Mandating IAP for older New Dealers: an interim report of the quantitative evaluation

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Department for Work and Pensions

Research Report No 362

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Richard Dorsett and Stefan Speckesser

A report of research carried out by the Policy Studies Institute on behalf of the Department for Work and Pensions

Corporate Document Services

First Published 2006.

ISBN 1 84712 044 X

ISBN13 978 1 84712 044 1

Printed by Corporate Document Services.

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Acknowledgements

This report uses data collected in the course randomly assigning individuals to mandatory participation in the Intensive Activity Period of New Deal 25 plus, linked to the New Deal Evaluation Database. The authors gratefully acknowledge the contributions of Department for Work and Pensions (DWP) colleagues, in particular: Tim Conway, Jayne Middlemas and Peter Weller for their ongoing support and guidance, Jenny Crook for her early management contributions and Graeme Connor for developing an excellent randomisation tool on which the entire evaluation relies. Joanna Bramhall of the Jobcentre Plus network provided considerable help with the data. We also thank James Riccio of MDRC and Jeffrey Smith of the University of Michigan for their invaluable comments and consultancy advice.

Abbreviations and acronyms

DiD	Difference-in-Difference
DWP	Department for Work and Pensions
ERA	Employment Retention and Advancement Demonstration project
IAP	Intensive Activity Period on ND25+
IB	Incapacity Benefit
IV	Instrumental Variables
JSA	Jobseeker's Allowance
ND	New Deal
ND25+	New Deal 25 plus
NDED	New Deal Evaluation Database
RA	Random assignment

Summary

Introduction

New Deal 25 plus (ND25+) provides job search assistance, training opportunities and work placements to people aged between 25 and the state pension age who have been claiming Jobseeker's Allowance (JSA) for 18 out of 21 months. It comprises three stages:

- **Gateway** - up to four months of intensive jobsearch assistance;
- **IAP (Intensive Activity Period)** - a variety of assistance (training, work experience etc.) lasting 13 to 52 weeks;
- **Follow-through** - further jobsearch assistance for up to three months.

At present, people aged 50 and over on ND25+ can volunteer to participate in the IAP.

The Pensions Green Paper *Simplicity, security and choice: working and saving for retirement* announced the intention to run a pilot study mandating participation. This has been running since April 2004 in 14 Jobcentre Plus districts. This report documents the evaluation to date of the effect of the mandate. Results are interim at this stage – final results will be available in a later report due in 2008.

Design of the evaluation

The evaluation follows a random assignment (or 'experimental') design. Individuals participating in the pilots are randomly assigned to either an 'action' group for whom IAP participation is mandatory or a 'control' group for whom participation remains voluntary. The strength of this approach is that it permits the most robust evaluation of the effects of the mandate as it avoids the selection bias that can result with alternative approaches – other approaches rely on untestable assumptions. In terms of internal validity (the ability to identify a causal effect) experimental estimates are unrivalled. However, as with any pilot study there is the need to consider how realistic the findings are for non-pilot areas, i.e. do the results have external validity? There are also potential problems that are unique to experiments.

In practical terms, the design of the evaluation was such that random assignment would take place at the time of ND25+ entry. Since individuals find out at the time of random assignment whether they will be required to participate in IAP or whether any participation will be voluntary, it is from this point onwards that the effects of the mandate can be observed. While it is conceivable that the existence of a mandate might affect behaviour before ND25+ entry, discussions with Department for Work and Pensions (DWP) suggest that this is unlikely.

The analysis is based on data collected at the time of random assignment and on the New Deal Evaluation Database. Up until August 2005, 2,622 individuals had participated in the pilots. The total number of participants expected by the end of the pilot period is about 4,000. This is less by a third than original expectations.

The customer group in pilot areas

The following is a summary of the customer group in the pilot areas:

- 75 per cent are men;
- they are fairly evenly spread throughout the 50-58 age range, with a slight dip at age 59;
- 86 per cent are white British, seven per cent are Asian or Asian British;
- 51 per cent have no qualifications; 17 per cent hold higher education qualifications;
- 40 per cent used to work in manual occupations, 32 per cent as semi-skilled or skilled workers and eight per cent in office jobs. There is also a high share of former professionals or managers among the clients – 20 per cent;
- only 29 per cent are partnered. Only ten per cent of all customers live with partners who are working;
- 93 per cent of participants are not working at all at the time of random assignment.

Internal validity of the experiment

As a summary of the tests on internal validity, the randomisation was successful and balanced almost all observable characteristics between both groups. This provides some reassurance that the control group is well suited to providing a counterfactual outcome for the action group. There are some slight differences at the level of the individual Jobcentre Plus district which may disappear as the sample size increases.

External validity of the results

Pilot areas account for only one-fifth of the target population nationwide. A comparison of pilot and non-pilot areas shows customers in pilot areas are older, they less frequently report disabilities and are more often white. Also, they tend to remain longer on benefit before entering the Gateway. It will be important to take account of these differences when considering the possible effect of extending the mandate to non-pilot areas.

There are also significant differences between pilot and non-pilot areas in the type of option implemented. This reduces the scope for generalising the results and may mean that the estimated effects would not apply to all areas but only those implementing IAP in a similar way to the pilot areas.

There are some indications of implementational deviations from the design of the experiment. Many eligible customers are not randomly assigned at all and some are assigned long after entering ND25+. This is potentially problematic when seeking to generalise the results since it is not currently possible to observe why such deviations occur. The consequence of late random assignment is that the role of anticipatory effects while on the Gateway is reduced. The estimated treatment effect is less likely to correspond to the effect which would be found if the mandate were introduced nationally since a nationwide implementation would affect the whole period on ND25+, rather than just the post-randomisation Gateway.

There are also deviations from the design of the mandate. In particular, while those who have previously participated in ND25+ after the age of 50 and those who do not have the required 18 months JSA spell should be excluded from the mandate, this is often not upheld in practice. However, these are deviations that are also likely to occur elsewhere were the mandate extended beyond the pilot areas so, rather than being problematic, they are likely to permit estimates to take better account of possible real-world imperfections in implementation.

Evaluation results – simple comparisons

Simple comparison revealed differences between the action and control groups in the ND25+ experience:

- Most participants of both the action and control groups leave the Gateway within seven months of random assignment. Action group members spend, on average, 20 days longer on the Gateway than those in the control group who voluntarily enter IAP.

- The share of participants in IAP is much bigger for the action group than for the control group. In addition, the action group were slower than the control group to return to regular signing for JSA.

- The employment outcomes are different for both groups, and the action group shows a higher share of participants starting unsubsidised employment after randomisation than the control group.

Evaluation results – regression analysis and duration analysis

The effects on unsubsidised employment and claiming non-JSA benefits were estimated using both regression analysis and duration analysis. An important caveat to the results is that they consider only the immediate destination on leaving ND25+ and do not allow for the possibility that individuals change their status after this time. The consequence of this is that the true effects are likely to be smaller than the results presented in this report. It has already been noted that these are only interim results; for the final report, it will be important to address such shortcomings in the data.

Regression analysis results

The estimated overall effect on unsubsidised employment is positive, suggesting that mandating IAP increases the outflows to employment. Thirty-six weeks after random assignment, those mandated to participate in IAP are five percentage points more likely to have found work than those for whom IAP is voluntary. This rises to about eight percentage points after about a year, although this is based on the small number of individuals for whom outcomes of this length can be observed. There appears to be some effect on claiming benefits other than JSA, although this is only significant between 24 and 27 weeks after random assignment, at around three percentage points.

There is some variation by Jobcentre Plus district. Taking the three biggest districts, the evidence suggests that mandating IAP has beneficial employment effects in Coventry and Warwickshire and Essex, without the adverse effect of moving individuals to other benefits. In Leicestershire, it appears that mandating IAP causes individuals to claim other benefits but not to enter unsubsidised employment.

There is also variation by level of qualification. The results show that mandating IAP appears to work well for those with no qualifications and those with GCSE or equivalent qualifications; they become more likely to find unsubsidised employment and no more likely to claim non-JSA benefits. It works poorly for those with qualifications equivalent to NVQ level 1; they tend to avoid employment and move to a non-JSA benefit as a result of being mandated to IAP. The outcomes of those educated to A-level standard or higher appear unaffected by the mandate.

Duration analysis results

Duration analysis is used as an alternative modelling approach that avoids the limitation with the regression analysis results that the longer-term outcomes can only be modelled for that subset of the sample who are observed sufficiently long after randomisation. The results of the duration analysis broadly agree with those of the regression analysis and provide some reassurance that those earlier results are not being driven by cohort effects. However, there are some differences, particularly when considering variations in the treatment effect across those with different levels of qualification. It was only among those with no qualifications that a significant employment effect of mandating IAP was found. Other individuals (apart from those with higher education qualifications) were more likely to claim a non-JSA benefit as a result of the mandate.

Conclusion

The 2006 welfare reform Green Paper *A new deal for welfare: empowering people to work* announced the intention to roll out mandatory IAP for the over-50s nationally. The results in this report provide some early evidence on the effect of mandating IAP. On the basis of the data available, the indications are that mandating IAP increases movements into employment, but has a weaker effect on movements from JSA to other benefits. The probability of moving from ND25+ to unsubsidised employment within a year of entering ND25+ was estimated at just below 29.8 per cent for those mandated to participate in IAP and 23.4 per cent for those not mandated – a difference of 6.4 percentage points. Corresponding estimated probabilities for movements to non-JSA benefits are 16.4 per cent and 11.3 per cent. The probability of moving to IB within a year of starting ND25+ was estimated at 13.9 per cent for those mandated and 10.1 per cent for those not mandated. However, this difference was only marginally significant. The results also suggest variation in effectiveness across region and level of qualification.

While the evaluation appears to be progressing well overall, two recommendations follow from the results presented:

- the reason for implementation of the experiment deviating from the design should continue to be investigated and proper implementation encouraged where appropriate for the remaining duration of the pilots; and
- adequate data should be made available to investigate outcomes beyond initial ND25+ exit. This additional data should include administrative benefit records and could also include administrative records on employment spells and even survey data, if that were felt to be appropriate.

1 Introduction

1.1 Policy background

The intention to run a pilot study mandating participation in the New Deal 25 plus (ND25+) Intensive Activity Period (IAP) for people aged 50-59 who have been claiming Jobseeker's Allowance (JSA) for 18 months was announced in the Pensions Green Paper *Simplicity, security and choice: working and saving for retirement* (December 2002). Long-term unemployed jobseekers aged 25 to 49 are already required to participate in the IAP because it offers extensive help back into work. At present, New Dealers aged 50 and over can volunteer to take up this extra help. However, many choose not to, perhaps reflecting their demoralisation about the chance of returning to work.[1] The aim of the pilot study is to assess whether mandatory participation can assist in the return to work.

1.2 An overview of ND25+

The ND25+ programme is focused on jobseekers aged between 25 and the state pension age. Like the other New Deal (ND) programmes, it aims to encourage jobseekers to improve their jobsearch efforts and at providing them with necessary skills, opportunities and motivation. Individuals must join the programme if they have been unemployed and claiming JSA for 18 of the previous 21 months. In some cases, participants may join the programme earlier. Individuals receiving Pension Credit may also volunteer to join.

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1 The Department for Work and Pensions (DWP) Tabulation Tool shows that, in November 2005, of those ND25+ participants aged 50-59, only 12 per cent were on the IAP.

Like other ND programmes, ND25+ consists of three stages:

1 After an initial interview, participants on ND25+ enter the **Gateway**. This usually lasts up to four months. During this time, individuals are provided with intensive assistance in their job search process.

2 Those not successful in finding employment in the Gateway period can subsequently start the **IAP**. A variety of assistance is offered under the IAP including: basic skills support, work focused training, work experience, work placements, jobsearch skills and other support. It typically lasts 13 weeks and, if necessary, can be extended to 26 weeks or 52 weeks for participants on the full time education and training option. Participants on IAP receive a training allowance, equivalent to their JSA amount plus a top-up.

3 The third stage is the **Follow-Through** period. During this period, those still on ND25+ are provided with further assistance in finding regular employment. The Follow-Through normally lasts for six weeks, but can be extended to 13 weeks in some cases to allow the jobseeker to undertake further activity similar to that available in the IAP.

Currently, IAP is mandatory for ND25+ participants aged 25-49 at the start of the Gateway but voluntary for customers older than 50 at the beginning of the Gateway.

1.3 Piloting mandatory IAP for those aged 50-59

1.3.1 Random assignment

The pilots were designed in such a way as to allow a robust evaluation of their effectiveness. To achieve this, eligible customers were randomly assigned to either one of two groups. For those in the first group (the so-called 'Action' group), participation in IAP was mandatory. For those in the second ('Control') group, there was no change; participation in IAP remained voluntary. For obvious reasons, such a procedure is termed 'random assignment' (RA) and evaluations based on RA are termed 'experimental'.

RA is widely acknowledged as the best approach for policy evaluation. The advantage over alternative approaches is that it results in two groups of people (the action and control groups) who are statistically identical[2] except for the fact that the action group is exposed to the policy intervention (the 'treatment', in the jargon of evaluation) while the control group is not. Since this is the only dimension over which these two groups differ, any post-randomisation differences in outcomes can be attributed to the effect of the treatment. The outcomes of the control group can

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[2] By 'statistical equivalence' we mean that there are no systematic differences between the action and control group; all observed differences arise purely from variation within the overall population.

properly be regarded as providing an unbiased estimate of what the outcomes for the action group would have been had they not been exposed to the treatment – the so-called counterfactual.

Evaluation methods that are not based on RA cannot identify two statistically identical groups and typically have to rely on a comparison of those who receive the treatment with those who do not receive the treatment. In the context of this evaluation, were RA not possible and pilots were instead carried out in some other way, customers mandated to participate in IAP would have characteristics different from those not mandated to participate. Since these differences may influence subsequent outcomes of interest, a simple comparison of outcomes could not be viewed as an estimate of the effect of mandating participation. Whereas RA automatically provides an unbiased estimate of the counterfactual that is needed to estimate the effect of treatment, with non-experimental evaluations untestable assumptions are required. Consequently, non-experimental evaluations are wholly dependent on the veracity of their underlying assumptions and must therefore always be viewed with this caveat.

Although RA represents something of a gold-standard for evaluation, it has rarely been used in the UK. There are a number of reasons for this including the potential expense of experiments and the fact that pilots have to be planned with RA in mind; it is not possible to carry out a post hoc experimental evaluation. Occasionally ethical objections to experiments are sometimes voiced, particularly where a service is denied to those who may benefit from it. In the context of mandatory IAP, such ethical concerns should not prove a problem since the potential benefit of treatment is not being denied to anybody; those in the control group can still participate in IAP voluntarily. A fuller discussion of RA is provided in Chapter 3.

1.3.2 Eligibility for inclusion in the experiment

The experiment includes those JSA claimants with a continuous JSA spell of 18 months who are entering ND25+ for the first time as a person aged 50-59. A substantial proportion of customers enter ND25+ with a JSA claim of less than 18 months, particularly in areas where the Incapacity Benefit (IB) reforms pilots are implemented (see below). Those leaving IB in these areas are excluded from the experiment.

The pilots have been running since 5 April 2004 in 11 Jobcentre Plus areas and since 10 January 2005 in an additional three Jobcentre Plus areas.[3] The pilots were designed to run continuously for two years and so were due to end in April 2006 for 11 areas and in December 2006 for the three remaining areas. Following the decision to roll mandatory IAP out nationally, it has been decided to end the pilot early in the three remaining areas.

[3] The pilots were delayed in three areas (Derbyshire; Gateshead & South Tyneside; and, Renfrewshire, Inverclyde, Argyll & Bute) since these were pilot sites for the Employment Retention and Advancement (ERA) demonstration project. It was decided that the IAP pilots should not start until recruitment to the ERA pilots ended. By avoiding overlap of the pilots, any contamination effect of ERA on the mandatory IAP pilots will be minimised.

In addition to the IAP pilots, some of the areas are also piloting other policy enhancements. An important example is the IB pilots. In IB pilot areas, IB recipients attend work focused interviews and those leaving IB for JSA may start ND25+ subsequently, including an IAP stage. Since IB recipients are also more likely to belong to older age groups than JSA claimants, many new customers aged 50-59 will start IAP in areas with both pilots. This might affect the IAP options available to other customers.

The Jobcentre Plus areas in which the pilots are running were chosen to include all seven IB Reform pilot areas. Additional factors influencing the selection of pilot areas include the number of eligible flow customers aged 50-59 and their geographical location.

Table 1.1 Mandatory IAP pilot areas

Jobcentre Plus area	IB pilot	ERA pilot	IAP pilot	IAP go-live date
Bridgend, Rhondda, Cynon & Taff	X		X	5 April 2004
Buckinghamshire & Oxfordshire			X	5 April 2004
Calderdale & Kirklees			X	5 April 2004
Coventry & Warwickshire			X	5 April 2004
Derbyshire	X	X	X	10 January 2005
East Lancashire	X		X	5 April 2004
Essex	X		X	5 April 2004
Gateshead & South Tyneside	X	X	X	10 January 2005
Hampshire			X	5 April 2004
Leicester			X	5 April 2004
Renfrewshire, Inverclyde, Argyll & Bute	X	X	X	10 January 2005
Shropshire			X	5 April 2004
Somerset	X		X	5 April 2004
Suffolk			X	5 April 2004

1.3.3 The timing of randomisation and the interpretation of the treatment effect

The experiment was designed such that individuals would be randomly assigned to the action or control group at the time of ND25+ entry.[4] In practical terms, this means that individuals find out at this stage whether they will be required to participate in IAP or whether any participation will be voluntary. From this point on, it is conceivable that the mandate will have an effect. In other words, the effect of mandating participation may be that individuals change their behaviour before the intended IAP participation as well as during/after such participation. This influences

[4] The description in this section relates to the design of the experiment. In practice, the experiment was not always implemented as planned.

how the estimated treatment effect should be viewed; it is the effect of changing from voluntary to mandatory IAP participation. It is important to be clear on this. The estimated effect is not simply an estimate of the effect of IAP participation. This is for two reasons: first, the effect can operate through those who do not participate (perhaps the requirement to participate encourages them to leave JSA or to find a job); second, those in the control group can still participate.

Designing the evaluation in this way allows for anticipation effects of the mandate to be observed from the point of ND25+ entry onwards. In principle, it is possible that such effects could occur at any stage of the JSA spell if people knew they would have to participate in IAP at some stage. If this were the case, randomisation should take place at the beginning of the JSA spell. However, the literature on anticipatory effects shows (e.g. Bergemann *et al*. 2005, Ashenfelter 1978, Heckman *et al*. 1999) that anticipation effects typically occur shortly before the treatment (and increase closer to the treatment). Consequently, it seems unlikely that individuals would change their behaviour in response to a treatment that – if ever started – would begin about 22 months into their JSA claim until nearer the time of the treatment. Furthermore, discussions with DWP have revealed that individuals are typically first informed about IAP when they enter ND25+ rather than at the start of their JSA claim. Providing additional information to individuals at the time of the start of their JSA claim therefore risks introducing the sort of randomisation bias discussed in Chapter 3.

1.4 Aim and structure of this report

This report serves two broad purposes. First, it is intended to document the approach to the evaluation. This includes a description of how the evaluation was designed and a discussion of the merits of the approach adopted and the possible associated pitfalls. Second, the report aims to present some preliminary results. Since it is an interim report, the results must be regarded as indicative and fuller results will become available later. As much as anything, they highlight the need for further results based on the completed pilot data. This indeed will be the chief purpose of the subsequent final report.

In terms of chapters, the approach to the evaluation is covered in Chapters 1 and 2 while the interim results are presented in Chapters 3 and 4. However, Chapter 3 also highlights some of the implementation issues that have arisen in the course of the evaluation and so goes some way to bridging the gap between the design of the experiment and its implementation. Chapter 5 concludes.

2 Evaluation method

This chapter presents a discussion of issues relating to the experimental approach adopted for this evaluation. In view of the rarity of such evaluations in the UK, such a discussion provides useful context.

2.1 The evaluation problem and approaches to solving it

The aim of evaluation is to assess the impact of a treatment. This treatment can be broadly defined; in the case of the evaluation considered in this report, 'treatment' means being mandated to participate in Intensive Activity Period (IAP) (rather than necessarily participating in IAP). To really know the impact of a treatment requires comparing the outcomes of those exposed to the treatment with the outcomes that they would have experienced had they not been exposed to the treatment. By definition, this counterfactual experience is impossible to observe; for those exposed to the treatment, only the outcome associated with treatment can be observed.

To attempt to address this fundamental difficulty, a number of methods are available. These vary in the assumptions they make – and therefore in their credibility – but all share the property that they attempt to estimate the counterfactual outcome for the treated using the outcomes of non-treated. At this stage, it is helpful to give an example to show how such comparisons can be misleading. Consider the case of a hypothetical training course for the unemployed. Participation in this course is voluntary. We might imagine that those who choose to participate will differ in some ways from those who do not do so. Perhaps they are more motivated, for example. Some time later, we observe that the proportion in work is higher among those who participated in the course than it is among those who did not participate in the course. Can the difference reliably be attributed to the effect of the course? The answer to this is almost certainly no; since those who participated are more motivated than those who did not participate, we would expect a higher proportion to be in work regardless of whether they had in fact participated in the course.

As indicated in Chapter 1, Random Assignment (RA) solves this problem by ensuring that there is no systematic difference between those in the treated group and those in the non-treated group. This means that the outcome of those in the non-treated group can reliably be viewed as an estimate of the counterfactual outcome. Any differences in outcomes following the date of random assignment can be attributed to the treatment. As recently described by Greenberg and Morris (2005) for the British Employment Retention and Advancement demonstration project (ERA), random assignment and experimental evaluation of programmes 'produce considerably more reliable estimates of programme impacts than any other method of estimating impacts'.

Non-experimental estimators are the alternative to RA. These are distinguished by the assumptions they make to estimate the counterfactual. Some of the common non-experimental estimators are summarised briefly below:

- Matching estimators are possible when all the sources of differences between treated and non-treated individuals can be observed. Essentially, a group of non-treated individuals who are similar in all important respects to the treated individuals is identified and their outcomes are regarded as an estimate of the counterfactual outcome for the treated. The assumption in this case is that all important differences are observed; this can sometimes be difficult to justify.

- Difference-in-differences (DiD) estimators are possible when longitudinal data (or cross-section data over two or more time periods) are available. The broad idea is that the bias that arises when comparing outcomes of the treated group with those of the non-treated group can be identified by carrying out an analogous comparison before the introduction of the treatment and then used to correct the post-treatment comparison. The assumption in this case is that the bias is constant over time.

- Instrumental variables (IV) estimators are possible when a variable exists – an 'instrument' – that is related to participation but not outcomes. In fact, RA can be viewed as an extreme form of an IV estimator, where the assignment to action or control group is the instrument. With IV estimators, the key difference is that the instrument does not *determine* treatment, it only influences it. IV estimators account for this weaker relationship in constructing a counterfactual outcome for those induced to participate due to the instrument. The key difficulty with this approach is finding a suitable instrument. Also, the effect that is identified applies only to those induced to participate due to the instrument.

- Control function estimators control for the fact that there are some unobserved characteristics (motivation in the earlier example) that influence participation but also influence outcomes. As with the IV approach, an instrument is needed for a credible application. Also, an assumption regarding the distribution of unobserved characteristics is needed.

It is not the purpose of this discussion to provide a comprehensive account of evaluation methods – this is provided in, for example, Heckman *et al*. (1999). The key point to observe is that, whereas RA can provide estimates without having to make any assumptions, the same is not true of non-experimental estimators. The importance of this should not be underplayed; although progress continues to be made with non-experimental approaches, detailed investigations show that they are generally unable to reproduce the benchmark experimental results (for example, Heckman *et al*., 1988).

2.2 Considerations when assessing microeconometric evaluations

The message from the previous section is that experiments are superior to any non-experimental method. In this section, possible problems facing evaluations in general are discussed. Points specific to experiments and to this particular evaluation are considered in more depth. Possible threats to the validity of the results are grouped into two broad headings: internal validity (the extent to which it is possible to view the results as capturing a true causal effect for those included in the evaluation) and external validity (the extent to which it is possible to generalise from the results beyond those included in the evaluation).

2.2.1 Internal validity

An evaluation is said to be internally valid if it can provide estimates that can be regarded as causal. In other words, internal validity implies that we can truly estimate the effect of the treatment. The discussion so far has suggested that experimental estimates are internally valid. Generally, threats to the internal validity of experiments do exist. Most commonly discussed in the economics literature is the idea of imperfect compliance such as individuals dropping out of treatment. In the case of this evaluation, dropping-out is a valid outcome. In terms of the earlier discussion, it can be viewed as a type of anticipation effect. In view of this, it poses no threat to the internal validity of the experiment.

A direct implication of internal validity is that characteristics are balanced (i.e. similar) across the treated and non-treated groups. The degree of balance for observable characteristics can be tested (see Section 3.4.2). Any significant differences would be a cause for concern. For obvious reasons, the degree of balance for unobservable characteristics cannot be tested but should result automatically from the RA process.

2.2.2 External validity

An evaluation is externally valid if its findings can be generalised from the study population to the population at large. It is important to consider the issue of external validity when generalising the results of any evaluation of a piloted intervention.

One indicator of external validity is the extent to which pilot areas are similar to non-pilot areas. If pilot areas are structurally different from non-pilot areas we might expect the findings from the pilot areas to be less relevant to other areas. A generalisation of the evaluation results may still be achieved in this case but may require some adjustment to reflect the difference in characteristics. The descriptive statistics in Section 3.3.1 compare pilot and non-pilot areas in terms of their observable characteristics to provide some insight into this.

Another particular potential difficulty when considering the effect of mandating a hitherto voluntary programme is that the voluntary programme may differ across pilot and non-pilot areas. Since the evaluation results provide the effect of mandatory, relative to a voluntary, IAP participation in the pilot areas, they can only be generalised if the treatment of voluntary IAP participants is the same in pilot and non-pilot areas. Section 3.4.3 analyses differences in the types of treatment offered to voluntary participants in the pilot areas and to participants in non-pilot areas. There are two possible reasons why such differences may be observed:

1 The pilot areas may offer IAP options different from those in the non-pilot areas in general, i.e. irrespective of the pilots. In this case, the IAP options started by action and control group members in the pilot areas will be similar, but will differ from all other areas.

2 The larger number of IAP participants in pilot areas may affect the quality of the IAP options available to both treatment and control groups in the pilot areas. If the total number of options available or the funding and the supply of these options do not increase accordingly, the experiment itself changes the participation pattern of both action and control groups in the pilot areas relative to non-pilot areas.

