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The longer-term impact of the New Deal for Young People

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Abbreviations

ALMB	Active Labour Market Benefits
EO	Employment Option
ETF	Environment Task Force Option
FTET	Full-Time Education and Training Option
IB	Incapacity Benefit
IS	Income Support
JCPlus	Jobcentre Plus
JSA	Jobseeker's Allowance
LAD	Local Authority District
ND25 Plus	New Deal Twenty-Five Plus
NDED	New Deal Evaluation Database
NDYP	New Deal for Young People
NIESR	National Institute of Economic and Social Research
PWD	Person With Disability
PSI	Policy Studies Institute
VS	Voluntary Sector Option
WBLA	Work Based Learning for Adults
WPLS	Work and Pensions Longitudinal Study

Summary

Introduction

The New Deal for Young People (NDYP) was introduced in 12 Pathfinder areas during early 1998 and rolled out nationally from April 1998. The programme is mandatory and provides support for 18 to 24 year olds who have been unemployed and claiming Jobseeker's Allowance (JSA) for six months. The NDYP is the largest government labour market programme with nearly 960,000 clients having started the programme by March 2005.

There has been extensive evaluation of the immediate impact of the NDYP on individuals and the level of unemployment but relatively little research into its longer-term effect and the longer-term outcomes of participants. This paper seeks to fill this gap by addressing the following questions:

• What is the impact of the NDYP?

• Which of the NDYP's options is the best?

• Who performs best on the NDYP?

To analyse these questions administrative databases have been used, including the Work and Pensions Longitudinal Study (WPLS) which brings together administrative benefit databases and employment databases from Her Majesty's Revenue and Customs.

Methodology

What is the impact of the New Deal for Young People?

Twelve monthly cohorts of male NDYP starters from July 1999 to June 2000 have been followed for four years to analyse the impact of the programme. The cohorts' Active Labour Market Benefit (ALMB) claims (JSA and programmes for which clients are ineligible for JSA)[1] have been compared to cohorts of JSA clients slightly too old

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[1] New Deal options, Basic Skills and Work Based Learning for Adults.

to have participated on the programme and whose claims had reached six months duration. It has not been possible to use a comparison group the same age as programme participants because the programme is mandatory for all JSA clients aged 18 to 24 years whose claim reaches six months duration.

Time off ALMBs has been used as the outcome measure so that a difference-in-differences approach could be adopted to remove systematic differences between the comparison and treatment groups. This is important because 18 to 24 year olds may behave differently to 25 to 30 year olds.

The comparison and treatment cohorts have also been matched on their observable characteristics to remove the effect of differences in the characteristics of the two groups.

Which of the New Deal for Young People's options is the best?

Analysing the relative effect of each of the NDYP's options is simpler than the net impact of the programme because all participants are drawn from the same age group. For this reason it has not been necessary to adopt a difference-in-differences approach to control for systematic differences between the two groups and it has been possible to use time in employment as the outcome measure.

The relative effect of each option has been estimated using cohorts of male and female option participants who started their option between July 1999 and June 2000. The cohorts have been followed for four years to test whether participants of one option subsequently spent longer in employment than those taking an alternative option. The cohorts have been matched on their observable characteristics to remove, as far as possible, any bias caused by differences in characteristics.

Who performs best on the New Deal for Young People?

A cohort of male and female leavers from the NDYP between July 1999 and June 2000 has been followed for four years. Clustering techniques have been used to group the leavers according to their post-programme benefit and employment experiences. Logistic regression has then been used to determine the relative likelihood of clients with certain characteristics being members of the successful clusters.

A cohort of leavers has been used because post-programme success is the variable of interest. The analysis of the previous two questions uses cohorts of starters because there is an investment period when participants are on the programme so it is important to test whether participation increases the length of time participants remain unemployed because they wish to remain on the programme.

Findings

What is the impact of the New Deal for Young People?

The key finding from the analysis is that the positive impact of NDYP on young people lasts for a number of years following participation. Over a four year period, NDYP participants spent, on average, 64 fewer days claiming ALMBs than the comparison group. Although the impact declines over time, this substantial difference suggests that participation on NDYP reduces ALMB claims by 12 per cent. Much of this amounts to additional savings to the Exchequer over and above those identified in earlier studies evaluating the short term impact of NDYP.

In 2005, the Department for Work and Pensions (DWP) published its Five Year Strategy. One of the key aims of the strategy is to move the inactive into work. From a policy perspective it is therefore interesting to analyse how effective the NDYP has been at reducing both active labour market and inactive benefit claims. The results are subject to greater uncertainty than the programme net impact estimates because it has not been possible to control for systematic differences in inactive benefit claims between the comparison and programme groups. They suggest however that programme participants spent less time claiming both active and inactive benefits than the comparison group. On average, programme participants spent 90 fewer days claiming active and inactive benefits than the comparison group over the four year period. The effect of the programme was also more sustained for active and inactive benefits combined, falling only two days over the four year period from 23 days in the first year to 21 days in the fourth year.

