onto MWA and those who are not referred. Differences in characteristics between these two groups arise as a result of the way referrals are selected by advisers, and are therefore known as *selection bias*.

3.2 Controlling for Selection Bias

This early study is focused mainly on the impacts of referral to MWA. Therefore to understand the differences between MWA referrals and non-referrals we must consider the mechanism by which jobseekers are referred onto MWA placements. This selection is very much determined by the referral made at the discretion of Jobcentre Plus advisers, guided by eligibility criteria and the approval of the Advisory Team Manager. The study also considers the impacts of starts to MWA, so we also consider the mechanism by which referrals participate.

Selection bias at the point of referral

Some emerging findings from qualitative fieldwork[19] provide some helpful insight in how selection for referral might have operated for the cohort of interest. We note that the fieldwork was conducted in March – May 2012, at a later point in the programme, when referrals and starts to placements were at higher levels, compared to the period of our cohort of interest for this study.

Although there was some variation in responses between Districts, interviews with advisers indicated two broad groups of claimants which made up most of the MWA referrals:

1. claimants for whom there was some element of doubt for example, about working and claiming; had a history of not turning-up to previous opportunities; were not engaging with Jobcentre Plus or whose jobsearch activity was deteriorating; and
2. claimants for whom there was a need to re-enforce the message that they needed to be serious about their commitment to engage with jobsearch and for whom the advisers considered work activity would be beneficial i.e. would provide some positive outcomes for the claimant.

In addition there was some evidence that the constraint on the number of places meant that there was some reluctance in some Districts to refer claimants. Although the cohort was from the beginning of the programme, when numbers of referrals and starts on placement were at lower levels than when the fieldwork was conducted, it is likely, that in some locations, this may have contributed in part to the decision to select.

To illustrate the claimant's perspective, a small number of claimants were also interviewed (given the qualitative nature of the research there is a risk the views may not have represented those who did not start). The responses suggested that most claimants understood that the placement was to provide experience of work and that it was mandatory; only a few were not clear why they were referred.

[19] Interviews with advisers, providers, host placements across 5 districts: these early findings will be published later in the year.

The questions did not go into sufficient detail to conclude whether or not the decision to be referred came as a surprise to claimants. It is also unknown whether the discussion prior to referral to explain that a claimant was being considered for referral may have affected the decision to pursue the referral. Nonetheless, it is difficult to envisage that the decision to refer is not unaffected by the claimant-adviser relationship leading up to the point of referral.

The final decision on a claimant's suitability for MWA is made in discussion with the Advisory Team Manager to discuss the individual customer and their circumstances and identify ways forward. The qualitative evidence suggested that the Advisory Team Managers agreed with the adviser decisions.

In summary, the early qualitative evidence suggests that advisers appear to be thinking carefully about whether to refer an individual to MWA. They are making what might be a subtle distinction between similar looking individuals and their motivations to work etc (and thinking about how to ration the places), and this decision will depend on a number of subjective views gained from the claimant- adviser relationship. Finally we note, it is difficult to assess the size of the potential pool of jobseekers that the adviser might have considered suitable for referral.

To allow us to control for the selection bias which results from this process of referral, we have brought together a wide range of variables which we hope will proxy the adviser's decision to refer, that might influence potential outcomes. The criteria to prioritise referrals focused on selection of claimants for whom there was need to re-engage with the labour market. Therefore we constructed a number of variables detailing a claimant's benefit and employment history to attempt to capture this lack of work focus. We also included the claimant's sanction record to capture a claimant's history of engaging with Jobcentre Plus and job search.

In addition we have collated demographic characteristics such as age, gender, ethnic group, marital, disability, qualification and lone parent status (Section 2.3) for the referral and non-referral samples. We have also obtained each individual's stated preferred choice of occupation. Since labour market prospects may be highly dependent on dynamic local labour market characteristics and the local availability of employment support, we have also collected each individual's geographical district and the labour market characteristics of the Local Authority where each individual lives.

It is likely that the referrals were claimants who had a record of participating on a number of DWP employment programmes (even if they did not fully engage with the programme). In principle we could control for this by removing those people who were on other DWP programme before their referral. However, this would bias the sample by deliberately removing a particular type of claimant because the remaining sub sample may no longer represent those on MWA. Therefore, measures of previous participation on other DWP programmes have also been used as matching variables. In the same light, it is not evident that we should remove claimants who have been

on other programmes after MWA, because it is possible that we might omit claimants who are more likely not to have left benefits or found work, thus affecting the size of the impact.

In summary then, we try to construct the counterfactual as accurately as we can, using the observable characteristics for which we do have data. There are likely to be many other unobserved variables, which to varying extents will play a role in the referral decision. However, the value of having a rich data set is that, some of the variables which we have observed will indirectly capture the influence of variables we have not observed. For example, while we have not been able to observe an individual's record for turning up to meetings, their qualifications, personality type, life experience, experience of discrimination, confidence, health, language skills or happiness, we hope that by controlling for gender, age, ethnicity, disability, local labour market characteristics, labour market history, prior programme participation etc. we hope the model will capture virtually all of their influence by proxy.

Nonetheless, there may well be factors that influence whether or not an individual is referred to MWA for which we cannot control. In particular, taking the qualitative evidence and policy design together, the greatest risk is that our variables will have underestimated the extent of lack of claimant focus; this in turn will lead to underestimating the counterfactual's detachment from the labour market and therefore an underestimation of the measured impact.

Selection bias at the point of starting

Additional analysis for this study was to estimate the impacts of referral separately for claimants who started on the programme, and for those who were referred but did not start. In this way, we get some insight into whether any impacts reflect the actual experience of undertaking a placement, or other types of effect including a deterrent effect. The analysis in section 1.2 indicated that just over half of claimants referred to provision start a placement. We believe that controlling for the selection bias which results from the decision to participate compared to claimants who have not been referred will be mainly driven by the initial selection at referral and then by decision to participate. The following paragraphs indicate other factors which drive participation, conditional on referral.

The decision not to participate could be for a variety of reasons: they may cease to claim JSA between referral and start because for example, if they found employment; or they maybe unwilling to participate but continue to claim JSA (in which case the MWA provider would make a referral for possible sanction action); or their circumstances may change so that other provision may be more appropriate.

Where a claimant chooses to end their JSA claim, rather than participate in MWA, it is clearly possible that these individuals are seeking to avoid participation, and the programme could be seen as having a deterrent effect. However, the possibility that the referral had a motivational effect on a claimant's jobsearch activity should also be considered; factors such as the

perceived 'usefulness' of the programme in increasing a claimant's chances of employment and the relevance of the placement to their job goals may impact on a claimant's decision not to comply with the requirements of the programme. However, given the nature of the target group and the mandatory nature of the programme, these factors are less likely and certainly not as significant as those in say voluntary work experience programmes.

Qualitative evidence was based on a small number of customer interviews, so it may not represent views, particularly of those who did not start, sufficiently. It suggested three main motivations for participation:

- positive - to gain experience of the workplace;
- less positive – risk of incurring a sanction;
- no individual was prepared to stop claiming to avoid MWA (particularly if there was a risk to Housing Benefit).

MWA Prime Providers are obliged to accept and arrange placements for claimants who are referred to the programme, so they do not exercise a choice over which claimants to accept. However, in certain cases it is possible placement hosts request an interview with a claimant before accepting them onto a placement. This may mean that for certain claimants, providers may have a smaller range of placement options, which may in turn impact on the attractiveness of the placement to the claimant.

The policy design allows employers choose to be involved in the selection process. In the small number of cases this probably affects referrals are likely to be chosen who the employer believes will benefit most from the experience and will be of most benefit to the employer organisation. In either case, it is only natural that they are likely to choose claimants, contrary to the referral criteria, who appear most motivated and keen to take up a MWA placement.