It is also possible that treatments will have macroeconomic effects that are not captured by a microeconometric evaluation. These could include:

- displacement effects – treated individuals gain jobs rather than non-treated individuals;
- deadweight effects – subsidising a treatment, which would have occurred anyway;
- substitution effects – treated individuals gain jobs previously held by non-treated individuals because of relative wage changes;
- general equilibrium effects – the treatment increases the supply of labour and so downward pressure is exerted on wages;
- tax effects – the effects of financing labour market interventions.

It is difficult to predict in advance the degree to which these will change the extent to which the estimated effects are scalable to the broader economy. In principle, for example, displacement or substitution effects could offset any positive effects that may be shown by the experiment. However, such effects are more likely to be important if a programme becomes large in size (Speckesser, 2004). With the intervention considered in this evaluation, it seems unlikely that such macroeconomic effects will be very important since the number of individuals affected by the treatment is small.

While the points raised in this section so far relate equally to experimental and non-experimental evaluations, Björklund and Regner (1996) identify four types of bias unique to experiments. These are discussed below in turn along with a consideration of their relevance to this evaluation.

1 Experiments can give rise to 'randomisation bias' whereby the existence of the experiment influences the behaviour of those potentially eligible for it. With the mandatory IAP evaluation, this might manifest itself in eligible Jobseekers Allowance (JSA) claimants opting to terminate their claims early rather than participate in the experiment. This would mean that both treatment and control groups included in the experiment would no longer be representative of the target group of eligible JSA claimants and so the estimated effects would be similarly unrepresentative. Since individuals only find out about the RA process at the time of randomisation, it seems unlikely that such bias will result in the case of this evaluation.

2 The fact of participating in an experiment can influence outcomes. 'Hawthorne' effects describe changes in behaviour of action and control groups that result simply from being involved in an experiment. For example, participants in the experiment may increase their search efforts purely due to the extra attention paid to them. 'Rosenthal' effects describe the situation where the existence of an experiment leads the action group to perform better simply because they are expected to do so (Rosenthal and Jacobson, 1968). With the mandatory IAP pilots, it is conceivable that both Hawthorne effects and Rosenthal effects could exist. However, one might hope that the size of any such effect is minimised due to the fact that, after the initial randomisation, there is no monitoring observable to members of the action and control groups so that they are not reminded of fact of their participation on an ongoing basis.

3 Experiments can also influence the behaviour of those delivering the programme. For example, caseworkers may be selective in who they include in the experiment in a way that was not intended when the experiment was designed. They might also alter the type of treatment provided. With the mandatory IAP pilots, advisers have, in principle, the opportunity to alter the experiment along these lines. The results presented in Chapter 3 show that there are indeed deviations from the intended design of the experiment that are consistent with such an 'adviser effect', although it is important to note that there may be other reasons for the observed deviations from the design. Whatever the cause, the consequence is that care must be exercised when generalising the results.

4 Finally, caseworkers may allocate those in the control group to an alternative programme that is a close substitute for the treatment received by those in the action group. With the mandatory IAP evaluation, such 'substitution bias' appears unlikely since the control group can still participate in IAP on a voluntary basis.

2.2.3 The 'black box' problem

Aside from the issues of internal and external validity, there is the fundamental issue of how an effect arises. In the case of the mandatory IAP evaluation, for example, while the existence of an effect on, say, employment may be identified, it is not possible to say which element of the treatment is causing the effect. For this reason, the effects estimated by microeconomic evaluations represent something of a 'black box' (see Greenberg and Morris (2005) for discussion of this in the context of the ERA project). Although this limitation is typically cited with respect to experimental evaluations, it is equally relevant when considering other microeconometric approaches. The results of the qualitative evaluation study will be important in understanding on how the identified effect estimated arises.

3 Characteristics of the customer group

The main focus of this chapter is a description of the characteristics of the customer group and their New Deal 25 Plus (ND25+) experiences. A comparison across pilot areas and non-pilot areas provides an insight into the issue of external validity. Internal validity is assessed by comparing action and control group members in the pilot areas. Before this, however, the data are described.

3.1 Evaluation databases

3.1.1 RA tool data

At the beginning of the Gateway, eligible customers participate in an initial interview in which they are randomly assigned to either the action or the control group. Random assignment (RA) is carried out using software developed by Department for Work and Pensions (DWP) specifically for this evaluation. We refer to this as the RA 'tool'. As well as performing the assignment, the RA tool also requires customers to provide information on a number of personal characteristics. These data provide a useful means of checking that the experiment is running as planned; PSI has produced monthly reports throughout the pilot period demonstrating that there are no worrying differences between the action and control groups in terms of their observable characteristics.

The RA tool data cover the period from April 2004 until end October 2005. For reasons of timing, this report is based on data until the end of August 2005 – a total number of 2,622 participants. Sub-section 3.3.2 describes the characteristics of the customers based on the RA tool data. Sub-section 3.4.2 provides tests for internal validity based on the RA tool data.

3.1.2 Merged RA tool data/ND25+ data

The RA tool data can be merged with the New Deal evaluation database (NDED). The NDED data are updated regularly, but do not include the most recent participants. Due to the reporting and data processing of the NDED, it has a lag of between six and eight weeks relative to the RA tool data. Therefore, the data used in this report cover only the period April 2004 until June 2005.[5]

The NDED data contain all customers' spells on ND25+. They report the beginning of the different stages on ND25+ (Gateway, Intensive Activity Period (IAP) (including type of option) and Follow-through) as well as the most important outcome variables (date of leaving the programme and destination state). All results reported in Chapter 5 of this report make use of NDED data. Since the merged data should cover all participants as recorded in the RA tool data until 24 June 2005, it should correspond to 2,305 individuals. However, some quality problems restrict the useable size of the merged data:

- 87 participants in the experiment (3.7% of the total) have no corresponding spell in the NDED data beginning after April 2004, i.e. there were no records found in NDED for their identifier.
- For another 28 participants (1.2%), the date of the ND25+ marker was set after the date of random assignment. Since the marker is the beginning of the ND25+ process and all further activity follows from it, random assignment should not have taken place for these cases.
- In the merged RA tool/NDED, we often observe that the Gateway began earlier than the day of random assignment. This indicates that advisers were not acting according to the guidance of the experiment. In some cases, random assignment was implemented immediately before the beginning of the options. Since we only observe effects from the time after random assignment onwards, this means that, for such cases, we cannot capture any effects that might appear during the Gateway. To keep the population of customers beginning mandatory IAP25+ relatively consistent, it was decided by DWP only to include customers who begin the Gateway no more than 28 days before the date of random assignment. Consequently, all cases with a random assignment earlier than this were excluded (129 participants – six per cent).

After these exclusions, 2,061 participants in the merged RA tool/NDED remain (89 per cent of the total number of participants). The analyses in Section 3.4.4 are based on these cases.

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[5] The latest release of these data should allow us to observe NDED information up to the end of August 2005. However, these data also require some corrections and the latest release of these data were supplied after the empirical analysis of this report was completed.

3.1.3 ND25+ customer data/NDED data

The third database used in this evaluation is a complete extract of all ND25+ customer records as recorded in the NDED. These data are used for the identification of the eligible customer group within and outside the pilot areas and for tests on the external validity of the experiment.

ND25+ customer data cover the period between June 1998 and May 2005, and provide data for all customers participating in the programme and their outcomes. Due to the size of the data (789,985 records), the analysis will be restricted to customers of the age-group 50 – 59 for the financial years 2003/4 and 2004/5. These data will be analysed for two reasons:

- Only ND25+ data allow the description of important observable characteristics of the customer group in pilot and non-pilot areas. Such a comparison is very important for testing the external validity of the experiment; if the characteristics between pilot and non-pilot areas differ considerably, external validity may not hold.
- ND25+ data are used for verification of the customer data provided by the RA tool. ND25+ customer data allow a complete description of the customers in the pilot areas and reveal whether all customers beginning Gateway have actually been randomly assigned if belonging to the eligible population.

The disadvantage of the databases used in this evaluation is the different reporting periods of the three sources. We cannot consistently relate all analyses of this interim report to the same calendar time. For the final report, data covering equivalent time periods should be provided to allow a systematic analysis of all different aspects of this analysis. A summary of the data sources is given in Table 3.1.

Table 3.1 Data used for the evaluation

	RA tool data	Merged RA/NDED data	ND25+ customer NDED data
Observations	All participants in the experiment	NDED spell related to entry participation in experiment (after April 2004)	All spells of customers beginning ND25+ since 1998
Main variables	Data generated by DWP implementing random assignment (qualification/age/ ethnicity/sex/area/ other characteristics)	Data generated by Jobcentres (status on ND25+ and IAP50+/ destination after end of programme/ ethnicity/sex/age/other characteristics)	Data generated by Job Centres (ND25+/ IAP50+/destination after end of programme/ethnicity/ sex/age/other characteristics) for all spells on ND25+
Period covered	April 2004- October 2005	April 2004- August 2005	June 1998- May 2005
Updates	Monthly	Monthly	-
Latest release with data up to	October 2005	August 2005	May 2005
Data used in this report up to	August 2005	June 2005	May 2005
Number of participants (Data used in this report)	2,622	2,061	769,985

3.2 Projected customers inflows

When designing the pilots, it was important to have an estimate of the likely size of the study sample. Jobseekers Allowance (JSA) claimant data for the period May 2002 – May 2003 showed 2,928 individuals aged 50 – 59 passing the threshold of 18 months continuous JSA receipt in the pilot areas over this period. Based on these figures, roughly 6,000 customers were expected to participate in the experiment over the period of two years.[6] This is shown in the first column of Table 3.2.

In practice, the number of participants in the experiment was much smaller than expected. We observe 2,567 customer entries until the end of August 2005, of which 2,296 were resident in non – Employment Retention and Advancement (ERA) areas and 271 in ERA areas. Based on these observed customer entries, the prediction of customers until the end of the experiment (31 December 2006 in ERA-areas, 31 March 2006 in all other areas) remains far below the number originally expected (Table 3.2, column two). By scaling up the customer inflows achieved until August 2005, we currently expect that the experiment will have a total number of participants of less than 4,100 by the end of the pilots.

6 Although this disregarded the fact that only those for whom this was the first ND25+ spell aged 50+ are eligible for the experiment.

Table 3.2 shows that there are important regional differences of the size of the realised customer group relative to the original expectation:

- in five areas the size of customer group corresponds very well to the original expectation: Bridgend & Rhonda, Cynon & Taff; Buckinghamshire and Oxfordshire; Coventry and Warwick; East Lancashire; and Hampshire. This can be seen by comparing the projection in column 4 of Table 3.2 with the first column of observed JSA spells exceeding the duration of 18 months. For example, in Bridgend & Rhonda, Cynon & Taff, a total of 206 participants are expected compared to the original expectation of 170 participants (85 a year for two years).
- in some areas, there are fewer participants than expected. This is especially the case in Calderdale and Kirklees, Gateshead and South Tyneside and Shropshire. In Calderdale and Kirklees, only 83 customers have so far participated, despite the relevant inflow to eligibility being 192 in the year before the experiment began.

The RA tool data do not provide evidence whether the unexpected drop in the size of the customer group arises from a declining customer group in these areas or whether some eligible customers are not being randomised. However, there are some indications for implementation problems in these areas:

- Sub-section 3.3.2 shows that many customers within the pilot areas are not included in the RA tool data, despite the ND25+ data showing that they should have been.
- Sub-section 3.4.1 shows that in one area – Calderdale and Kirklees – the majority of participants in the experiment were randomly assigned after the beginning of their Gateway. Obviously, implementation may differ across areas and problems might appear more often in some areas than in others. This might help explain why some areas have many fewer customers than expected.

Table 3.2 has been regularly updated during the course of the experiment to identify possible implementation problems. The projected number of participants has declined only slightly over time. Therefore, a total of about 4,000 customer entries until the end of the experiment is a reasonable expectation.

Table 3.2 Projected and achieved number of participants

	Annual 18 month threshold flow (05/02-05/03)	Number of participants – RA data achieved 04/04(01/05) – 31/08/05	Projected until end of experiment	Predicted until end as % of 18-months threshold for two years
Bridgend & Rhonda, Cynon, Taff	85	145	206	121
Buckinghamshire & Oxfordshire	163	250	355	109
Calderdale and Kirklees	192	83	118	31
Coventry and Warwick	218	295	419	96
Derbyshire*	355	124	373	52
East Lancashire	107	158	224	105
Essex	378	384	545	72
Gateshead and South Tyneside*	278	78	234	42
Hampshire	236	324	460	97
Leicester	356	381	541	76
Renfrewshire, Inverclyde, Argyle & Bute*	167	69	207	62
Shropshire	115	72	102	44
Somerset	92	75	107	58
Suffolk	186	129	183	49
Total non-ERA pilot areas	2,128	2,296	3,261	77
Total	2,928	2,567	4,075	70

N.B. Area missing for 55 cases of the RA tool data (as of August 2005, N=2,622,)
* ERA areas started on 10 January 2005
Scaling factors (based on data until 31 August 2005)
Non-ERA 1.42 (730 days/514 days)
ERA 3.00 (730 days/243 days)
Source: JSA data supplied by DWP and RA tool data until 31 August 2005

3.3 Descriptive analysis of the customer group

3.3.1 Comparing pilot and non-pilot areas

This section describes all customers who were aged 50-59 and who were beginning their first Gateway after the pilots began. The description is based on ND25+ data for the period 5 April 2004 to 27 May 2005 for the non-ERA areas and 10 January 2005 to 27 May 2005 for the ERA areas.

Table 3.3 shows a total of 14,854 customers: 2,945 in the pilot areas and 11,909 in other areas. The regional information was taken from the NDED rather than the RA tool data. Seven customers of the experimental sample are reported to live outside the pilot areas; these customers might have moved out of the area after random assignment. The experimental group that could be retrieved in the ND25+ data until the end of May 2005 consists of 1,925 customers. 1,020 clients who should have been randomly assigned did not take part in the experiment (column 3). There are substantial differences between the pilot areas: Buckinghamshire and Oxfordshire, Essex and Leicestershire show the lowest shares of customers starting Gateway without random assignment with around 20 per cent. For Calderdale and Kirklees, this figure is 57 per cent - this is consistent with the discussion in section 3.2.

Table 3.3 Eligible customers, in pilot and non-pilot areas

Jobcentre district	Experimental groups Number of customers Action group	Number of customers Control group	Number of customers without random assignment	Share of customers without RA (%)	Local area as % of total	Total customers
All non-pilot areas	3	4	11,902	100	80	11,909
Bridgend & Rhonda, Cynon, Taff	67	29	82	46	1	178
Buckinghamshire & Oxfordshire	103	103	52	20	2	258
Calderdale & Kirklees	29	25	73	57	1	127
Coventry & Warwickshire	121	99	95	30	2	315
Derbyshire	33	37	33	32	1	103
East Lancashire	56	73	79	38	1	208
Essex	166	146	81	21	3	393
Gateshead & South Tyneside	24	17	41	50	1	82
Hampshire	116	94	185	47	3	395
Leicestershire	159	174	84	20	3	417
Renfrewshire, Inverclyde, Argyll & Bute	12	18	46	61	1	76
Shropshire	25	41	24	27	1	90
Somerset	32	27	57	49	1	116
Suffolk	54	45	88	47	1	187
Total	1,000	932	12,922		100	14,854

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-ERA areas: 5 April 2004 – 27 May 2005, ERA areas 10 January 2005 – 27 May 2005)

Table 3.3 also provides important information about the validity of the experimental estimates for other areas: pilot areas account for only 20 per cent of the population of interest. Comparisons presented in the next sub-section show significant differences between pilot and non-pilot areas with respect to some characteristics. As already noted, this may have implications for the external validity of the experimental estimates.

We can also compare customers in pilot and non-pilot areas who are beginning the Gateway after the start of the pilots using data from the NDED until 27 May 2005.[7]

[7] Note that the distinction between pilot and non-pilot areas is time-varying, as the ERA pilot areas start on 10 January 2005. These areas are treated as non-pilot areas for the earlier period.

Tests for the significance of these differences are implemented using standard t-tests.

Table 3.4 shows the gender difference between the areas. In pilot areas, 27 per cent of all eligible participants on the Gateway are female. The share of women is slightly higher in non-pilot areas (28 per cent) but the difference in the sample means is not significantly different from zero.

Table 3.4 Eligible customers in pilot and non-pilot areas, by sex

	Pilot area (Col. %)	Non-pilot area (Col. %)	Difference (%points)
Male	73	72	1
Female	27	28	-1
Base	2,945	11,909	

* significant >= 95%

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-ERA areas: 5 April 2004 – 27 May 2005, ERA areas 10 January 2005 – 27 May 2005)

Table 3.5 compares the age distribution of participants. Some significant differences are evident: in non-pilot areas, there are significantly more participants in the age groups 50 – 53. Pilot areas show higher shares of participants aged 54 or 55 at the beginning of the Gateway. A significantly higher age of participants in pilot areas might be important when considering the external validity of the results. One might expect that older participants are less likely to benefit from IAP. For example, the benefits of re-training are reduced for those with fewer economically productive years ahead of them. Another possibility is that older participants might be more likely than younger participants to respond to the treatment by retiring. Clearly, such differences between the pilot and non-pilot areas could mean that the effects of mandating IAP could differ in other areas of the UK.

Table 3.5 Eligible customers in pilot and non-pilot areas, by age

	Pilot area (Col. %)	Non-pilot area (Col. %)	Difference (%points)
50	12	16	-4*
51	11	13	-2*
52	10	11	-1
53	10	10	0*
54	10	9	1*
55	11	8	3*
56	10	9	1
57	10	9	1
58	8	8	1
59	7	6	0
Base	2,945	11,909	

* significant >= 95%

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-ERA areas: 5 April 2004 – 27 May 2005, ERA areas 10 January 2005 – 27 May 2005)

Table 3.6 shows that the pilot areas differ also in the distribution of ethnic groups. The overall picture shows that 89 per cent of all Gateway participants in the pilot areas are of British white origin. In other areas, the corresponding figure is 87 per cent. A difference of two percentage points might not seem large, but it is significant for the client group of eligible JSA claimants who are or might become subject to a mandatory IAP. 'Asian or Asian British' customers are more common in the pilot areas than elsewhere while 'black or black British' and 'Chinese or other ethnic group' customers are less common.

Table 3.6 Eligible customers in pilot and non-pilot areas, by ethnicity

	Pilot area (Col. %)	Non-pilot area (Col. %)	Difference (%points)
White	89	87	2*
Mixed	0	0	0
Asian or Asian British	7	4	3*
Black or Black British	1	3	-3*
Chinese or other ethnic group	1	3	-1*
Prefer not to say	3	3	0
Base	2,945	11,909	

* significant >= 95%

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-pilot and non-ERA pilot areas: 5 April 2004-27 May 2005, ERA pilot areas 10 January 2005 – 27 May 2005)

The ND25+ data also show that the eligible group in pilot areas has a higher share of disabled customers than the remaining areas, see Table 3.7. On average, 43 per cent are disabled in the pilot areas, compared to 39 per cent in non-pilot areas. Again, this difference of four percentage points is significant.

Table 3.7 Eligible customers in pilot and non-pilot areas, by disability

	Pilot area (Col. %)	Non-pilot area (Col. %)	Difference (%points)
Not disabled	57	62	-4*
Disabled	43	39	4*
Base	2,945	11,909	

* significant >= 95%

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-pilot and non-ERA pilot areas: 5 April 2004-27 May 2005, ERA pilot areas 10 January 2005 – 27 May 2005)

Table 3.8 shows the differences in the duration of JSA benefit before the beginning of the ND25+ Gateway. All JSA claimants with a benefit receipt of more than 18 months have to participate in ND25+. Customers in non-pilot areas begin the Gateway after 18 months, on average. In pilot areas, the mean duration on benefit for customers is significantly longer. Note that ND25+ should begin after roughly

546 days. In pilot areas, customers have an average duration on benefit of 609 days, 85 days longer than in non-pilot areas. This difference is significant.

Table 3.8 Eligible customers in pilot and non-pilot areas, by days on benefit before ND25+

	Pilot areas		Non-pilot areas		
	Mean duration of claim at ND start	Number of customer entries	Mean duration of claim at ND start	Number of customer entries	Average difference (in days)
ND25+ IAP participants age group 50-59	609	2,930	524	11,810	85*

* significant >= 95%

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Non-pilot and non-ERA pilot areas: 5 April 2004 – 27 May 2005, ERA pilot areas 10 January 2005 – 27 May 2005)

To summarise, most of the previous tables show significant differences between pilot and non-pilot: customers in pilot areas are older, they less frequently report disabilities and are more often white. Also, they tend to remain longer on benefit before entering the Gateway. All these differences may be important when considering the possible effect of mandating IAP in other areas for this age group. Any generalisation of the results should take into account that the population of interest in pilot areas is slightly different from the national average or the non-pilot areas and so issues of external validity arise. Appropriate measures to address this might include:

- weighting experimental estimates based on average socioeconomic characteristics in all areas; and
- carrying out additional non-experimental analyses for specific groups of interest.

3.3.2 The customer group in pilot areas

Following the introduction of the pilots, all customers with a JSA duration of 18 months or more should be randomly allocated to either the action group or the control group if they are living in a pilot area and start their first ND25+ Gateway aged 50 – 59.

By selecting all eligible customers whose ND25+ spell begins after the start of the pilots, the ND25+ data until May 2005 should roughly correspond to the data provided by the RA tool. However, Table 3.9 shows that a substantial number of eligible customers in the pilot areas started ND25+ without random assignment; 1,020 customers have no corresponding entries in the RA tool data. Qualitative research by the project group within DWP shows that there are numerous reasons why such cases might occur; most importantly customers were not randomly assigned at the adviser's discretion, either because they would have turned 60 shortly after beginning their Gateway or because of health reasons that are unobservable in ND25+ data.

Table 3.9 shows that the implementation of the experiment matters a lot and that the advisers have a crucial role in the delivery of the programme. As discussed in Section 3.2, roughly 3,000 eligible customers per annum were expected when designing the evaluation (see Table 3.2). Based on data covering the first 13 months of the experiment, Table 3.9 shows that roughly these numbers of customers are beginning ND25+ in pilot areas. However, a substantial proportion of eligible customers do not participate in the experiment.

This is of great importance for the experimental estimates for two reasons:

1 There is no quantitative information on how advisers are selecting individuals. This creates a problem when attempting to generalise the results.

2 The number of participants available for the quantitative analysis is substantially reduced. This affects the precision of the estimates reported in the Chapter 4.

Table 3.9 also shows that some of the clients who are randomly assigned would not be eligible for the programme if the rules of eligibility were consistently applied. Column two of the table reports that 495 customers (26 per cent of the experimental group) are not in fact eligible to participate since they have already participated in ND25+ aged 50 – 59. There are numerous reasons why such ineligible customers might be included in the experiment. For example, customers might not have reported their earlier spell. Alternatively, an adviser may have decided to randomly assign an ineligible individual because an earlier Gateway spell ended after a very short duration.

Table 3.9 Customers in pilot areas, by eligibility and random assignment status

	Eligible	Ineligible	Total
No random assignment	1,020	0	1,020
Action	735	262	997
Control	695	233	928
Total	2,450	495	2,945

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Eligible: Customers beginning the first Gateway participation aged 50-59 in pilot areas after the introduction of mandatory IAP50+ in the period 5 April 2004 – 27 May 2005; eligibility in ERA pilot areas starts 10 January 2005. Ineligible: Customers beginning a second or later start of Gateway aged 50-59)

Table 3.9 does not control for the duration on JSA before the random assignment. According to the guidance for the mandatory IAP pilots, customers should only participate in RA to mandatory IAP if their benefit claim exceeds 546 days at the starting date of the Gateway. Therefore, some of the 'eligible' customers without RA (as reported in Table 3.9) might not be eligible according to this criterion. Table 3.10 explores this additional restriction on eligibility and reports the duration on JSA before the beginning of Gateway. If the experiment were implemented according to the guidance, early entrants would not be randomly assigned.

Table 3.10 shows that 595 out of 1,020 clients beginning Gateway without RA in pilot areas should have been randomly assigned as their JSA claim was longer than 546 days at the beginning of the Gateway. It also shows that 288 participants beginning the Gateway with RA should not have been part of the experiment because they started ND25+ early.

Given that the experimental sample partially consists of early entrants, this table provides further evidence that implementation is not wholly consistent with the formal rules of eligibility, and that the experimental samples obtained differ from the groups originally expected based on the programme design.

Table 3.10 Customers in pilot areas, by benefit duration and random assignment status

Length of JSA claim		Random assignment		
	Missing	Action	Control	Total
546+ days	595	846	791	2,232
< 546 days	425	151	137	713
Total	1,020	997	928	2,945

Source: Gateway entrants aged 50-59 with first Gateway after the age of 50, ND25+ data (Early entrants with less than 546 days on benefit within the last 21 months are not supposed to be allocated by random; even if they are starting their first participation on the Gateway aged 50-59)

But is it really a problem that the experimental estimates rely on customers that differ from those originally planned in the design of the pilots? Not if one takes into account the importance of practical implementation of the programme. All programmes in all areas are implemented by advisers. Advisers in other areas might apply a selective implementation of the programme similar to the one we observe in the pilot areas. Therefore, a nationwide introduction of mandatory IAP would presumably lead to similar selection processes in other areas, and the external validity of the experiment might not be affected by the selective implementation of the pilots. Should similar selection processes occur elsewhere, estimates based on the experimental samples provided will still be informative for all areas, because they show the outcome of a programme as delivered in the local Jobcentres. For this reason, we do not exclude from subsequent analyses those individuals who are ineligible due to having previously participated in ND25+ aged 50 or over or to not having claimed JSA for 18 months.

The remainder of this sub-section describes the characteristics of customers participating in the experiment between 5 April 2004 and 31 August 2005 using information collected by the RA tool. Table 3.11 shows the numbers of customers in the action and control group: a group of 2,622 was randomly assigned over this period, of which 48 per cent belong to the control group and 52 per cent to the action group.

Table 3.11 Customers starting Gateway with random assignment, pilot areas only

	Number of customers until end August 2005	Col %
Action group	1,353	52
Control group	1,269	48
Total	2,622	100

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.12 shows the inflows on a monthly basis. In April 2004, only 90 customers were randomly assigned to the action and control group. June 2004 shows the highest number of customer inflows with a total of 196 new clients. Between July and October 2004, the customer numbers are around 160. There are considerably fewer new customers in December 2004. As three additional pilots participate in the experiment after 10 January 2005, the average customer numbers were expected to increase for 2005. However, Table 3.12 shows little change; after a peak of 193 new customers in January 2005, numbers return to around 165 a month until the summer, when slightly fewer customers were randomly assigned: 152 in July 2005 and 123 in August 2005. In May 2005, there are only 121 new customers due to one week without any random assignments because of staff shortages at DWP.

