



Department
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A Quantitative Impact Assessment of the Supervised Jobsearch Pilots

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Executive Summary

The Supervised Jobsearch Pilots (SJP) programme was a mandatory Provider-led intervention of up to 13 weeks duration that required eligible jobseekers to carry out up to 35 hours of job-search activities per week. The purpose of the programme was to make claimants' job search more effective through support and supervision, and increase their likelihood of moving off benefit and into work. There were two separate pilots, one for those who had been claiming Jobseeker's Allowance for less than a year (pre-Work Programme pilot) and another for those who had completed two years on the Work Programme and still not found employment (post-Work Programme pilot).

For the pre-Work Programme pilot we estimate that, per participant, SJP led to an average of ten fewer days spent on Department for Work and Pension's (DWP) primary benefits and an average five more days spent in employment. For the post-Work Programme pilot we estimate that, per participant, SJP led to an average of 19 fewer days on DWP benefits and 5.7 more days in employment. Whilst there appears to be a net financial benefit to the participants, the costs of running the SJP programme are not compensated for by the reduction in benefit expenditure and increased tax returns.

Contents

	Page
1. Background	1
2. Pilot Impacts	4
3. Cost Benefit Analysis	21
4. Conclusion	23
Annex A. Overview of the Pilot Recruitment Process	25
Annex B. An Assessment of the Pilot Participant Data	29
Annex C. Assumptions Underpinning the Cost Benefit Analysis	35

List of Tables

Table 1.1	Number of people in each pilot and each group according to the LMS data.....	2
Table 1.2	Number of people in each pilot and each group according to the ITT analysis	2
Table 2.1	Characteristics of Pre-WP Pilot Participants	4
Table 2.2	Characteristics of Post-WP Pilot Participants	13
Table 2.3	Proportion of Pilot Participants with a Sanction	19
Table 3.1	Net Benefits of the SJP Pilots	21
Table B.1	Pilot and Group of SJP Participants	30
Table B.2	Comparison between nominal and actual group allocation for the pre-WP pilot	31
Table B.3	Comparison between nominal and actual group allocation for the post-WP pilot	31
Table B.4	Proportion of LMS Markers that are Consistent with the Underlying Administrative Data	32
Table B.5	Proportion of Pilot Participants with a PRaP Start and/or Payment...	33
Table C.1	Drivers of Net Cost/Benefits.....	35

List of Figures

Chart 2.1	Proportion of Pre-WP pilot participants on DWP benefits.....	6
Chart 2.2	Difference in the proportion of pre-WP pilot intervention and control participants that are claiming DWP benefits	7
Chart 2.3	Difference in the proportion of pre-WP pilot participants on DWP benefits in the period prior to recruitment.....	9
Chart 2.4	Difference in the proportion of pre-WP pilot participants on JSA and ESA.....	10
Chart 2.5	Pre-WP pilot employment rates.....	11
Chart 2.6	Pre-WP pilot employment impact.....	12
Chart 2.7	Proportion of Post-WP pilot participants on DWP benefits.....	14
Chart 2.8	Post-WP Benefit Impact.....	15
Chart 2.9	Difference in the proportion of post-WP pilot participants on JSA and ESA.....	16
Chart 2.10	Post-WP Employment Rates.....	17
Chart 2.11	Post-WP Employment Impact.....	18

Supervised Jobsearch Pilots Impact Assessment

List of Abbreviations

ASE	Actively Seeking Employment
DMA	Decision Maker Activity
DMAS	Decision Maker and Appeals System
DWP	Department for Work and Pensions
FTA	Failure To Attend
HMRC	Her Majesty's Revenue and Customs
ITT	Intention to Treat
JCP	Jobcentre Plus
JSA	Jobseeker's Allowance
LMS	Labour Market System
NiNo	National Insurance Number
PRAP	Provider Referral and Payment System
RCT	Randomised Controlled Trial
SJP	Supervised Jobsearch Pilots
UC	Universal Credit
WP	Work Programme
WPCI	Work Programme Completer Interview

Supervised Jobsearch Pilots Impact Assessment

Author

This report was prepared by Martin Moran who is an analyst at the Department for Work and Pensions.

Summary

This report describes a quantitative analysis of the impact of the 'Supervised Jobsearch Pilots (SJP) trial. The purpose of the pilots was to make claimants' job search more effective through support and supervision, and increase their likelihood of moving off benefit and into work. The pilots sought to test the impact of a prolonged period of supervised job-search activity with two groups of claimants: those who had been claiming Jobseeker's Allowance for less than a year (pre-Work Programme pilot) and those who had completed two years on the Work Programme and still not found employment (post-Work Programme pilot). Claimants were referred to the pilots by Jobcentre Plus and were required to attend a local provider centre for up to 35 hours per week for 13 weeks, unless they stopped receiving Jobseeker's Allowance during this time.

For the pre-Work Programme (pre-WP) pilot we estimate that, per participant, SJP led to an average of ten fewer days spent on Department for Work and Pension's (DWP) primary benefits and an average five more days spent in employment. We estimate that the post-Work Programme (post-WP) pilot has led to participants spending 19 fewer days on DWP benefits and 5.7 more days in employment. The post-WP impact may increase as we track participants for longer, but only very marginally.

Participation in SJP is associated with higher levels of sanctions, with 16.2 per cent of the pre-WP intervention group incurring a sanction during the pilot period compared with seven per cent of the controls. The equivalent figures for the post-WP pilot are 16.9 per cent and 6.2 per cent respectively. This may be, in part, due to the greater demands of the programme compared with the 'business as usual' requirements.

Finally, whilst there appears to be a net financial benefit to the participants, the costs of running the SJP programme are not compensated for by the reduction in benefit expenditure and increased tax returns.

1 Background

The Supervised Jobsearch Pilots (SJP) were launched in September 2014. These pilots delivered 35 hours of mandatory attendance, for up to 13 weeks, with a Provider where job-search activities were required to be undertaken. The intent of the pilots was that claimants would improve their job searching skills, develop positive work-related behaviours and ultimately secure employment.¹

There were two pilots. One recruited people prior to the point in their Jobseeker's Allowance (JSA) claim when a referral to the Work Programme (WP) would be mandatory. We call this the pre-WP pilot. The other pilot recruited people after they had completed the WP but had not secured employment. We call this the post-WP pilot. The pilots operated in four Jobcentre Plus Districts and recruited up to 12 December 2014. Full details on the recruitment policy are provided in Annex A.

Both pilots used a Randomised Controlled Trial (RCT) approach. Initially potential recruits' suitability (according to criteria outlined in Annex A) for the pilots was considered and some people were excluded at this stage. Some people were also excluded at this stage on the basis of a 'Work Coach Decision' (again, details are outlined in Annex A). Then, half the eligible population were put in an 'intervention' group and were referred onwards to join the SJP programme and the other half were put into a control group.

We note that people could be deemed unsuitable after they had been randomly allocated and effectively be taken out of the intervention or control groups. We also note that mid-way through the pilots the random allocation ratio for the post-WP pilot was altered to 75:25 in favour of the intervention group. This was done to increase the number of SJP referrals (for commercial reasons) but this change should not in principle alter the average characteristics or the outcomes of the post-WP intervention or control group. The participant data used for the analysis that is presented in this report was recorded in a case management system, called the 'Labour Market System', which is operated by DWP. Details on how we interpret that data are provided in Annex B. Annex B also corroborates the LMS data with other sources that, in principle, confirm the stage in the claim that participants were at when they were recruited to SJP and their participation or otherwise in the programme.

¹ For more detailed information on the content and delivery of the pilots, please refer to the externally commissioned evaluation of SJP, which is published alongside this report.

Supervised Jobsearch Pilots Impact Assessment

According to the LMS data the number of people in each pilot and in each of the intervention, control and unsuitable groups is shown in Table 1.1. For simplicity we have combined those deemed unsuitable with those who were excluded on the basis of a Work Coach decision, but we acknowledge that these two categories have different interpretations. Detailed information on the criteria that the unsuitable group comprise is in Annex A.

Table 1.1 Number of people in each pilot and each group according to the LMS data

Pilot	Intervention	Control	Unsuitable
Pre-WP	415	721	1003
Post-WP	1958	1980	1942

The LMS data describes how people were treated in the implementation of the pilots. However, the analysis presented in the next section of this report is based upon an ‘Intention to Treat’ (ITT) approach. This simply means that we treat people according to how they should have been treated according to the trial design and not how they were actually treated. This is a commonplace approach that minimises the chance of there being systematic differences (aside from the piloted intervention) between the intervention and control groups that could potentially influence the measured impacts. More details on the specific assumptions we made in respect of our ITT method are presented in Annex B, however, the consequence is that our impact assessment is based upon the numbers of people presented in Table 1.2 and not Table 1.1.

Table 1.2 Number of people in each pilot and each group according to the ITT analysis

Pilot	Intervention	Control
Pre-WP	365	655
Post-WP	1977	2077

We do not include those deemed unsuitable or those with a Work Coach decision in our ITT analysis. In principle, these are out of scope of the pilots. However, there is some evidence that the unsuitability and Work Coach decision criteria were applied differently to the intervention and the control groups. If we include these people in the ITT analysis then only a small proportion of people in the intervention groups would

Supervised Jobsearch Pilots Impact Assessment

have actually taken part in the programme. In principle, this would make detecting an impact more difficult. As the next section will show, if we exclude both those deemed unsuitable and those with a 'Work Coach Decision' we have little evidence of bias within our data and therefore we believe that the control groups still provide a good estimate of the outcomes that the intervention groups would have achieved in the absence of the pilots.