In summary, a combination of evidence and our own conjecture suggests that individuals who start are more likely to be motivated and compliant to engaging with Jobcentre Plus than an average referral, although it is difficult to quantify the difference. However, given that the selection is driven in the first place by selection by the advisers we believe that the selection of starts will closely resemble the decision to refer. Therefore, we conjecture that, overall, the variables used for matching which we hope leads to a credible counterfactual for the referral sample, will also not be far off the counterfactual for claimants that start.

3.3 Propensity Score Matching

The aim of the Propensity Score Matching process is to construct a comparison group of individuals who were not referred to the MWA programme, but who in aggregate have identical characteristics to those who were referred, in those characteristics which influence selection and outcomes. If this is successfully achieved, we can then use the labour market outcomes of non-referrals in the comparison group as an approximation for

the counterfactual, i.e. what the labour market outcomes of referrals in our treatment group would have been if they had not been referred.

When there are a large number of observed characteristics, as is the case in the present evaluation, direct matching on all characteristics becomes a limited device as the number of dimensions relative to the number of observations increases (Rosenbaum and Rubin, 1983)[20]. Therefore, we follow the literature in using a single balancing score on which to match, which is a function of all the observed variables. The balancing score used is a propensity score, which is the probability of an individual being referred to the programme given all of their observed characteristics.

Below is a summary of the Propensity Score Matching (PSM) protocol used in this evaluation to construct suitable treatment and comparison groups from the referral and non-referrals samples (Appendix 4 shows a step-by-step guide to the protocol).

Firstly, the probability of participation (dependent variable) was modelled[21] using the observed individual characteristics of referrals and non-referrals, as independent variables. From this model, the predicted probability of referral - the 'propensity score' - was calculated for each referral and non-referral in the sample. Secondly, a matched comparison group was constructed by matching[22] each referral with the average of all non-referrals with similar propensity scores, giving more weight to those whose score was nearest.

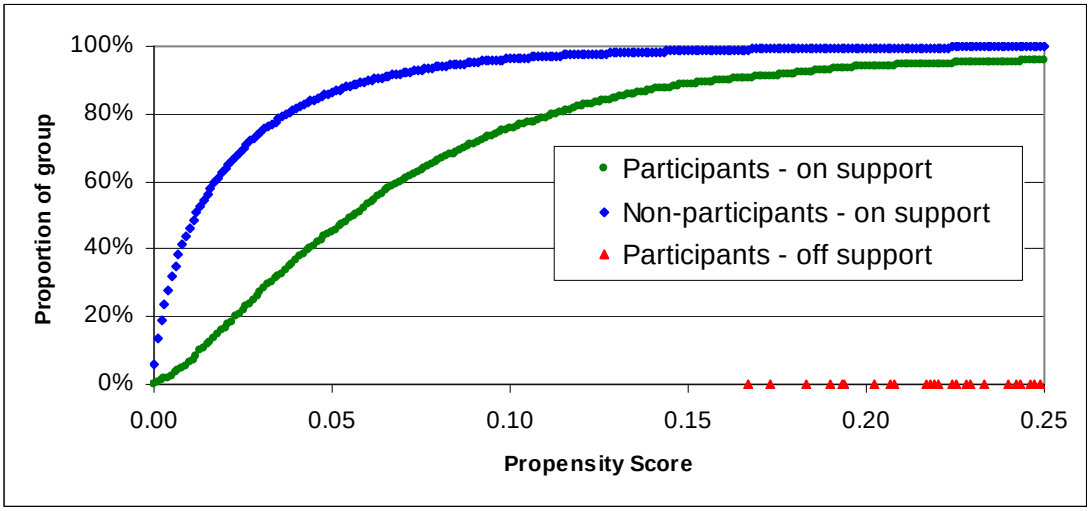
3.3.1 Common Support for Referrals

For Propensity Score Matching to be a successful methodology for estimating the counterfactual, there must be sufficient common support for referrals among the non-referral sample. This means that we must be able to find matching non-referrals for the vast majority of our referrals. This is important as any impact estimates are only valid for those referrals for whom common support is available.

The propensity score distribution for the sample in our main analysis is given below; each point is the cumulative proportion of the group with propensity scores in increments of 0.001.

[20] An accessible explanation of how it can be applied to evaluation of labour market policy can be found in Bryson, et al. 2002.
[21] Using a probit regression
[22] Matching used 'Kernel' matching. For each referral in the sample, all non-referrals with propensity scores within the Kernel bandwidth were selected and weighted using an Epanechnikov distribution. The bandwidth determines how closely the propensity score of a non-referral must be to that of a referral for selection into the final matched comparison group. A bandwidth of 0.0001 was used for our analysis. This bandwidth was shown by Ainsworth and Marlow (2011) to provide a model that retained a high level of common support whilst also ensuring a tight match between non-referrals and referrals. The matching was carried out using an adaptation of the *Stata* code applied by Thomas (2006), which relies on the *Stata* module *psmatch2* written by Leuven and Sianesi (2003).

Figure 3.1: Propensity Score distributions of referrals and non-referrals:
(3,190 referrals; 124,740 non-referrals)



As expected, Figure 3.1 shows that propensity scores tend to be higher among referrals. The overall mean propensity score is 0.025 (this is the proportion of the sample receiving MWA support). 31% of non-referrals have a score of more than the mean propensity score (0.025) compared with 97% of referrals. The small proportion of referrals (<0.1%) for whom no common support is available all have propensity scores between 0.17 and 0.68.

The propensity score distribution provides a very high degree of overlap between referrals and non-referrals, with over 99% of referrals finding common support. We can be confident therefore in generalising from those referrals for whom there was a good match to the entire cohort.

3.3.2 Matching Quality

We found the propensity score model to be highly effective in constructing treatment and comparison groups that are well balanced on the observed characteristics. Table 3.1 shows specification statistics for the matching of the referral and non-referral groups. The chi-squared test shows that prior to the match, there was approximately zero probability that the referral and non-referral samples had the same set of characteristics. After matching there is statistically no difference between the matched groups in terms of observed variables at the 5% level.

Table 3.1: Specification statistics for the group matching

Sample	Pseudo R-sq	LR chi-sq	p > chi-sq
Unmatched	0.15	4,600	0.00
Matched	0.01	54	1.00

Table 3.2 below shows the unmatched and matched means of a range of variables for the referral and non-referral groups. The variables in this table

are non-exhaustive and are provided to illustrate the extent to which the PSM methodology selects non-referrals with similar characteristics across a range of variables. A full list is not provided here as there are over 350 individual variables included in the propensity score model. The table illustrates that the matching has been effective in balancing the groups on the listed covariates. For example, that before matching the proportion of referrals who are male is 77%, while the proportion of non-referrals who are male is 68%. After matching, the referral and non-referral groups both comprise equal proportions of males (76%).

Overall the matching appears to have been very effective. The matching results in a reduction in bias for 350 out of the 358 variables used in the propensity score model[23]. Prior to matching, there were significant differences (at the 5% level) between referrals and non-referrals for 281 variables. After matching there were *no longer* significant differences (at the 5% level) between referrals and non-referrals for any of the 358 variables used. Nonetheless, while the matching appears to be of good quality for the observed variables we cannot know if the same is true for the unobserved variables.

[23] These include the following dummy variables: two employment programme variables and six benefit start month variables.