Which of the New Deal for Young People's options is the best?

The results show that after controlling for a limited number of observable characteristics, those taking the Employment Option outperformed those taking other options. Excluding time in employment during year one because some Employment Option spells have been included as employment spells, those taking the Employment Option spent seven percentage points longer in employment than a matched cohort of Full-Time Education and Training Option participants. The difference is larger for the Employment Option compared to Voluntary Sector and Environment Task Force Options at nine percentage points each.

The results also show that participants taking the Environment Task Force Option, on average, performed the least well. These findings are, however, likely to contain a bias because participants have been matched on only a limited number of observable characteristics. It has not been possible to match on clients' motivation, drive or employability, which are likely to be determinants of the client's choice of option. Without matching on these characteristics, the effects of the Employment Option are likely to be overestimated because the most able clients tend to take this option and the effects of the Environment Task Force Option are likely to be underestimated because it is often the option of last resort.

The results using time in employment (for males only) were compared to the results using time off ALMBs (for males only) to test whether the results were significantly different. The comparison suggests that the estimated effect of an option using time off ALMBs overestimates the estimated effect using time in employment. Although the options results are not directly comparable to the net impact results, they do suggest that the net impact estimates for the programme may overestimate the impact of the NDYP in terms of time in employment.

Who performs best on the New Deal for Young People?

The cohort of leavers was grouped according to their post-NDYP outcomes and this resulted in five clusters being obtained: Off Benefit, Long JSA, Other Benefits, Employment and JSA and New Deal. The Off Benefit cluster contains clients who were recorded as spending about 40 per cent of their time in employment but less than five per cent claiming benefits. The Long JSA, Other Benefits, Employment and, JSA and New Deal clusters spent significant proportions of their time on JSA, on other benefits, in employment and, on JSA or New Deals respectively.

Logistic regression was used to estimate the relative importance of factors affecting the likelihood of achieving successful longer-term outcomes. Participants were deemed to have had successful outcomes if they were included in the Off Benefit or Employment clusters because these clusters spent the least time claiming benefits and the greatest time in recorded employment. The results suggest that qualifications have a large effect on the likelihood of achieving successful outcomes: clients with qualifications at level four and above were 308 per cent more likely to have had a successful outcome than those with no qualifications. Clients' previous claim history and age were also important factors with older clients and clients with shorter previous claims more likely to achieve successful outcomes.

The stage at which participants left the programme was also a significant factor. Leavers from the Employment Option are 317 per cent more likely to have achieved successful outcomes than leavers from Follow-Through. These results are in agreement with the options analysis with leavers from the Employment Option being the most likely to achieve successful outcomes followed by Full-Time Education and Training participants, Voluntary Sector participants and Environment Task Force participants.

The results also suggest that the immediate destinations of NDYP participants are a good guide to their longer-term outcomes. Leavers to unsubsidised employment were most likely to have spent their time in employment or off benefits, leavers to other benefits were likely to have remained on other benefits and leavers to JSA were likely to have spent most of their time on JSA or New Deals.

Conclusions

What is the impact of the New Deal for Young People?

NDYP participants spent, on average, 64 fewer days claiming ALMBs than a group of JSA clients slightly too old to be eligible for the NDYP . The estimated impact of the NDYP gradually fell over the four year period from 24 days in the first year to ten days in the fourth year. The impact of the NDYP was, however, positive over the four year period.

This estimate only includes the direct impact of NDYP on participants and not the indirect effects of the programme on the economy as a whole. Riley *et al.* (2000) found that by the first half of 2000 NDYP had generated about 25,000 jobs (including those in subsidised employment) of which 10,000 went to people outside the NDYP age group. Wilkinson (2003A) also found similar results with NDYP reducing unemployment by between 30,000 and 40,000, with around 25,000 resulting from fewer young people having claims lasting six months. These results suggest that our analysis has not included a significant part of the programme's benefit: its impact on non-participants.

Analysis including active and inactive benefits suggests that programme participants spent fewer days claiming benefits than the comparison group. This finding is subject to uncertainty since it has not been possible to control for systematic differences in the proportion of time that both age groups spent claiming inactive benefits prior to the introduction of the NDYP. The programme group spent on average 12 per cent of its time on inactive benefits in the fourth year.

Which of the New Deal for Young People's options is the best?