Table 3.12 Customers starting Gateway, by month of random assignment

	Number of customers until end August 2005	Col. %
Apr-04	90	3.4
May-04	133	5.1
Jun-04	196	7.5
Jul-04	159	6.1
Aug-04	150	5.7
Sep-04	159	6.1
Oct-04	167	6.4
Nov-04	188	7.2
Dec-04	123	4.7
Jan-05	193	7.4
Feb-05	165	6.3
Mar-05	167	6.4
Apr-05	167	6.4
May-05	121	4.6
Jun-05	169	6.5
Jul-05	152	5.8
Aug-05	123	4.7
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Turning to the socio-economic characteristics of the customer groups, Table 3.13 shows that the customers are predominantly male; 1,965 or 75 per cent of all participants are men.

Table 3.13 Customers starting Gateway, by sex

	Number of customers until end August 2005	Col. %
Male	1,965	74.9
Female	657	25.1
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.14 shows the age distribution of the customer group. Only those between 50 and 59 years of age were supposed to be included in the experiment, but 17 of the participants were outside this range. Participants are spread fairly evenly throughout the eligible age range, although there is a dip in numbers for those aged 59.

Table 3.14 Customers starting Gateway, by age at random assignment

	Number of customers until end August 2005	Col. %
49	1	0.0
50	296	11.3
51	288	11.0
52	249	9.5
53	261	10.0
54	290	11.1
55	292	11.1
56	260	9.9
57	271	10.3
58	239	9.1
59	159	6.1
60	5	0.2
61	4	0.2
62	4	0.2
63	3	0.1
Total	2,499	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.15 gives a breakdown of the participants by ethnicity. The key customer group consists of white British persons with a total share of 86 per cent of all participants in the experiment, followed by the group of Asian or Asian British participants with seven per cent of total. Those belonging to other ethnic groups or who refuse to provide information about their ethnic origin account for seven per cent of the total.

Table 3.15 Customers starting Gateway, by ethnicity

	Number of customers until end August 2005	Col. %
White – British	2,257	86.1
White – Irish	31	1.2
White – Other	38	1.5
Mixed	6	0.2
Asian or Asian British	195	7.4
Black or Black British	41	1.6
Chinese or other ethnic group	14	0.5
Prefer not to say	40	1.5
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

The pilot areas differ in size. As shown in Table 3.16, more than 50 per cent of the participants come from the four areas of Coventry and Warwickshire, Essex, Hampshire and Leicestershire. The biggest share is found for Essex and Leicestershire (both about 15 per cent) and Hampshire (13 per cent). The three ERA areas (Gateshead & South Tyneside, Renfrewshire, Inverclyde, Argyll & Bute and Derbyshire) began the experiment on 10 January 2005 and show smaller numbers of participants in general. As suggested by Table 3.2, Derbyshire is the biggest of the later pilots. Fewer than 100 participants come from each of Somerset, Shropshire and Calderdale and Kirklees. On the basis of Table 3.2, small numbers were expected for Somerset and Shropshire at the beginning of the experiment. However, the number of participants was expected to be higher for Calderdale & Kirklees.

Table 3.16 Customers starting Gateway, by pilot area

	Number of customers until end August 2005	Col. %
Bridgend & Rhonda, Cynon, Taff	145	5.7
Buckinghamshire & Oxfordshire	250	9.7
Calderdale & Kirklees	83	3.2
Coventry and Warwickshire	295	11.5
East Lancashire	158	6.2
Essex	384	15.0
Hampshire	324	12.6
Leicester	381	14.8
Shropshire	72	2.8
Somerset	75	2.9
Suffolk	129	5.0
Derbyshire	124	4.8
Gateshead & South Tyneside	78	3.0
Renfrewshire, Inverclyde, Argyll & Bute	69	2.7
Total	2,567	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

The RA tool also collects detailed information about the customer's level of education. Table 3.17 shows that more than half of all customers (51 per cent) report not having any formal qualification. NVQ level 1 or equivalent is the highest level of qualification for 11 per cent of participants. Another 12 per cent have GCSEs or an equivalent qualification. Roughly nine per cent of participants passed A-levels and 17 per cent hold higher education (i.e. college or university) qualifications.

Table 3.17 Customers starting Gateway, by level of education

	Number of customers until end August 2005	Col. %
None	1,331	50.8
NVQ level 1 or equivalent	286	10.9
GCSEs or equivalent	318	12.1
A levels or equivalent	231	8.8
Higher education	456	17.4
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

The RA tool also collects information about the skill level of most recent employment before the current JSA claim. Table 3.18 shows that the majority of customers used to work in manual occupations (40 per cent). Roughly 32 per cent were employed as semi-skilled or skilled blue collar workers and 8 per cent formerly worked in office jobs. There is also a high share of former professionals or managers among the clients (20 per cent).

Table 3.18 Customers starting Gateway, by last occupation

	Number of customers until end August 2005	Col. %
Manual	1,060	40.4
Semi-skilled	409	15.6
Skilled	427	16.3
Office	200	7.6
Professional	274	10.5
Managerial	252	9.6
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.19 shows the partnership status of participants. Only 29 per cent are married or live in partnerships compared to more than 70 per cent who are either singles (41 per cent), divorced (22 per cent), separated (five per cent) or widowed (three per cent).

Table 3.19 Customers starting Gateway, by partnership status

	Number of customers until end August 2005	Col. %
Single	1,080	41.2
Married	681	26.0
Living together	68	2.6
Divorced	573	21.9
Separated	137	5.2
Widowed	75	2.9
Prefer not to say	8	0.3
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

For customers with a partner, that partner is also unemployed in more than half of all cases. Only 10 per cent of all customers live with partners who are working.

Table 3.20 Customers starting Gateway, by partner's employment status

	Number of customers until end August 2005	Col. %
No partner	1,865	71.1
Retired	42	1.6
Working over 30 hours per week	151	5.8
Working between 16 and 30 hours per week	66	2.5
Working less than 16 hours per week	44	1.7
Unemployed - seeking work	74	2.8
Unemployed - not seeking work	325	12.4
Other	37	1.4
Prefer not to say	18	0.7
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.21 shows that the majority (93 per cent) of participants are not working at all while on JSA.

Table 3.21 Customers starting Gateway, by own employment status

	Number of customers until end August 2005	Col. %
Do not work	2,425	92.5
Work for less than 5 hours per week	33	1.3
Work between 5 and 10 hours per week	91	3.5
Work between 10 and 16 hours per week	73	2.8
Total	2,622	100.0

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

3.4 Randomisation and validity of the experiment

As discussed in the methodological section of this report, random assignment should have divided the customer groups into a treatment sample and a control group sample that do not differ with respect to any characteristics so that any differences in subsequent outcomes can be viewed as being caused by the treatment. Furthermore, random assignment was supposed to take place at the time of entering ND25+. This section provides a number of checks:

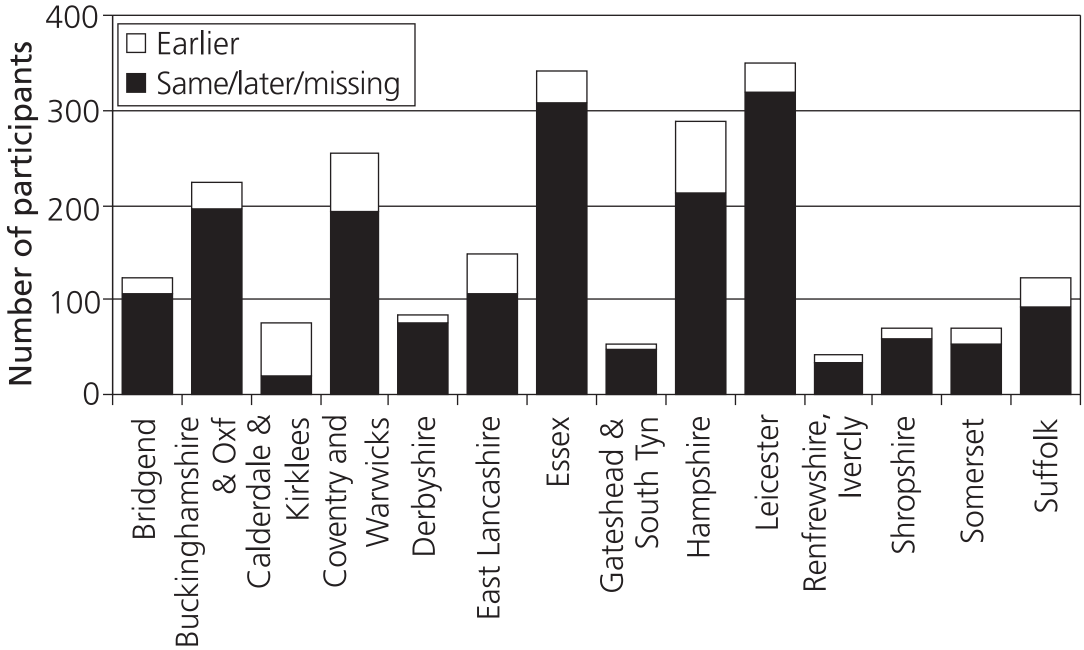
- a description of the timing of randomisation relative to the date of Gateway start;
- an exploration of internal validity, i.e. tests on the sample averages of observable characteristics for action and control groups;
- an exploration of the external validity of the experiment that describes differences between policies implemented in the pilot and non-pilot areas;
- a comparison of progress through ND25+ for action and control group members and of the IAP options they participate in.

3.4.1 Timing of randomisation

As already noted, since participants know at the time of random assignment whether they will later have to enter the IAP, it is possible that the effect of the mandate may happen before entering IAP. Randomisation should take place at the date of the initial interview for the ND25+, i.e. at the start of the Gateway. There are however quite a number of cases where the Gateway begins earlier than the date of random assignment. On average, 78 per cent of all customers are randomly assigned on the date of the initial Gateway interview or before, but this varies widely across areas. Based on data up to 27 June 2005, Figure 3.1 shows the proportion of customers beginning the Gateway on or after the date of random assignment relative to participants who were randomly assigned after starting the Gateway. Random assignment after starting the Gateway contradicts the guidance of the programme but is commonly observed. This is most obvious in the case of Calderdale and Kirklees where only 25 per cent of participants are assigned on or before the beginning of the Gateway. In such cases, the behaviour of those on the Gateway will be unaffected until randomisation takes place so the scope for the treatment to have an effect is curtailed.

While Calderdale and Kirklees provides the most extreme example, there are also substantial numbers of participants randomly assigned late in other areas. In Hampshire, Somerset and Suffolk, around 25 per cent of all participants start the Gateway earlier than the random assignment. Other areas are more successful in implementing the experiment as planned. In Essex, for example, random assignment is late in only ten per cent of cases.

Figure 3.1 Timing of Gateway start relative to random assignment, by area



Source: Merged customers'/ND25+ data, up to 27 June 2005

Late randomisation of participants on the Gateway has important consequences for the evaluation:

- As mentioned above, variation in the timing of randomisation relative to the start of Gateway means that the estimated treatment effect is an average: for some participants, it includes the whole Gateway period, for others only a part of it.
- Attempting to generalise the results becomes more complicated. The estimated treatment effect is less likely to correspond to the effect which would be found if the mandate were introduced nationally since a nationwide implementation would affect the whole period on ND25+, rather than just the post-randomisation Gateway as in this evaluation.

3.4.2 Internal validity of the experiment

As already noted, random assignment should create an action and a control group which are statistically equivalent to each other. By construction, this 'balance' is achieved for both observable and unobservable characteristics. As a check of whether the experiment has worked, it is helpful to compare the distribution of observable characteristics across the action and control groups. In the following, tests of differences are carried out based on data provided by the random assignment tool.

Table 3.22 shows the distribution of the action and the control groups sample across areas. Among the action group, seven per cent of all customers are based in the district of Bridgend & Rhonda, Cynon, Taff, whereas only four per cent of the control group are resident in this area. This difference is significant as it is for East Lancashire and Shropshire: participants assigned to the action group are less likely to be residents in these districts than participants that are allocated to the control group of the experiment.

Despite this indication that randomisation was not successful with respect to the geographical distribution across Jobcentres, the problem is unlikely to be serious for two reasons:

1 these three areas are relatively small in size, accounting for only 15 per cent of all customers (see Table 3.16). Put another way, the experiment was successful for 85 per cent of customers.

2 related to the first point, since the differences are based on small numbers of customers it might only be a temporary problem. That is, as the study group grows in size, the averages between both groups are likely to balance out.

Table 3.22 Differences in characteristics at date of RA, by area

	Action group (Col. %)	Control group (Col. %)	Difference (%points)
Bridgend & Rhonda, Cynon, Taff	7	4	3*
Buckinghamshire & Oxfordshire	9	10	-1
Calderdale & Kirklees	3	3	1
Coventry and Warwickshire	12	11	1
East Lancashire	5	7	-2*
Essex	15	15	0
Hampshire	12	13	-1
Leicester	15	15	0
Shropshire	2	4	-1*
Somerset	3	3	0
Suffolk	5	5	0
Derbyshire	5	5	0
Gateshead & South Tyneside	3	3	1
Renfrewshire, Inverclyde, Argyll & Bute	3	3	0
Base	1,320	1,247	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

As shown in Table 3.23, no significant difference was found in the gender mix between the action and the control groups. In both samples, roughly 75 per cent of the participants are male and 25 per cent female.

Table 3.23 Differences in characteristics at date of RA, by sex

	Action group (Col. %)	Control group (Col. %)	Difference (%points)
Male	75	75	1
Female	25	25	-1
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.24 shows the ethnicity of the participants. Again, there are no significant differences between both groups, although there are small differences in the shares of customers for the group of white British and black British persons.

Table 3.24 Differences in characteristics at date of RA, by ethnicity

	Action group (Col. %)	Control group (Col. %)	Difference in (% points)
White – British	86	86	1
White – Irish	1	1	0
White – Other	1	2	0
Mixed	0	0	0
Asian or Asian British	7	8	0
Black or Black British	2	1	1
Chinese or other ethnic group	1	0	0
Prefer not to say	1	2	-1
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.25 shows the shares of customers from action and control groups for the age categories 49 – 63 as observed at the date of random assignment. We observe differences of up to two percentage points for the age of 51 and 54; 12 per cent of the action group is 51 years old at the date of random assignment, but only ten per cent of the control group. However, neither this difference nor a similar difference for the 54 year old customers is statistically significant.

Table 3.25 Differences in characteristics at date of RA, by age

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
49	0	0	0
50	11	12	-1
51	12	10	2
52	9	10	-1
53	9	10	-1
54	12	10	2
55	11	12	-1
56	10	10	0
57	10	11	-1
58	10	9	1
59	6	6	-1
60	0	0	0
61	0	0	0
62	0	0	0
63	0	0	0
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

As shown in Table 3.26, there are virtually no differences in the partnership status of the two groups. Exactly the same share of participants in action or control groups are single, married, divorced or widowed. There are small differences in the share of cohabiting couples and separated customers; however, none is significant.

Table 3.26 Differences in characteristics at date of RA, by marital status

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
Single	41	41	0
Married	26	26	1
Living together	3	2	1
Divorced	22	22	-1
Separated	5	6	-1
Widowed	3	3	0
Prefer not to say	0	0	0
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

There are differences in the shares of action and control group participants that reported the lowest or the highest level of qualification; 52 per cent of those in the action group have no formal qualifications compared to 49 per cent of the control group. On the other hand, there is a higher share of highly skilled jobseekers among control group participants (19 per cent compared to 16 per cent for the action group). However, none of these differences is statistically significant (Table 3.27).

Table 3.27 Differences in characteristics at date of RA, by education

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
None	52	49	3
NVQ level 1 or equivalent	11	11	-1
GCSEs or equivalent	13	11	2
A levels or equivalent	8	10	-2
Higher education	16	19	-3
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.28 illustrates the differences between action and control group with regards to the last occupation. Again, there are no significant differences between both groups.

Table 3.28 Differences in characteristics at date of RA, by last occupation

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
Manual	41	40	0
Semi-skilled	16	16	0
Skilled	17	16	1
Office	8	7	2
Professional	10	11	-2
Managerial	9	10	-1
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Tables 3.29 and 3.30 summarise the employment status of participants in action and control groups as well as their partners' employment status. Generally, there are only very small differences between the groups and none is statistically significant.

Table 3.29 Differences in characteristics at date of RA, by employment status

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
Do not work	93	92	1
Work for less than 5 hours per week	1	1	0
Work between 5 and 10 hours per week	3	4	-1
Work between 10 and 16 hours per week	3	2	1
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

Table 3.30 Differences in characteristics at date of RA, by partner's employment status

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
No partner	70	72	-2
Retired	2	1	0
Working over 30 hours per week	5	6	-1
Working between 16 and 30 hours per week	3	3	0
Working less than 16 hours per week	2	1	0
Unemployed - seeking work	3	3	0
Unemployed - not seeking work	13	12	2
Other	2	1	0
Prefer not to say	1	1	0
Base	1,353	1,269	

* significant >= 95%

Source: Customers' data allocated by DWP RA tool, until 31 August 2005

As a summary of the tests on internal validity, the randomisation was successful and balanced almost all observable characteristics between both groups. This provides some reassurance that the control group is well suited to providing a counterfactual outcome for the action. The randomisation has not been successful in balancing the geographical distribution of participants but this is likely to be overcome as the sample size increases.

3.4.3 External validity

This section explores whether the estimated treatment effects can be regarded as externally valid. As shown in Section 3.3.1, there are differences between pilot and non-pilot areas with respect to the ethnic origin of ND25+ participants, the age group and the length of the benefit claim before the beginning of the Gateway. However, the conclusion in Section 3.3.1 was that such differences would not violate external validity if the results obtained by the experimental estimates are weighted accordingly.

In this section, we present further tests on the similarity of pilot and non-pilot areas with respect to the policies implemented, i.e. the time participants spend on the Gateway and the quality of the IAP options. While it is possible to weight experimental results to address compositional differences between pilot and non-pilot areas (as discussed above) it is not possible to proceed in this way when there are differences between pilot and non-pilot areas in the actual treatment being delivered. Intuitively, this is obvious – there is no reason to expect two different treatments to have the same effect.

Table 3.31 compares the time spent on the Gateway before the participation in a first option between pilot and non-pilot areas. As can be seen in the first row, there are significantly different durations on the Gateway between pilot and non-pilot areas even before the introduction of the pilots. In the year 2003/2004, participants in pilot areas spent 94 days on the Gateway before starting the first option of the IAP compared to 86 days in the non-pilot areas.

After the beginning of the experiment, the average duration of the Gateway for participants in IAP is 91 days in the pilot areas and only 71 days in non-pilot areas for the group of interest; the difference has grown from eight to 20 days. Comparing the duration on Gateway for voluntary participants, we find that control observations in pilot areas stay on Gateway for an average of average 93 days before starting an option versus 71 days in non-pilot areas.

Table 3.31 Time until first IAP option before and after introduction of pilots, in pilot areas and non-pilot areas

	Pilot areas		Non-pilot areas		
	Days until first Option	Number of cases	Days until first Option	Number of cases	Mean difference (in days)
ND25+ IAP participants age group 50+, April 2003-April 2004	94	645	86	3,763	8*
ND25+ IAP participants age group 50+, April 2004-May 2005	91	820	71	2,628	20*
ND25+ IAP voluntary** participants age group 50+, after introduction of mandatory IAP	93	121	71	2,628	22*

* significant >= 95%

** Voluntary participants in pilot areas are control group customers only

Source: Merged customers'/ND25+ data, up to 27 June 2005

Apart from differences in the duration of Gateway, pilot areas also implement different types of options than non-pilot areas. This pattern can be found before and after the introduction of the pilots. In pilot areas, 20 per cent of all IAP participants start Basic Skills/Basic Employability Training as their first option in the financial year 2003/4 compared to 17 per cent in non-pilot areas. Fewer participants begin an option promoting self-employment in pilot areas and a much higher share of participants begin a Work Placement Option (25 per cent compared to 19 per cent). In non-pilot areas, 24 per cent of all IAP participants start a Work Experience option compared to 19 per cent in pilot areas (see Table 3.32).

Table 3.32 Differences by type of first IAP option before introduction of pilots, in pilot areas and non-pilot areas

	Non-pilot areas		Pilot areas		
	Total	Col. %	Total	Col. %	Percentage point difference
Employment Option	415	16	104	16	-1
FTE/training	404	16	96	14	-1
BET/BS Option	434	17	133	20	3*
Self-employment (n. NDFM)	147	6	21	3	-3*
Self-employment (NDFM)	2	0	0	0	0
ETO Option	30	1	13	2	1
Work Placement Option	475	19	166	25	6*
Work Experience Option	610	24	126	19	-5*
IAP training	3	0	2	0	0
Job search Option	6	0	1	0	0
Training for Work	19	1	3	0	0
Work-based Learning	2	0	2	0	0
Total	2,547	100	667	100	

* significant >= 95%

Source: ND25+ data for customers aged 50-59, 1 April 2003 – 4 April 2004

These differences in the programmes provided under IAP are more noticeable after the introduction of the pilots: Table 3.33 shows that a much lower share of IAP participants in pilot areas begins an employment option (five per cent versus 15 per cent) than in non-pilot areas and many fewer begin IAP training as their first option (14 per cent versus 21 per cent). The biggest difference is with work experience options; these account for 42 per cent of all participants in the pilot areas but only 19 per cent in the non-pilot areas. Overall, these figures indicate that pilot areas generally carry out different programmes under IAP than non-pilot areas for the population of interest and that there might be some concern whether external validity is credible.

Table 3.33 Differences by type of first IAP option after introduction of pilots, in pilot areas and non-pilot areas

	Non-pilot area		Pilot area**		
	Total	Col. %	Total	Col. %	Percentage point difference
Employment Option	265	15	32	5	-9*
FTE/training		0		0	0
BET/BS Option	423	23	130	21	-2
Self-employment (not NDFM)	287	16	65	11	-5*
Self-employment (NDFM)	2	0	0	0	0
ETO Option	84	5	9	1	-3*
Work Placement Option	20	1	33	5	4*
Work Experience Option	338	19	253	42	23*
IAP training	386	21	86	14	-7*
Job search Option	12	1	0	0	-1*
Training for Work	4	0	1	0	0
Work-based Learning	1	0	0	0	0
Total	1,822	100	609	100	

* significant >= 95%

** ERA areas from January 2005

Source: ND25+ data for customers aged 50-59, 5 April 2004 – 27 May 2005

Table 3.34 compares the IAP options of those in the control group in pilot areas with those in the non-pilot areas. These differences mirror those shown in Table 3.33; again, the work experience option is much more widely used among voluntary participants in pilot areas than in non-pilot areas. The fact that this is the only difference that is statistically significant despite the fact that many of the differences are similar in size to those in Table 3.33 reflects the smaller sample size available in Table 3.34.

Table 3.34 Differences by type of first option after introduction of mandatory IAP pilots for voluntary customers, pilot areas and non-pilot areas

Type of Option 1, voluntary customers aged 50+ only+, Period 5 April 2004 - 27 May 2005	Non-pilot areas		Pilot areas**		
	Total	Col. %	Total	Col. %	Percentage point difference
Employment Option	265	15	4	3	-11
FTE/training					
BET/BS Option	423	23	30	25	2
Self-employment (not NDFM)	287	16	13	11	-5
Self-employment (NDFM)	2	0	0	0	0
ETO Option	84	5	3	3	-2
Work Placement Option	20	1	8	7	6
Work Experience Option	338	19	45	38	19*
IAP training	386	21	17	14	-7
Job search Option	12	1	0	0	-1
Training for Work	4	0	0	0	0
Work-based Learning	1	0	0	0	0
Total	1,822	100	120	100	

* significant >= 95%

** ERA areas from January 2005

Source: ND25+ data for customers aged 50-59, 5 April 2004 – 27 May 2005 (voluntary customers in pilot areas correspond to the control group)

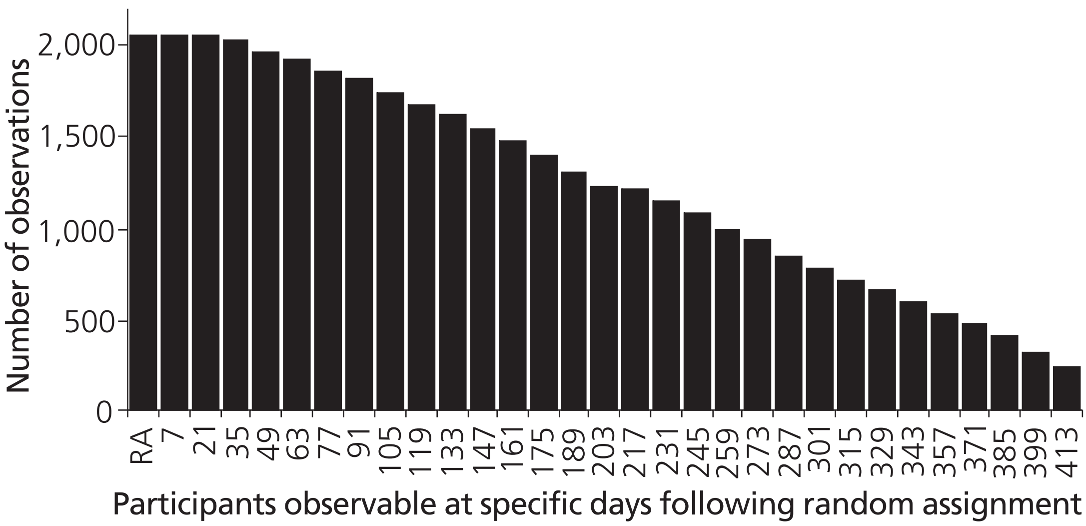
These comparisons indicate that there are significant differences between pilot and non-pilot areas in the type of option implemented and that this might harm external validity. In particular, differences between pilot and non-pilot areas in the treatment offered reduce the scope for generalising the results and may mean that the effects estimated in this evaluation would not apply to all areas but only those implementing IAP in a similar way to the pilot areas.

3.4.4 Participation on ND25+

This section describes participation in the programme for action and control groups as recorded in the merged RA/NDED data until the 27 June 2005. As the experiment is still ongoing, the time period observed following the date of random assignment varies for all customers. In this sub-section, we trace progress through ND25+ from the point of random assignment onwards. Those who became participants in April 2004 can be observed for up to 413 days. Consequently, consideration of longer term outcomes is based on a smaller number of participants than is consideration of shorter term outcomes.