Table 3.2: Unmatched and matched means for main analysis

Variable	Sample	Treatment	Comparison	% bias	% reduction in |bias|	t	p>|t|
Age (mean years)	Unmatched	32	35	-21.7		-11.9	0.0
	Matched	32	33	-0.6	97	-0.2	0.8
Male (%)	Unmatched	77	68	20.7		11.0	0.0
	Matched	76	76	-0.1	100	0.0	1.0
Disabled (%)	Unmatched	24	23	3.8		2.1	0.0
	Matched	24	24	-0.4	90	-0.1	0.9
White ethnicity (%)	Unmatched	70	77	-15.7		-9.1	0.0
	Matched	71	70	1.2	92	0.5	0.6
Asian ethnicity (%)	Unmatched	8	6	7.9		4.7	0.0
	Matched	8	8	0.0	100	0.0	1.0
Single (%)	Unmatched	55	47	16.3		9.1	0.0
	Matched	55	55	0.4	97	0.2	0.9
On JSA, ESA, IB, IS or TA 52 weeks before MWA referral date (%)	Unmatched	67	56	22.7		12.4	0.0
	Matched	66	66	-0.2	99	-0.1	0.9
In work 4 weeks before MWA referral date (%)	Unmatched	17	28	-24.6		-12.8	0.0
	Matched	18	18	0.0	100	0.0	1.0
Number of sanctions in the last two years (mean)	Unmatched	1.0	0.4	45.6		35.6	0.0
	Matched	0.9	0.9	2.0	96	0.8	0.4
Chosen occupations are Elementary trades, plant and storage-related (%)	Unmatched	24	17	16.6		9.9	0.0
	Matched	24	24	0.0	100	0.0	1.0
Local Authority Unemployment Rate (%)	Unmatched	9	9	-2.0		-1.1	0.3
	Matched	9	9	-1.1	45	-0.5	0.7
Days out of the past two years spent on Flexible New Deal (mean)	Unmatched	40	50	-8.5		-4.7	0.0
	Matched	41	42	-0.9	90	-0.4	0.7
Low Qualified (%)	Unmatched	19	21	-4.8		-2.6	0.0
	Matched	20	19	0.5	90	0.2	0.9

Notes:

The % bias is the difference between the sample means in the treatment and comparison groups as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (Rosenbaum and Rubin, 1983).

3.3 Measuring Outcomes and Impacts

To estimate the average effect of MWA on referrals (the *average effect of treatment on the treated, ATT*), we have measured three main sets of outcome data for each referral and non-referral. In each of the 21 weeks following an MWA referral date (pseudo referral date in the case of non-referrals), we measure whether the individual was:

- in receipt of out of work benefits (JSA, IS, IB, ESA) or training allowance;
- in employment; and

- in receipt of payment of JSA, adjusted to take into account of sanctions, or in receipt of other work benefits (IS, IB, ESA) or training allowance.

The first two outcomes are standard and we use DWP administrative data to determine individual benefit spells, and data originally sourced from the HM Revenue and Customs (HMRC) tax system to determine employment spells. Outcomes are identified at weekly intervals following a MWA referral or a pseudo referral date (comparison group). A definitive outcome is assigned where a weekly point in time (7, 14, 21….. days after the programme referral) is identified as being between a benefit spell start and end or an employment spell start and end (or there is a start but no accompanying end recorded). The outcomes measured are not mutually exclusive, so in any given week an individual may appear as both 'in employment' and 'in receipt of benefit'. The outcome period covers an independently calculated period of time for each individual, spanning from the individual's MWA/pseudo referral date to the end of the maximum period of the data available for the cohort (21 weeks later).

The third measure is helpful to our understanding because while an individual is in receipt of a sanction it is still possible technically to be in receipt of JSA but not receiving any payment. Therefore the third measure adjusts the outcome (described in Appendix 2), and thus impact measure, to consider only those who are in payment of JSA; in other words it is excluding from the outcome measure individuals who are still technically on JSA but have had their benefit sanctioned.

By comparing the outcome data of the matched treatment and comparison groups, we are able to estimate the impact of the MWA referral on each outcome over time. When we present our results in Section 4, we therefore refer to the programme impacts on the likelihood of referrals claiming benefit, the likelihood of referrals in payment of benefit after adjusting for sanctions and the likelihood of referrals being in employment.

To calculate the net impacts of the programme on a particular outcome for a given week, we first take the mean outcome value of the treatment group (i.e. the proportion of the group who are receiving benefit or are in employment) and subtract the weighted mean outcome value of the comparison group. Thus a raw net impact measure is the absolute percentage point difference between the treatment and weighted comparison groups for the corresponding outcome.

However, some small differences in labour market history between the groups may exist after matching. Therefore, we use a difference-in-differences approach to adjust our impact measure to further reduce this bias. The estimated impacts are adjusted by the average pre-programme differences in labour market outcomes. However, for this particular analysis, this works out at an average of only -0.5 percentage points across the weekly benefit history variables. For example, if the estimated impact on benefit receipt were found to be -2.5 percentage points, but the average pre-programme difference in benefit receipt were -0.5 percentage points, then the impact estimates would

be adjusted to gain a final difference-in-differences adjusted estimate of +3 percentage points (-0.5 subtracted from -2.5).

The impacts presented in Section 4 are therefore the difference-in-differences adjusted impacts of the programme on each of the outcomes of interest.

4. Results

In this section, estimates of the average effect of the MWA programme on referrals (the *average effect of treatment on the treated, ATT*) are presented.

Section 4.1 presents our main impact estimates for MWA referrals. Section 4.2 examines the impact estimates on starts and non starts. Finally section 4.3 explores the sensitivity and heterogeneity of the estimated impacts by performing the analysis with several different groups of referrals and non-referrals.

4.1 Impacts of Mandatory Work Activity referrals

The main estimates describe the impact of being referred to MWA, for each of the 21 weeks following referral, on a referral's probability of being:

- in receipt of benefit (JSA, IS, ESA, IS or training allowance); and
- in employment; and
- in receipt of payment of benefit (adjusted benefit outcome to exclude claimants whose benefit was sanctioned) or training allowance.

Impact graphs are extended over a 104-week period prior to starting on MWA to illustrate the extent to which the Propensity Score Matching controls for labour market history over the pre-programme period. The impact graphs also show a 95% confidence interval around the central impact estimates[24].

Figure 4.1 shows the comparison of the proportion on benefit between the referrals and matched non-referrals and Figure 4.2 shows the impacts after the "difference in differences" adjustment.

Before describing the impacts we confirm the finding in Section 3.2 that the benefit history of non-referrals in the 104 weeks prior to referral matches very closely to the benefit history of MWA referrals. This gives us confidence that the matching on labour market history has been successful. However, it is important to keep in mind that the closeness of the match is probably more of a reflection of the fact that we have matched on that specifically, so there is no guarantee that we have eliminated selection bias.

[24] Standard errors are calculated using a linear probability model. The standard errors suffer from heteroscedasticity and non-normality. There is some debate in the literature as to the best way to calculate errors without being too computationally intensive. Our method probably gives an overly cautious approach to errors; errors calculated by psmatch2 are up to half the size that we have reported and errors in other evaluations appear to be smaller for similar sample sizes, for example Lechner and Wunsch (2009).