It is nonetheless important to bear in mind that there remain a number of people in the ITT intervention groups that did not join the programme (and some people in the control groups did join the programme). With respect to the intervention groups, it is quite reasonable for some people to not join the programme. Some people will have left benefits at that stage in their claim regardless of SJP and others might have left benefits because of some deterrence effect. So, an ITT approach is a correct approach to take but it does mean that when referring to an 'intervention group' we are not necessarily saying that everybody in that group attended the 13 week SJP programme.

We now go on to summarise the main findings from our analysis. For each of the pre-WP pilot and post-WP pilot in turn, we first explore the impacts upon the numbers of people receiving DWP benefits and as part of that work we also explore the impacts upon individual benefits. We then examine employment impacts.

We complete our impact analysis with a short summary of sanctions activity and after that present an assessment of the net costs/benefits of the SJP pilots, to the participants, to the employer and to the Exchequer.

2 Pilot Impacts

This chapter examines differences between the intervention and control groups’ benefit and employment outcomes after pilot participation. As mentioned in the previous section, we take an ITT approach whereby we treat people according to how they should have been dealt with according to the trial design and not how they were actually dealt with.

2.1 Pre-WP Pilot Impacts

Before examining any impacts for the pre-WP pilot we first compare the characteristics of the intervention and control group. Table 2.1 summarises a range of personal characteristics as well as participants’ partner and parental status. No differences evident from Table 2.1 are statistically significant, so we can assume for the purposes of analysis that there is no systematic difference in the characteristics of those in the intervention and control group. That said, the absence of any statistically significant differences is partly because our samples sizes are small. There is a margin of error of four to five percentage points around many measures which means that any differences would have to be reasonably large (as high as nine to ten ppts in some instances) before we could be confident that they were not due to random variation. So, whilst the data in Table 2.1 does not give us particular cause for concern, the low sample sizes limit the value of those data.

Table 2.1 Characteristics of Pre-WP Pilot Participants

Characteristic	Intervention	Control
Number of Participants	365	655
Gender		
Male	65%	60%
Female	35%	40%
Ethnicity		
White	77%	79%
Black	5.5%	4.1%
Asian	10%	7.3%
Mixed	3.6%	3.5%

Supervised Jobsearch Pilots Impact Assessment

Chinese/Other	1.4%	2.7%
Prefer Not To Say	2.5%	3.5%
Unknown	0%	0.2%
Age At Start of Pilot		
16 to 24	33%	39%
25 to 29	14%	13%
30 to 39	20%	17%
40 to 49	18%	16%
50 to 59	13%	13%
60 or Over	2%	2%
DDA¹ Disabled		
	28%	28%
Known Skills Needs		
Basic Skills Need	8.2%	8.4%
English as a Second Language	1%	1%
Number of Children		
No Children	81%	80%
1 Child	8%	11%
2 Children	6.8%	4.7%
3 Children	2.5%	2.3%
4 or More Children	2.2%	1.7%
Age of Youngest Child		
0 to 2	4.1%	3.4%
3 or 4	1.1%	1.8%
5 to 10	9.0%	9.8%
11 to 15	3.0%	2.3%
16 or Over	1%	1.7%
Unknown	1.6%	1.2%
In Receipt of Partner Allowance		
	10%	11%

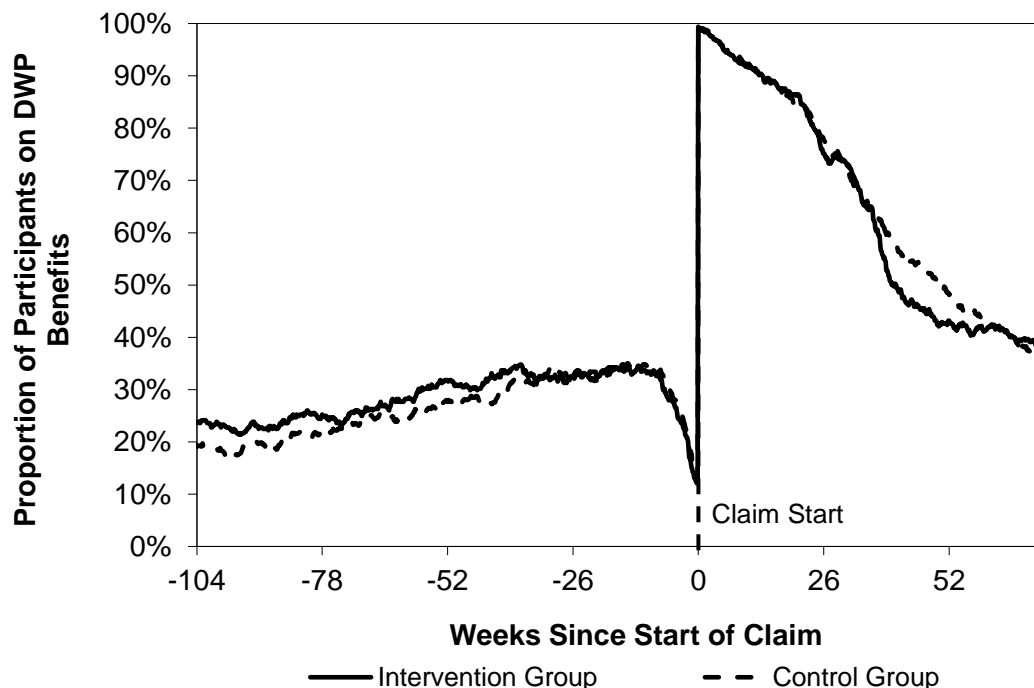
1 As defined in the Disability Discrimination Act of 1995

Source: DWP benefits administrative data: November 2015 and Labour Market System: February 2016

Supervised Jobsearch Pilots Impact Assessment

We next compare the degree to which pre-WP pilot participants were reliant upon benefits prior to joining SJP. Past presence on benefits is a good predictor of future reliance on benefits, so benefit history is an important indicator to check if we are to be confident that our sample is bias free. Chart 2.1 shows, with respect to the first day of the JSA claim that led to pilot participation, the proportion of the intervention and the control group that were claiming any primary DWP benefit on any particular day. The time scale is referenced to day one of the JSA claim because that is a common reference point. Particularly in the first 12 months, the probability of leaving benefits is dependent upon the duration of the JSA claim and because people joined SJP at different points in their claim their subsequent presence on benefits will not just be influenced by pilot participation.

Chart 2.1 Proportion of Pre-WP pilot participants on DWP benefits



Source: DWP Benefits Administrative Data: November 2015

Chart 2.1 suggests that around ten per cent of participants were on DWP benefits immediately before the JSA claim that led to trial participation. These people will have moved from one benefit, typically Employment Support Allowance (ESA), to another (JSA in this instance). The chart also shows that the control group were as likely as the intervention group to be on benefits in the period shortly prior to the JSA claim of interest. However, in the longer term the agreement is not as good. Past receipt of benefits does have an influence on future benefit outcomes but we are less clear on the extent to which the more recent past has a stronger influence than the

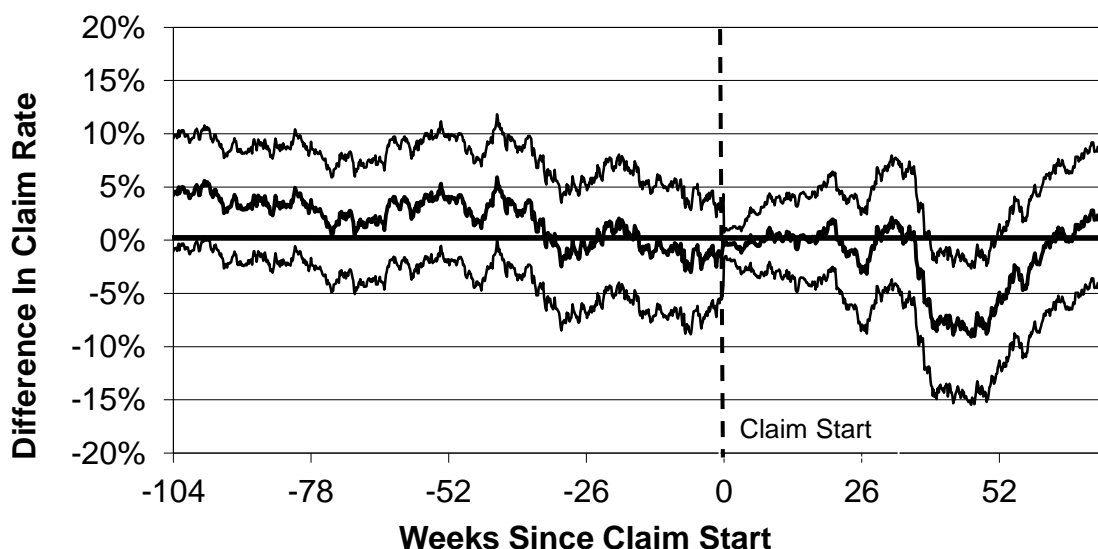
Supervised Jobsearch Pilots Impact Assessment

more distant past. Therefore, we cannot say whether the differences evident in Chart 2.1 will contribute to post-pilot differences in benefit receipt.