Figure 3.2 shows the numbers of participants that can be observed for different durations following RA. As described in Table 3.1, there are 2,061 participants in the merged RA/NDED data for which we can observe the outcomes following RA, but outcomes can only be observed for three weeks post RA for the entire sample. All descriptions related to later dates and outcomes for periods longer than one month are based on a subset of the sample, which can be observed for the corresponding duration. Figure 3.2 shows that 1,748 are observed for at least 105 days after RA. After a year, roughly 500 participants remain in the sample. Descriptions of participation and outcomes 59 weeks (413 days) after RA use a sample of only 235 participants, i.e. only 11 per cent of the total sample and only those who started Gateway in April and May 2004.

Figure 3.2 Number of participants observable at specific times post random assignment

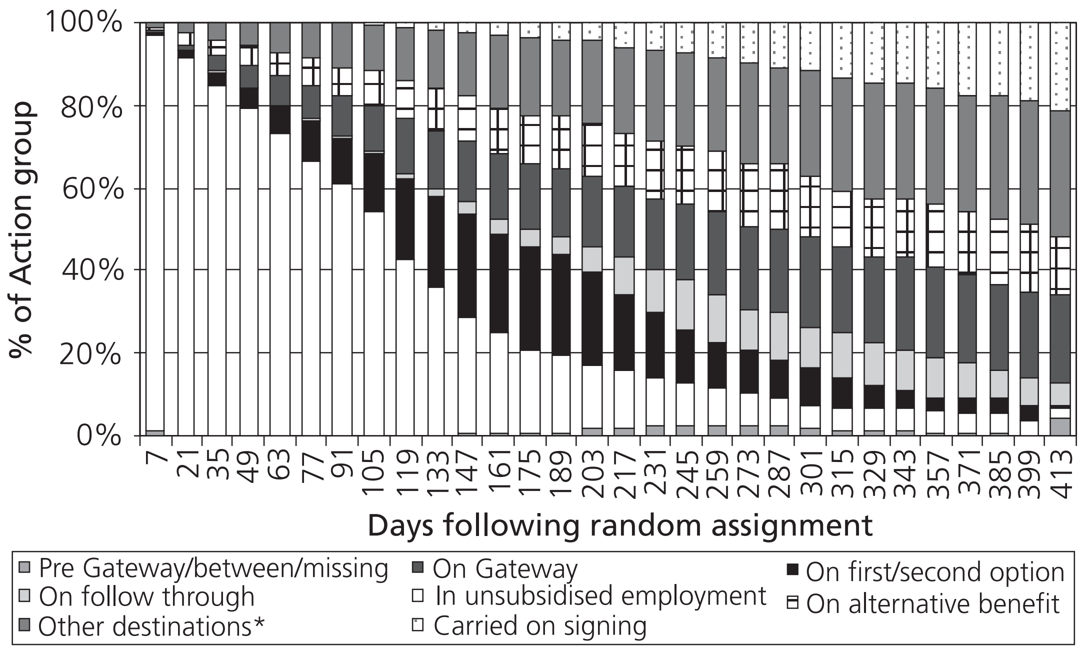


Source: Merged customers'/ND25+ data, up to 27 June 2005

Figures 3.3 and 3.4 show the programme status of the action and control groups beginning the Gateway in pilot areas by RA status based on the merged RA/NDED data for up to 60 weeks after RA. Outcomes are related to the end of these weeks rather than to weekly averages and the time axis shows days following RA instead of weeks. The columns of Figures 3.3 and 3.4 show the destination states of the participants in the action and control groups respectively.

Figure 3.3 shows the participation and outcomes of the action group. Seven days after RA, most are on the Gateway, only 0.5 per cent of these customers have left ND25+ to unsubsidised employment and about one per cent to an alternative benefit. After several weeks, the share of customers leaving the programme increases as well as the number of customers beginning the IAP or leaving to any alternative benefit. After nine weeks (63 days), 75 per cent of the action group are still on the Gateway, but seven per cent have left to employment and another seven per cent have started a first option. After 35 weeks (245 days), 11 per cent are still on the Gateway, although they should have started the IAP after four months. There are still 11 per cent on the first option at this date, while some 18 per cent have left the programme to unsubsidised employment. Those completing ND25+ return to regular signing after they finished the IAP. Eight per cent of all action group participants show this outcome. At the end of the period of observation – about 59 weeks after random assignment – we observe a small group of three per cent remaining on the Gateway, however most of the customers have participated in the programme and show programme-related outcomes. There is a one per cent share of participants still on options, while five per cent are on the follow-through. 22 per cent have left the programme to unsubsidised employment, 14% claim an alternative benefit and another 22 per cent have returned to regular pattern of signing for JSA.

Figure 3.3 Programme status on ND25+ and outcomes, 0-60 weeks after RA, action group

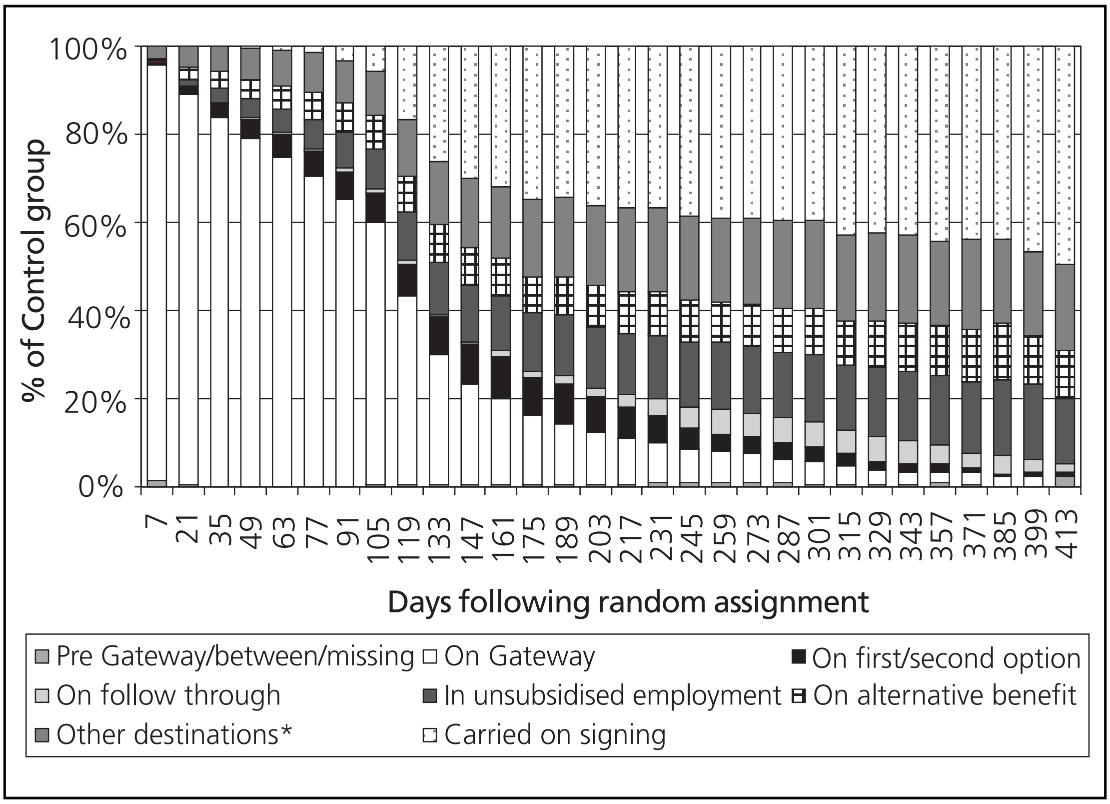


Source: Merged customers'/ND25+ data, up to 27 June 2005

Figure 3.4 shows the programme status and outcomes for the control group. As participation in IAP is voluntary for this group, fewer participants from the control group are expected to participate in the IAP stage. This indeed appears to be the case; participation in the IAP is less common than for the action group. After 133 days (19 weeks), only nine per cent participate in IAP compared to 22 per cent of the action group (Figure 3.3). This difference remains almost constant for 10 weeks. During the first 11 weeks following RA, both groups show very similar employment outcomes with up to eight per cent leaving the programme to unsubsidised employment. After that, participants of the control group have lower shares of participants leaving the programme to unsubsidised employment; 37 weeks after RA, 15 per cent of the control group have left to unsubsidised employment compared to 20 per cent of the action group.

As there is no mandatory participation on IAP, participants in the control group return to the regular pattern of signing directly after the Gateway if they remain on JSA, and there are very few participants observed on the follow through stage of ND25+ as this stage is related to previous IAP participation. As expected, there are many more participants leaving the programme to return to regular signing among the control group than among the action group; 175 days after random assignment, 11 per cent of the control group have left ND25+ to return to regular signing for JSA compared to only four per cent of the action group.

Figure 3.4 Programme status on ND25+ and outcomes, 0-60 weeks after RA, control group



Based on the descriptions of Figures 3.3 and 3.4, we summarise the following:

- Most participants of both groups leave the Gateway within seven months after random assignment; ten per cent of participants remain on the Gateway for a much longer duration.
- The share of participants in IAP is much bigger for the action group than for the control group. Selection into the action group increased the IAP participation as planned and postponed the return to regular signing for JSA for this group relative to the control group.
- The employment outcomes are different for both groups, and the action group shows a higher share of participants starting unsubsidised employment after randomisation than the control group. Chapter 5 explores this in more detail.

Table 3.35, shows that the duration of the Gateway for participants in options differs between action and control groups. While those in the action group show an average duration of 113 days, the mean duration is only 93 days for the control group beginning IAP. It is possible that this difference arises since control group members only volunteer for IAP if they believe it will help them find work. With this potential benefit in mind, they might actively press their ND25+ adviser to enter IAP as soon as possible.

Table 3.35 Time until first IAP option, by RA status

	Action group	Control group
Mean duration on Gateway at date of Random assignment (in days)	113	93
Number of participants on (until end June 2005)	310	121

Difference significant >= 95%

Source: Merged customers'/ND25+ data, up to 27 June 2005

Table 3.36 shows the extent to which the type of the options started by the action or the control group differ. A significant difference in the take up of work experience options is evident; more than half of the action group have first start an option of this type compared to only 38 per cent for the control group. The proportion starting self-employment or basic skills options is lower in the action group than the control group but these differences are not statistically significant.

Table 3.36 Differences by type of first option, by RA status

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
Employment Option	2	3	-1
BET/BS Option	17	25	-7
Self-Employment (not NDFM)	6	11	-5
ETO Option	2	2	-1
Work Placement Option	7	7	0
Work Experience Option	51	38	13*
IAP Training	15	14	0
Base	310	121	0

* significant >= 95%

Source: Merged customers'/ND25+ data, up to 27 June 2005

4 Evaluation results

This chapter presents preliminary estimates of the effects of mandating Intensive Activity Period (IAP) in the pilot areas. Since the piloting stage of the evaluation has not been completed at the time of writing, the results are necessarily of an interim nature. Fuller results will follow in the final report for this evaluation.

An important caveat to the results is that they consider only the immediate destination on leaving New Deal 25 plus (ND25+) and do not allow for the possibility that individuals change their status after this time. For example, a proportion of those entering unsubsidised employment, will later return to Jobseeker's Allowance (JSA). On the other hand, a proportion of those returning to regular signing after ND25+ will later find work. Not allowing for these post-exit changes may result in overstating estimated effects. In view of this, the true effect of the treatment on the probability of an individual having a particular economic status at a given time after random assignment is likely to be smaller than the results presented in this chapter. Consequently, the results should be interpreted with care. For the final report, it will be important to address this shortcoming in the data.

This chapter examines the effects of mandating IAP in three separate ways. First, and most intuitive, simple outcomes estimates are provided by comparing average outcomes among those in the treatment group with average outcomes among those in the control group. Second, analogous estimates are produced using regression models that control for the effects of observable characteristics and so produce more precise estimates of effects. Third, duration analysis is used to model the time until an event (entering work, for example) and the extent to which this is affected by being in the treatment rather than the control group. These three approaches are covered in turn below.

4.1 Simple outcome estimates

A simple way of estimating the effect of mandating IAP is by comparing the average outcomes of the action and control group members. Table 4.1 shows that 60 per cent of participants in the action group and 70 per cent of the control group have left ND25+ by end June 2004. 16 per cent of the action group and 13 per cent of the control group entered unsubsidised employment from ND25+. This difference is not significant at the conventional level but is on the margins of statistical significance. However, there are significant differences when considering those leaving to other destinations. Those in the action group are more likely to move to Incapacity Benefit (IB) or another benefit; in total, 13 per cent of the action group move to a non-JSA benefit compared to nine per cent of the control group. Furthermore, while nine per cent of the action group return to a regular pattern of signing for JSA, the corresponding figure for the control group is 31 per cent.

Table 4.1 Destination states on ND25+, by customer group

	Action group (Col. %)	Control group (Col. %)	Difference in percentage points
Still on ND25+	40	30	10*
Left ND25+ to			
Unsubsidised employment	16	13	3&
Incapacity benefit	8	6	2*
Other (non-IB) benefit	5	3	2&
Gone abroad	1	1	-1
Government training	3	1	2*
Other known destination	4	5	0
Unknown destination	14	10	4*
Carried on signing JSA	9	31	-22*
Number of participants	1,073	988	

* significant >= 95%, & significant 90%

Source: Merged customers'/ND25+ data, up to 27 June 2005 (41% are still on ND25+)

Table 4.2 compares how long individuals spend on ND25+. For those in the action group, the mean duration is 137 days. This is 23 days longer than the mean duration for the control group. A similar difference can be observed between the date of random assignment and the date when the participants finally left ND25+. This suggests that one consequence of mandating IAP is that individuals remain longer on ND25+.

Table 4.2 Duration on ND25+, by customer group

	Action group	Control group	Mean difference (in days)
Mean duration between beginning of and end of programme	137	114	23*
Mean duration between RA date and end of programme	136	113	23*
Number of participants	1,073	988	

* significant >= 95%

Source: Merged customers'/ND25+ data, up to 27 June 2005

To get a clearer insight into how the treatment affects outcomes one must address the fact that individuals vary in how long they are observed post-ND25+ entry. The results that follow do this by considering outcomes relative to the date of random assignment, as discussed in Section 3.4.4.

Table 4.3 reports the proportion of customers who leave ND25+ at specific periods following random assignment. This is a similar approach to that described in Section 3.4.4. It has the advantage of maximising the number of observations on which each effect is estimated. A natural consequence of this is that estimates of outcomes shortly after random assignment are based on a larger number of observations than estimates of outcomes a long time after random assignment. In view of this, subsequent tables report the numbers of participants that can be observed at the specific dates chosen.

The results in Table 4.3 show that the action group has higher outflows to employment than the control group. 105 days after random assignment, 11 per cent of the action group has entered unsubsidised employment compared to 9 per cent of the control group. The difference becomes more pronounced over time and eventually becomes statistically significant: 39-45 weeks after random assignment (273-315 days), the employment rate is five-six percentage points higher for the action group than for the control group. This corresponds to an unsubsidised employment entry rate of around 21-22 per cent for the action group compared to only 14-15 per cent for the control group. The significance of this effect subsequently falls. It is likely that this is due to the smaller number of observations on which it is possible to estimate longer term effects – clearly, the final report from this evaluation will be informative here.

Table 4.3 Proportions leaving to unsubsidised employment 0-60 weeks after RA, by customer group

	Action group		Control group		
Days after random assignment	% of customer group with exit to unsubsidised employment	Number of customers of group	% of customer group with exit to unsubsidised employment	Number of customers of group	% point difference in employment outcome
7	0	1,070	0	985	0
21	2	1,071	2	985	0
35	4	1,056	3	977	1
49	5	1,019	4	942	1
63	7	995	5	925	2
77	8	964	7	902	2
91	10	948	8	869	2
105	11	910	9	838	2
119	13	874	11	805	2
133	14	840	12	779	2
147	15	808	13	743	2
161	16	773	13	708	3
175	16	741	13	666	3
189	16	689	14	622	2
203	17	643	14	591	3
217	17	638	14	582	3
231	18	603	15	551	3
245	18	563	15	520	3
259	19	518	15	478	4
273	20	487	15	454	5*
287	20	444	15	406	5*
301	22	410	15	380	6*
315	21	373	14	346	6*
329	21	342	16	321	5
343	22	312	16	292	6
357	21	280	16	256	6
371	21	246	16	233	5
385	21	214	17	195	4
399	21	160	17	158	4
413	22	116	15	119	6

* significant >= 95%

Source: Merged customers'/ND25+ data, up to 27 June 2005

Table 4.4 considers exits to non-JSA benefits. The results show very little difference between the action and control groups to begin with. However, after 25 weeks (day 175), the table shows a significant difference of four percentage points. The timing of the effect corresponds closely to the timing of IAP entry for those mandated to participate. This is suggestive of a deterrent effect – individuals may choose to exit

JSA rather than participate in the IAP against their will. This difference remains significant and fairly constant until 41 weeks (287 days) after random assignment. Beyond this point, it is insignificant. Again, this is likely to be due to the reduced sample size available for the analysis of long-term outcomes.

Table 4.4 Proportions leaving to non-JSA benefit 0-60 weeks after RA, by customer group

	Action group		Control group		
Days after random assignment	**% of customer group with exit to non-JSA benefit**	**Number of customers of group**	**% of customer group with exit to non-JSA benefit**	**Number of customers of group**	**% point difference in exit to non-JSA benefit**
7	1	1,070	1	985	0
21	3	1,071	3	985	0
35	4	1,056	4	977	0
49	5	1,019	4	942	0
63	5	995	5	925	0
77	6	964	6	902	0
*91	7	948	7	869	0
105	8	910	7	838	1
119	9	874	8	805	1
133	10	840	9	779	2
147	11	808	9	743	2
161	11	773	9	708	3
175	12	741	8	666	4*
189	12	689	9	622	4*
203	13	643	9	591	3*
217	13	638	10	582	4
231	14	603	10	551	4
245	14	563	10	520	4*
259	15	518	9	478	5*
273	15	487	10	454	6*
287	15	444	10	406	5*
301	15	410	11	380	4
315	14	373	10	346	4
329	13	342	11	321	3
343	14	312	11	292	3
357	15	280	11	256	4
371	15	246	12	233	3
385	16	214	13	195	3
399	16	160	11	158	5
413	14	116	11	119	3

* significant >= 95

Source: Merged customers'/ND25+ data, up to 27 June 2005

As an additional outcome, Table 4.5 shows the effect of the mandate on exits to IB. As exits to IB account for the majority of all exits to non-JSA benefits, the results are very similar to those shown in Table 4.4, although generally not as significant. As before, there is a significant effect of the mandate 175 days after random assignment, by which time around nine per cent of the action group will have exited to IB, compared to six per cent of the control group.

Table 4.5 Proportions leaving to IB 0-60 weeks after RA, by customer group

	Action group		Control group		
Days after random assignment	% of customer group with exit to IB	Number of customers of group	% of customer group with exit to IB	Number of customers of group	% point difference in exit to non-JSA benefit
7	1	1,070	0	985	0
21	2	1,071	2	985	0
35	2	1,056	3	977	0
49	3	1,019	3	942	0
63	3	995	3	925	0
77	4	964	4	902	0
*91	4	948	5	869	-1
105	5	910	5	838	0
119	6	874	5	805	1
133	7	840	6	779	1
147	7	808	6	743	2
161	8	773	6	708	2
175	9	741	5	666	3*
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203	9	643	6	591	3
217	9	638	6	582	3
231	9	603	7	551	2
245	9	563	6	520	3
259	10	518	6	478	4
273	10	487	6	454	4*
287	10	444	7	406	3
301	10	410	7	380	3
315	10	373	7	346	3
329	10	342	7	321	2
343	10	312	8	292	3
357	11	280	7	256	4
371	11	246	8	233	3
385	11	214	9	195	2
399	12	160	8	158	4
413	10	116	7	119	4

* significant >= 95

Source: Merged customers'/ND25+ data, up to 27 June 2005

4.2 Impact estimates based on regression models

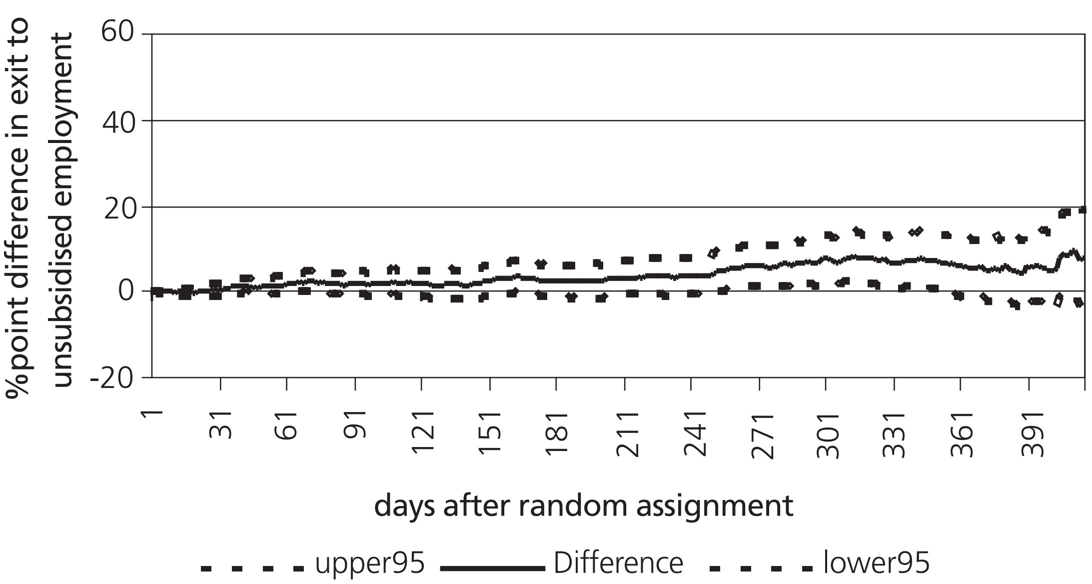
This section reports the estimated treatment effects on three outcomes: exits to unsubsidised employment, exits to non-JSA benefits and exits to all non-benefit destinations (i.e. exits from benefit). These effects are estimated as average differences in the shares of the action group leaving the programme to these destination states compared to the control group, as already shown in Tables 4.3, 4.4 and 4.5. In contrast to the results in the last section, the results in this section are estimated using regression models and so can control for the effect of observable characteristics on the outcome in question. This helps to estimate the treatment effect more precisely.

The estimated effects are presented graphically and show how the effect changes as longer term outcomes are considered. The graphs all follow an identical format. Length of time since random assignment is shown on the x-axis. The y-axis shows the size of the estimated treatment effect as a percentage point difference between the action and control group. A positive effect indicates that the treatment increases the probability of the outcome in question. The significance of the estimates is illustrated using confidence intervals. An estimate is significantly positive (or negative) if its surrounding confidence interval are above (or below) zero (the x-axis). As in the previous section, estimates of the effect on outcomes of different post random assignment durations can only be estimated for those who are observed sufficiently long after random assignment. Because of this, effects on longer-term outcomes are estimated on a smaller sample than effects on shorter-term outcomes. With this proviso in mind, estimates are based on all those randomised on or before 27 June 2005. Outcomes of up to 60 weeks post random assignment are considered.

4.2.1 Employment and benefit outcomes

Figure 4.1 shows the estimated effect on unsubsidised employment. Generally, the treatment effect is positive suggesting that mandating IAP increases the outflows to employment. However, the positive effect in not significant for 36 weeks following random assignment, i.e. the period of the Gateway. 253 days after the participants were randomly assigned, we find a significantly positive outcome; the presence of the mandate increased exits to unsubsidised employment by 4.9 percentage points. This positive effect increases for later periods. For customers who can be observed for a period of 343 days (49 weeks), the estimated effect is 7.9 percentage points. Very few observations are available for consideration of longer term estimates and this reduces the precision of such estimates. Consequently, the positive findings are no longer significant for outcomes more than one year after random assignment.

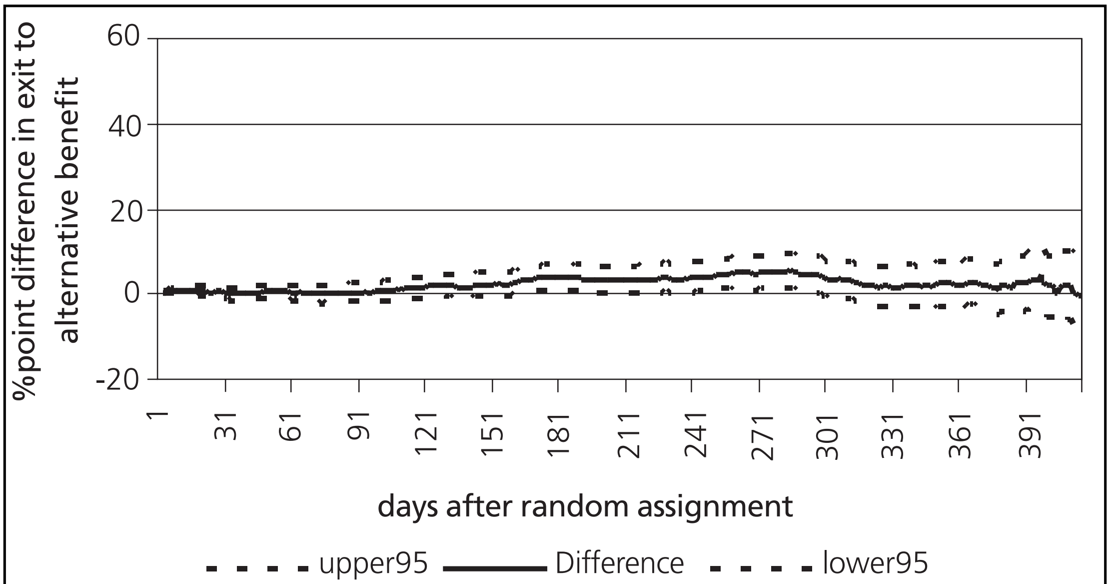
Figure 4.1 Employment effect of mandating IAP



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.2 shows the estimated effect on claiming benefits other than JSA. Again, there is evidence of positive effects, although these are not always significant. The effect of mandatory IAP on exits to non-JSA benefits is insignificant for a period up to 24 weeks (168 days) after random assignment. For longer-term outcomes, no significant effects were found. For outcomes between 24 weeks and 27 weeks after random assignment, the regression models show a significant positive effect of around three percentage points.