The findings in this paper are in agreement with Bradley (2004): Employment Option participants perform the best followed by Full-Time Education and Training, the Voluntary Sector and the Environment Task Force participants.

The use of matching is central to this result because clients choose which option to take. If there are characteristics systematically correlated with the choice of option that have not been included in the matching then the results presented in this paper may be biased. A number of characteristics have been controlled for but it has not been possible to control for all relevant characteristics. Clients who are more highly motivated may be more likely to take the Employment Option than an alternative option. Since it has not been possible to observe and control for this factor, the results may overstate the effect of the Employment Option and understate the effect of the Environment Task Force Option.

Who performs best on the New Deal for Young People?

Leavers from the Full-Time Education and Training option and the Employment Option are the most likely to have successful long-term outcomes from the NDYP. Participants who left to unsubsidised employment had the best long-term outcomes.

The results also suggest that immediate outcomes are a good indicator of longer-term outcomes. Participants who left to other benefits were likely to have spent the majority of their time on other benefits and those returning to JSA were likely to have spent significant periods of time on JSA or New Deals.

There are large differences in the rates of success for participants with different characteristics. Older participants, those with higher qualifications and those with shorter JSA claims prior to starting the programme are more likely to have successful outcomes in the long-term. Geography also plays an important role with local labour market conditions having a large effect on the likelihood of success in the longer-term.

These results are, however, based on gross outcomes and not net impacts. Hence, participants with higher qualifications may be more likely to have successful outcomes, but it is not possible to determine whether this is because they are more able or because the programme helps them more than participants without qualifications. No or low qualifications may also be acting as a proxy for other forms of disadvantage that it has not been possible to include in the logistic regression.

An interesting finding is that gender does not seem to play an important role in determining long-term success after other factors have been controlled for. Females are likely to spend longer on other benefits but these claims are offset by males being more likely to spend longer claiming JSA.

1 Introduction

1.1 New Deal for Young People

The New Deal for Young People (NDYP) was introduced in 12 Pathfinder areas during early 1998 and rolled out nationally from April 1998. It is a mandatory programme providing support for 18 to 24 year olds who have been unemployed and claiming Jobseeker's Allowance (JSA) for six months.

The programme includes three distinct phases: Gateway, Options and Follow-Through. The Gateway is designed to last for around four months during which participants receive intensive jobsearch support and help to overcome possible barriers to work. The options stage allows participants to experience structured activities including the Employment Option (EO) for up to six months, Full-Time Education and Training (FTET) for up to 12 months, work in the Voluntary Sector (VS) for up to six months and work as part of an Environment Task force (ETF) for up to six months. Participants remaining on the programme after the options stage return to Follow-Through, an additional period of intensive jobsearch support for up to three months, before they leave the programme.

This model sets out the original design of NDYP. Since its introduction in 1998, NDYP has undergone a number of changes including streamlining of options and, in practice, provision may deviate from the programme design. For instance, participants may spend more or less time on each stage than set out in the programme design.

NDYP is the largest government labour market programme with nearly 960,000 individuals having started the programme up to March 2005. Most participants leave the programme during Gateway before they reach the relatively expensive options. For instance, of the 170,000 clients referred to NDYP between March 2003 and March 2004, 150,000 started the Gateway, 50,000 started an option and 30,000 started Follow-Through.

1.2   Aims and objectives

The Government offers a variety of programmes and interventions for jobseekers and the inactive. Evaluation of labour market programmes has to date been dominated by whether the programmes have an effect on the immediate outcomes of participants. Focusing only on immediate destinations may however lead to suboptimal programme development or investment if programmes take different lengths of time to have an impact on employment outcomes. For instance, jobsearch may have better immediate employment outcomes than training programmes, but if the jobs are not sustained the effect of jobsearch could be overstated by analysing only immediate outcomes. It is important to evaluate programmes over a longer period to test whether the effects of programmes are different in the longer-term than the short-term and whether the effects of programme participation are sustained or short-lived.

Exploiting new administrative datasets this paper seeks to test whether NDYP has had an effect on participants in the longer-term by addressing the following questions:

• What is the impact of the NDYP?

• Which of the NDYP's options is the best?

• Who performs best on the NDYP?

1.3   Nature of longer-term impact

Analysing the longer-term impact of programmes exacerbates one key problem of programme evaluation: can changes in behaviour be attributed to programme participation? There are two issues raised by this question. First, if someone participates on NDYP and then attends a Basic Skills course, participates on Work Based Learning for Adults (WBLA) or repeats a spell on NDYP, how should their outcome be attributed to their original NDYP spell? This analysis has assumed that subsequent programme participation is the result of participants' original NDYP participation. Hence, if non-participants subsequently spend longer on other programmes this has been attributed to them not participating on NDYP.