Turning to the post-claim period, Chart 2.1 shows that there is a short period of time when the intervention and control group have a distinctly different likelihood of being on benefits. To illustrate this more clearly, Chart 2.2 shows the difference between the curves in Chart 2.1. Also shown are the 95 per cent confidence intervals. The range described by these intervals represents the statistical uncertainty in the value of the true impact given that we tested SJP on a sample of participants and not whole 'population'. By population we do not mean all JSA claimants. Rather, we mean all those who fulfil the eligibility criteria outlined in Annex A of this report.

We can see the pre-pilot agreement and longer term disagreement in the period prior to the start of the JSA claim of interest. We also see a clear reduction in the intervention group's presence on benefits relative to the control group around 39 weeks after the start of the JSA claim. 18 to 24 year olds were recruited during weeks 20 to 24 of their JSA claim and those aged 25 or over were recruited in weeks 33 to 37. An emergent impact after 39 weeks is consistent with that time frame considering that SJP is of up to 13 weeks duration.

Chart 2.2 Difference in the proportion of pre-WP pilot intervention and control participants that are claiming DWP benefits.



Source: DWP Benefits Administrative Data: November 2015

Charts 2.1 and 2.2 raise the question as to why we do not measure the impact of SJP with respect to the date of pilot recruitment. The reason for this is that the

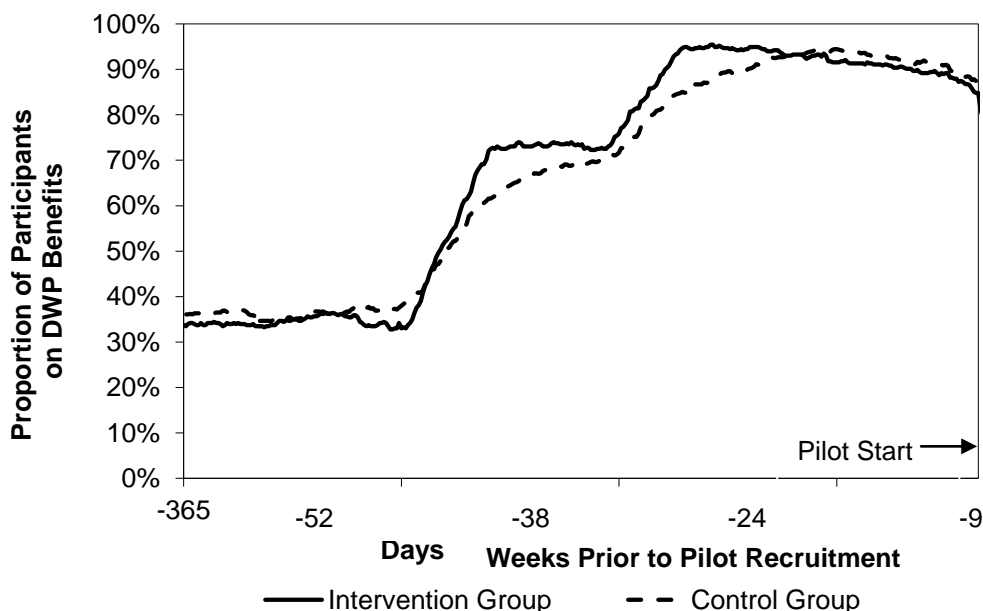
Supervised Jobsearch Pilots Impact Assessment

distribution of recruitment dates is not the same for both the intervention and the control group. We can see this from Chart 2.3 below. This chart shows the proportion of people in the intervention and the control groups that were on benefits in the period of time prior to pilot recruitment. The two discontinuities in the curve for the intervention group reflect the different recruitment periods for 18 to 24 year olds and the over 25s. The control group curve is however, a lot more graduated suggesting that the pilot recruitment process was not applied as rigidly (in time) as it was for the intervention group.

This means that control group claimants joined SJP at different points in their claim compared with the intervention group and therefore might not provide a good estimate of what the intervention group's outcomes would have been in the absence of the pilot. To rule out this possibility we can and do measure the impact with respect to a reference point common to both groups, namely, the first day of the JSA claim that led to trial participation.

Our choice of reference point does mean that we effectively 'spread out' the impact over a longer period of time, and that the measured peak impact will be lower than it would be had we measured the impact with respect to the pilot recruitment date. However, what matters is the totality of the difference in benefit receipt, and measuring that difference with respect to the point of claim will not distort the total impact (in terms of additional days off benefit). Furthermore, it will remove the differences evident in Chart 2.3 that are due to the recruitment process and not the pilot impact. This is why the point of claim is our preferred date of reference.

Chart 2.3 Difference in the proportion of pre-WP pilot participants on DWP benefits in the period prior to recruitment.



Source: DWP Benefits Administrative Data: November 2015

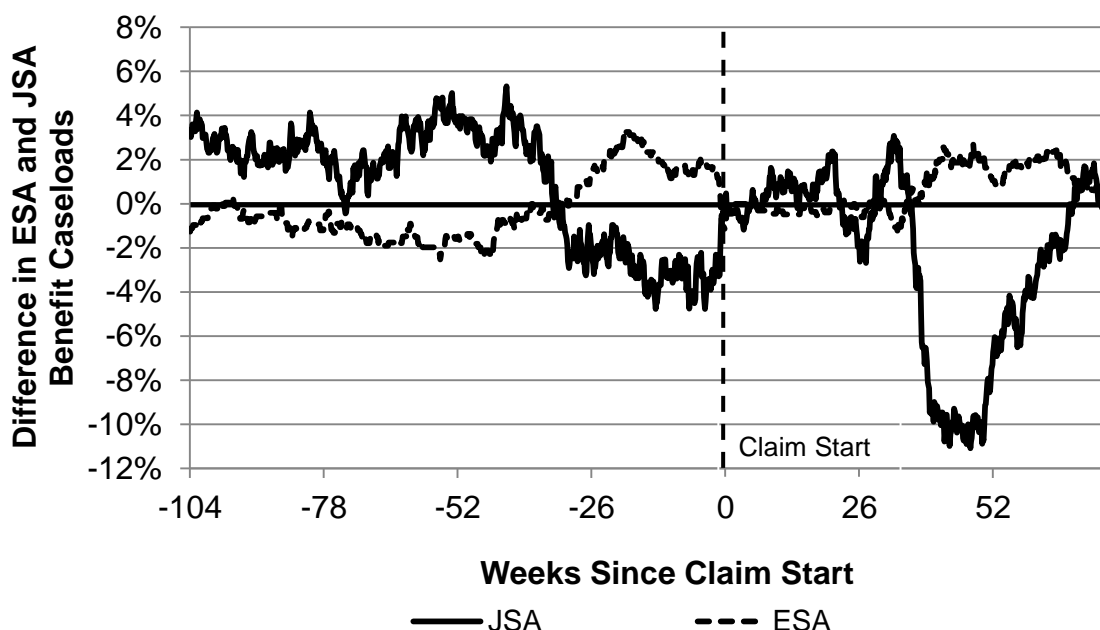
In total, we estimate the pre-WP pilot impact to amount to an average ten days off benefit per participant. There is a wide margin of error surrounding this central estimate. We can only be 95 per cent sure that the true impact lies between + one and -21 days, where the '+' sign indicates that the intervention group spend more time on DWP benefits than the control group. Nonetheless, there is a period of time where the difference is statistically significant (with a 95 per cent degree of confidence) and that time period coincides well with the timing of the intervention. Therefore, we regard it as very unlikely that that cumulative difference occurred through chance alone, particularly because the central estimate is very close to zero both before and after the period of time where an impact is evident. The figure of ten days off benefit is based on the sum of the central estimates of the daily impacts, measured from the point at which a difference between the intervention and control group emerges. We chose not to measure from when the daily impacts were statistically significant with a 95% degree of confidence as, in our view, this would provide an unduly conservative measure of the overall impact.

Chart 2.2 confirms the total impact of the pre-WP pilot on all DWP primary benefits. We checked for evidence of an impact upon individual benefits because it is possible that participants were moving to other benefits as well as leaving benefits altogether.

Supervised Jobsearch Pilots Impact Assessment

At the time of the pilots it was most likely that participants might transition to ESA. Chart 2.4 shows that the JSA benefit saw the largest change in the number of people claiming that benefit. The increase in the numbers claiming ESA is much lower and not greater than levels seen prior to the pilot, so there isn't strong evidence that the pre-WP pilot has led to more claimants moving to other benefits than would otherwise have been the case. As we shall see later in this report, this is not the case for the post-WP pilot.

Chart 2.4 Difference in the proportion of pre-WP pilot participants on JSA and ESA.



Source: DWP Benefits Administrative Data: November 2015

Chart 2.5 shows the measured employment rates for the pre-WP pilot participants as inferred from HMRC's P45 data. There are known shortcomings with the P45 data. These include:

- Jobs that pay below the Lower Earnings Limit are sometimes not reported.
- Self-employed jobs are under-reported.
- Employment start and end dates are sometimes missing.
- Employment start and end dates are sometimes set to the beginning and end of the tax year respectively.