Figure 4.1:Benefit receipt rate for MWA referrals and matched non-referrals

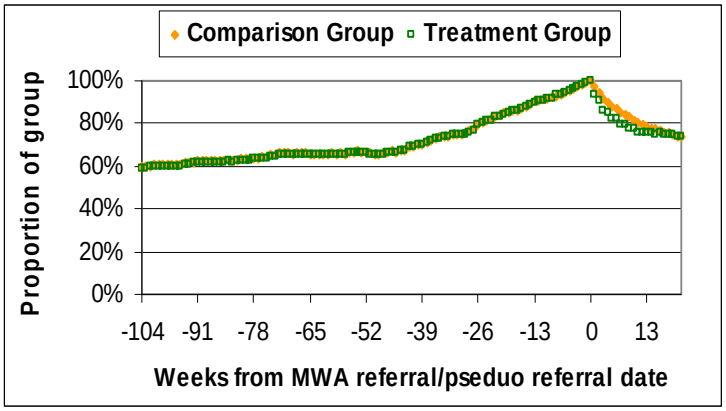
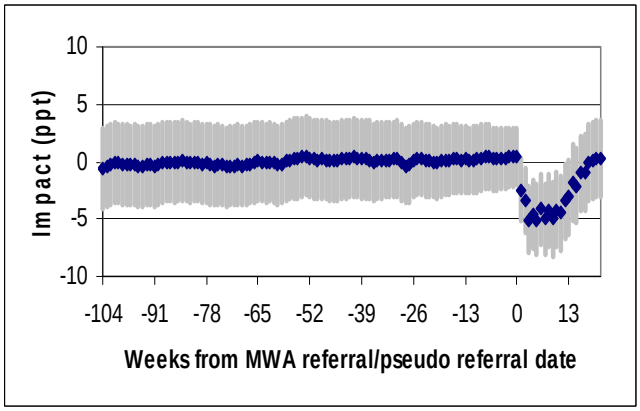


Figure 4.2: Impact of MWA on the likelihood of a referral in receipt of benefit



The results show in the first three months a referral to MWA had a strong and beneficial impact on the likelihood of receiving benefit compared to non-referrals - referrals were much less likely to be on benefit (77% at week 10) than non-referrals (82% at week 10). Therefore, the central impact estimate is about -5 percentage points in weeks 4 to 10. This suggests for this period MWA may have been acting as a deterrent to claiming benefit. However, between 3 and 5 months the impact returns to 0 (benefit receipt is about 74%), indicating that the referrals returned to benefit on average more than the comparison group. Overall, the benefit impact over the first 21 weeks equates to referrals being off benefit for an average of about 4 days more than if they had not been referred.

Figure 4.3 shows the comparison of the proportion on ESA/IB between the referrals and matched non-referrals and Figure 4.4 shows the impacts after the “difference in differences” adjustment. The results show in the first three months a referral to MWA had a negative impact on the likelihood of receiving ESA/IB compared to non-referrals - referrals became more likely to be on benefit than non-referrals; the central estimate is about +3 percentage points by week 21. Therefore, a proportion of the return to benefit observed in Figures 1 and 2 is driven by a number of referrals moving from Jobseeker’s Allowance to Employment and Support Allowance.

Figure 4.3:ESA/IB receipt rate for MWA referrals and matched non-referrals

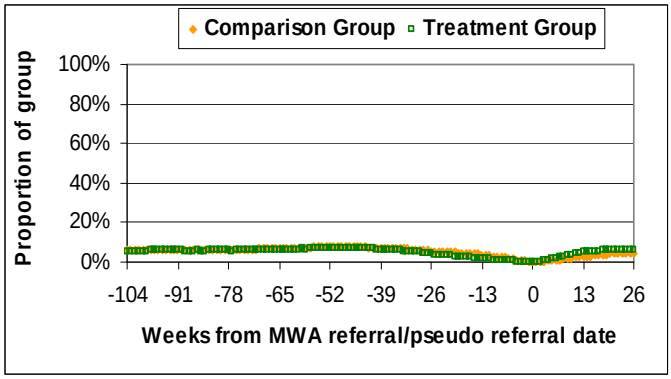


Figure 4.4: Impact of MWA on the likelihood of a referral in receipt of ESA/IB

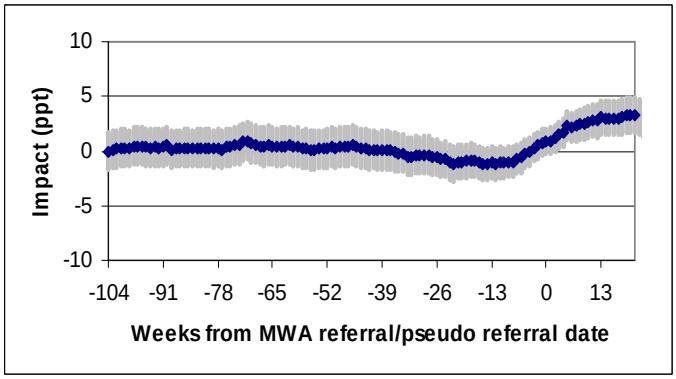


Figure 4.5 shows the comparison for the proportion in employment between the referrals and matched non-referrals and Figure 4.6 shows the impacts after the difference-in-differences adjustment. The results show that, unlike for benefit, a MWA referral had no impact on the likelihood of being employed compared to non-referrals. This suggests that although the benefit impacts suggested a deterrent effect from claiming benefit for the first 10 weeks, individuals were not going into employment, as recorded on HMRC P45 data, more than the comparison group. Therefore it is consistent with the observation that for individuals between 3 and 5 months the impact returns to zero percentage points, indicating that the referrals returned to benefit on average more than the comparison group.

Figure 4.5: Employment rate for MWA referrals and matched non-referrals

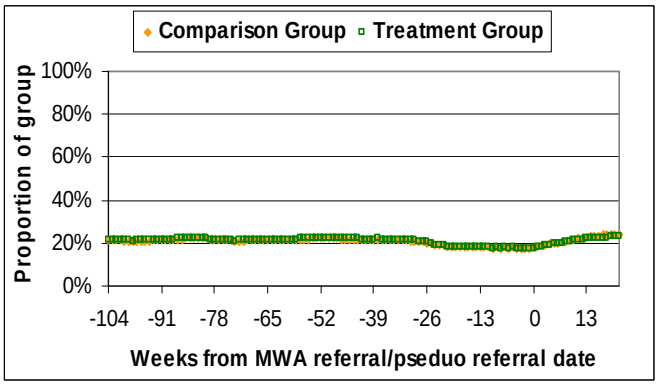
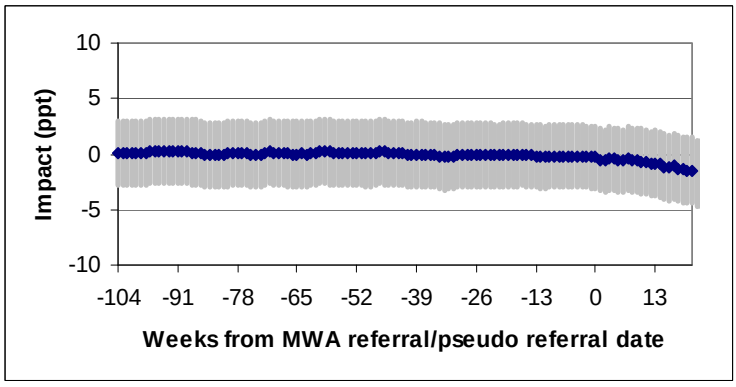


Figure 4.6: Impact of MWA on the likelihood of a referral in employment



Important note on employment impacts
Figure 4.4 highlights imperfections in the P45 HMRC employment data and exposes gaps in our understanding of an individual's status. In Figures 4.1 and 4.5, at 104 weeks before referral, 20% were in employment and 60% were on benefit (there will be some overlap between these two figures). This means that at least 20% (how many more depends on the degree of overlap) who are recorded as neither on benefit nor in employment. Their status is unknown; some might be in education but others will be in employment but are not recorded to be in work at that time. Moreover, in the week of being referred, 18% were employed; we would expect the real employment rate to be close to zero because all referrals were actually claiming benefit in this week. As discussed in Section 2.2.2, we do not believe that the imperfections in the employment data will systematically bias our impact estimates. However, we acknowledge that the employment impact estimates rely on imperfect data and should be treated with a degree of caution.