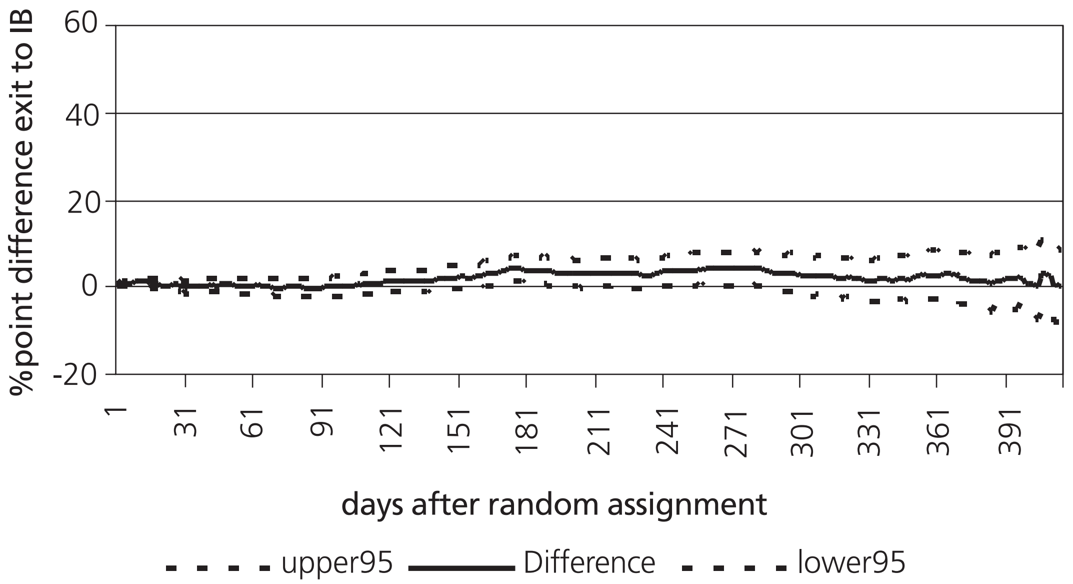
Figure 4.2 Non-JSA benefit effect of mandating IAP



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Participants beginning an IB claim after leaving the ND25+ are the most important subgroup of those leaving JSA to other benefits. The results for this group are similar to those for non-JSA benefits as a whole (Figure 4.3). Significant effects for leaving the programme to IB were only found for a period between 24 weeks and 27 weeks after random assignment, when the exit to IB is around three percentage points higher for the action group than for the control group. However, exit rates of the action group do not differ from those of the control group in the long run.

Figure 4.3 IB effect of mandating IAP



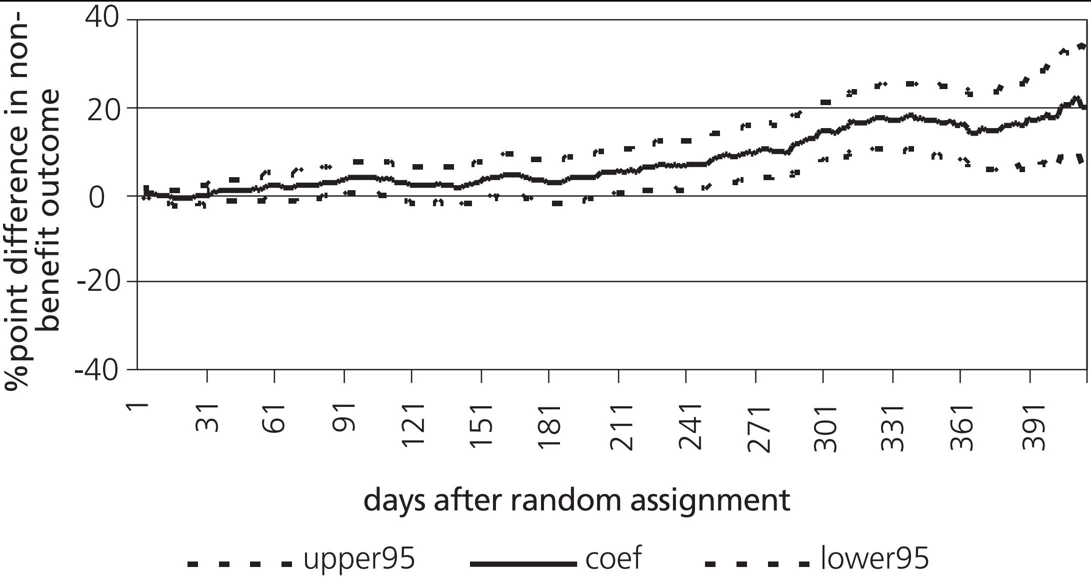
Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.4 shows estimates of the effect on exits to all non-benefit destinations. This consists of participants who have left to employment as well as those leaving to 'other known' and 'other unknown' destinations or who have moved abroad.[8] For the first 35 weeks, the estimated effect was positive but insignificant. A significant positive effect of about five percentage points emerged after 35 weeks. From this point on, the size of the effect grows (and remains significant). One year after random assignment, the estimated effect is 13 percentage points. At the latest date around 60 weeks after random assignment, the estimated effect is around 19 percentage points.

[8] Note that those on government training are regarded as still being on benefit.

An estimated effect of this size should prompt concern about the underlying data. It is possible that individuals who spend longer on IAP are more likely to be recorded as having left to an unknown destination. This would artificially increase the proportion of those in the action group who appear to have left to a non-benefit destination. Informal investigation of the data does not rule out this possibility. For now, the longer-term results for this outcome must be regarded as tentative. We would hope that by the time of the final report, any data uncertainties along these lines will be addressed so more definite results can be presented.

Figure 4.4 Outcomes of mandatory IAP on non-benefit destinations



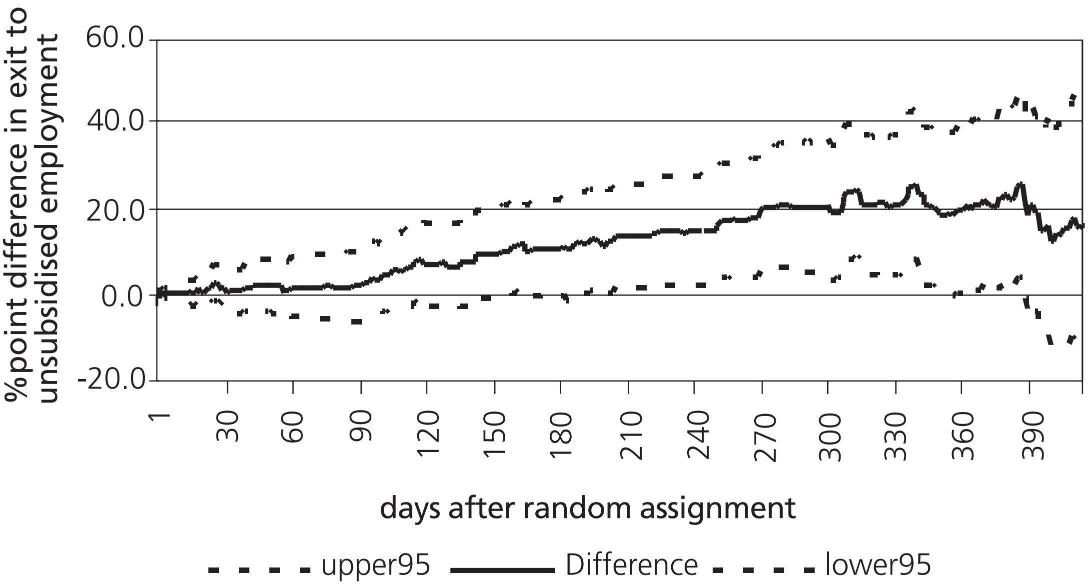
Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

4.2.2 Variations in effectiveness by region

In this sub-section, results are presented showing the extent to which effectiveness of the mandate appears to vary across three of the biggest Jobcentre Plus districts – Coventry and Warwickshire, Leicester and Essex. It should be noted that sub-group results such as these are less precisely estimated than the overall results presented in the previous sub-section since they reflect the smaller number of individuals in a particular sub-group. This results in the wider confidence intervals evident in the following graphs. With longer-term outcomes, it compounds the problem of shrinking sample size already mentioned. By the time of the final report, more precise estimates will be possible.

The estimates in Figures 4.5 to 4.7 show pronounced differences by district in the effectiveness of mandatory IAP on employment. In Coventry and Warwick, the positive effect becomes significant around 160 days following random assignment (RA) at a level of about ten percentage points (as shown in Figure 4.1). This effect is much higher (and still significant) around 38 weeks (270 days) after RA, when the estimates show a difference of around 17 percentage points based on remaining observations. The longest-term outcomes are not significant.

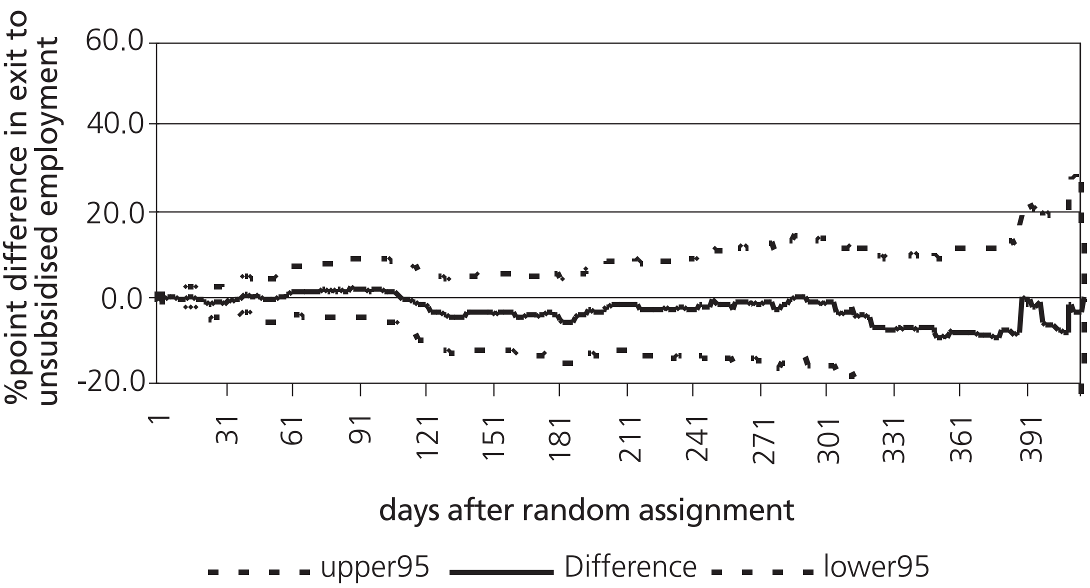
Figure 4.5 Employment effect of mandating IAP, Coventry



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.6 shows the same outcome for Leicester. Here, the estimated effect is insignificant throughout.

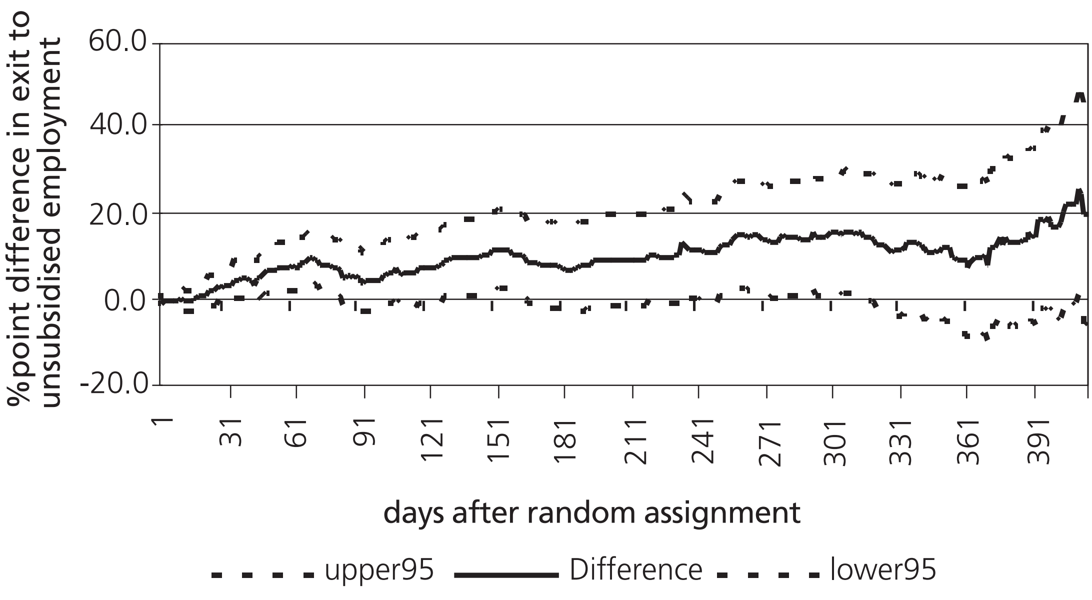
Figure 4.6 Employment effect of mandating IAP, Leicester



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.7 represents the same outcome for Essex. A positive effect is evident for some of the periods observed following RA but, for most of the period, the effects are insignificant. Essex is an interesting area because the employment outcome of the programme is significant for three periods. The first significant positive effect is estimated for the period from 47 days until 84 days following RA, corresponding very well to the part of the Gateway period that leads into IAP participation and, therefore, might reveal some anticipatory effects in Essex that do not exist in a similar way in other areas. The second period for which the estimates are significantly positive lies around 129 days until 165 days after random assignment, at around the time when IAP actually takes place. In both periods, the estimated treatment effect is about ten percentage points. The long-term outcomes based on cohorts that can be observed sufficiently long, are also significantly positive: for the period between 36 and 45 weeks following random assignment (252 to 318 days), there are positive employment effects of around 11 and 14 percentage points. After that, the effect remains positive, although insignificant.

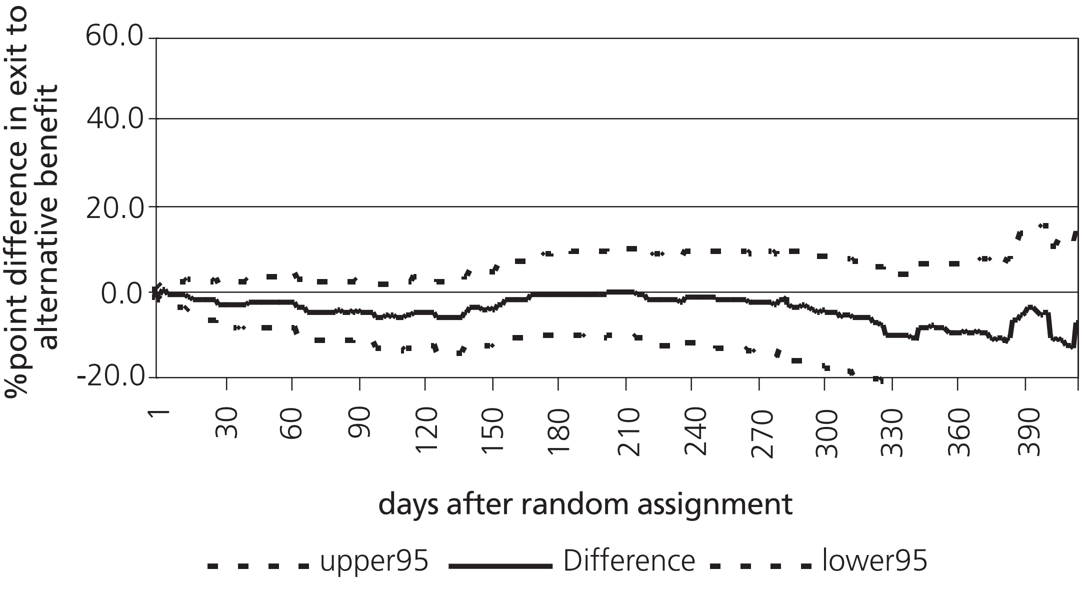
Figure 4.7 Employment effect of mandating IAP, Essex



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figures 4.8 to 4.10 consider the effect on moving to non-JSA benefits. In Coventry and Warwickshire, no significant effects are found.

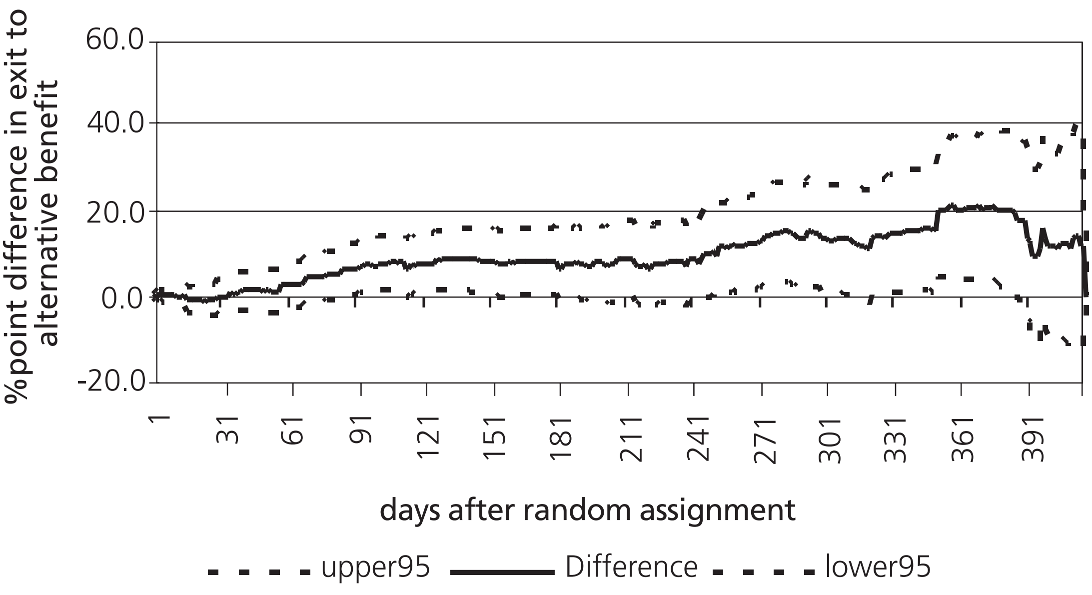
Figure 4.8 Non-JSA benefit effect of mandating IAP, Coventry



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

The results for Leicester (Figure 4.9) are significantly positive for most of the time between 91 and 155 days following RA that should roughly correspond to the period of the mandatory IAP. This effect then becomes insignificant (although only marginally) until 255 days following RA. The effects on outcomes one year after RA are again positive and significant.

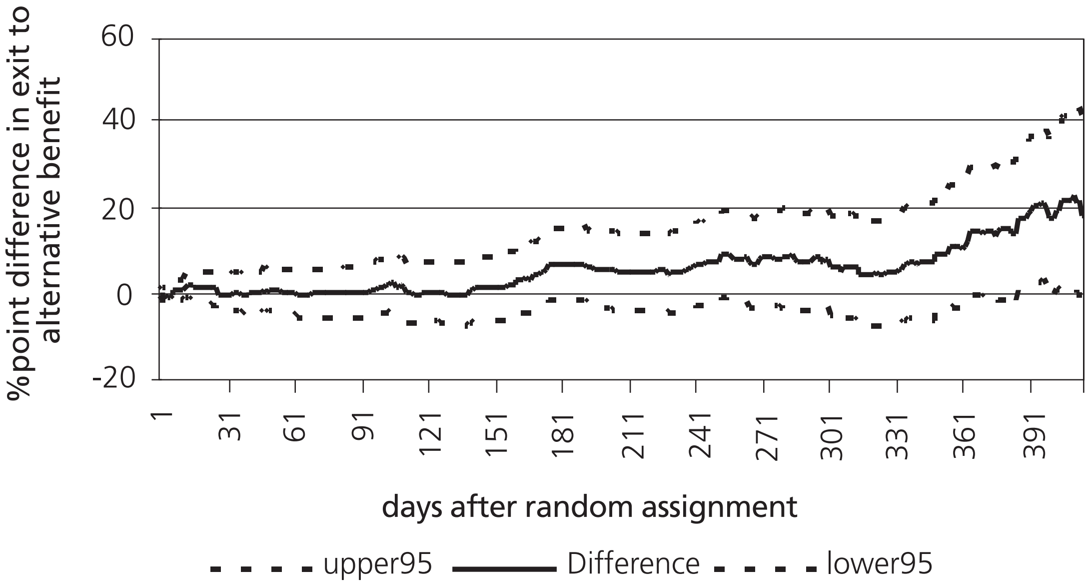
Figure 4.9 Non-JSA benefit effect of mandating IAP, Leicester



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

The effects in Essex appear to be positive but never reach statistical significance, although this is marginal in some cases (Figure 4.10).

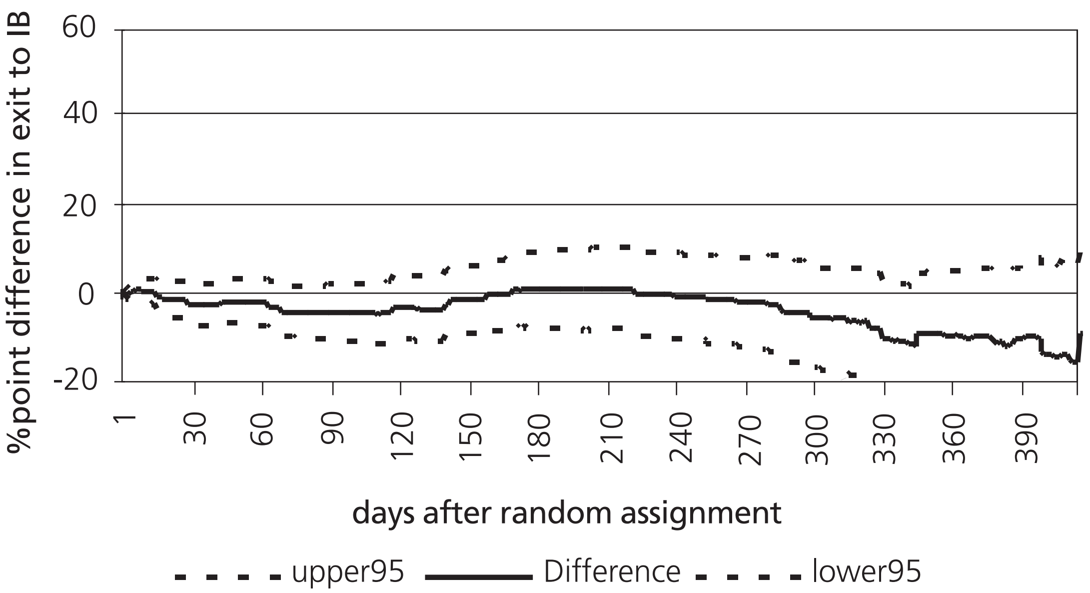
Figure 4.10 Non-JSA benefit effect of mandating IAP, Essex



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figures 4.11 – 4.13 show that the effects on movements into IB are broadly similar to those for non-JSA benefits as a whole. In Coventry and Warwickshire, no significant effects are found (Figure 4.11).

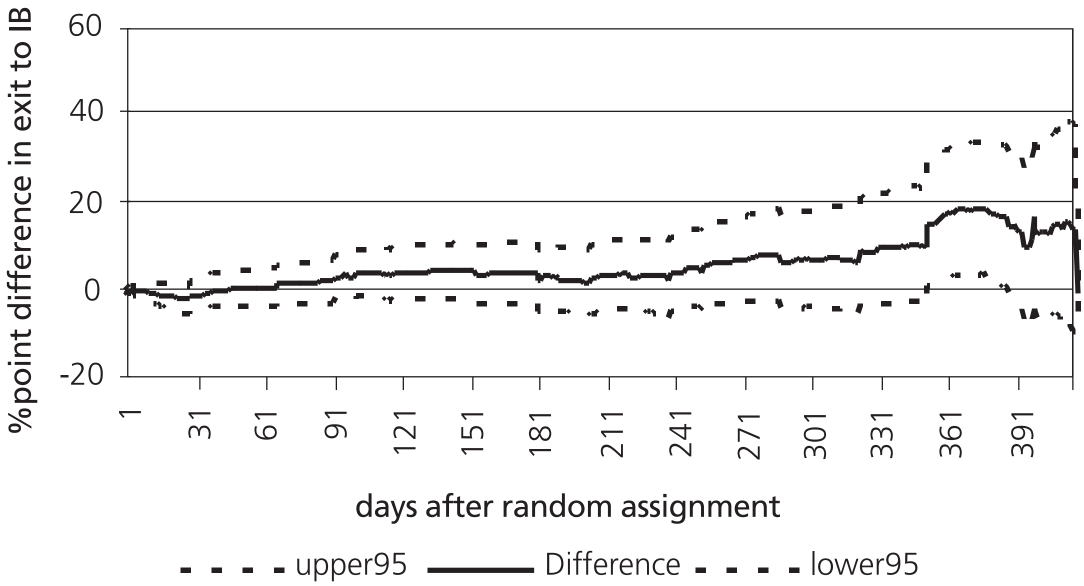
Figure 4.11 IB effect of mandating IAP, Coventry



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

We found significant positive effects on exits to IB in Leicester (Figure 4.12). The differences in exits to IB are insignificant for the period between 91 and 155 days following RA, where a positive effect of the mandate was found for participants leaving to any non-JSA benefit. Around one year after RA there is again a positive and significant effect, which is slightly smaller compared to the exit to all non-JSA benefits as shown in Figure 4.9.

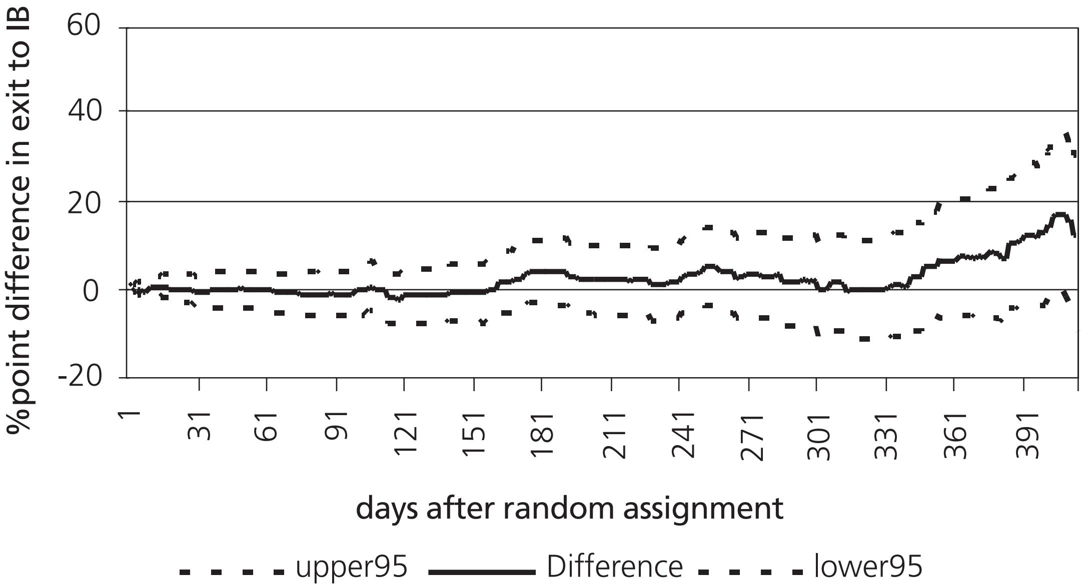
Figure 4.12 IB effect of mandating IAP, Leicester



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

The effect is insignificant in Essex (Figure 4.13), as it was when considering exits to all non-JSA benefits.