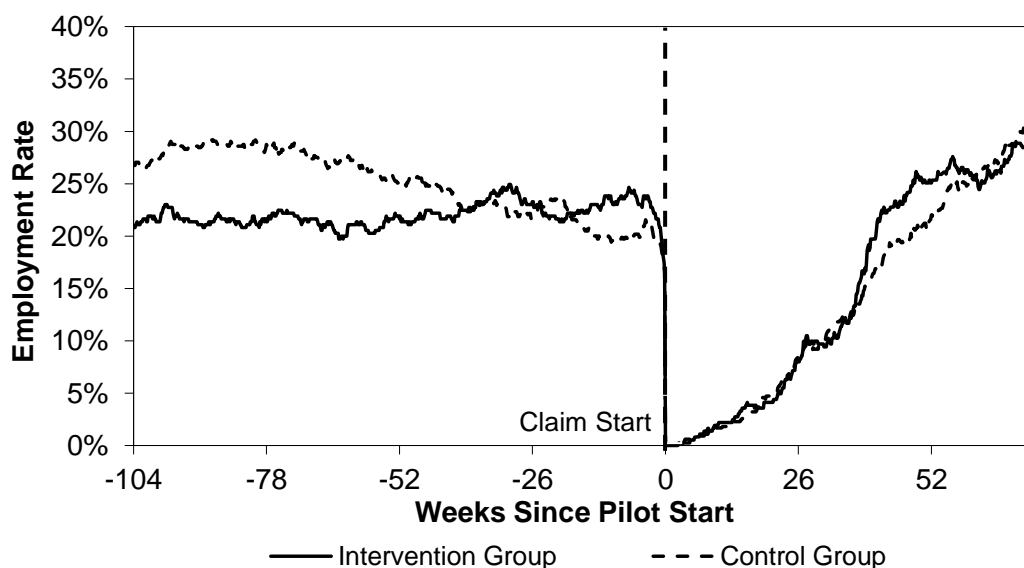
Supervised Jobsearch Pilots Impact Assessment

- Data matching errors mean that we may not always identify jobs gained by SJP pilot participants.
- Large time lags in the data mean that it can be many months before we receive information about a job entry.

Owing to these shortcomings, we acknowledge that our measures of participant employment rates probably fall short of the true levels and therefore so will the differences that we measure. Further, we do not expect the P45 data to be complete and stable beyond August 2015 which means that we will only be able to confidently track people who joined the pilots at the end of the recruitment period for five months or so, though we can track people who joined earlier in the recruitment period for longer than that.

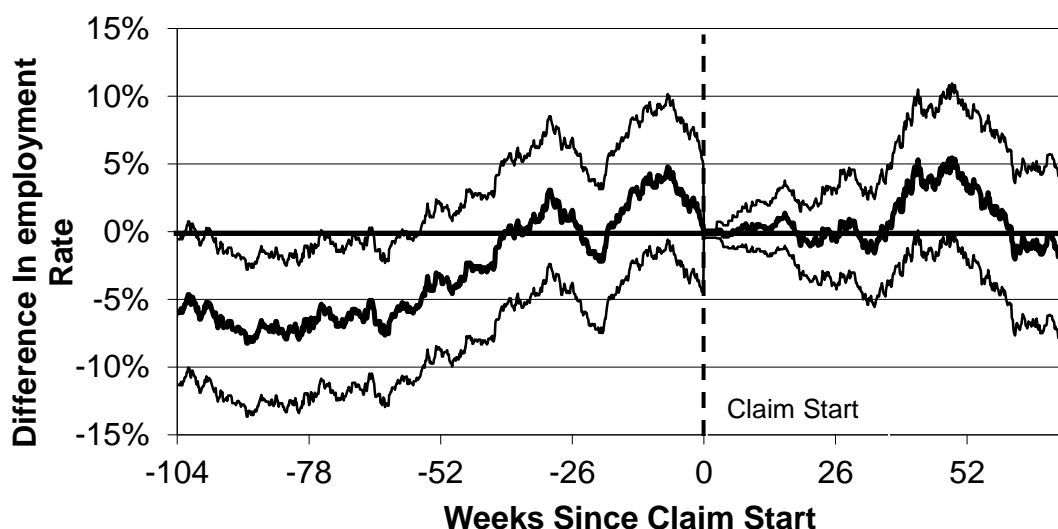
Notwithstanding these comments on the quality and completeness of our employment data, Charts 2.5 and 2.6 respectively show the measured employment rates and the differences between these rates. The charts broadly corroborate the benefit impacts. Taken at face value, the cumulative impact to date is an extra five days in employment (with a margin of error of - four to 14 days). The true impact may be higher than this owing to the data quality issues summarised earlier. If we are undercounting employment then we may be undercounting the difference in employment. Also, there is a systematic difference in the measured employment rates prior to the claim start and we cannot rule out this having some influence in the pilot period.

Chart 2.5 Pre-WP pilot employment rates



Source: HMRC P45 data: February 2016

Chart 2.6 Pre-WP pilot employment impact



Source: HMRC P45 data: February 2016

2.2 Post-WP Pilot Impacts

Turning to the post-WP pilot, we first assess the personal characteristics of the intervention and control group participants. Table 2.2 contains similar information to Table 2.1 but for the post-WP pilot. The sample sizes are much larger and therefore it is easier to identify differences between the intervention and the control group as being non-random. There is in fact only one statistically significant difference in Table 2.2, but considering that we have made many different comparisons of various characteristics it would not be unexpected for one comparison to appear to be statistically significant through chance alone.²

Nonetheless, the one significant difference is in the proportion who are disabled (as defined by the Disability Discrimination Act of 1995). The difference is 4.2per cent and is mainly in relation to participants' capacity to lift, carry and/or move large objects. The intervention group are less likely to have or report a disability and this may have had some influence on the decision to refer them to the pilot. Whilst we cannot rule out this difference having some influence on the measured impacts, as we shall see, the scale of the impacts is such that any influence is likely to be very minor.

² It is possible to adjust for multiple comparisons but our preference is to present the unadjusted tests and caveat accordingly.

Supervised Jobsearch Pilots Impact Assessment

Table 2.2 Characteristics of Post-WP Pilot Participants

Characteristic	Intervention	Control
Number of Participants	1977	2077
Gender		
Male	76%	74%
Female	24%	26%
Ethnicity		
White	78%	77%
Black	6.4%	6.3%
Asian	8.4%	8.6%
Mixed	1.8%	2.5%
Chinese/Other	1.6%	1.2%
Prefer Not To Say	3.6%	4.0%
<i>Unknown</i>	0.2%	0%
Age At Start of Pilot		
16 to 24	12%	12%
25 to 29	16%	14%
30 to 39	21%	20%
40 to 49	25%	26%
50 to 59	22%	24%
60 or Over	4%	4%
DDA¹ Disabled*	33%	37%
Known Skills Needs		
Basic Skills Need	30%	29%
English as a Second Language	1.4%	2.0%
Number of Children		
No Children	84%	83%
1 Child	7%	8%
2 Children	5.2%	4.4%
3 Children	2.6%	2.6%
4 or More Children	1.4%	1.8%
Age of Youngest Child		
0 to 2	2.4%	2.0%

Supervised Jobsearch Pilots Impact Assessment

3 or 4	1.5%	1.6%
5 to 10	3.2%	3.7%
11 to 15	5.1%	5.9%
16 or Over	2.8%	2.3%
Unknown	1.5%	1.4%
In Receipt of Partner Allowance		
	9%	10%

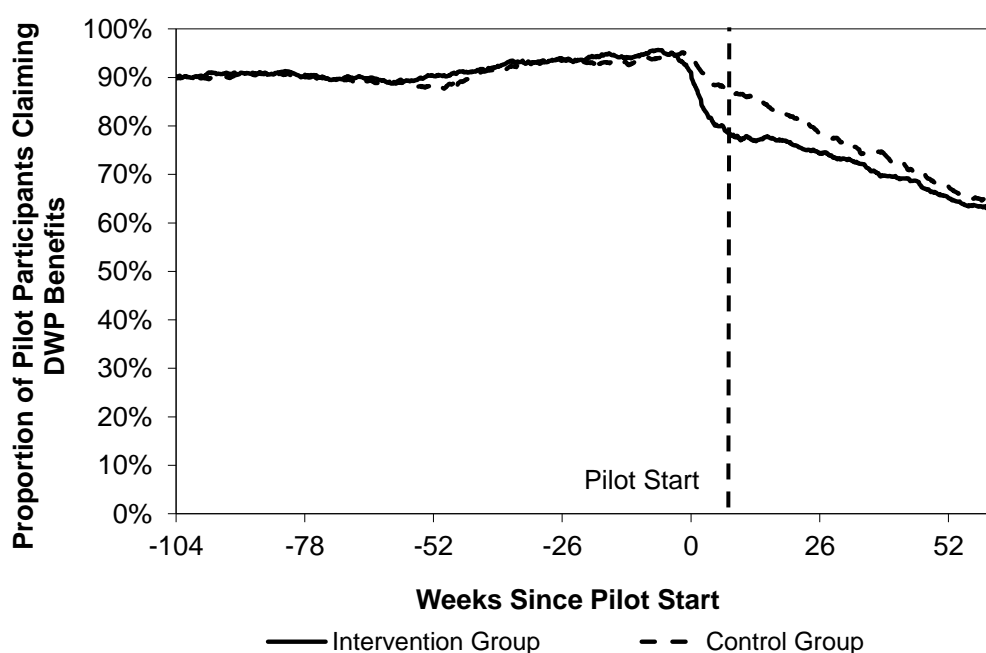
¹ As defined in the Disability Discrimination Act of 1995

*Difference is significant with a 95 per cent degree of confidence.