Second, how long would it be reasonable to expect NDYP to have an effect for? Could differences in employment rates between the comparison and treatment groups ten or 20 years after participation be attributed to NDYP? There are two approaches that can be adopted: (1) all post-programme differences between the comparison and treatment groups can be attributed to the programme, or (2) differences can be attributed to the programme until the impact of the programme falls to zero (any subsequent difference between the comparison and treatment groups would not be attributed to the programme).

Dolton *et al.* (2002) analysed the effect of Restart interviews and attributed all differences between the comparison and treatment groups to Restart. They found an initial impact of ten percentage points falling to one percentage as the comparison group received the treatment six months later. They also found that a six percentage point gap re-emerged and was maintained for three remaining years. Dolton *et al.* argued that the re-emerging difference could be attributed to the treatment group being 'more able to cope with the adverse labor market conditions that prevailed at the beginning of the 1990s'.[2]

This paper has not assumed either approach because the impact of the programme as a whole does not fall to zero.

Substitution and displacement are potentially serious problems for this analysis. Substitution occurs when labour market programmes cause employers to swap programme participants for their employees or choose programme participants over unemployed non-participants. This has two implications for the analysis. First, programme participants' employment outcomes may improve but at the detriment of non-participants. Although there may be some benefits to redistributing jobs from those closer to the labour market to those further away from the labour market,[3] if substitution is high the net benefit of the programme for society may be low. Second, if employers substitute programme participants for the comparison group then the programme effect will be overestimated. The programme effect will be a composite of two parts: the positive effect of the programme on participants (the programme impact) and the negative effect of the programme on non-participants or the comparison group (a potential bias).

Displacement is similar to substitution except non-participants lose their jobs or potential jobs through the market mechanism. Wage subsidies may allow new firms to enter a market and undercut firms already in the market by reducing the new firm's labour costs. If the increased competition between the new and old firms causes the older firms to make a loss and eventually leave the market, then the employees and potential employees of the older firm will be displaced by the employees of the new firm.

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[2] Dolton *et al.* (2002) p. 392.

[3] See Layard *et al.* (1991) Ch. 4 for more information. The authors argue that the long-term unemployed exert less downward pressure on wages than the short-term unemployed. Hence, if programmes cause the long-term unemployed to be substituted for the short-term unemployed or the employed then there will be greater downward pressure on wages and the equilibrium level of unemployment will fall.

This paper has not examined potential substitution and displacement effects from NDYP but previous research suggests that substitution and displacement are not significant issues for this analysis. Neither the National Institute of Economic and Social Research's (NIESR) nor the Policy Studies Institute's (PSI) macro evaluations of NDYP found evidence of a negative impact from substitution.[4] The NIESR report found a temporary effect on off-flows from long-term unemployment for other age groups, but concluded that this was caused by a temporary shift of resources to the roll out of NDYP and not a result of the programme itself.

Discussion of the datasets used in the analysis is covered in Chapter 3.

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[4] See White *et al.* (2002) for further information.

2 Literature survey

There are three literatures of interest for this paper: the shorter-term impact of NDYP literature, the longer-term impact of labour market programmes literature and the longer-term impact of NDYP literature.

2.1 Short-term impact of NDYP

There is a large body of quantitative and qualitative research evaluating the effect of NDYP and other New Deal programmes.[5] Two key analyses of the macroeconomic impact of NDYP were undertaken by NIESR and PSI. These analyses examined the impact of NDYP on both participants and non-participants. This is an important point because this working paper analyses only the impact of NDYP on participants.

2.1.1 New Deal for Young People: implications for employment and the public finances[6]

NIESR split their analysis into the direct and indirect effects of NDYP. The direct effects include a reduction in youth unemployment and an increase in youth employment as a result of the programme. The indirect effect includes the wider impact of the programme on the levels of unemployment and employment for other age groups.

To estimate the direct impact of NDYP, NIESR analysed the inflows to unemployment, the outflows from unemployment and the stock level of unemployment. Using a difference-in-differences approach with 25 to 29 year olds as a counterfactual, NIESR found that by March 2000 long-term youth unemployment had fallen by 45,000. There had however been an increase in inflows to unemployment for young people by around 10,000 suggesting that total youth unemployment had fallen by

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[5] See Hasluck (2000) for a review of the evidence.