Figure 4.13 IB effect of mandating IAP, Essex



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

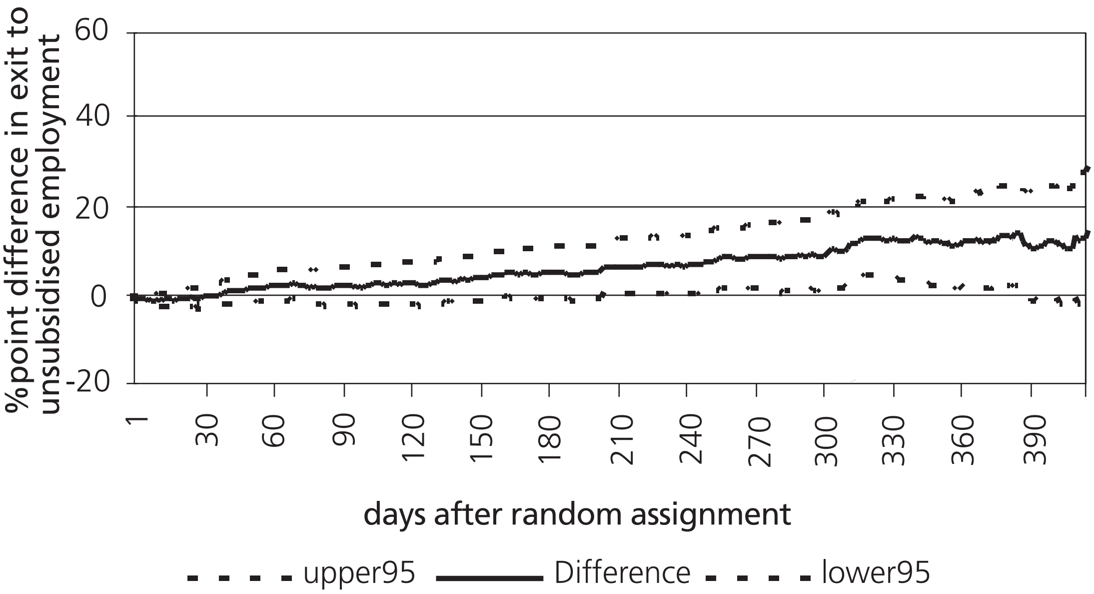
As a summary, the evidence suggests that mandating IAP has beneficial employment effects in Coventry and Warwickshire and Essex, without the adverse effect of moving individuals to other benefits. For Leicestershire, the results are different and it appears that mandating IAP causes individuals to claim other benefits but not to enter unsubsidised employment.

4.2.3 Variations in effectiveness by level of qualification

This sub-section considers how the treatment affects individuals with a particular level of education. The results show large differences between the outcomes of participants without formal qualifications and the other groups. As shown in Table 3.17, half of all customers report no qualifications and are therefore a key subgroup of participants.

Figure 4.14 shows the effect on employment for those with no qualifications. Significant positive effects averaging about six percentage points are evident after 29 weeks (roughly 200 days). This effect increases over time to 13 percentage points around one year and two weeks following random assignment (day 384) before becoming insignificant (although it remains similar in size).

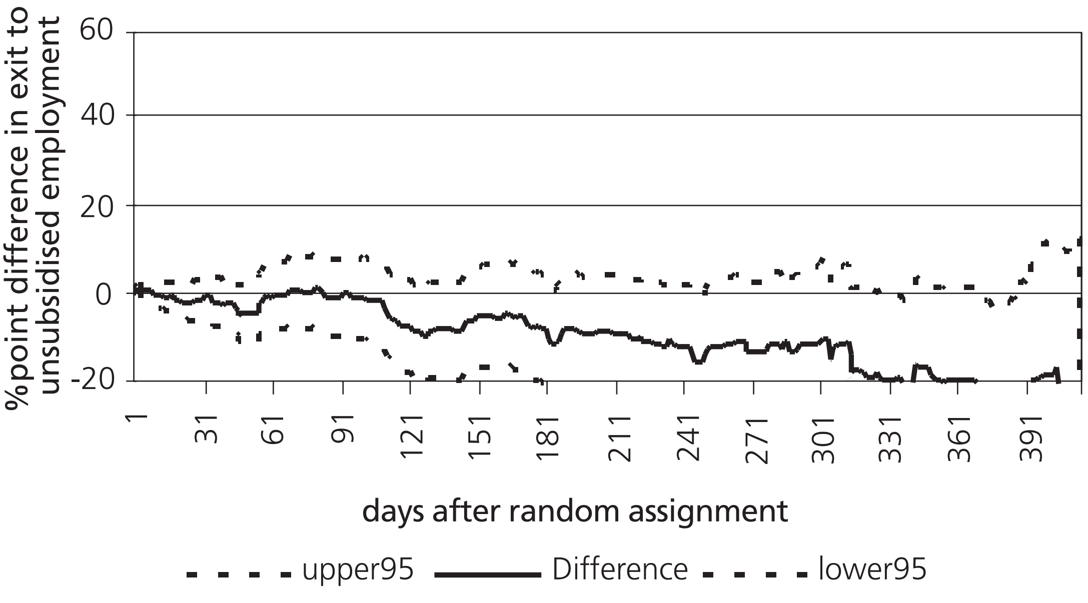
Figure 4.14 Employment effect of mandating IAP, without formal education



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

A different effect is found for those with low level qualifications. Figure 4.15 shows the estimated effects are not significant in most cases. Generally, the effect appears negative indicating that, if anything, mandating IAP reduces the probability of finding unsubsidised employment for this group. Mostly, the insignificance is marginal and indeed some significant negative effects are found (e.g. for the period around 35 weeks/245 days following RA, between 320 and 340 days after RA and again for the period after one year and one week following RA).

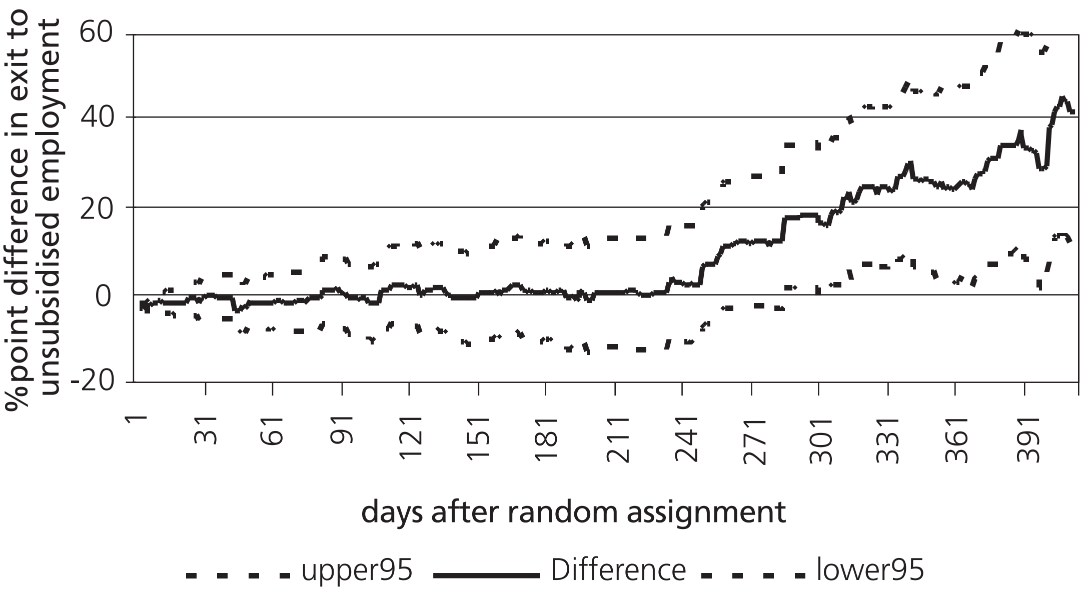
Figure 4.15 Employment effect of mandating IAP, NVQ level 1 or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.16 shows that there is a positive effect for participants with GCSEs or equivalent qualifications. For this group, the regression models find significant positive effects around 290 days following RA. The effect is stronger in the long run, increasing to a level of around 25-30 percentage points.

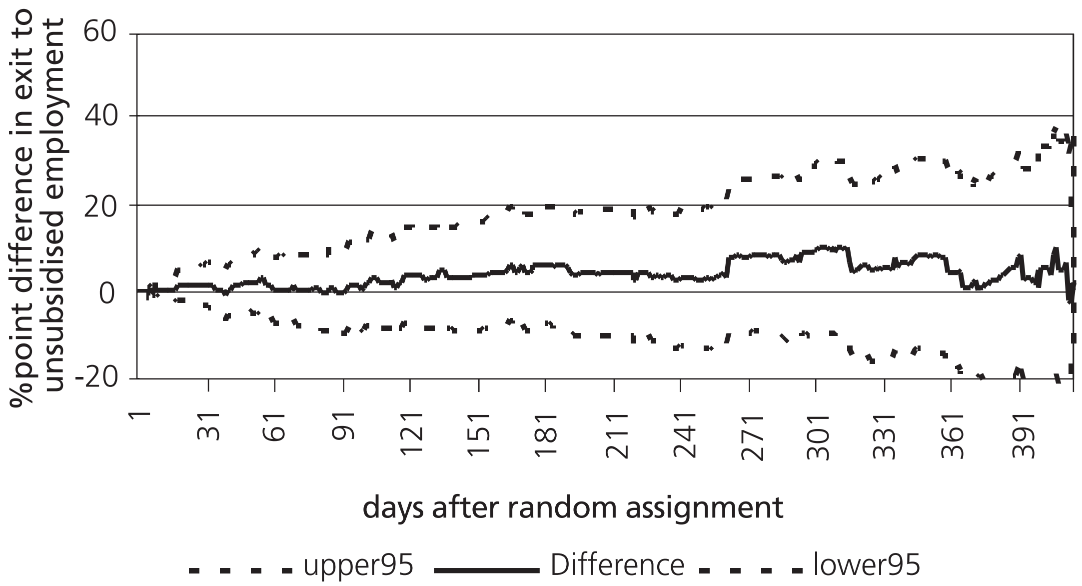
Figure 4.16 Employment effect of mandating IAP, GCSEs or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

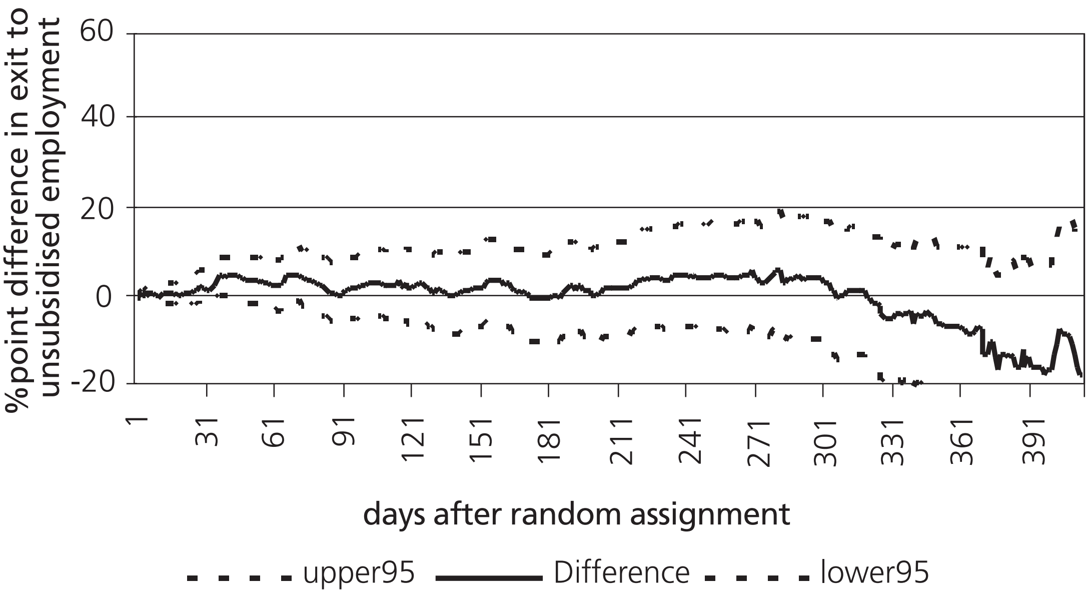
For groups with a qualification at A-level standard or higher, mandatory IAP failed to increase their likelihood of unsubsidised employment in the short- as well as in the long-run (Figures 4.17-4.18).

Figure 4.17 Employment effect of mandating IAP, A levels or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.18 Employment effect of mandating IAP, Higher education

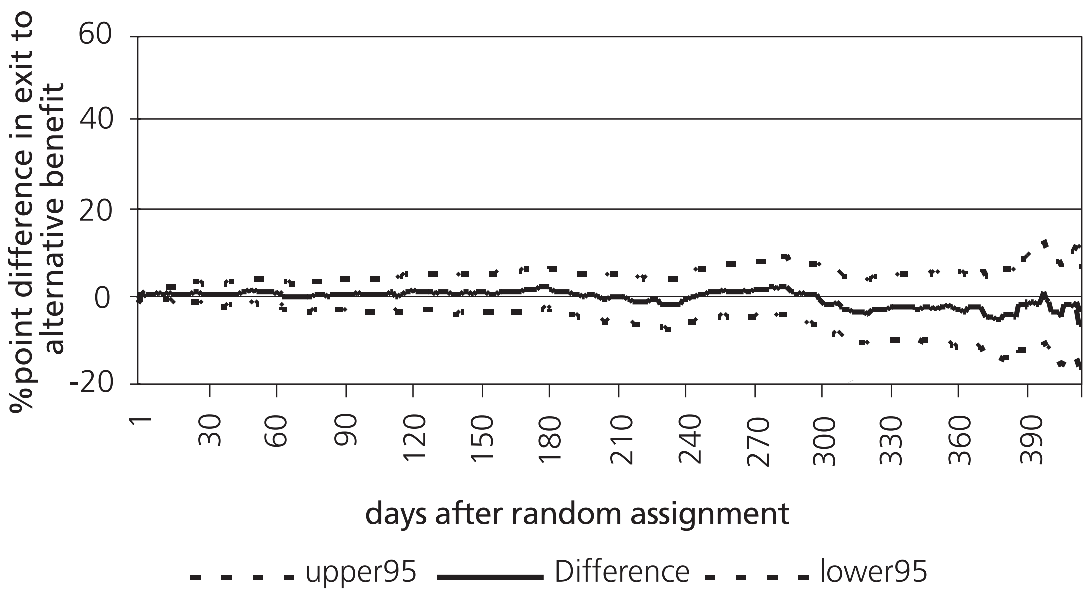


Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

The remaining results in this sub-section relate to the effect of mandating IAP on claiming non-JSA benefits by level of qualification.

No significant effect was found for those with no qualifications (Figure 4.19) indicating that mandating IAP does not cause such individuals to move to another benefit.

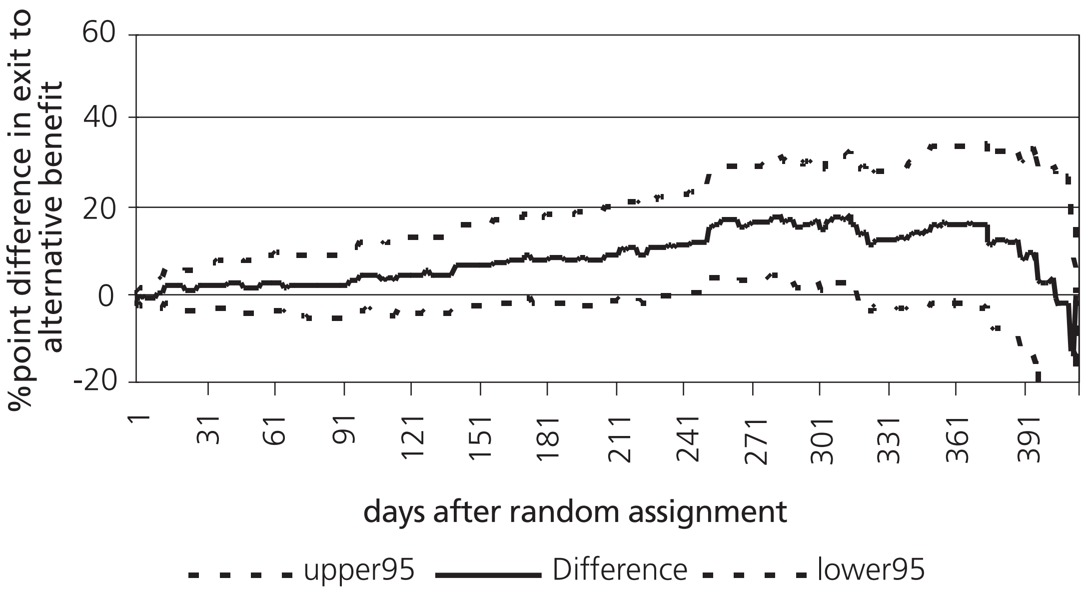
Figure 4.19 Non-JSA benefit effect of mandating IAP, without formal education



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.20 considers those with low level qualifications. A significant positive effect is evident between 242 days and 323 days following the day of RA indicating that mandating IAP significantly increased the probability of claiming a non-JSA benefit for this group. These differences become insignificant for longer-term outcomes.

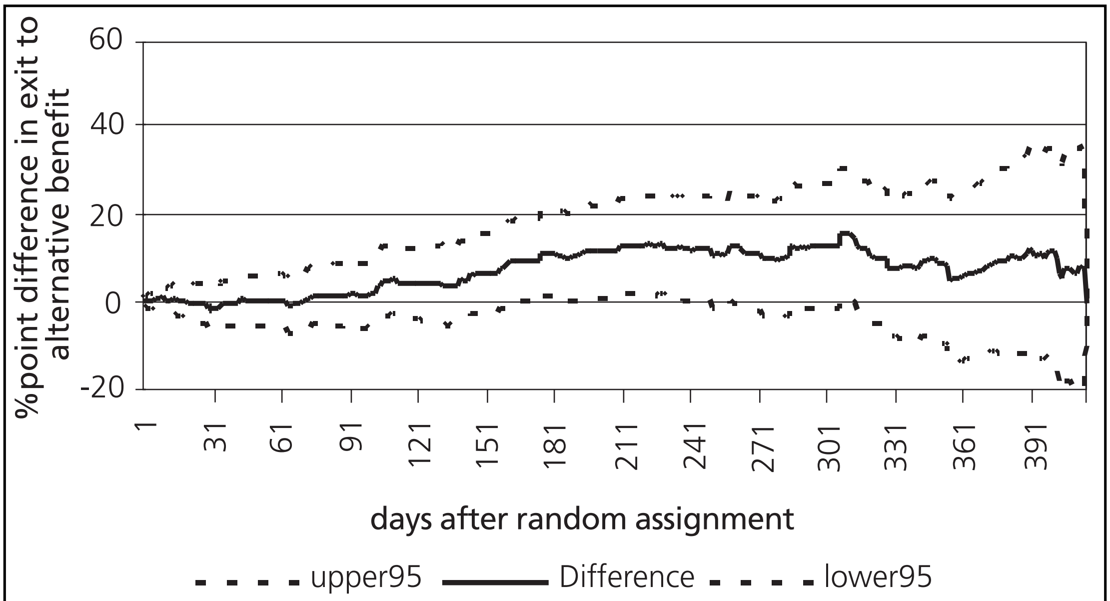
Figure 4.20 Non-JSA benefit effect of mandating IAP, NVQ level 1 or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

A positive effect was also found for those with GCSE standard qualifications. Figure 4.21 shows an increased exit to non-JSA benefits between 160 days and 240 days following RA.

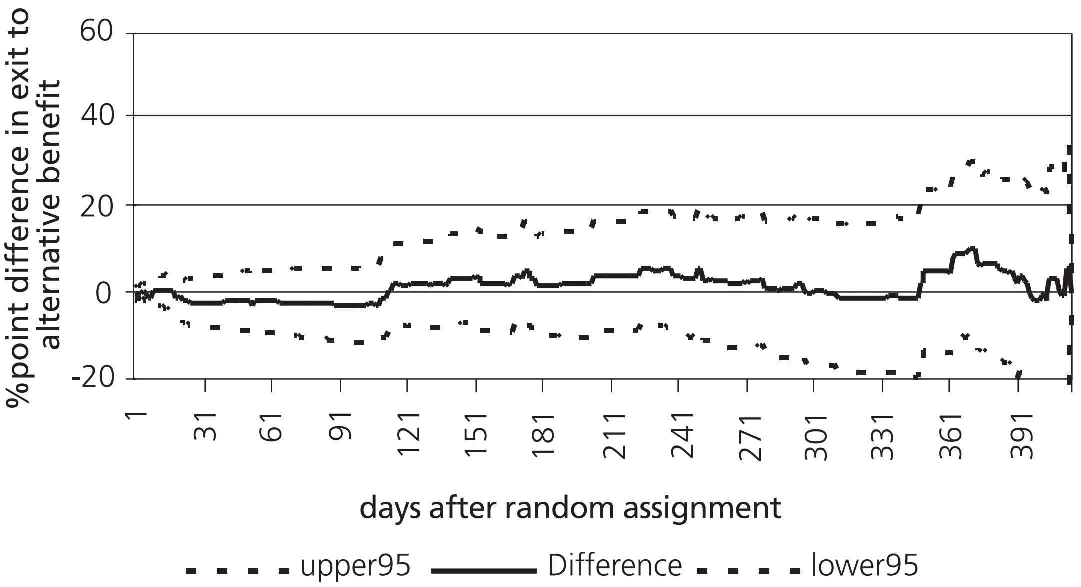
Figure 4.21 Non-JSA benefit effect of mandating IAP, GCSEs or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

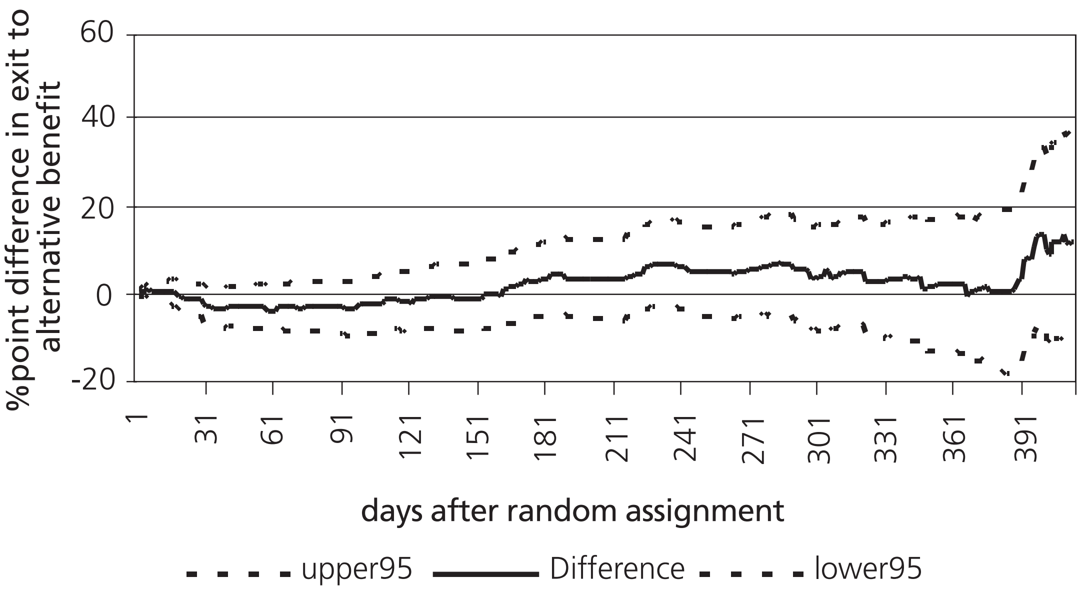
For participants with A-level standard qualification or higher, mandating IAP had no significant effect on exits to non-JSA benefits (Figures 4.22 and 4.23).

Figure 4.22 Non-JSA benefit effect of mandating IAP, A levels or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

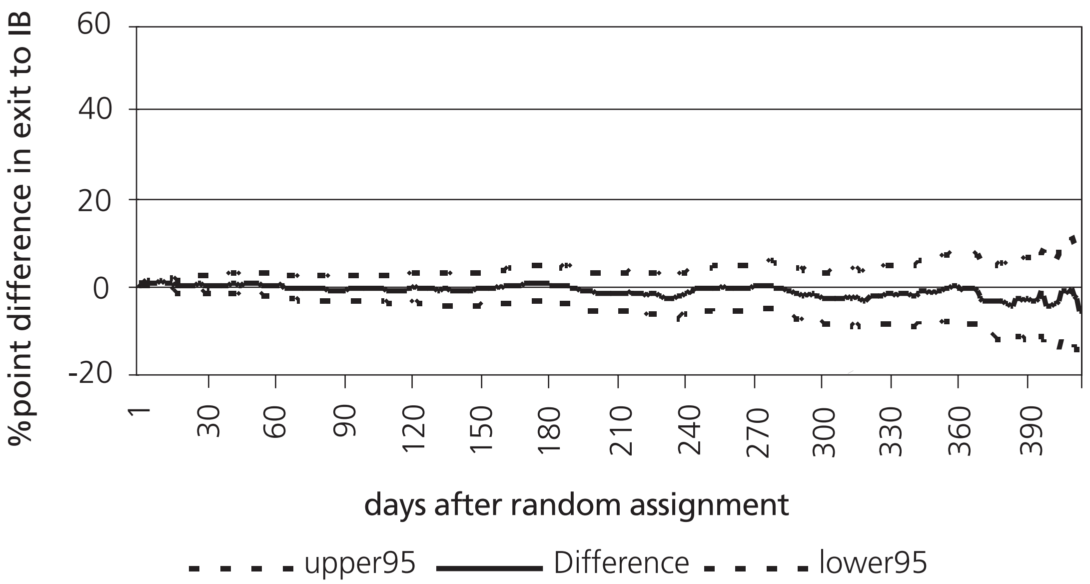
Figure 4.23 Non-JSA benefit effect of mandating IAP, Higher education



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

The effect on exits to IB for participants with no qualifications is shown in Figure 4.24. As with exits to all non-JSA benefits, no significant effect was found for this group.