The main reason that there is a difference between the benefit and employment impacts in the first 10 weeks is to realise that the outcomes and therefore the impacts are not mutually exclusive; table 4.1 illustrates the impacts at weeks 9 and week 21 with regard to the four mutually exclusive labour market groups. It shows that at week 9 referrals were slightly more likely (+3.3 percentage points) to be neither in employment nor on benefit and slightly less likely to be in both employment and in receipt of benefit; this indicates that referrals were more likely to leave benefit for a destination other than employment.

Table 4.1: Impacts (percentage points) on mutually exclusive outcomes

Week	Benefit only	Work Only	Neither	Both
9	-2.6	+1.8	+3.3	-2.4
21	+1.4	-0.4	+0.1	-1.1

Our final impact estimates reflect benefit outcomes that consider only individuals that were in payment of benefit after adjusting for sanctions. Figure 4.7 shows the comparison of the proportion in payment of benefit between the referrals and matched non-referrals and Figure 4.8 shows the impacts after the "difference in differences" adjustment.

Not surprisingly the results accentuate the impacts shown in Figures 4.1 and 4.2. In the first 3 months referrals became much less likely to be in payment of benefit than non-referrals; the central estimate is about -8 percentage points in weeks 4 to 10. However, between 3 and 5 months, the effect of sanctions does not prevent the impact returning back towards zero. Overall, the benefit impact over the first 21 weeks equates to referrals not being in payment of benefit for an average of about 8 days more than if they had not been referred.

Using the unmatched outcomes in Figures 2.3 and 2.4 as an indication, it is reasonable to say that the results have not changed as much as one might have assumed. The main reason for this is that the matching has given more weight to non-referrals with poor sanctions records, which increases the likelihood of the matched non-referral receiving a non-MWA sanction post pseudo-referral.

Figure 4.7:Benefit payment rate for MWA referrals and matched non-referrals

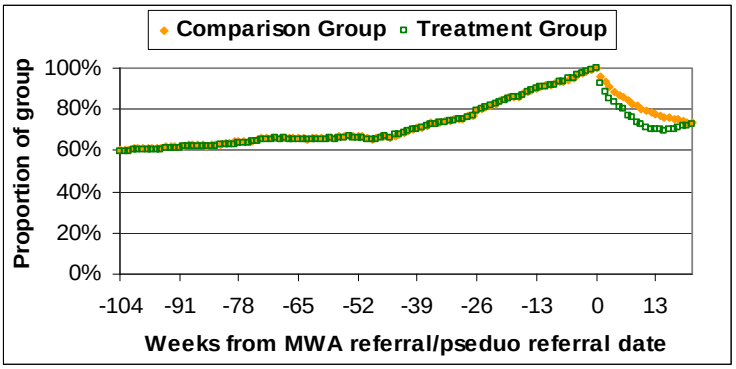
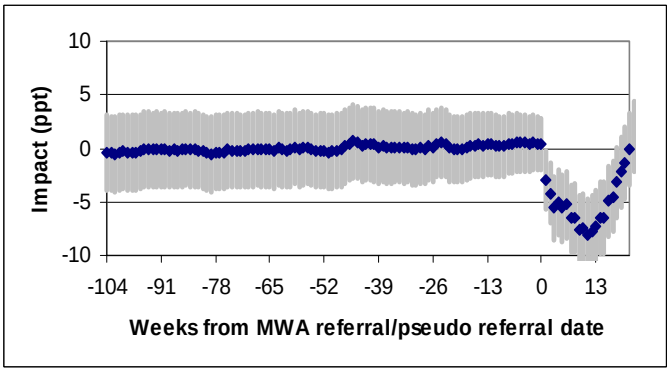


Figure 4.8: Impact of MWA on the likelihood of a referral in payment of benefit



The effects of other support on the impact estimates

The impact estimates do not describe the impact of MWA against a baseline of 'no support', but rather the impact of MWA against a baseline of other Jobcentre Plus support and time on other DWP employment programmes.

The Department does not hold centrally comprehensive data for the amount of Jobcentre Plus support that each claimant receives. Therefore, as with other evaluations of this type, there is no other option but to assume that referrals and non-referrals receive similar levels of standard Jobseeker support of fortnightly signings and contact with Jobcentre Plus advisers.

As outlined in Sections 2 and 3, we have used a number of variables which capture time spent on other DWP programmes *before* the MWA referral or pseudo referral date in our propensity score model. The aim of including these variables was both to balance the groups with regard to the effects that previous programmes may have had on selection.

However, referrals and non-referrals may also have spent time on alternative DWP programmes *after* their MWA start/pseudo referral date. The notable result is that 29% of non-referrals and 32% of referrals went onto be referred to the Work Programme. In addition non-referrals spent an additional 5 days on other DWP programmes in the 21 weeks following their pseudo referral date than referrals spent over the equivalent period.

If we estimate the impacts of MWA against 'no support' we may therefore expect the impacts to be slightly different. Over this short period of time, whether the impacts are weaker or stronger will largely depend on the strength of the 'lock-in' on alternative programmes, in particular the Work

Programme, relative to MWA. However, as it is difficult to disentangle other support from our estimates of MWA impacts, we do not attempt to adjust the impacts for this effect. A fully comprehensive analysis would need to consider the impacts of the Work Programme and other programmes.

4.2 Impacts for Starts to MWA and non-starters

So far the analysis has been focused on the cohort of referrals compared to a group of non-referrals taken from JSA claimants. Here we estimate benefit impacts for starts and non-starters, *but still following their referral date*.

Figures 4.9 and 4.10 show that in the first three months non-starters had a strong and beneficial impact on the likelihood of receiving benefit compared to non-referrals – non-starters became much less likely to be on benefit than non-referrals; the central estimate is about -12 percentage points in weeks 4 to 10. This suggests for this period MWA may have been acting as a deterrent to claiming benefit. However, between 3 and 5 months the impact returns to zero percentage points, indicating that non-starters returned to benefit on average more than the comparison group.

Figures 4.11 and 4.12 show that there are no significant impacts on benefit for starts. Note that given that the confidence interval stretches over 10 percentage points the impact would have to be large to be observed.

Figure 4.9:Benefit receipt rate for MWA non-starters and matched non-referrals

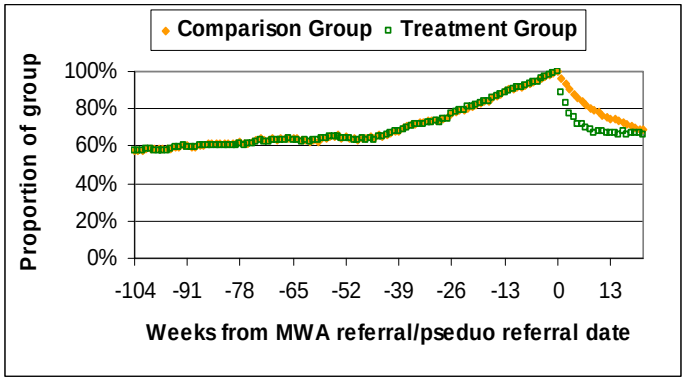


Figure 4.10: Impact of MWA on the likelihood of non-starters in receipt of benefit