[6] Riley *et al.* (2000).

around 35,000. NIESR also estimated that by March 2000 youth employment had increased by about 15,000.[7]

The indirect effects of NDYP were evaluated by analysing the programme's impact on wage pressure. NIESR estimated that by transforming long-term unemployed into short-term unemployed, NDYP had reduced wage pressure in the economy and allowed employment to increase without increasing inflationary pressure. Using the estimated impact of NDYP on wage pressure in the National Institute's model of the UK economy, NIESR found that by the first half of 2000 NDYP had reduced total unemployment by around 45,000. NDYP had also generated about 25,000 extra employee jobs of which around 10,000 went to people outside the 18 to 24 year old age group.

2.1.2 New Deal for Young People: evaluation of unemployment flows[8]

Wilkinson analysed the impact of NDYP on the probability of participants being unemployed at different durations following their JSA claim reaching six months (the theoretical date of entry to NDYP). Using 30 to 39 year olds as a counterfactual and difference-in-differences approach, Wilkinson analysed both direct and indirect effects of NDYP. Two indirect effects were identified: (1) clients leaving prior to reaching six months unemployment to avoid programme participation and (2) clients not leaving JSA so that they could enjoy the programme benefits.

Wilkinson found that unemployment was around 20,000 lower for men and 8,000 for women 12 months after their JSA claims reached 12 months duration. Part of this fall can however be explained by participants being on options and not receiving JSA. The impact 18 months after participants claims reach six months duration (when most participants should have left options) was lower at around 10,000 for men and 5,000 for women.

In addition, Wilkinson found that fewer young people remained unemployed for six months than in previous years. This indirect effect of NDYP indicates that in addition to the programme's direct impact, NDYP reduced unemployment by a further 25,000. Wilkinson also found that NDYP had a significant impact on the probability of participants leaving unemployment for government sponsored training. At 18 months after qualifying for NDYP about 8,000 men and 4,000 women had left unemployment for government sponsored training and not returned to unemployment.

Adding together the direct and indirect effects of NDYP, Wilkinson concluded that NDYP had reduced unemployment by between 30,000 and 40,000.

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[7] Employment includes those on the Subsidised Employment Option.

[8] Wilkinson (2003A).

2.2 Longer-term impact of labour market programmes literature

There has been a growing literature on the longer-term impact of labour market policies since the 1990s [Couch (1992); Hamilton *et al.* (2001); Dolton *et al.* (2002)]. The longer-term impact of labour market programmes has become important with the increasing focus on 'work first' approaches rather than programmes to enhance human capital. 'Work first' programmes offer relatively low-cost support for the unemployed and appear to move the unemployed into employment at a faster rate than human capital approaches. 'Work first' approaches may however have only a short-lived effect with participants quickly returning to unemployment when compared to those participating in education and training programmes. The key question the literature has sought to answer is whether the greater investment in education and training than 'work first' approaches leads to greater returns over a longer period.

Greenberg *et al.* (2004) have brought together data from 64 US welfare to work programmes to produce a meta-analysis. The authors defined programmes emphasising jobsearch and de-emphasising job training as 'work first' and programmes not emphasising jobsearch as training programmes. They found that although the outcomes of training programmes did catch up to 'work first' programmes, the effect of both fell towards zero after 24 months and 'work first' dominated training. Jobsearch typically increased participants earnings by $4,134 over the 24 month period following referral compared to $1,567 for training programmes (2000 prices).

These results suggest that 'work first' support clearly dominated training programmes. As the authors point out however, in most instances the training provided only modest support lasting 'no longer than a month or two when it did occur'.[9] If the programmes directed towards training did not provide useful training then the difference between the two types of support may represent the difference between pressure to find employment and the absence of that pressure for a couple of months. Hence, the results in Greenberg *et al.* may not offer a fair comparison between 'work first' programmes and programmes that actually increase human capital.

Evidence from West Germany (Lechner *et al.* (2004)) suggests that the returns from long training programmes can be substantial. Lechner *et al.* analysed the effect of participating on different training programmes varying in length from only a few days to three years. They found that those receiving retraining (the most substantial training available) were between ten and 15 percentage points more likely to be employed than the comparison group seven years after they started the programme. Those receiving short (less than six months) and long (more than six months) training were between five and nine percentage points more likely to be in employment than non-participants after seven years.

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[9] Greenberg *et al.* (2004) p. 46.

Over the entire seven year period the shorter training programme outperformed the other programmes because it had a shorter lock-in period during training. Over the seven years, those taking shorter courses spent an additional eight months in employment compared to non-participants. Those taking longer training courses spent four months longer in employment and no significant difference could be detected for those taking retraining. If the differences in employment rates at seven years persist, however, those taking retraining will eventually overtake those taking the other training programmes.