Source: DWP benefits administrative data: November 2015 and Labour Market System: February 2016

If we now look at the trial participants' presence on benefit before being recruited onto SJP (Chart 2.7) we see very little difference between the intervention and control group. That said, given that these people are WP completers (who did not get a sustained job) most people will have spent most of their time over the prior few years on DWP benefits, leaving little room for a difference to exist (hence the 87 per cent to 95 per cent level of benefit receipt in Chart 2.7). Note that benefit receipt is not at 100% prior to the pilot because people can have short breaks in their claim and re-join their claim journey at the same stage. Regardless, we have checked most of the data at our disposal and have identified little that would lead us to suspect that the outcome data is biased.

Chart 2.7 Proportion of Post-WP pilot participants on DWP benefits



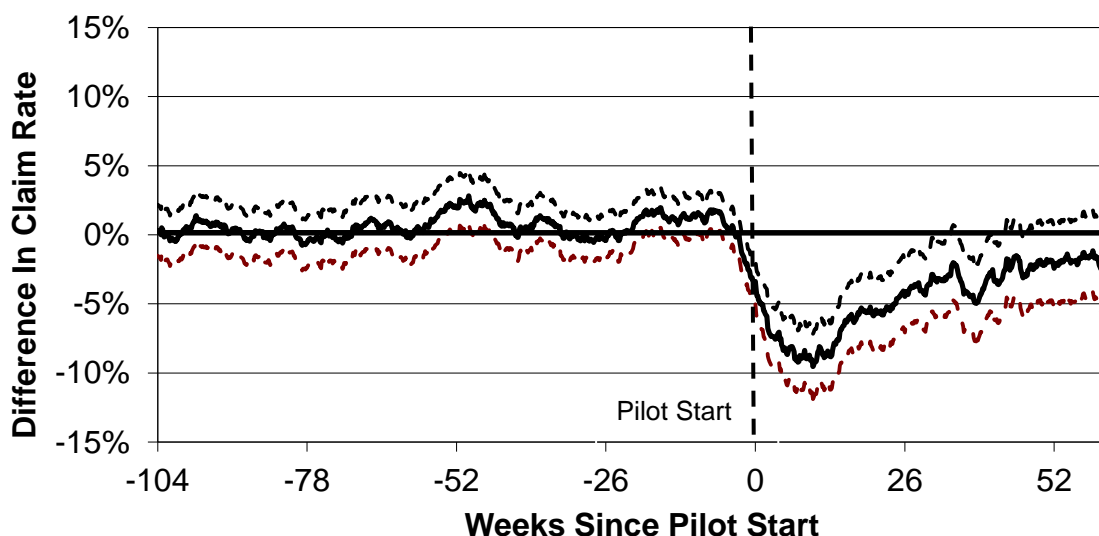
Source: DWP Benefits Administrative Data: November 2015

Supervised Jobsearch Pilots Impact Assessment

Chart 2.8 shows the post-pilot difference in presence on benefits. The chart also shows the pre-pilot difference in order to confirm that the post-pilot differences do represent a real change in participant behaviours and not just a continuation of a prior trend. An impact emerges approximately three weeks prior to the date when the LMS marker was first set. At first glance this might seem as though the trial has a deterrence effect. A letter was sent out prior to the pilot informing people of their potential inclusion, but this was sent to both the intervention and the control groups (i.e. they had not been allocated at that stage), so any deterrent effect should have impacted both groups equally and not manifest as a pre-pilot difference in claim rates.

A simple explanation is that some markers were being set late. We do not know whether this was the case but we raise the possibility to acknowledge that there are various explanations for the early onset of the impact and in the absence of any direct evidence for the cause we leave this question open.

Chart 2.8 Post-WP Benefit Impact



Source: DWP Benefits Administrative Data: November 2015

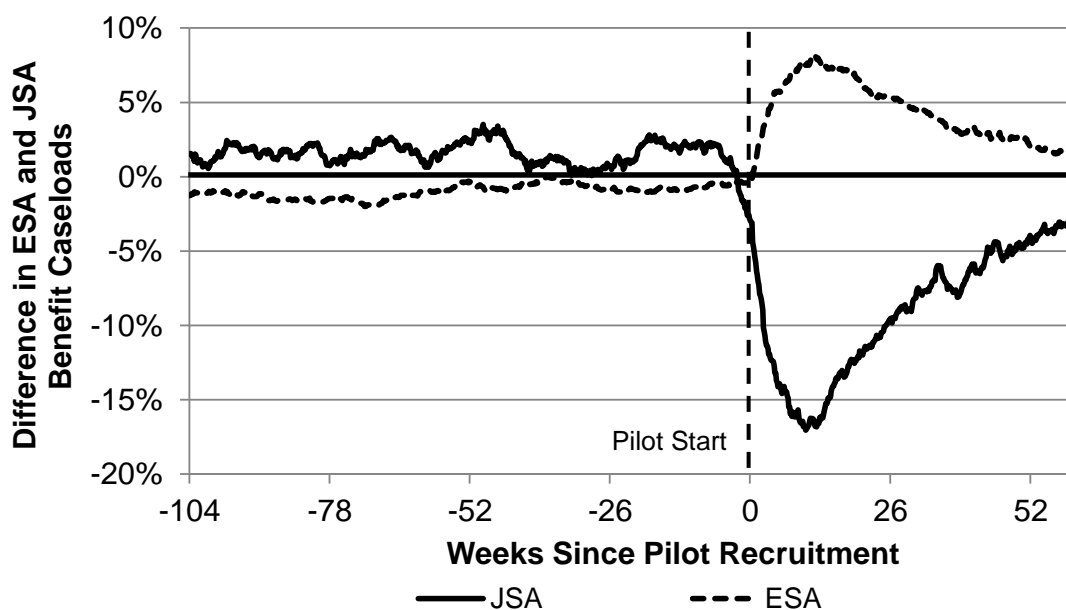
Taking the data on trust, we would conclude that the pilot impact begins shortly before the intervention and extends to over one year after the intervention (we track people up to 59 weeks), with a cumulative effect of 19 days off benefit to date. Given that the impact has not quite reduced to zero in the time window shown in Chart 2.8 the total impact might be expected to increase in due course. However, we extrapolated the apparent impact and did not estimate an overall impact much larger than that quoted here. The margin of error, measured from the point at which an impact emerges, is eight to 30 days off benefit.

Supervised Jobsearch Pilots Impact Assessment

As with the pre-WP impact we measured the additional days off benefit from the point at which a difference between the intervention and control group emerges and not when the daily impacts were statistically significant. However, unlike the pre-WP pilot, we do not track the post-WP impact from the point of claim because this is a very long time in the past and for long term claimants the claim duration is not as strong an influence upon the likelihood of leaving benefits. Further, we observed no inconsistency in the claim histories between the intervention and the control group, but we did for the pre-WP pilot and that was the motivation for the approach we took when measuring the pre-WP pilot impact.

Chart 2.8 shows the net impact upon all DWP primary benefits. We explored the impacts specific to the different DWP benefits. Chart 2.9 shows the main features of that analysis, which are limited to the JSA and ESA benefits. The chart shows that the impact upon JSA (i.e. fewer people on JSA) is partly offset by an opposite impact upon ESA (i.e. more people on ESA). At its peak, the impact is -17ppts for JSA and + eight ppts for ESA, leading to the overall impact of – nine ppts shown in Chart 2.8. Expressed in terms of days off benefit, a JSA impact of 38 days has been offset by an opposite ESA impact of 18 days and small number moving to other benefits.

Chart 2.9 Difference in the proportion of post-WP pilot participants on JSA and ESA.



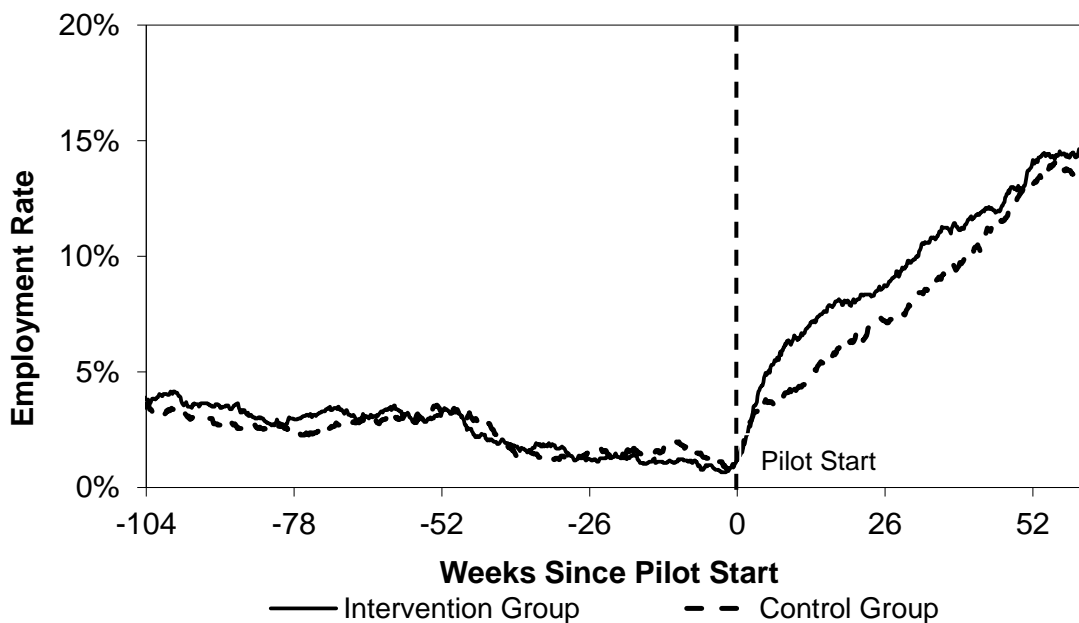
Source: HMRC P45 data: February 2016

Supervised Jobsearch Pilots Impact Assessment

Clearly then, some post-WP pilot participants are moving to ESA in response to the pilot intervention. Those with a health condition may choose whether to claim ESA or JSA. If they claim ESA they will have a Work Capability Assessment and the outcome of this assessment will decide whether they are eligible for ESA or not.