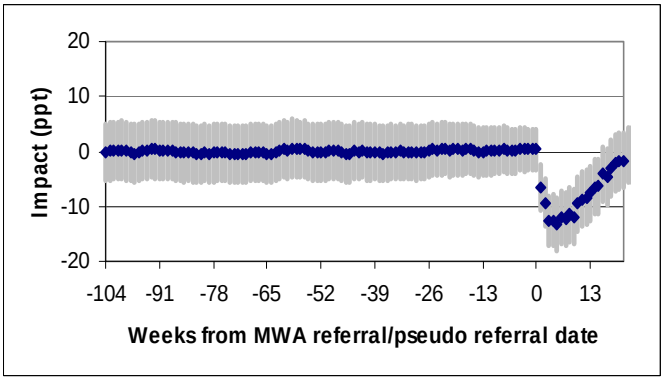


Figure 4.11:Benefit receipt rate for MWA starters and matched non-referrals

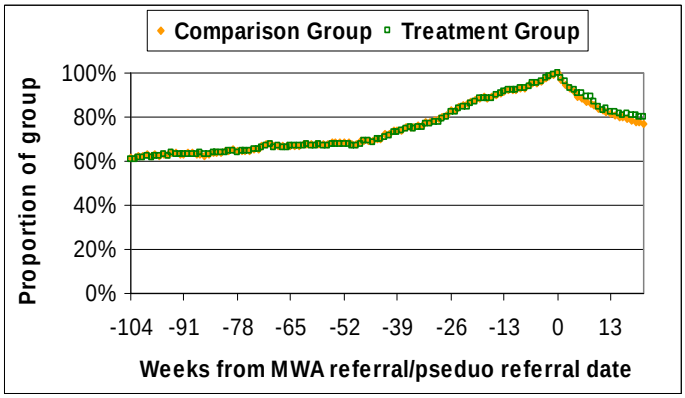
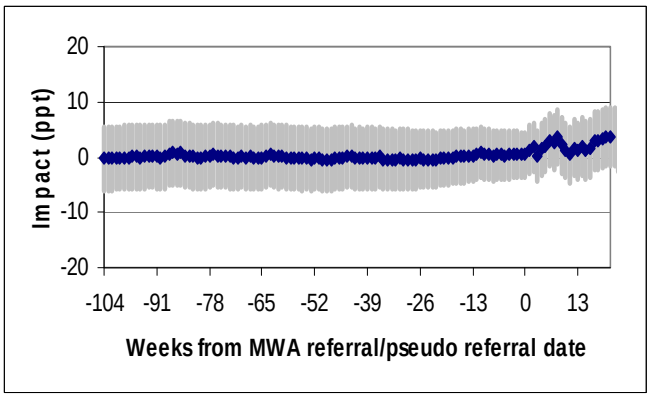


Figure 4.12: Impact of MWA on the likelihood of starters in receipt of benefit



4.3 Sensitivity Analysis

Seven additional analyses are presented in Appendix 7 to explore the sensitivity and the heterogeneity of the estimates:

- Impacts of MWA on a later cohort of referrals from August to September;
- Impacts of MWA using a nearest neighbour matching estimator;
- Impacts of MWA on sub-groups aged 18-24 and 25 plus;
- Impacts of MWA by northern regions and southern regions;
- Impacts of MWA by gender;
- Impacts of MWA by length of time on JSA before referral; and
- Impacts of MWA using additional matching variables.

Overall we found that the impact estimates were largely insensitive to each of these alternative implementations. Some impacts looked greater or returned to zero in a shorter timescale, but due to the errors of the estimates of the impacts, we cannot be certain that these differences are statistically significant. This provides increased confidence that the methodology is robust and that our findings are not biased by the definition of our referral and non-referral samples. However, future work using larger and more recent cohorts should examine further sub-groups, in particular smaller geographical locations to check heterogeneity between districts and Contract Package Areas.

5. Conclusions

We have performed an early impact analysis of Mandatory Work Activity (MWA) by comparing the benefit and employment outcomes of a cohort of referrals with those of a matched comparison group, as an estimate of the counterfactual.

We emphasise that the analysis is complex and caution should be applied to the results, least of all because this is a preliminary impact analysis, based on a small cohort of referrals from the early months of the programme. Also for reasons we set out below there is a strong possibility that the impacts have been underestimated. In addition to exploring the impacts for more recent cohorts and for longer tracking times, further work might also consider exploring alternative methodologies.

Before summarising the impact results it is important to remember that the intention of MWA was to target individuals who were lacking or failing to demonstrate the disciplines and behaviours needed to seek out and secure employment. It is difficult to a form an objective assessment, but overall the cohort of referrals for this study had long benefit histories and poor sanctions record, suggesting that advisers selected individuals that were further from the labour market than the average JSA claimant, in line with the policy design. In turn this means that many of the referred individuals were likely to pose a significant challenge for any employment support designed to help them regain the focus needed to access sustained employment.

The central impact estimates for referrals between May 2011 up to and including July 2011 showed that in the first 3 months MWA decreased the likelihood of claiming benefit by up to 5 percentage points: the proportion of referrals in receipt of benefit was 77% at week 10 compared to 82% for non-referrals at week 10. This strongly suggests that MWA had caused a deterrent effect of claiming benefit - a very high proportion (64%) of those who did not start signed off their JSA claim.

However, in the subsequent period the impact decreased, returning to zero by 21 weeks following the referral (benefit receipt for both referrals and non-referrals was 74%). This means that in this period a higher proportion of individuals returned to benefit amongst referrals than non-referrals. Overall, the benefit impact during the first 21 weeks equated to referrals being off benefit for an average of about 4 days more than had they not been referred.

Many MWA referrals incurred a sanction but remain on benefit, so outcomes were adjusted to reflect only those individuals who were in payment of benefit. This accentuated the size of the impacts so that in the first 3 months MWA decreased the likelihood of being in payment of benefit by up to 8 percentage points. However, in the subsequent period the impact decreased, returning to zero by 21 weeks following the referral. Overall, the impact over the first 21 weeks equated to referrals being not in payment of benefit for an average of about 8 days more than had they not been referred.

Unlike for benefit impacts, MWA referrals showed no employment impacts. However, in common with other evaluations we should approach this result with caution owing to imperfect employment data.

It is possible that the impacts will change later on as the programme develops. In particular, we know that MWA sanction decision rates have risen. To look for signs of this possibility, impacts from a cohort from August to September were also examined. Although the results followed a similar pattern over the first 13 weeks (the maximum tracking time) as for the main cohort, future analysis is certainly required for later cohorts.

Some sensitivity tests were performed using sub-groups. We found that the impact estimates were largely insensitive to each of these alternative implementations. This provides increased confidence that the methodology was credible and that the findings were not biased by the definition of the chosen referral and non-referral samples.

Impacts were also tentatively estimated for cohorts of non-starters and starts, from the referral date, against individuals who were not referred. The results showed that non-starters were much more likely to be off benefit (by up to 12 percentage points) than the comparison group in the 10 weeks as individuals signed off more than the non-referred sample; but then the impact returned to zero by week 21 as these individuals returned to benefit to similar levels as the non-referred sample. On the other hand no impacts were observed for starters.

Overall our judgment is that the method is more likely than not to generate an underestimate of the impacts for two main reasons:

- The decision to refer is based on adviser discretion. Given that this decision was guided by the policy intent to select claimants who were not engaged with the labour market, the greatest risk is that the lack of claimant focus and motivation has been underestimated; this in turn will lead to underestimating the counterfactual's detachment from the labour market and therefore an underestimation of the measured impact.

- The design is that a referral should never come as a surprise which means that the possibility of a referral to MWA should be discussed in meetings prior to the date of the referral. This means that if claimants anticipated that they were going to be referred to MWA before the date of referral and then left benefit before this happened to avoid being referred, some of the deterrent effect of MWA would not be captured.

Other caveats to note are:

- It is very likely that insufficient tracking time has been allowed to observe the full extent of the impacts, particularly from the effects of new referrals, re-referrals and sanctions.