Lechner *et al.* did not consider 'work first' approaches, but their results suggest that substantial training packages can have a large effect on employment rates seven years later. It is not possible to compare the findings from Lechner *et al.* and Greenberg *et al.* but Lechner *et al.'s* findings show that comparing substantial training support and 'work first' approaches requires a long-term perspective.

2.3   Evidence of the longer-term impact of the New Deal for Young People

The evidence base for the longer-term impact of NDYP is limited to a survey of participants by Bonjour *et al.* (2001) and an MSc dissertation by Bradley (2004).

2.3.1   New Deal for Young People: National Survey of Participants: Stage 2

Bonjour *et al.* (2001) surveyed a cohort of NDYP participants 18 months after they started the programme. The cohort was drawn from starters between September and November 1999. Matched groups of participants weighted for non-response and survey attrition[10] were used to analyse the relative effect of each option compared to an extended period on the Gateway.[11] The principal findings of Bonjour *et al.* were:

- In terms of improving employability, the EO performed best followed by the VS option, the FTET option, and the ETF option.

- Participants taking EO were the most likely to be in employment and least likely to be claiming JSA after controlling for observable characteristics.

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[10] The cohort of participants was based on the cohort used for the Stage 1 Survey (Bryson *et al.*, 2000). The Stage 1 Survey took place six months after participants started NDYP.

[11] Bonjour *et al.* argued that participants experiencing an extended period on Gateway received little additional support compared to participants experiencing the normal Gateway period of four months. Under this argument a period of extended Gateway is equivalent to not participating on an option.

- ETF and VS were found to encourage movement into employment at a quicker rate than FTET.

- Job satisfaction was higher for FTET participants than for those taking VS.

- 'The relative performance of those with multiple disadvantages tended to be boosted most by the FTET option, since this would have addressed their most fundamental needs.'

Bonjour *et al.'s* sample may however have suffered from selection bias. The Stage I survey response rate was 54 per cent and the Stage II survey attrition rate was 44 per cent so the realised sample included only 30 per cent of the original. Although Bonjour *et al.* attempted to reduce the bias by weighting their realised sample, the sample is unlikely to have been unbiased if factors systematically influenced the probability of individuals participating in the survey. Bradley (2004) argued that DWP clients with the greatest disadvantage could have been under-represented in the sample because 'they are more likely to be people who move around a lot, or are the least co-operative with the department'.[12] Bradley also argued that the cohort may not be representative of subsequent NDYP participants because the sample was drawn from starters over a short period.

The most salient weakness of Bonjour *et al.* for analysing the longer-term effect of NDYP is that participants were surveyed only 18 months after they started the programme. A participant experiencing four months on Gateway, six months on an option and four months on follow through would have been off the programme for only four months when they were surveyed and FTET participants may have still been on the programme when they were surveyed. This suggests that the relatively poor performance of FTET participants may be explained by its participants not having had sufficient time to successfully use the skills they had acquired in the labour market.

2.3.2 Analysis of the long-term outcomes of New Deal for Young People

Bradley (2004) was the first piece of research to analyse the outcomes of NDYP participants over a substantial length of time: four years. Bradley analysed five questions:

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[12] Bradley (2004) p. 15.

- *Question 1: How long do people stay off benefits following a spell on NDYP?* Using a cohort of leavers between July 1999 and June 2000 Bradley found that 25 per cent of leavers returned immediately to benefits[13], 50 per cent returned to benefits within six months of leaving NDYP and there was a 23 per cent chance that 1,500 days after leaving NDYP a participant would not have returned to a benefit. The survival curve levelled off around 20 per cent indicating that around a fifth of leavers are predicted to never return to benefits.

- *Question 2: What were participants' destinations each year after leaving the programme?* Using the same cohort, Bradley examined snapshots of what participants were doing one, two, three and four years after they left NDYP. He found that only a minority of participants moved between the states of claiming benefits and not claiming benefits over the four snapshots. He also found that the proportion of participants claiming benefits remained stable at around a third (35 per cent after one year falling slightly to 32 per cent in year four).

- *Question 3: What factors influence the probability of long-term success from NDYP?* Bradley used logistic regression to estimate the importance of a set of characteristics on the probability of experiencing a successful outcome (defined as spending less than 25 per cent of their time on benefits during the four years since leaving NDYP). The results were not significant but indicate the relative importance of the following characteristics in decreasing importance: stage of leaving NDYP, claim history, age, disability status, ethnicity and gender.

- *Question 4: Do leavers from some stages perform better than leavers from other stages?* A group of participants with similar characteristics[14] was used to analyse the proportion of time participants spent claiming benefits during the four years since leaving NDYP. Bradley found that EO participants performed the best, followed by FTET, VS and ETF participants.