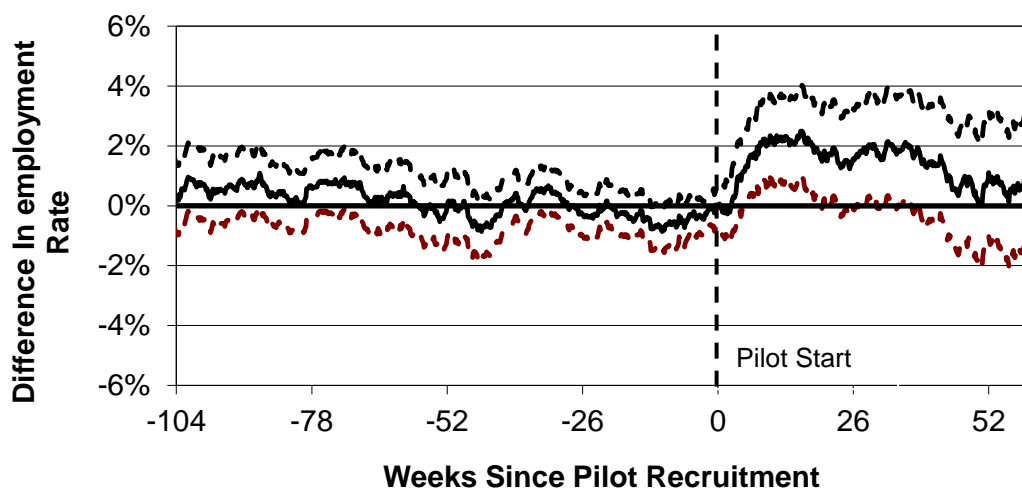
To complete the analysis of the post-WP pilot's impact, Charts 2.10 and 2.11 respectively show the measured employment rates as inferred from HMRC's P45 data and the differences between these rates. We reiterate earlier comments on the quality and completeness of our employment data, but given that shortcoming, Chart 2.11 does broadly corroborate the benefit impacts. We observe an insignificant difference in the pre-pilot employment histories (possibly because most participants were claiming JSA for that period of time, as evidenced by Chart 2.7). After the pilot period, the cumulative impact to date is an extra 5.7 days in employment (with a margin of error of -1.5 to 13 days) and as with the benefit impact, we might expect this to increase when we can track participants for longer periods of time but only very marginally.

Chart 2.10 Post-WP Employment Rates



Source: HMRC P45 data: February 2016

Chart 2.11 Post-WP Employment Impact



Source: HMRC P45 data: February 2016

2.3 Analysis of Sanctions

We complete this report with a short account of sanctioning activity during the pilot period. An intuitive definition of the ‘pilot period’ for the intervention group is the 13 weeks that they were attending the programme. However the control group did not join the programme so we have no equivalent period for that group. Around 90 per cent of SJP participants started the programme within four weeks of the LMS pilot marker being set so to ensure that we track both the intervention and the control groups for a consistent and meaningful period of time we measured sanctions incurred during the 17 weeks following the setting of the LMS pilot marker. The 17 weeks comprises four weeks to account for the time lag between setting the marker and joining SJP plus 13 weeks to account for the maximum duration of the programme.

Using this definition of the pilot period, Table 2.3 shows the proportion of the intervention and control groups who had a sanction applied to their claim. Proportions are shown for both the pre-WP pilot and the post-WP pilot and, for each, proportions are shown for the intervention and control groups. It is important to note that the figures in Table 2.3 are somewhat arbitrary. The time period over which we track sanctions is an approximate reflection of the time that participants were engaged with the pilots. The figures may not capture every single sanction associated with the pilots and conversely they may capture some sanctions that are not associated with

Supervised Jobsearch Pilots Impact Assessment

the pilots. Further, the figures are not comparable with any other measure of sanction rates.³

Table 2.3 Proportion of Pilot Participants with a Sanction

	Pre-WP		Post-WP	
	Intervention	Control	Intervention	Control
Proportion With a Sanction	16.2%	7%	16.9%	6.2%

Source: DWP Decision Maker and Appeals System: August 2015

Table 2.3 shows a clear increase in sanctions amongst the intervention groups when compared to the controls. The difference is a factor 2.3 and 3.7 in the level of sanctions for the pre-WP and post-WP pilots respectively. Most of the sanctions are due to failing to attend SJP and many have been incurred by people who did not actually start SJP (according to our interpretation of the PRaP data).

Some sanctions will have led to a JSA claim being closed so some of the difference in benefit receipt will be due to the increased level of sanctions. The impact specifically due to sanctions cannot be truly disentangled from the pilot intervention. This is partly because the increased level of sanctions is a consequence of taking part in the pilot (though not an inevitable one). Also, whilst we can identify which participants left benefits following a sanction we cannot say how long they remained off benefits solely because of the sanction and therefore we cannot measure how many of the additional days off benefit are specifically due to the additional sanctioning activity.

We did check prior sanctioning activity to rule out the possibility that the observed post-pilot difference was due to pre-existing behaviours. For the post-WP pilot participants, prior to pilot participation there is no evidence of a greater prevalence of sanctions amongst the intervention group compared with the control. For the pre-WP pilot participants the same is true during the six months before pilot participation. Sanction rates do diverge before that (with the intervention group having a higher

³ DWP publishes JSA sanction rates via its website here: <https://www.gov.uk/government/collections/jobseekers-allowance-sanctions> However, because these figures cover different types of JSA claimant and different time periods they do not provide a suitable context for the analysis presented in this report.

Supervised Jobsearch Pilots Impact Assessment

propensity to incur a sanction), so some of the pre-WP pilot difference in sanction rates may be due to innate behavioural differences, but these behaviours were not displayed in the six months before pilot participation and therefore would need to have been 'triggered' by the specifics of the SJP regime.

We note that the figures in Table 2.3 are based upon the same ITT approach that we took with the benefit and employment impact analysis. In other words, some of the people in our intervention group did not actually attend the SJP programme. This means that sanctions were not just incurred by those who attended the programme. Rather, some of the sanctions will be due to people who never attended the programme.

3 Cost Benefit Analysis

This short section of the report summarises our best estimates of the costs and benefits of SJP. We have followed the standard DWP framework for estimating net benefits and have used generalised (to the wider JSA client base) assumptions for metrics such as earnings and transport or childcare costs. Those assumptions relate to the 2014/15 financial year which most closely aligns to the pilot period. We do not have information on the specific earnings and outgoings of the pilot participants so it is possible that the standard assumptions are not reflective of SJP participants. Annex C gives further detail on the assumptions underpinning the cost benefit framework so we do not repeat that content here.

We also note that three different providers successfully bid for the SJP contracts and each charged a different amount for their services. Each provider operated in a different area and therefore the overall SJP unit cost is in part a function of the geographical distribution of the pilot referrals. This means that the actual costs during the pilots may not be reflective of any future costs should the SJP programme be provided more widely.

These caveats accepting, we have estimated an average unit cost of the pilots. Combined with the central estimates of the impacts upon benefit receipt and employment that we outlined earlier, Table 3.1 summarises the net benefits implied by DWP’s cost benefit framework and assumptions.

Table 3.1 Net Benefits of the SJP Pilots

	Net Benefit to the Participant	Net Benefit to the Exchequer
Pre-WP Pilot	£50	-£1700
Post-WP Pilot	£150	-£1600

The figures in Table 3.1 have been rounded to the nearest £50. Distributional weights have been applied to the participant benefits. This assumes that people on lower incomes (and this will be true of most of the people who benefited from SJP) value a marginal increase in their income more than people on higher incomes. On the basis of this and the other assumptions underpinning the cost benefit framework, we estimate that SJP participants are better off for the pre-WP and post-WP pilots. This is because the additional wages received by participants are expected to outweigh the costs associated with a net reduction in benefit receipt and net increases in tax liabilities, travel and childcare costs. The assumption that participants value

Supervised Jobsearch Pilots Impact Assessment

additional income more highly than the average taxpayer means that the programme is also estimated to result in a net redistributive benefit of £50 and £150 for the pre-WP and post-WP pilot participants respectively.

Table 3.1 however, suggests that there is a net loss to the Exchequer of £1700 and £1600 per pre-WP and post-WP pilot participant respectively. This is because the programme costs are expected to be greater than the benefits associated with any net increase in tax revenues and net reductions in benefit expenditure, operational costs and NHS expenditure.

4 Conclusion

In conclusion, we have identified various ways in which the implementation of SJP appears to differ from the nominal design, namely:

- We have significantly fewer people in the pre-WP intervention group compared with the pre-WP control group.
- A small number of people have been allocated to the wrong (intervention or control) group.
- We have people who on the basis of their LMS marker should have joined the programme but did not and people who on the basis of their LMS marker should not have joined the programme but did,.
- Some LMS markers indicate that someone was at the pre-WP stage in their claim whereas DWP's administrative data suggests they were in the post-WP stage (and vice versa).