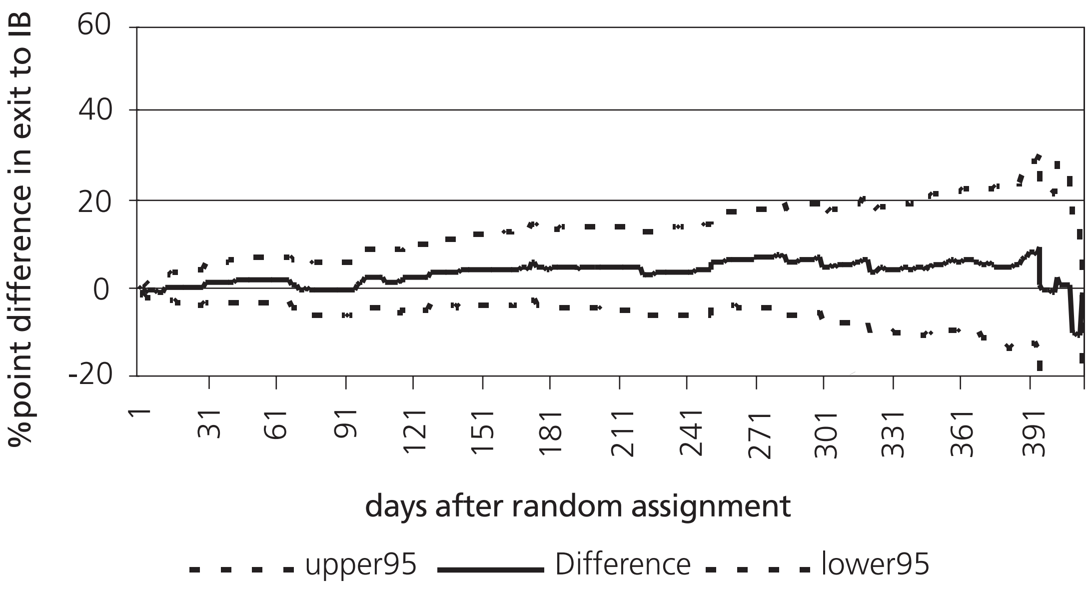
Figure 4.24 IB effect of mandating IAP, without formal education



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

For those with low-level qualifications, no effect on movement to IB is evident (Figure 4.25). This differs from the result for non-JSA benefits as a whole (Figure 4.20) which shows a significant positive effect between 242 and 323 days following the day of RA.

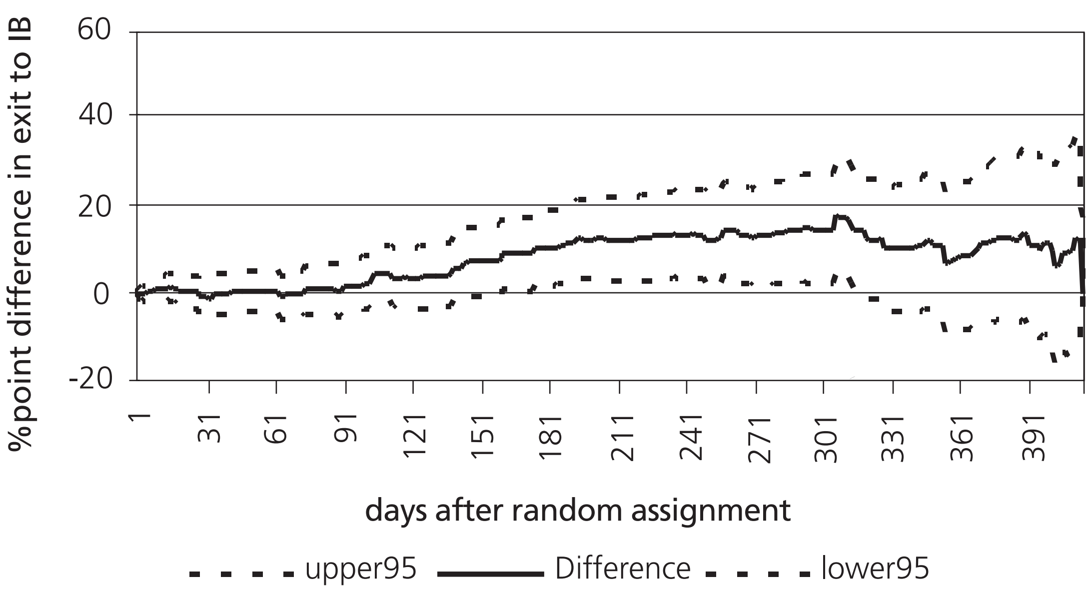
Figure 4.25 IB effect of mandating IAP, NVQ level 1 or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Positive effects are found for participants with intermediate qualifications. Figure 4.26 shows the mandate increases exits to IB among those with GCSE level qualifications by around ten percentage points between 160 days and 240 days following RA. This is a similar result to that found for all non-JSA benefits as a whole.

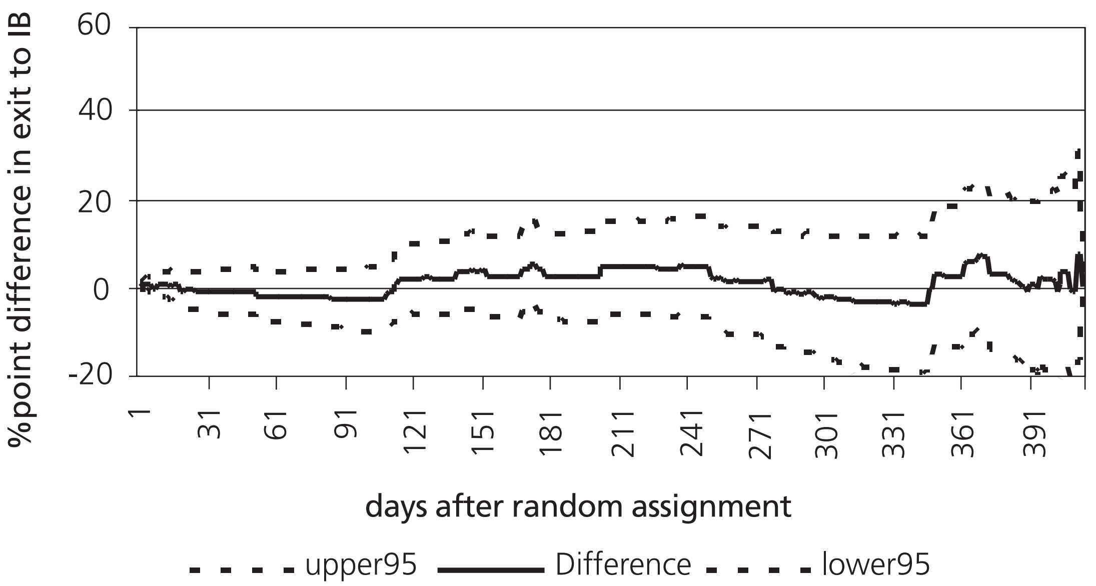
Figure 4.26 IB effect of mandating IAP, GCSEs or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

For those with A-level or equivalent qualifications, no effect on movement to IB is found (Figure 4.27).

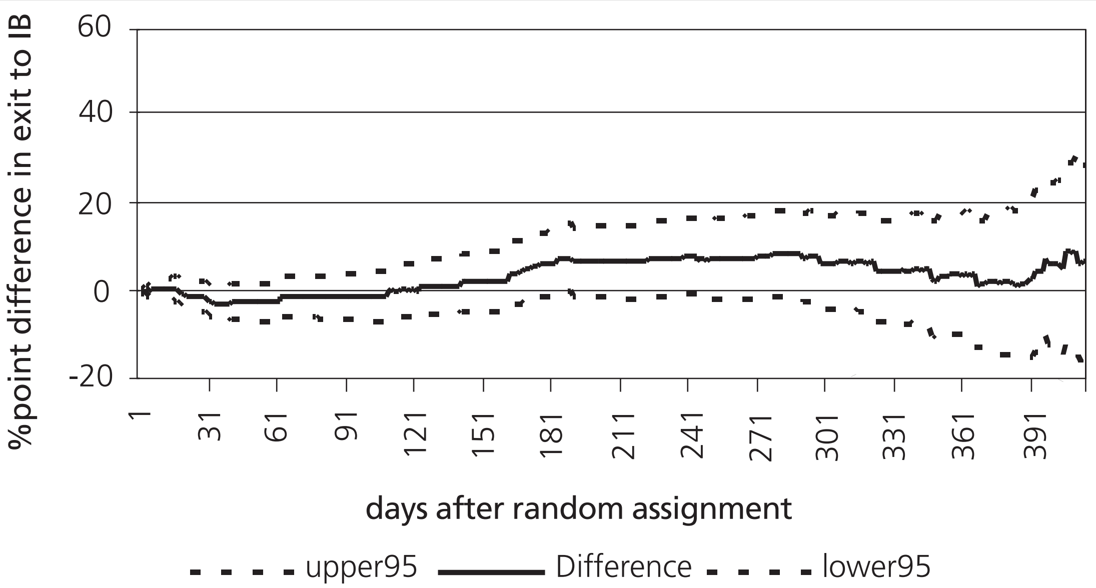
Figure 4.27 IB effect of mandating IAP, A levels or equivalent



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.28 suggests no effect on movement to IB for those with higher education qualifications.

Figure 4.28 IB effect of mandating IAP, Higher education



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

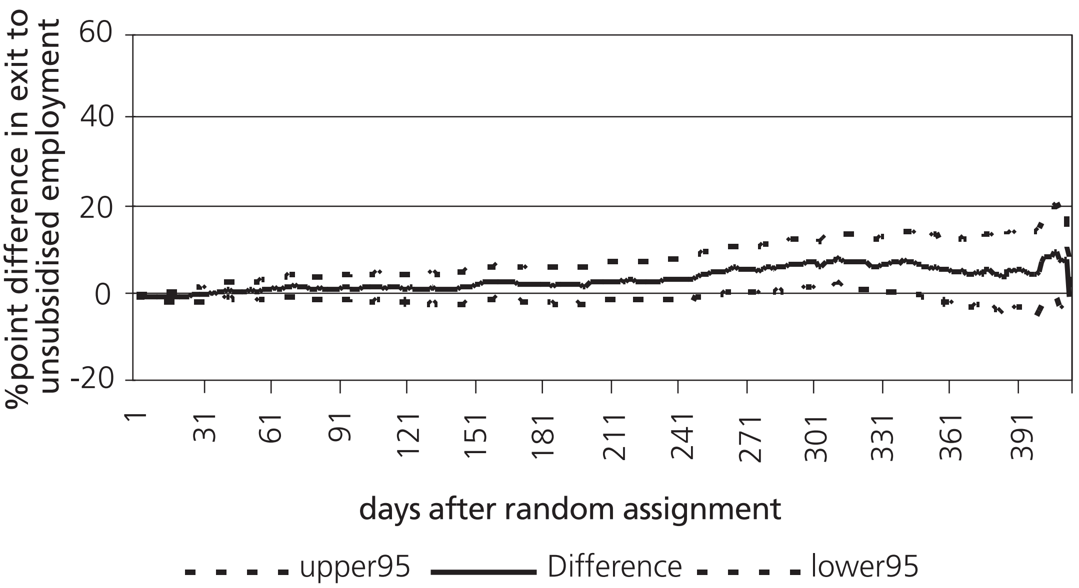
As a summary, the results show that mandating IAP appears to work well for those with no qualifications and those with GCSE or equivalent qualifications. It works poorly for those with qualifications equivalent to NVQ level 1; they tend to avoid employment and move to a non-JSA benefit as a result of mandating them to IAP. The outcomes of those educated to A-level standard or higher appear unaffected by the mandate.

4.2.4 Variations in effectiveness by IB pilot areas

As shown in Table 1.1, some pilot areas are also pilot areas for Pathways to Work. In this sub-section, we consider the extent to which the results differ between Pathways and non-Pathways areas.

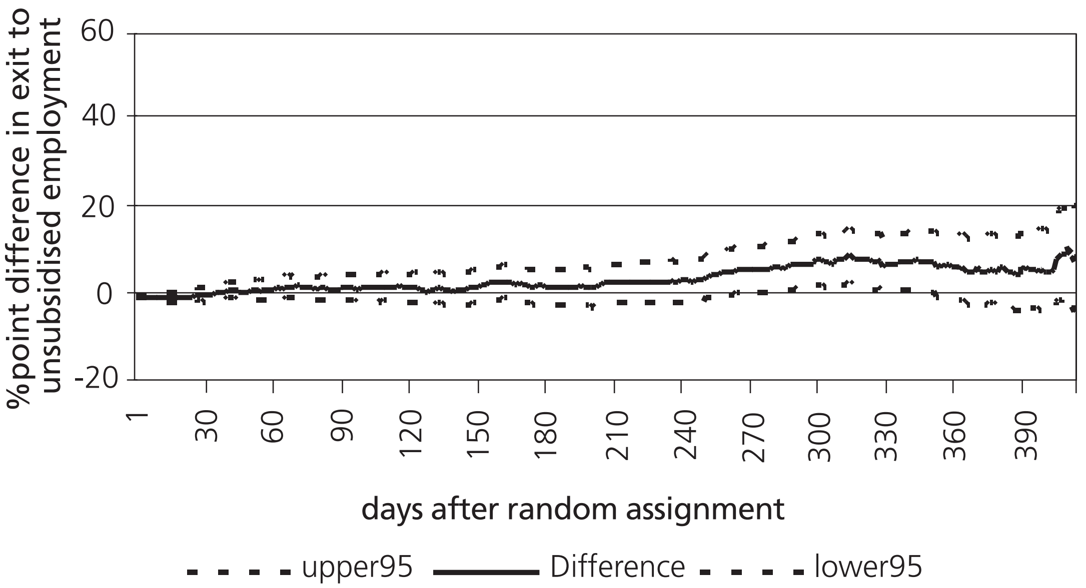
Figures 4.29 and 4.30 show the effects on exits to unsubsidised employment. Overall, the results are very similar for both types of area and none of the observed differences is significant.

Figure 4.29 Employment effect of mandating IAP, IB Pilot areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

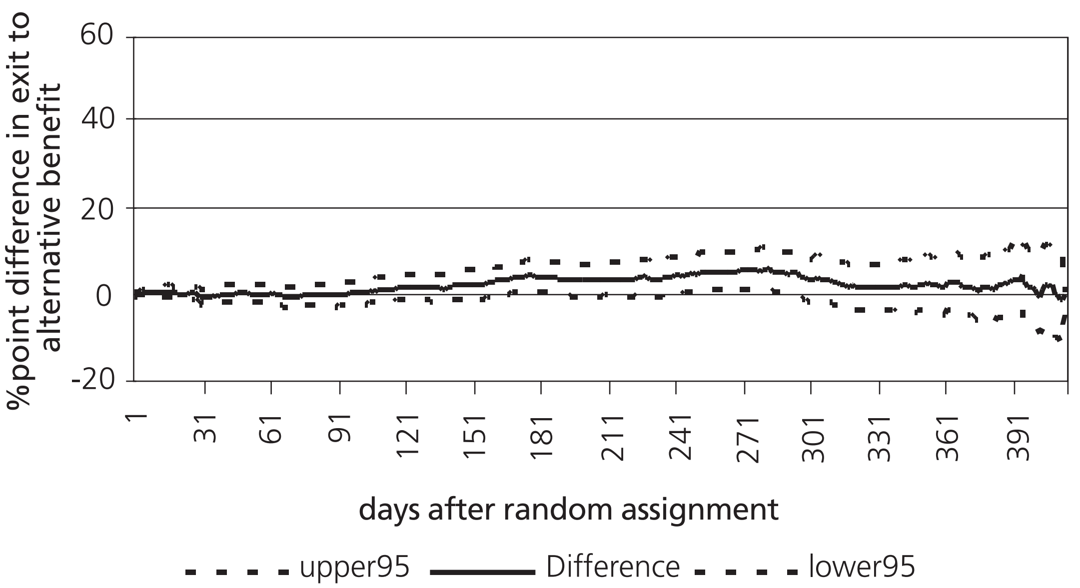
Figure 4.30 Employment effect of mandating IAP, other areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

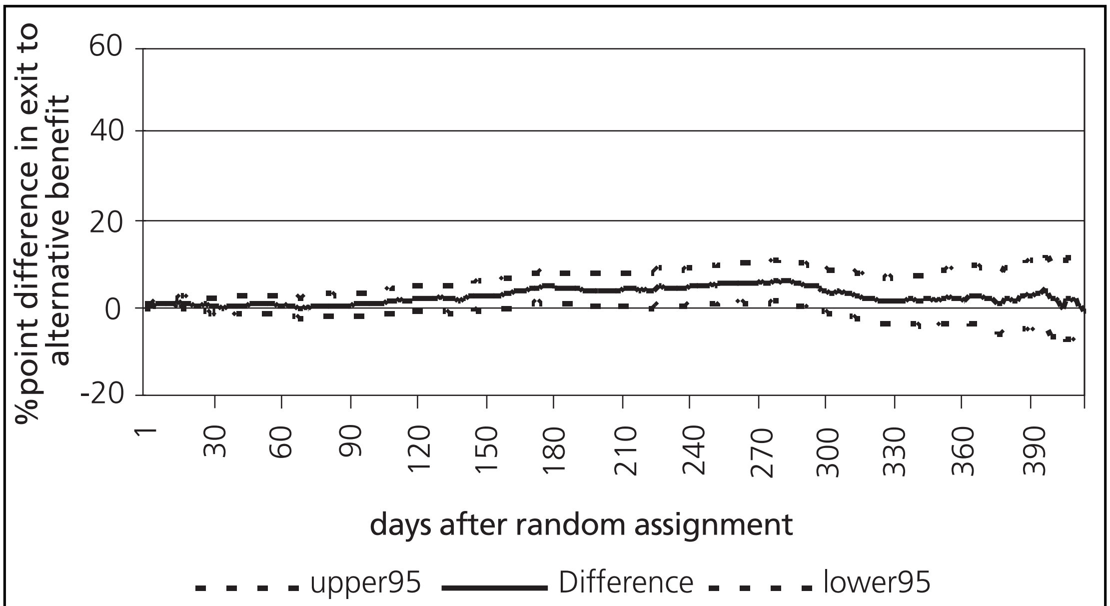
When considering exits to non-JSA benefits, the differences between Pathways and non-Pathways areas are even smaller (Figures 4.31 and 4.32). As before, these differences are not significant.

Figure 4.31 Non-JSA benefit effect of mandating IAP, IB pilot areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

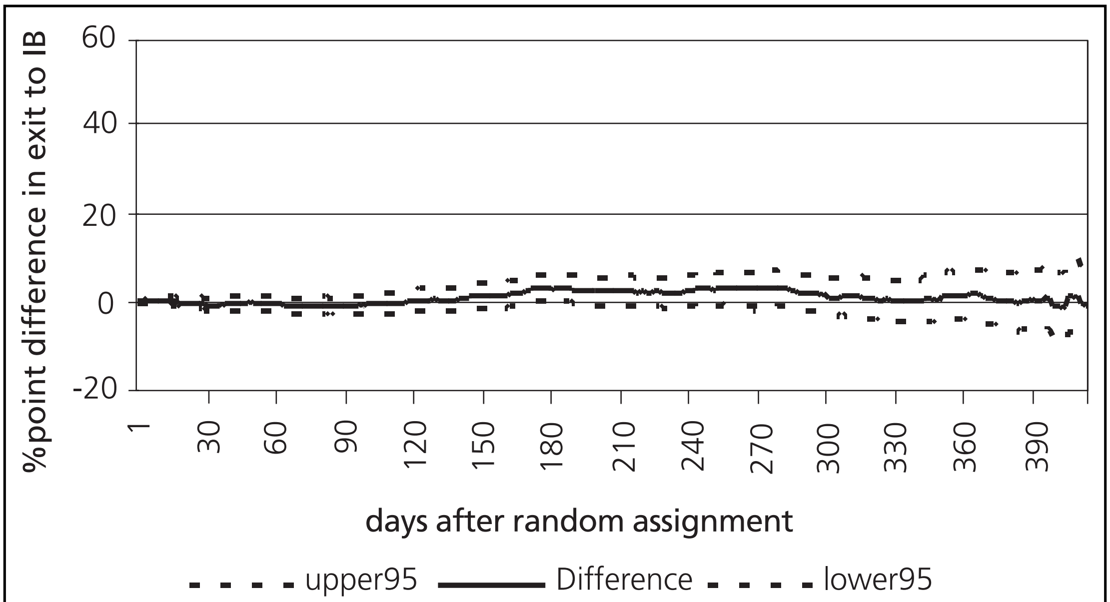
Figure 4.32 Non-JSA benefit effect of mandating IAP, other areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

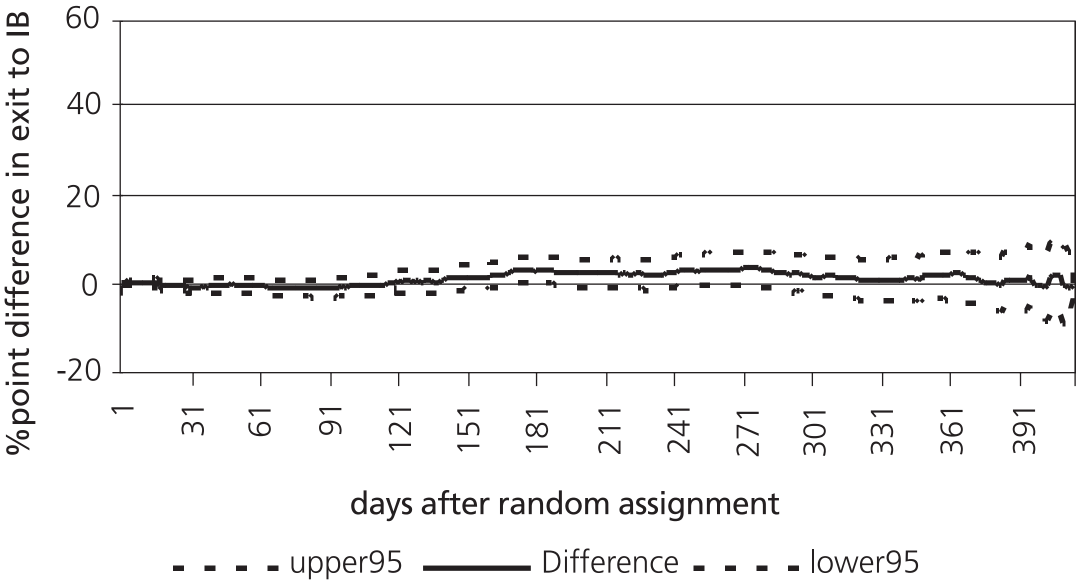
As shown in Figures 4.33 and 4.34, the results do not differ for exits to IB either. Again, no significant differences are evident.

Figure 4.33 IB effect of mandating IAP, IB pilot areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Figure 4.34 IB effect of mandating IAP, other areas



Source: Regression based estimates for merged customers'/ND25+ data, up to 27 June 2005

Summarising the results in this sub-section, the evidence suggests that mandating IAP had the same effects in Pathways and non-Pathways areas.

4.3 Impact estimates based on duration analysis

The key limitation of the results presented in Section 4.2 is that it is not possible to assert that the change over time in the estimated treatment effect represents a true evolution of the treatment effect rather than simply reflecting the changing population on which the different estimates are based. One could argue, for example, that the eventual emergence of a significant treatment effect is due to those entering the experiment earlier in the pilot period (and therefore observed for a longer period of time) being affected more than those entering the pilot later. This means that it is not possible to distinguish changes over time in the treatment effect from cohort effects.

In this section, the results of a different approach to estimating the treatment effect are presented. Duration analysis[9] is a standard econometric tool for modelling the time until an event. Unlike the approach used in Section 4.2, duration models can take account of the fact that individuals vary in the length of time post-RA they are observed.[10] Consequently, the treatment effect can be estimated in a single model

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[9] Also called survival analysis or hazard regression.

[10] However, the results still suffer from the limitation that it is only the first exit from ND25+ that is captured so no account is taken on sustainability of employment or other longer-term issues.

for the whole sample. It is important to note that such models can still provide estimates that can be regarded as causal – the fact of random assignment guarantees this. Indeed, Dolton and O'Neill (1996) used duration models to examine the effect of Restart interviews on unemployment duration – Restart was also piloted using random assignment.

4.3.1 An overview of duration analysis

The central concept in duration analysis is the 'hazard' rate. In the case of this evaluation, this would be the probability of an individual exiting JSA at a particular time conditional on not having exited before that point. It is also possible to consider exits to a specific destination. When considering exits to unsubsidised employment, for example, the hazard rate relates to the probability of exiting JSA to unsubsidised employment at a given time, conditional on not having left JSA to employment before that time.[11]

A key decision in duration analysis is how to characterise the hazard rate. It is often unrealistic to assume that it is constant over time. For example, exits from unemployment tend to be concentrated in the early stages of a spell and become increasingly less likely as the spell lengthens. Such 'duration dependence' corresponds to a declining hazard rate. The approach that is taken in this report is to avoid imposing a particular form on the hazard rate but instead to allow it to vary flexibly over the duration of the spell. This approach is dominant in modern duration analysis.

While the underlying hazard may be flexible in the way outlined above, being in the action group rather than the control group is assumed to have the same effect on the hazard at all stages of the spell. This is quite a restrictive assumption since one might imagine that the hazard rate is likely to vary over the duration of the spell. For example, one possibility is that the hazard rate will increase more for the action group than the control group as the time of entering IAP approaches. However, the assumption of a constant effect is imposed since to do otherwise risks having the results influenced by cohort effects – the very thing that the duration analysis is being carried out to avoid. In the final report, the assumption of a constant effect will be relaxed.

4.3.2 Describing exits from JSA

Before presenting the estimation results, it is helpful to describe the rate at which individuals leave JSA. Figure 4.35 shows exits from JSA into unsubsidised employment. Both lines in the chart represent the probability of remaining on JSA. The solid line represents the control group while the dashed line represents the action group. Basically, the steeper the line, the quicker individuals are to leave JSA and enter employment. From this, it is clear that there is little difference between the action and control groups over the first four months. After this point, those in the action group are more likely to leave JSA for employment than those in the control group.

[11] It is possible to consider multiple destinations simultaneously. However, this places more demands on the data and so is not considered in this interim report.