- *Question 5: Is the finding of Bonjour et al. (2001) that remaining on extended Gateway significantly reduces the chances of claiming JSA relative to VS and to a lesser extent FTET and ETF supported by a larger sample of participants?* Using survival analysis, Bradley found that there was no evidence to suggest that extended Gateway leavers stay off benefits longer than VS or FTET customers.

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[13] Bradley adopted a 42 day rule: benefit claims were required to last at least 42 days following the participant leaving NDYP for the claim to be included as a valid claim. Forty-two days was chosen as the cut off point because 42 days is the length of time between the most infrequent benefit database downloads. This rule ensured that benefit claims picked up in the analysis were live after the participant left NDYP.

[14] The group shared the following characteristics: it was their first spell on NDYP in July 1999, they were normal entrants to NDYP, they were white, they were male and they had claimed JSA for between 160 and 365 days between January 1995 and the date they joined NDYP.

3   Datasets

Several administrative databases have been used in this analysis. They include the Master Index, the WPLS, the Client Extract, the New Deal Evaluation Databases (NDEDs) and the Joint Unemployment and Vacancies Operating System (JUVOS) five per cent sample. All of the databases are drawn from administrative sources and are not designed specifically for programme evaluation: the quality of the databases depends on the quality of the input into the administrative systems and the algorithms that are used to interpret the administrative inputs.

The Master Index brings together a number of datasets to provide information on all benefit spells for all DWP clients.

The WPLS is a new and rich database which pools the Master Index and employment databases from the Her Majesty's Revenue and Customs to provide information on benefit and employment spells for DWP clients since 6 April 1998. The WPLS is currently under development with additional information such as earnings data due to be included. At the time of analysis the WPLS was subject to a number of issues, the most significant for the analysis were:

• Employment records are matched to individuals held on the Master Index using fuzzy matching which is subject to some uncertainty. This analysis has included only fuzzy matches where either the National Insurance Number matches or the following five variables match: surname, forename initial, postcode sector, gender and date of birth.

• Some employment records have missing start or end dates. Where the start and end dates are unknown they have been recorded as the start and end of the tax year (5 April and 6 April respectively). These records have been excluded from the analysis because although they indicate that there has been an employment spell during a financial year, it is not possible to estimate the length of that spell.

• Records for the self-employed and for most workers not earning enough to pay tax at the time of analysis are not included in the WPLS at present.

- Some benefit spells are included as employment spells because of the way Inland Revenue records taxable spells. All employment spells which match benefit spells have been removed from this analysis.

The Client Extract provides information on the characteristics of all DWP clients since May 1998. The client extract includes a single record for all clients, updated with the latest information. This may cause problems if the information contained in the client extract is different to what would have been recorded when the client started NDYP. Updated details are unlikely to be a problem for most characteristics because they are fixed (e.g. gender, age and ethnicity) but some may have changed (e.g. Local Authority District (LAD) and disability status). It has not been possible to remove this potential problem.

The New Deal evaluation databases provide information on New Deal participants' spells on NDYP.[15]

The JUVOS five per cent sample is collected by the Office of National Statistics and includes JSA records for five per cent of JSA clients since October 1982.[16]

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[15] For further information on the NDED see Daly *et al.* (1999).

[16] For further information on JUVOS see Ward *et al.* (1995).

4 Methodology

4.1 What is the impact of the New Deal for Young People?

The effect of NDYP has been estimated using 12 monthly cohorts of starters between July 1999 and June 2000. Each cohort has been followed for four years to test whether participation on NDYP reduced their Active Labour Market Benefits (ALMB) claims in both the short and the longer-term.

4.1.1 Outcome measure

The proportion of time off ALMBs has been used as the outcome measure in this analysis.[17] Time on the following benefits has been included as time on ALMBs: JSA, New Deal options[18], Basic Skills[19] and WBLA. Time on the New Deal options, Basic Skills and WBLA have been included because jobseekers become ineligible for JSA when they participate on these programmes. Hence, time off ALMBs corresponds to time off JSA and time off programmes for which clients are ineligible for JSA.

Time off ALMBs has been used instead of time in employment for two reasons: first, JSA claims data is available prior to the introduction of NDYP so a difference-in-differences approach is feasible (this is not possible with time in employment because data is not available before 1998), and second, benefit claims data is more reliable than employment spells data. A difference-in-differences approach is

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[17] It has been assumed that there are 365.25 days in a year. This assumption has been made throughout this paper.

[18] Time on New Deal options has been used because participants leaving New Deal return to the programme if they leave for less than 13 weeks. Using time on New Deals instead of time on options would overestimate the length of time participants spend on New Deals because spells off the New Deals lasting less than 13 weeks would not be included as time off ALMBs.