Nonetheless, we believe that we can discern an impact upon the DWP primary benefit caseload amounting to an average ten days off benefit per participant in the pre-WP pilot. The pre-WP pilot employment impact is harder to discern but we estimate that the intervention group have spent an additional five days in employment on average. The pre-WP pilot impacts appear comparatively short lived and our analysis suggests that we have measured the totality of those impacts.

We estimate that the post-WP pilot has led to participants spending 19 fewer days on DWP benefits in the 59 weeks following the nominal pilot start date. However, a JSA impact of 38 fewer days has been offset by an opposite impact of 18 more days for ESA (with the one ppt difference to the overall off benefit impact of 19 days being accounted for by rounding and other benefits). We measure an employment impact of 5.7 days over the same time period. The post-WP pilot impact has so far not reduced to zero, but we expect further increases to be only very marginal.

Participation in SJP is associated with higher levels of sanctions, with 16.2 per cent of the pre-WP intervention group incurring a sanction during the pilot period compared with 7 per cent of the control group. The equivalent figures for the post-WP pilot are 16.9% and 6.2% respectively. This may be, in part, due to the greater demands of the programme compared with the 'business as usual' requirements.

Supervised Jobsearch Pilots Impact Assessment

Finally, whilst there appears to be a net financial benefit to the participants, the costs of running the SJP programme are not compensated for by the reduction in benefit expenditure and increased tax returns.

Annex A: Overview of the Pilot Recruitment Process

1. The pilots operated in four Jobcentre Plus (JCP) Districts though one of these (West Yorkshire) did not take part in the pre-Work Programme (WP) pilot. Those Districts were:
 - Surrey & Sussex
 - Black Country
 - Mercia
 - West Yorkshire
2. All sites in the Mercia District took part in the trial apart from Rugby Jobcentre. This was because that site has rolled out Universal Credit (UC) and therefore was not a suitable location for a Jobseeker's Allowance (JSA) pilot. Similarly, not all sites in West Yorkshire took part in the pilots. The sites that did are listed in the footnote.⁴ All Surrey and Sussex and Black Country sites took part in the trial.
3. Referrals to the SJP pilots began on 29 September 2014 and finished on 12 December 2014. The final referral date was to ensure that all spells on the pilots had started by 19 December 2014 so that the pilots could end by 31 March 2015.

The Pre-WP Pilot

4. Eligibility within the pre-WP pilot was dependent upon the claimant's age. People aged 18 to 24 were considered for recruitment during weeks 20 to 24 of their JSA claim. Those aged 25 or over were considered for recruitment during weeks 33 to 37 of their JSA claim. Also, claimants must not have previously attended the WP and nor should participation in the pilot have delayed any later referrals to the WP. Over and above these specific criteria there were other, more subjective criteria:

⁴ Bradford Eastbrook Court, Bradford Westfield House, Guiseley, Keighley, Leeds Eastgate, Leeds Park Place, Leeds South, Leeds Southern House, Morely, Pudsey, Seacroft and Shipley.

Supervised Jobsearch Pilots Impact Assessment

- Claimants should have needed more intensive support and assistance, and should have benefited from more intensive assistance with job applications.
 - The pilot was intended to focus on those claimants not taking sufficient and effective steps to secure employment (indicated by a lack of recent work history and failing to secure job interviews).
 - The pilot also intended to focus on those claimants who had a history of benefit sanctions for Actively Seeking Employment (ASE) or Fail To Attend (FTA).
5. Where somebody did meet the above criteria it was acknowledged that the pilot may nonetheless have been unsuitable for them. The 'unsuitability' criteria that Work Coaches were asked to assess people against were as follows:
- UC and 'New-style' JSA claimants (the pilots were for JSA claimants only);
 - JSA credits-only claimants;
 - Multi Agency Public Protection Arrangements cases;
 - Special Customer Records cases;
 - Postal Work Search Review claimants;
 - Claimants who were pregnant;
 - Claimants who were not required to meet availability and/or actively seeking employment, or were being treated as having met them;
 - Claimants who were participating (or awaiting placement) in Work Choice/Residential Training;
 - Claimants who were participating in other provision (e.g. New Enterprise Allowance, English Language, IT skills) to which they had been referred by JCP, the timing of which meant they would not have been able to complete 13 weeks of SJP prior to WP referral or the end of the pilot.
 - Those who were undertaking voluntary or part-time work;
 - Claimants for whom travelling to the SJP provider premises would have been outside their daily travel to work time – 90 minutes each way unless restrictions had been agreed;
 - Claimants in Mercia District who were participating in another co-located pilot (the 18-21 year-old pilot). This applied to pre-WP claimants only.
6. As well as the unsuitability criteria listed above Work Coaches were allowed to exempt a person from the pilot if they presented 'exceptional circumstances' and after having consulted with their Line Manager. Where a

Supervised Jobsearch Pilots Impact Assessment

person was not deemed suitable for the pilot or a Work Coach decision was made to exempt them then that person progressed no further in the trial process and they continued with their existing programme of support.

The Post-WP Pilot

7. Initially, the post-WP pilot eligibility required that 26 weeks had elapsed since their Work Programme Completer Interview (WPCI) before people came in scope of the pilot. Further, participants should have been receiving 'Post Work Programme Support' or 'Help to Work'. However, owing to low numbers of referrals to SJP, from 27 October 2014 this criterion was expanded to include people who had had their WPCI more than 26 weeks beforehand. Originally, the earliest date of participants' WPCI was 7 April 2014 but this was pushed back to 1 December 2013. To ensure that the additional participants were as similar as possible to the original participants, the WPCI attendees were recruited in reverse order of attendance. That is, those who attended their WPCI closest to the 26 week point were considered for recruitment first. Unlike the pre-WP pilot, Work Coaches did not have more subjective eligibility criteria relating to the need for additional job search support (as set out in the bullet points in paragraph 4).
8. The unsuitability criteria listed in paragraph 5 also applied to the post-WP pilot, except of course the exclusion of '18 to 21 year-old' pilot participants in the pre-WP pilot (the final bullet point in paragraph 5). As for the pre-WP pilot, Work Coaches had the discretion to exempt people from the pilot where they presented exceptional circumstances.

On the Random Allocation

9. SJP was a Randomised Controlled Trial (RCT) so where a person was considered suitable for the trial they were then randomly allocated to either the trial 'intervention' group or a 'control' group, with the control group experiencing the 'business as usual' level of conditionality and employment support. This is an important consideration because whilst the impact measured with respect to the control group may have good 'internal validity' one cannot assume that impact would apply more widely to, for example, people who are at an earlier stage in their claim, people who do not have a history of benefit sanctions for ASE or FTA, or people who are a lot further into the post-WP stage of their claim.
10. Once deemed suitable for the pilots, participants were randomly allocated to either an intervention or a control group. The allocation was done on the basis of the last three digits of the National Insurance Number (NiNo) on the

Supervised Jobsearch Pilots Impact Assessment

assumption that this number bears no relationship with any participant characteristic and no pilot outcome. This assumption can be confirmed by inspection of the characteristics of the intervention and control groups, which we cover in the main body of this report.

11. For the majority of the recruitment period the random allocation was done on a 50:50 basis, save random fluctuations around this figure, i.e. half of all eligible participants were put in an intervention group and half in a control group. However owing to low numbers of referrals in the first part of the recruitment period, from 20 November 2014 onwards people in the post-WP stage of their claim who had a NiNo that ended in a number from 000 to 749 were allocated to the intervention group of the post-WP pilot and the remainder allocated to the control group of the post-WP pilot.

Annex B: An Assessment of the Pilot Participant Data

1. This Annex summarises the data that defines which individuals participated in the pilots and when. This information was recorded on a case management system called the 'Labour Market System', which is how JCP Work Coaches administer some elements of benefits conditionality and employment-related support. We initially characterise the LMS data that identifies the pilot participants and we then compare that data with other sources associated with pilot eligibility such as the underlying DWP benefits data and information on WP participation.
2. Once a pilot recruit is allocated to an intervention or control group an LMS marker should be set in order to record the group that each person has been put in. The marker also records whether the participant was in the pre-WP or post-WP pilot and whether the person was considered for inclusion but deemed unsuitable for the pilots, though the marker does not record the reason why the person was deemed unsuitable.
3. We examined those markers with a view to identifying a group of intervention and control individuals and the point in time when they were recruited onto the pilots. For brevity, in some parts of this annex we have pooled the pre-WP and post-WP pilot data and present the data as a single population of records. However, when discussing the recruitment process and pilot impacts we return to considering the pilots separately.
4. The marker data used by this analysis comprises 10,025 records covering all pilot groups. 8,019 individuals are represented in the data because 1,475 people had two or more records. This 1,475 had between them 3,474 records. A number (323) of the duplicates indicated no material change. The remaining duplicates were due to:
 - Changes on the same day, which probably do not indicate any real change in status, rather, just a correction to the marker: 254 (15 per cent).
 - Markers being moved on to a 'Break/Exit' or 'Transfer' (even when the marker was formerly 'Not Suitable'): 590 (34.9 per cent).