- Impact is not against ‘nothing’ but against a background of support, which includes Jobcentre Plus support and other programmes. Consideration of the time spent on other programmes suggests that the impacts against a background of standard Jobcentre Plus support might increase or depress the impacts marginally. In particular a high proportion of both referrals and non-referrals went onto to be referred to the Work Programme. As the availability of this other support increased towards the end of 2011 and in 2012, any future analysis must deal with this issue more comprehensively.

- The adjustment of outcomes to reflect sanctions is a helpful guide to understanding the impact on who is actually in payment of benefit; however some caution must be applied to these estimates because of uncertainty around some sanction start and end dates for calculating sanction durations and also a small number of individuals may have been awarded hardship payments and so strictly speaking are still in payment of some amount.

- In common with other evaluations, the employment impacts are less robust than benefit impacts because the HMRC tax data does not capture all employment outcomes, and cannot always be matched to benefit records. This means the benefit and employment impacts cannot be directly compared, and the benefit impact is likely to be a more robust estimate.

Appendices

Appendix 1 – Defining the Referral Sample

Table A.1 below shows how the referral sample of 3,190 was derived from the number referred to MWA placements between May 2011 and July 2011. It shows that of the total 24,010 MWA referrals, 3,380 were referred during our cohort period (May 2011 to July 2011). Of these, 3,190 were recorded on our systems as receiving Jobseeker's Allowance in the week of their MWA referral date.

Table A.1 Sampling valid referrals using selection conditions

Condition for selection	Valid referrals remaining
MWA referrals (May 2011 to November 2011)	24,010
Referrals during the cohort period (May 2011 to July 2011)	3,380
Referrals recorded as receiving Jobseeker's Allowance in the week of their referral	3,190

Appendix 2 – Adjusting JSA outcomes by sanctions

This Appendix gives details of the sanctions process and the methodology for adjusting benefit outcomes so that if an individual is in receipt of JSA and a sanction they are considered to be not in payment of JSA.

Sanction process

To claim JSA, claimants must satisfy a number of conditions. Failure to meet these conditions, without good cause, can result in:

- ***Disentitlement:*** where customers do not meet the basic conditions for JSA (to be available and actively seeking employment) their claim for benefit is terminated; or

- ***Sanction:*** where a customer does not satisfy their labour market conditions (e.g. refusing a job offer) no benefit is paid for a set period of time.

Two types of sanctions can be currently applied:

- **Fixed-length sanctions** are applied to acts or omissions relating directly to employability; for example, refusing a place on a mandatory training course. In general, fixed sanctions can be applied for two weeks for a first sanction; four weeks for any subsequent sanction within 12 months and a 26-week sanction can be imposed for a third (or subsequent) offence for some claimants in contracted provision. Also one- or two-week sanctions can be applied for failing to attend an interview. The only exception is MWA sanctions for which a 13-week sanction is applied for a first offence and 26-week sanctions for subsequent offences.

- **Variable sanctions** are applied to acts or omissions related directly to employment and can be applied for up to 26 weeks. The duration of the sanction is set at the discretion of the decision make.

The process followed is that an adviser would make a referral to a Decision Maker if they had doubts that a claimant was meeting requirements. The Decision Maker would then make one of three decisions – adverse, favourable or no decision (decision cancelled or reconsidered). It is possible this could happen 'instantly', but would depend on individual circumstances. For example, an adviser might suggest to a claimant that they feel they could do more and remind them of their responsibilities before following up with a referral to the Decision Maker at the next meeting if the doubt remains. In another example, where a claimant attends on the right day, but attends late, they can be given a warning letter setting out the date and time they are next required to attend the Jobcentre and informing them they could be sanctioned for one or two weeks if they do not attend at the right time on the right date.

Adjusting outcomes using sanction information

In order to adjust outcomes using sanctions information we:

a) Identified all MWA specific sanctions, other fixed sanctions and variable sanctions for referrals and non-referrals – disentitlements were not included as in such cases the benefit claim should be terminated;

b) Calculated the average duration of fixed and variable sanctions occurring within the latest year for which we have complete data – i.e. 2010/11. This was only done for cases where the sanction start and end dates were recorded and considered reliable[25]. Volumes were then compared to those for the previous year and differences between the treatment and control groups were considered. The results were as follows:

   - Average length of fixed sanction = 1 week
   - Average length of variable sanction = 11 weeks

c) Calculated the average time taken between a sanction decision being recorded and when the sanction actually started. The results were as follows:

   - MWA sanction = 26 days
   - Fixed sanction = 10 days
   - Variable sanction = 20 days

d) Matched in sanctions (excluding disallowances) occurring within the 30 weeks following the MWA referral/pseudo referral and created new outcome variables incorporating sanctions information where if an individual is in receipt of benefit but also in receipt of a sanction, the outcome is reset to say not in payment.

Note: We do not have access to hardship payment data and we would expect a small number of sanctioned individuals to be awarded hardship. Therefore, we may have overestimated the number considered to be not in payment of JSA.

[25] 94% of fixed sanction records were found to have correctly recorded start and end dates compared to only 78% of variable sanction records.

Appendix 3 – Generating Pseudo Referral Dates

The benefit and employment outcomes of individual referrals in our treatment group are measured weekly from the date on which they are referred to MWA. However, because a referral can occur at any time during an individual's claim, non-referrals in our comparison group have no natural referral date from which outcomes can be measured. It is therefore necessary to assign a 'pseudo referral date' to each non-referral so that a time-based comparison between groups can be made. We must ensure that these pseudo referral dates identify a period of time over which non-referrals can best represent what would have happened to MWA referrals if they had not been referred.

We use the method for generating pseudo referrals as described in Ainsworth and Marlow (2011). This aims to align the non-referrals and referrals with respect to two time dimensions: calendar time and length of time on benefit.

The method used for generating pseudo referral dates is as follows:

1. All referral records were separated according to the benefit start month of the referral – i.e. separate data sets were created for referrals starting benefit in October 2008, November 2008, December 2008, and so on up to July 2011 (the latest MWA referral month included in the analysis);

2. For each of these referral data sets, we calculated the proportion of MWA referrals occurring in each possible MWA referral month.

3. We then separated all non-referral records according to the benefit start month of the non-referral, in the same manner as carried out in Step 1 for referrals;

4. For each non-referral benefit start month file, we randomly assigned a pseudo referral month from the distribution of MWA referral months gained from the referral file with the equivalent benefit start month.

5. We then assigned a random date in the assigned month from a flat distribution (i.e. all dates within the month were equally likely).

6. If an assigned pseudo referral date occurred at a time when the non-referral was not claiming JSA, then the pseudo start was considered 'invalid' and the record was removed from the sample.

7. Table A.2 shows that the distribution of MWA referrals closely mirrors the distribution of pseudo referrals (unmatched) and then after matching the distribution is identical to the distribution of pseudo referrals.

Table A.2: Comparison of distribution of referrals and pseudo-referrals

	Unmatched		Matched	
	Non-Referrals	MWA Referrals	Non-Referrals	MWA Referrals
May-11	6%	5%	5%	5%
Jun-11	43%	40%	40%	40%
Jul-11	51%	54%	55%	55%
Total	**611,230**	**3,190**	**3,080**	**3,080**

Appendix 4 – Matching on other DWP Programmes

Referrals and non-referrals may have been on employment programmes other than MWA prior to their start or pseudo referral date. To ensure that the impacts we measure are balanced with regard to the amount of past additional support received, we match on the time spent on each of 13 DWP employment programmes in the year prior to the start or pseudo referral date. Programmes included are as follows:

- Flexible New Deal (FND);
- New Deal for Young People (NDYP);
- New Deal for Long Term Unemployed (NDLTU)
- New Deal for Disabled People (NDDP);
- Pathways to Work (PtW);
- European Social Fund (ESF).
- New Deal for Lone Parents (NDLP);
- Basic Skills (BS);
- Work Based Learning for Adults (WBLA);
- Employment Zones (EZ);
- New Deal for Partners (NDP);
- Young Person's Guarantee (YPG);
- Six Month Offer (6MO)
- Work Programme (WP)
- Get Britain Working Measures (GBW).