Figure 4.35 Probability of remaining on JSA rather than exiting to employment

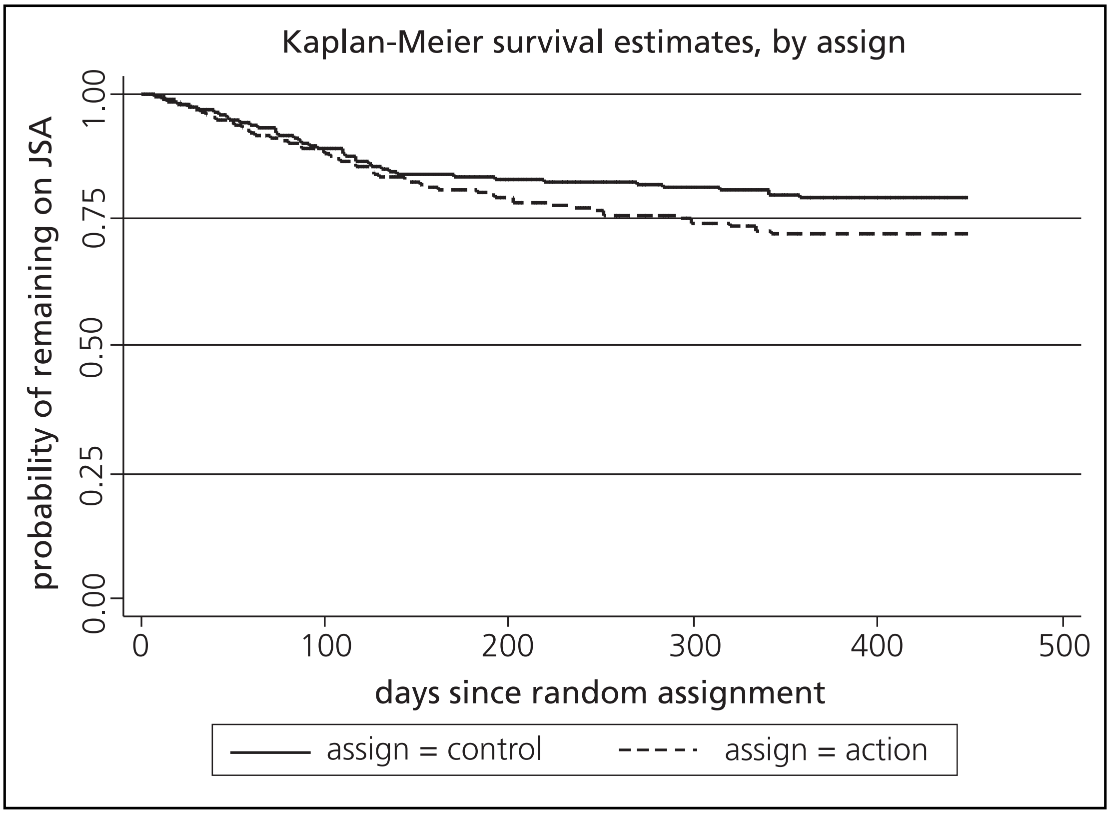
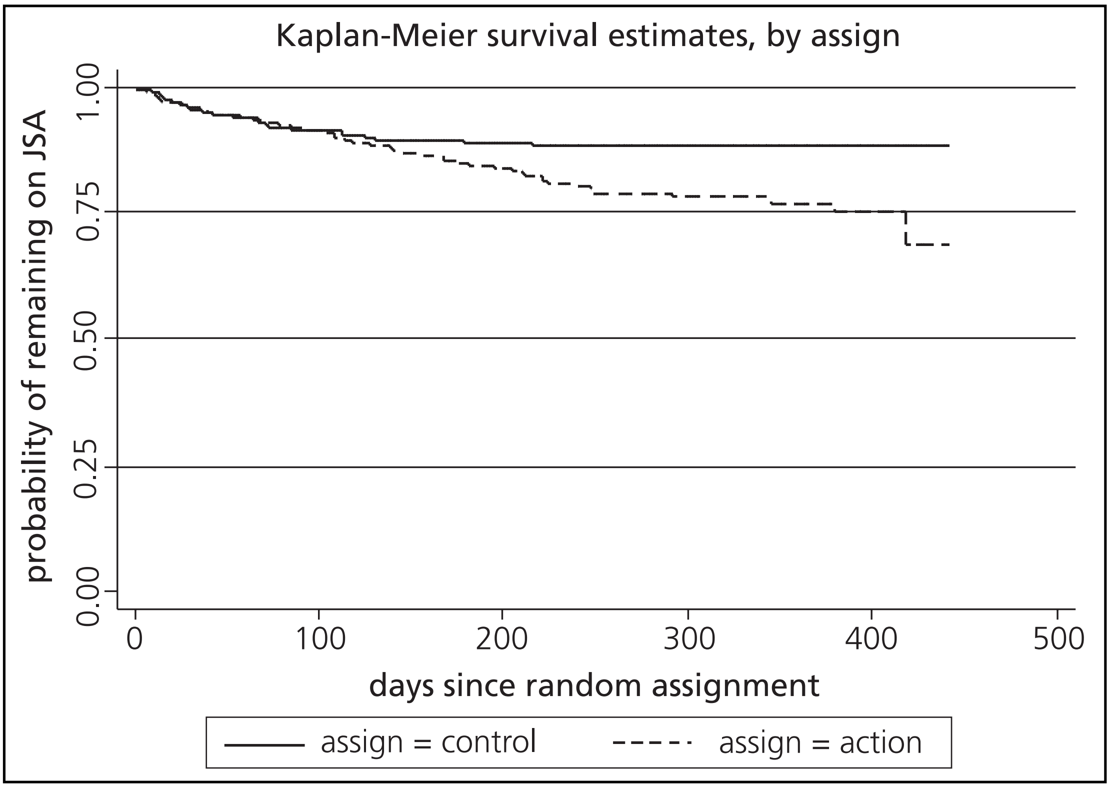


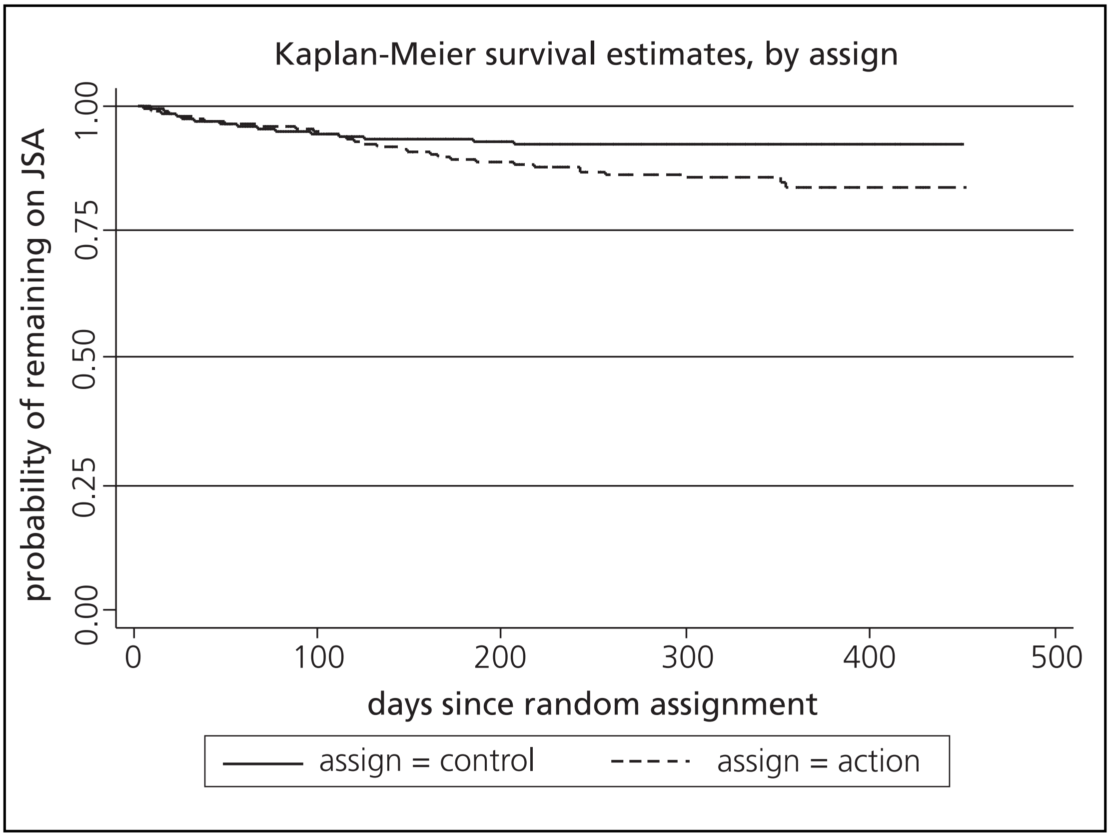
Figure 4.36 presents analogous results for exits to benefits other than JSA. The same general pattern is evident with no difference found for the first four months or so and then those in the action group being more likely than those in the control group to move from JSA to a different benefit.

Figure 4.36 Probability of remaining on JSA rather than exiting to a non-JSA benefit



The same broad pattern is evident when considering exits to IB (Figure 4.37).

Figure 4.37 Probability of remaining on JSA rather than exiting to IB



4.3.3 Estimation results

Duration analysis allows us to proceed beyond the descriptive stage and to examine the extent to which mandating IAP has an effect on exits from JSA. As with the main regression analysis results presented in the previous section, three outcomes are considered: exits to employment, exits to non-JSA benefits and exits to IB. The full results are presented in detail in Table A4.1 in the appendix to this chapter. This gives three columns of results showing the overall effect of treatment on the hazard rate for exits to employment, exits to non-JSA benefits and exits to IB respectively. We discuss the key results below.

The hazard rate and the extent to which it changes over time can be seen in the first six rows of Table A4.1. The coefficients represent the probability that, on any one day, the representative individual[12] leaves JSA to the destination given in the column heading. For example, the first row of coefficients give the hazard rate for those who are in the first post-random assignment month of their spell, the second row of coefficients give the hazard rate for those who are in the second post-random assignment month of their spell and so on. Across both columns, these effects are very significant at all stages of the spell. To give some feel for the size of the hazards, an estimate of 0.001 corresponds to a probability of exit on a given day of one – tenth of one per cent.

The next rows in Table A4.1 show how the hazard rate varies with particular characteristics. The coefficients represent the size of the hazard for those individuals with that characteristic relative to the size of the hazard for individuals without that characteristic. Accordingly, a value close to one indicates that the characteristic in question has no effect on the hazard. The greater than one the estimated coefficient is, the more it is associated with an increased hazard rate. The closer to zero the estimated coefficient is, the more it is associated with a decreased hazard. Scanning the table of results, it is easy to see which characteristics are associated with quicker exit from JSA.

However, the main interest in this report is not in the effect of these characteristics but rather in the effect of the treatment. This is captured by the final coefficient labelled 'Action group' (shown in bold). Looking at the first column, a significant effect on the hazard rate of exit to employment is evident. The coefficient is greater than one, showing that mandating IAP increases the hazard by 32 per cent. Similarly, the second column shows that mandating IAP increases the hazard rate of exit to non-JSA benefits by 49 per cent. This is also significant. The third column suggests that mandating IAP increases the hazard of exit to IB. However, this is only marginally significant. So it appears that the treatment causes an increase in exits to unsubsidised employment, to non-JSA benefits and possibly also to IB.

To make the estimated effects more meaningful, Table 4.6 uses the modelled hazard rates to show the cumulative effect on exits over time. Six points in time are considered: one, two, three, four, six and 12 months after random assignment. At

[12] See the footnote to Table A4.1 for the definition of the representative individual.

each point, the size of the estimated effect is given. This represents the difference in percentage points between those in the action group who are predicted to have left by this point and those in the control group who are predicted to have left by this point. A positive effect indicates that mandating IAP increases exits while a negative effect indicates the opposite. Significant (at the five per cent level) effects are indicated by an asterisk in the second column. Significance at the ten per cent level is shown by a '+'.

Table 4.6 Estimated exits to employment and non-JSA benefits over time

	Destination:	Months post random assignment:					
		1	2	3	4	6	12
	Employment*	0.6	1.3	2.0	2.8	3.6	6.3
	Non-JSA benefit*	0.8	1.2	1.7	2.2	2.8	5.0
	IB+	0.6	0.8	1.2	1.6	2.0	3.8
Variation by district							
- *Coventry*	Employment*	2.0	4.4	6.9	9.4	12.2	20.9
	Non-JSA benefit	0.1	0.1	0.1	0.2	0.2	0.4
	IB	0.0	0.0	0.1	0.1	0.1	0.2
- *Leicester*	Employment	-0.7	-1.6	-2.5	-3.4	-4.4	-7.7
	Non-JSA benefit *	1.9	2.8	4.0	5.2	6.6	11.7
	IB+	1.5	2.1	2.9	3.9	5.1	9.4
- *Essex*	Employment*	1.9	4.2	6.5	8.9	11.6	19.5
	Non-JSA benefit	1.0	1.5	2.1	2.7	3.4	6.0
	IB	0.6	0.8	1.2	1.6	2.0	3.7
Variation by qualification							
- *No quals*	Employment*	0.9	2.1	3.3	4.5	5.9	10.3
	Non-JSA benefit	0.2	0.4	0.5	0.7	0.8	1.5
	IB	-0.2	-0.3	-0.5	-0.7	-0.8	-1.5
- *NVQ1*	Employment	-0.9	-2.1	-3.3	-4.4	-5.7	-9.5
	Non-JSA benefit *	1.5	2.3	3.2	4.2	5.3	9.7
	IB+	1.2	1.7	2.3	3.2	4.2	7.9
- *GCSE*	Employment	0.9	2.0	3.1	4.3	5.5	9.0
	Non-JSA benefit *	1.4	2.1	3.0	3.9	5.0	9.1
	IB*	1.9	2.7	3.9	5.2	6.8	12.5
- *A-Level*	Employment	0.6	1.3	2.1	2.8	3.7	6.5
	Non-JSA benefit *	1.5	2.2	3.2	4.1	5.2	9.5
	IB	1.6	2.2	3.2	4.3	5.6	10.3
- *Higher Ed.*	Employment	0.6	1.3	2.1	2.8	3.7	6.3
	Non-JSA benefit	0.5	0.7	1.0	1.3	1.7	3.0
	IB+	1.2	1.7	2.4	3.3	4.3	7.9

The first three rows in Table 4.6 correspond to the results already presented. As discussed, the effects of the mandate on exits to employment and to other benefits are both significant at the five per cent level, while the effect on exits to IB is significant only at the ten per cent level. They are also broadly comparable in size. After a year, the effect on exits to employment is in the order of six percentage points. The corresponding effect on exits to non-JSA benefits is five percentage points. For exits to IB, the effect is about four percentage points.

Further insight is permitted by allowing the effect to vary across different characteristics. Subsequent rows in Table 4.6 replicate the approach of the regression analysis by considering how the results vary by district and level of education. The results are quite consistent with the regression results in finding significant employment effects in Coventry and Warwickshire and in Essex but not in Leicester. The results for Coventry and Warwickshire (about 21 percentage points after a year) are similar in size to those presented in the previous section while the effects in Essex (about 20 percentage points) are slightly larger. The estimated effects on exits to non-JSA benefits are also consistent with the results of the previous section. Mandating IAP appears to cause those in Leicester to move to other benefits (an effect of about 12 percentage points after a year) but not those in Coventry and Warwickshire or in Essex (the estimated effects are insignificant). The estimated effects on exits to IB also resemble the regression results; it is only for those in Leicester that a significant effect is detected.

The final set of results show how the treatment effects vary by level of education. As with the earlier regression results, we see that the mandate causes those without any formal qualifications to enter unsubsidised employment. The size of this effect is also consistent at about ten percentage points a year after random assignment. However, no other significant effects on employment were found. Turning to the effect on movement onto non-JSA benefits, no significant effect is found for those without qualifications. This is consistent with the regression results. However, significant effects of about nine-ten percentage points a year after RA are found for those with qualifications at the level of NVQ1, GCSE or A-level. Finally, the effects on exits to IB resemble to some extent the effects found using regression analysis. Specifically, the most significant effects are found among those with GCSE or equivalent level qualification. For them, mandating IAP appears to increase the probability of moving to IB by about 12 percentage points.

Overall, the results of the duration analysis broadly accord with those of the regression analysis and provide some reassurance that those earlier results are not being driven purely by cohort effects. However, there are some differences, particularly when considering variations in the treatment effect across those with different levels of qualification.

4.4 Summarising the main results

As noted at the start of this chapter, the results presented above should be interpreted with caution and a fuller analysis, based on more observations and looking beyond first destination on ND25+ exit, will be available in the final report. However, it is of interest to consider the early indications of the effect of mandating IAP.

The results suggest that mandating IAP increases the probability of moving from ND25+ into work by about five or six percentage points. Movements onto other benefits also appear to increase, although the significance of this finding varies over time. The effects found using duration analysis appear more significant than those resulting from regression analysis. It will be interesting to investigate whether this difference remains when basing estimates on the final sample. When considering movements to IB, the regression results and the duration analysis results both suggest effects of marginal significance.

The results indicate some variation by region. Movement into employment appears to have been increased in Coventry and Warwickshire and Essex but not in Leicester, which was more likely to show an increase in movements to other benefits as a result of mandating IAP.

Considering education, it is among those with no qualifications that the most consistent employment effects were found. This is a positive finding since this group accounts for roughly half the sample. The regression results also suggest an increase in job entry among those with GCSE level qualifications. However, this is not found by the duration analysis. This apparent contradiction is perhaps indicative of a cohort effect; since the sample on which the regression results are based changes as longer-term outcomes are considered, it may be that there is something about those randomised earlier in the experiment that predisposes them to being more affected (the duration analysis results are not subject to this changing sample definition).

The patterns of movements onto other benefits or movements onto IB are a little more mixed. The regression results suggest the mandate may induce those with NVQ level 1 or GCSE equivalent qualifications to move to other benefits, but full consideration is made difficult by the small number of observations. The duration analysis finds similar results but also suggests a positive effect among those with A-level qualifications. The strongest effects on movements to IB appears among those with GCSE level qualifications.

5 Conclusion

This report documents the progress to date with the evaluation of mandating Intensive Activity Period (IAP) for those on ND25+ aged 50-59. Unusually for a UK evaluation, this is based on a random assignment (RA) approach.

Overall, the evaluation appears to be progressing well and robust estimates of the effect of the mandate as implemented should be possible by the time of the final report. However, a number of problems have been described in the report. Most importantly:

- the number of individuals included in the experiment is smaller than predicted;
- the implementation of the experiment has not always been as planned.

Both of these problems reduce the size of the sample on which the evaluation is based. A strong recommendation of this interim report is that the reasons for such deviations from the design continue to be investigated and proper implementation be encouraged where appropriate for the remaining duration of the pilots.

In considering the substantive findings, it should be borne in mind that this is an interim report and fuller results will become available later. With this caveat in mind, the early indications are of a generally positive effect of the mandate, with increased employment and only a small effect on movement to other benefits. The results also suggest variation in effectiveness across region and level of qualification.

However, an important limitation of this analysis is that the data only allow consideration of first exit from ND25+. A fuller analysis will allow sustainability of employment, for example, to be examined. Should the mandate only encourage short-term jobs, the view of its effectiveness will change accordingly. In view of this, the second strong recommendation of this interim report is that adequate data be made available to investigate outcomes beyond initial ND25+ exit. This additional data should include administrative benefit records and could also include administrative records on employment spells and even survey data, if that were felt to be appropriate.

Appendix
Duration analysis results

Table A4.1 Duration analysis results

	(1) unsubsidised employment	(2) Non-JSA benefits	(3) IB
Baseline hazard:			
- 1 month since RA	0.001	0.001	0.000
	[20.87]**	[17.17]**	[14.69]**
- 2 months since RA	0.001	0.000	0.000
	[20.15]**	[17.83]**	[15.28]**
- 3 months since RA	0.001	0.000	0.000
	[20.00]**	[17.47]**	[14.97]**
- 4 months since RA	0.001	0.000	0.000
	[19.47]**	[17.23]**	[14.63]**
- 5-6 months since RA	0.001	0.000	0.000
	[20.76]**	[18.19]**	[15.54]**
- more than 6 months since RA	0.000	0.000	0.000
	[22.37]**	[19.36]**	[16.65]**
Age at time of RA:			
-51	0.895	1.682	1.615
	[0.45]	[1.35]	[1.06]
-52	0.671	2.038	1.807
	[1.46]	[1.86]+	[1.28]
-53	0.882	2.059	2.067
	[0.51]	[1.98]*	[1.69]+
-54	0.767	2.629	2.552
	[1.06]	[2.73]**	[2.23]*

Continued

	(1) unsubsidised employment	(2) Non-JSA benefits	(3) IB
-55	0.994	2.001	1.721
	[0.03]	[1.85]+	[1.21]
-56	0.795	2.029	1.670
	[0.91]	[1.86]+	[1.12]
-57	0.631	2.048	2.168
	[1.74]+	[1.91]+	[1.77]+
-58	0.432	2.929	2.633
	[2.58]**	[2.87]**	[2.16]*
-59	0.740	4.368	1.723
	[0.98]	[3.93]**	[1.03]
Non-white ethnic group	0.492	1.295	1.440
	[3.05]**	[1.26]	[1.46]
Lives with partner	1.132	0.751	0.682
	[0.78]	[1.41]	[1.47]
Lives with partner who works	0.928	0.946	0.918
	[0.33]	[0.16]	[0.19]
Disabled	0.809	1.692	1.893
	[1.63]	[3.68]**	[3.53]**
Has drivers licence	0.960	0.897	1.086
	[0.23]	[0.61]	[0.38]
Has drivers licence and access to car	2.302	1.379	1.138
	[4.65]**	[1.50]	[0.49]
Working part-time at time of RA	2.366	0.758	0.630
	[4.93]**	[0.94]	[1.16]
Occupation before ND25+:			
- *semi-skilled*	1.156	1.316	1.170
	[0.81]	[1.41]	[0.66]
- *skilled*	1.142	1.234	0.883
	[0.70]	[0.93]	[0.43]
- *office*	0.757	1.136	0.809
	[0.96]	[0.42]	[0.53]
- *professional*	0.564	1.622	1.448
	[2.06]*	[1.55]	[0.98]
- *managerial*	1.301	1.720	0.986
	[1.15]	[1.86]+	[0.04]
Type of accommodation tenure at time of RA			
- *owned*	1.248	0.818	1.048
	[1.34]	[1.03]	[0.20]
- *private tenant*	0.864	0.907	0.959
	[0.82]	[0.53]	[0.18]
- *other*	0.773	0.818	0.901
	[1.03]	[0.75]	[0.31]

Continued

	(1) unsubsidised employment	(2) Non-JSA benefits	(3) IB
Pension	0.817	0.611	0.582
	[1.39]	[2.44]*	[2.08]*
Highest qualification at time of RA:			
- *NVQ level 1 equivalent*	1.322	0.591	0.509
	[1.40]	[2.03]*	[1.90]+
- *GCSE equivalent*	1.433	0.699	0.934
	[1.94]+	[1.47]	[0.24]
- *A level equivalent*	1.002	0.627	0.874
	[0.01]	[1.63]	[0.40]
- *higher education*	1.176	0.709	0.760
	[0.77]	[1.31]	[0.81]
District:			
- *Renfrewshire, Inverclyde, Argyll & Bute*	1.133	1.225	2.230
	[0.23]	[0.32]	[1.22]
- *Gateshead & South Tyneside*	3.717	2.752	3.155
	[3.26]**	[2.27]*	[2.12]*
- *East Lancashire*	1.798	1.228	1.239
	[2.07]*	[0.70]	[0.61]
- *Calderdale & Kirklees*	1.491	1.057	1.626
	[1.08]	[0.13]	[1.04]
- *Bridgend, Rhonda , Cynon & Taff*	0.721	0.974	1.093
	[0.88]	[0.08]	[0.22]
- *Coventry & Warwick*	1.148	0.637	0.649
	[0.55]	[1.51]	[1.17]
- *Shropshire*	1.908	1.092	1.173
	[1.96]*	[0.21]	[0.32]
- *Derbyshire*	1.071	1.747	2.003
	[0.16]	[1.24]	[1.28]
- *Essex*	1.165	1.002	1.059
	[0.66]	[0.01]	[0.19]
- *Suffolk*	1.930	0.983	0.712
	[2.45]*	[0.05]	[0.68]
- *Buckinghamshire & Oxfordshire*	1.039	0.484	0.326
	[0.15]	[2.00]*	[2.04]*
- *Hampshire*	0.873	1.419	1.186
	[0.50]	[1.41]	[0.53]
- *Somerset*	1.110	1.231	1.610
	[0.27]	[0.54]	[1.06]
Month of RA:			
- *Apr-04*	0.936	0.964	0.832
	[0.21]	[0.12]	[0.47]
- *May-04*	1.004	0.432	0.322
	[0.02]	[2.27]*	[2.25]*

Continued

	(1) unsubsidised employment	(2) Non-JSA benefits	(3) IB
- Jul-04	1.267	0.737	0.754
	[0.88]	[0.98]	[0.78]
- Aug-04	1.022	0.481	0.539
	[0.08]	[2.11]*	[1.58]
- Sep-04	1.210	1.069	0.739
	[0.72]	[0.23]	[0.80]
- Oct-04	0.996	0.712	0.517
	[0.01]	[1.10]	[1.63]
- Nov-04	0.595	0.953	0.801
	[1.66]+	[0.17]	[0.66]
- Dec-04	0.980	0.864	0.590
	[0.06]	[0.44]	[1.24]
- Jan-05	0.949	0.529	0.510
	[0.19]	[1.88]+	[1.72]+
- Feb-05	1.058	0.887	0.590
	[0.19]	[0.36]	[1.25]
- Mar-05	1.102	0.534	0.353
	[0.31]	[1.57]	[1.99]*
- Apr-05	0.998	0.697	0.574
	[0.00]	[0.90]	[1.13]
- May-05	1.552	0.951	0.440
	[1.00]	[0.10]	[1.08]
- Jun-05	2.936	1.056	1.310
	[1.94]+	[0.09]	[0.41]
Action group (treatment effect)	1.324	1.487	1.401
	[2.31]*	[2.78]**	[1.91]+
Observations	2049	2049	2049

Absolute value of z statistics in brackets.

+ significant at 10%; * significant at 5%; ** significant at 1%.

Coefficients represent the hazard for an individual with a given characteristic relative to an individual without that characteristic. The exceptions to this are the 'baseline' hazard terms (the first six terms in the table) which give the hazard rate for the reference individual at a given time post-random assignment. The reference individual has the following characteristics: age – 50; ethnic group – white; partnership status – unpartnered; disability status – not disabled; drivers licence – yes; working part-time at time of RA – no; previous occupation – manual; accommodation tenure type – council or social tenant; pension arrangement – none; qualifications – none; district – Leicestershire; month of random assignment – June 2004.

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