Supervised Jobsearch Pilots Impact Assessment

- People changing from an intervention group to a control group, or vice versa: 78 (4.6 per cent).
 - Markers changing from pre-WP to post-WP, or vice versa: 43 (2.5 per cent).
 - Markers being moved on to 'Unsuitable' after having been set to intervention or control: 340 (20 per cent).
 - The remaining 385 (23 per cent) duplicates are for various changes in the marker that do not seem particularly meaningful, such as movements from a 'Work Coach Decision' to 'Unsuitable' or from 'Exit/Break' to 'Transfer'.
5. We assume that changes in the LMS marker are due to JCP staff having improved information about an individual or there has been a change in their situation and therefore we take the most recent record for an individual as indicating their intended status.
6. Table B.1 summarises which group and which pilot each participant was put in according to the LMS marker data when using the assumptions mentioned above. We have combined 'Unsuitable' cases with 'Work Coach Decision' cases because both represent a form of exemption, though we acknowledge that there are significant differences to the interpretation of these categories. For clarity, where a person was deemed unsuitable or had a Work Coach decision to exempt them from the trial, after having been put in an intervention or control group, we remove them from that intervention or control group.

Table B.1 Pilot and Group of SJP Participants

	Intervention	Control	Unsuitable
Pre-WP Pilot	415	721	1003
Post-WP Pilot	1958	1980	1942

Source: LMS Bespoke Scan

7. In the pre-WP pilot, 36.5 per cent of people are in the intervention group and 63.5 per cent are in the control group. 36.5 per cent is lower than we would expect due to random variation alone. To some extent this difference could be due to SJP participants being deemed unsuitable after having been recruited to an intervention group (marker movements from an intervention group to 'Unsuitable' or 'Work Coach Decision' outnumber similar movements from a

Supervised Jobsearch Pilots Impact Assessment

control group by four to one). However, it is also possible that the trial recruitment process has also been exposed to selection bias. The potential consequence of this bias is explored in the main body of this report.

8. 49.7 per cent of the people in the post-WP pilot are in the intervention group, which would be very much in line with a 50:50 intervention to control group allocation ratio. However, as stated in Annex A, the random allocation split was changed to 75:25 in favour of the intervention group mid-way through the post-WP recruitment so we would not expect the split we observe in the post-WP pilot.
9. Tables B.2 and B.3 summarise the extent to which people appear to have been put into the nominal (intervention or control) group in the pre-WP and post-WP pilot respectively. The row headings in the tables show which group a person should have been put in on the basis of their NiNo and the column headings show which group people were actually put in. The figures in the tables suggest that, for the pre-WP pilot 5.3% of the people who should be in the intervention group were in fact put in the control group and 5.7 per cent of the people who should have been put in the control group were put in the intervention group. The equivalent figures for the post-WP pilot are 3.4 per cent and 3.9 per cent respectively. These misallocation rates provide scope for biases to enter the participant data but as we explain shortly our approach to the analysis is able to reduce this risk.

Table B.2 Comparison between nominal and actual group allocation for the pre-WP pilot

	According to LMS Marker			
According to NiNo		Intervention	Control	Unsuitable
	Intervention	373	21	583
	Control	42	700	420

Source: LMS Bespoke Scan

Table B.3 Comparison between nominal and actual group allocation for the post-WP pilot

	According to LMS Marker			
According to NiNo		Intervention	Control	Unsuitable
	Intervention	1881	67	1372
	Control	77	1913	570

Source: LMS Bespoke Scan

Supervised Jobsearch Pilots Impact Assessment

10. The LMS markers, at face value, tell us which pilot each person was recruited in to. However, we can check whether that person was in the right stage of their claim and at the right age for that marker. Table B.4 summarises the proportion of markers that did agree with the stage of the claim and the age that we measured from DWP's administrative data.

Table B.4 Proportion of LMS Markers that are Consistent with the Underlying Administrative Data

	Recruitment Policy			
	Pre-WP 18 to 24	Pre-WP Over 25	Post-WP Original	Post-WP Revised
Surrey & Sussex	98%	97%	95%	96%
Black Country	99%	95%	97%	97%
Mercia	100%	99%	92%	96%
West Yorkshire	-	-	98%	99%
All	99%	96%	96%	97%

Source: DWP Administrative Data: November 2015 and Bespoke LMS Scan

11. In the main, the markers do seem to be consistent, though there are exceptions. Mercia's post-WP participants who fulfilled the original criteria were least likely to have consistent markers. A small proportion of people in West Yorkshire had pre-WP markers even though that District was not recruiting from that stage in the claim.
12. We next examine the extent to which people appear to have actually participated in SJP. In principle, this should be indicated by a record within the Provider Referral and Payment system (PRaP). We looked at all PRaP records that related to a SJP contract. We checked both whether a person appears to have started the programme and whether a payment was made. We found nine cases where a person appears to have started SJP but no payment was made. We nonetheless include these people amongst our 'verified' cases because there is at least *some* evidence of a start. Regardless, their small numbers will not significantly change our conclusions.
13. We also found a small number of PRaP starts or payments when we would not expect to on the basis of the LMS marker (because they had a 'Control', 'Unsuitable' or 'Work Coach Decision' marker). Table B.5 summarises the degree of consistency between the LMS and PRaP data by showing the proportion of people in each pilot and group with a PRaP start or payment.

Supervised Jobsearch Pilots Impact Assessment

14. A sizeable number (1722) of all the people with an LMS marker did have some presence in PraP that was associated with the pilot contract, but they did not appear to start SJP proper. We do not know the reason for this in most cases – where a reason is recorded on PRaP, that reason simply restates the fact that SJP was not attended, not started or in a small number of instances rejected. Benefit exits may have accounted for some of these cases, but it is unlikely to be more than one-third to a half of all cases.

Table B.5 Proportion of Pilot Participants with a PRaP Start and/or Payment

	Pre-WP	Post-WP
Intervention	61%	77.6%
Control	0.4%	0.4%
Unsuitable	1%	1%

Source: PRaP Data: February 2016 and Bespoke LMS Scan

15. Taking this information together with all the previous analysis, we are left with a choice as to how to frame the question of the pilots’ impacts. In short, we choose to:

- Allocate people into the group they are nominally in on the basis of their NiNo. The reason for this is to reduce any selection bias that might have been introduced into the intervention and control groups as a result of incorrect allocations.
- Exclude ‘Unsuitable’ and ‘Work Coach Decision’ cases from both the intervention and the control group. The reason for this is to ensure that as many of the people in the intervention group have actually undergone the intervention. There is some evidence that the unsuitability and Work Coach decision criteria may have been applied differently to the intervention and control groups. However, the differences that we observe may be due to unobserved behaviours such as LMS markers not being set in the first place, so we take on trust that no intervention or control group participants in our ITT analysis fulfil the unsuitable or Work Coach decision criteria.
- Allocate people to the pilot that they appear to be in on the basis of DWP’s administrative data and not the LMS marker data. The reason for doing this is because we believe that it is more likely that the marker

Supervised Jobsearch Pilots Impact Assessment

has been set incorrectly than it is that the administrative data is incorrect.

- Include people in the intervention group who may not have actually started SJP and leave in the control group people who appear to have taken part in SJP. The reason for doing this is because it is likely that the reason for not attending (in respect of the intervention group) and attending (in respect of the control group) SJP is likely to be related to some characteristic associated with the likelihood of a labour market outcome. Therefore, to remove these anomalous cases risks introducing differences in outcomes that are not due to the trial intervention.

16. We have described in the above a so called 'Intention to Treat' (ITT) approach, which simply means that we base our analysis upon the nominal design of the trial and not the way it was actually implemented. So, for example if someone was at the post-WP stage and should have attended SJP, in the analysis we treat them as though they were on SJP even if in actual fact they did not undergo the intervention. Our approach is the most likely to be bias-free but it has the consequence that 44 per cent of the people in our pre-WP intervention group and 25 per cent of the post-WP intervention group did not actually attend SJP (according to our interpretation of the PRaP administrative data). Conversely, 4.1 per cent of the pre-WP control group and 3.7 per cent of the post-WP control group did attend SJP. These figures are different to those in Table B.5 because Table B.5 is based upon the intervention or control group indicated by the LMS marker whereas the figures presented here are based upon the nominal intervention or control group (which is dependent upon the NiNo).

17. It is likely therefore that the impacts we have presented underestimate the true impact (though not necessarily in proportion to the number of discrepancies mentioned above). However, we can be relatively confident that the impacts are due to SJP and not selection biases, which is why the ITT approach is our preferred method.

Annex C: Assumptions Underpinning the Cost Benefit Analysis

1. The cost benefit analysis presented in this report follows the standard DWP Cost Benefit Framework. It is beyond the remit of this report to provide the full detail of that framework. However, we note that the analysis does not account for:
 - The additional leisure time which participants forego (this represents a potential cost to participants and therefore society).
 - The non-pecuniary benefits associated with additional time in unsubsidised employment (these represent a potential benefit to participants and therefore society).
 - The cost of hiring and training incurred by employers (this represents a potential cost to employers and therefore society).
 - The economic multiplier effect which may result from the programme (this represents a potential benefit to society).

2. Table C.1 summarises, for both the pre-WP and the post-WP pilot, the elements of the cost benefit calculations which act to increase or decrease the net benefits to each of the three potential beneficiaries considered.

Table C.1 Drivers of Net Cost/Benefits

Programme Impact	Participants	Employers	Exchequer
Increase in output	0	+	0
Increase in wages	+	-	0
Programme costs	0	0	-
Reduction in operational costs	0	0	+
Reduction in benefit payments	-	0	+
Increase in taxes	-	0	+
Increases in travel & childcare costs	0	0	0
Reduction in healthcare costs	0	0	+
Redistributive costs and benefits	+	0	0
Social cost of Exchequer Finance	0	0	0