We use the number of days spent on each of these programmes in the year prior to referral or pseudo referral date as matching variables in the propensity score model. The exceptions are for YPG, 6MO, WP and GBW for which we do not have accurate end dates for all spells. Therefore, we use a single binary variable, which indicates whether an individual has had one or more spells on each of these programmes.

Appendix 5 - Controlling for Labour Market History

As discussed in section 3, we believe that the labour market history of an individual provides an important proxy for unobserved characteristics, such as motivation to work, which will jointly influence both the decision and the outcomes in the absence of referrals. It is therefore important to control for benefit and employment history in our propensity score model.

A common method described in the literature for controlling for labour market history is the approach adopted by Card and Sullivan (1988), in which a single variable is constructed to describe the labour market position of each individual over time. However, Ainsworth and Marlow (2011) proposed an alternative method of controlling for labour market history, which we have adopted in the present analysis. This appendix outlines the advantages of using the Ainsworth and Marlow approach.

In the approach utilised by Card and Sullivan, a single variable is constructed to represent an individual's labour market history. For example, a string variable of eight binary characters could represent whether an individual was in or out of employment in each of eight time periods. This approach has the advantage that a single variable can indicate not just the length of time an individual has spent receiving benefit or in employment, but also represents a timeline of moving in and out of each labour market state.

A disadvantage of this approach is that the number of permutations of the constructed string variable is 2^N, where N is the number of time periods independently represented in the history string. Therefore, each additional time period included in the string doubles the number of possible permutations. Constructing a variable which describes eight periods of three months (i.e. two years of benefit history) therefore results in 255 (2^8 -1) dummy variables. Using such labour market history variables therefore requires a trade-off between ensuring the quality of the labour market variable (in terms of describing labour market history with sufficient resolution over a sufficiently long duration) and ensuring that the variable is not over-specified by producing too many dummy variables in the propensity score model.

We have therefore adopted the alternative approach proposed by Ainsworth and Marlow to control for labour market history, which is adapted to control for labour market history with higher resolution over longer durations. To implement this method we generate 104 independent binary variables which represent an individual's benefit receipt or non-receipt in each of the 104 weeks prior to the MWA referral or pseudo referral date. We then generate a further 104 independent binary variables which represent whether an individual is in or out of employment in each of the 104 weeks prior to the MWA start or pseudo referral date. In this way, we are able to control for 104 weeks of labour market history using the resulting 104 variables in the propensity score model. To gain equivalent resolution and duration using the

approach adopted by Card and Sullivan would have required approximately 2^{52} variables in the model.

Finally to reflect labour market churn we also include a variable that counts the number of benefit claims made in the two years prior to the referral date.

Appendix 6 – Matching Protocol

We describe below the matching protocol used in this evaluation to construct suitable treatment and comparison groups from our referral and non-referral samples.

1. Define a referral (treatment) sample within the analysis cohort period, as specified in Section 2.1.1;
2. Define a non-referral (comparison) sample within the same cohort period, as specified in Section 2.1.2;
3. Combine the records from steps 1 and 2 to produce a single sample comprising treatment and comparison records;
4. Code an indicator variable *Z,* which is 1 for treatment records and 0 for comparison records;
5. Specify and estimate a binary probit for p(x) := P(Z=1 | X=x);
6. Restrict the sample to common support: remove all treatment records for which no comparison record falls within the Kernel bandwidth (a bandwidth of 0.0001 was used in our primary analysis);
7. Implement a Kernel 'one-to-many' matching approach:
    a. Select a treatment record and identify all comparison records with propensity scores lying within the Kernel bandwidth of the treatment record score;
    b. Apply a weighting to the comparison records using an Epanechnikov distribution such that those with closer propensity scores to the treatment record are weighted more than those with more distant propensity scores;
    c. Repeat steps *a* and *b* until all treatment records have been selected. The weighting applied to comparison records for each repeated step is added to the cumulative weighting from all previous steps (the total weighting of all comparison records is therefore equal to the number of treatment records).
8. Use the final weights for each comparison record to calculate a weighted mean for each outcome variable in Z=0;

Appendix 7 – Sensitivity Analysis

Impacts of MWA on a later cohort of referrals

The impacts described below do not include the impact on moving into work because the impact from the original analysis has been small.

Impacts of MWA on a later cohort of referrals from August to September

The main analysis conducted used a cohort of referrals from May to July 2011. This is when MWA referrals could first be made, so there is a risk that the referrals made during the early part of this time period do not represent the performance of referrals subsequently made once MWA had become more established. We therefore also tracked a cohort of referrals made in August and September 2011 for 13 weeks.

The benefit impact for the later cohort appears to be of a similar magnitude to that for the original cohort, although it seems to be taking longer to return to zero. It is too early to say but this may indicate that the original analysis may slightly underestimate impacts from later referrals.

Impacts of MWA using a nearest neighbour matching estimator

The matching for the main analysis uses kernel matching, in which we remove all treatment records for which no comparison record falls within a fixed bandwidth. We tested an alternative method which uses a record's "nearest neighbour".

Statistical tests of the overall effectiveness of the PSM model showed that "nearest neighbour" matching in this instance to be inferior to kernel matching thus giving less credible impact results.

Impacts of MWA on sub-groups aged 18-24 and 25 plus

The rationale for this analysis is to explore the sensitivity of impacts by age. This is important because younger benefit claimants may show different impacts and also do not have as much work and benefit history as older participants to control for.

The benefit impact in the weeks 4 to 10 appears to be slightly more accentuated for 18 to 24-year olds but, for both groups, it returns to zero at about 19 or 20 weeks after referral.

Impacts of MWA by northern regions and southern regions

The rationale for this analysis is to see whether there are any geographical differences. Ideally we would like to compare each CPA, but at this point in

time we only have sufficient referrals to divide into two groups – northern and southern regions.

The benefit impacts in the north and south follow a similar pattern as all referrals, with referrals in the north and south returning to zero around 17 weeks after referral. Referrals in the north are slightly more likely to be on benefit at the end of the tracking period.

Impacts of MWA by gender

MWA referrals seem to have less of an impact on female claimants than male claimants with respect to benefit receipt. The benefit impact looks less pronounced for female referrals, taking half as long to return to zero and with impact increasing after this.

Impacts of MWA by length of time on JSA before referral

We also compared the impact on referrals and non-referrals that had been on JSA for up to six months and over six months before referral. The non-referred have spent an average of 11 weeks less on JSA in the two years prior to pseudo referral, compared to the referred, so the referred might potentially be harder to help.

Those who had spent less time on JSA experienced a larger impact early on, but the impact for both groups returned to zero later.

Impacts of additional matching variables

We also looked at the sensitivity of adding additional matching variables: numbers of fixed sanctions, variable sanctions and disallowances experienced in the two years prior to the MWA referral/pseudo referral date, instead of the total number of referrals experienced and the proportion of time spent on benefit in the last 24 months/12 months/6 months and the number of benefit spells experienced in the last 24 months/12 months/6 months. We found that doing this did not improve the quality of the matching significantly and the results followed a similar pattern.

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Authors

Adam Hillmore
Jasbir Lally
Simon Marlow
Stuart Prince
Deborah Pritchard

Department for Work and Pensions