[19] Only spells on Basic Skills lasting longer than one day have been included as valid spells because clients taking a basic skills test are recorded as having participated on the programme for one day.

necessary because the comparison group may be systematically different from programme participants.

Using the proportion of time off ALMBs as the outcome measure has one significant drawback: clients are treated as having successful outcomes whenever they are not claiming ALMBs. Clients going to prison, moving to inactive benefits, e.g. Income Support (IS) or Incapacity Benefit (IB), or dropping out of the labour force are treated as having positive outcomes. To undertake the analysis using time off ALMBs it has been necessary to make two assumptions. First, it has been assumed that clients spend their time in only three states: on ALMBs, in employment and in 'other states'. Secondly, it has been assumed that the proportion of time spent in 'other states' is constant, so less time on ALMBs equates to more time in employment. The second is a strong assumption, but it has not been possible to test this assumption because data is not available for what members of the comparison and treatment groups were doing when they were not claiming ALMBs.

The comparison and treatment groups have also been restricted to males only. Males are less likely than females to move from ALMBs to inactive labour market benefits such as IS, so time off ALMBs is more likely to be correlated with time in employment. Including females may cause an overestimation or an underestimation of the effect of NDYP if the proportions of females in the comparison and treatment groups are different.

4.1.2   Counterfactual and method

NDYP is a mandatory programme so there is no group of the same age with similar characteristics (having claimed JSA for six months) that can be used as a comparison group. JSA clients who were slightly too old for NDYP and had been claiming JSA for six months have been used as a counterfactual.[20] Using this comparison group raises four problems: first, former NDYP participants may become eligible to be included in the comparison group, second, members of the comparison group may become eligible for New Deal Twenty Five Plus (ND25 Plus),[21] third, the raw differential between the comparison and treatment groups may be biased by differences in the characteristics of the two groups, and fourth, unobserved time dependent factors may have differential impacts on the two groups with the effect varying over time.

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[20] Aged 25 to 30 years when their JSA claim became six months duration. If a client has been included in the analysis once then they have been prevented from being included for a second time. This restriction has been made to remove double counting of ALMB claims. Without this restriction a single benefit claim could be counted more than once, i.e. once for each cohort of the analysis in which the client is included.

[21] Before April 2001, JSA clients aged 25 to 49 became eligible for ND25 Plus when their claim reached 24 months duration or they met the early entry criteria. After April 2001, clients became eligible when they had been claiming JSA for 18 months during a 21 month period or they met the early entry criteria.

The first problem has been removed by dropping anyone in the comparison group who had participated on NDYP because their inclusion may bias the estimate of the programme impact.[22]

It has not been possible to remove the second problem: the counterfactual is ND25 Plus and not the JSA regime. To mitigate this problem all early entrants[23] to NDYP and ND25 Plus have been removed from the analysis so that the length of time before the effect of ND25 Plus can be seen is maximised.

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[22] Including former NDYP participants in the comparison group has two potential effects on the estimated net impact: a programme effect and a client group effect. It is not possible to state *a priori* which effect will dominate or whether they will cancel each other out, so former NDYP participants have been excluded from the analysis.

Programme effect:

Assuming NDYP reduces participants' ALMB claims, former NDYP participants are likely to spend less time claiming ALMBs than those who have not participated on NDYP. Including former NDYP participants in the comparison group would reduce the proportion of time that the comparison group spends claiming ALMBs because former NDYP participants will contaminate the comparison group. The programme effect will cause the net impact of NDYP to be underestimated.

Client group effect:

Former NDYP participants who become eligible to be included in the comparison group are likely to be the most disadvantaged NDYP participants. This is because the least disadvantaged are likely to enter and remain in employment following programme participation whereas the most disadvantaged are likely to remain on ALMBs. Since clients are only eligible for NDYP up to the age of 24, disadvantaged clients who remain on ALMBs for long periods will eventually become eligible to be included in the comparison group when they reach 25 years of age. Including former NDYP participants in the comparison group will cause the net impact of NDYP to be overestimated because the most disadvantaged former NDYP participants would be disproportionately included in the comparison group.

[23] No reliable marker for early entrants exists on the NDYP and ND25 Plus databases so the following assumptions have been made. Someone is an early entrant to NDYP if they join within 167 days of the start of their JSA claim. Someone is an early entrant to ND25 Plus if they join within 23 months of the start of their claim (before April 2001) or they have been claiming JSA for 17 months in a 21 month period (after April 2001).

To mitigate the third problem, the comparison and treatment groups have been matched on observable characteristics using propensity scores.[24] Matching the cohorts reduces the observable differences between the comparison and treatment groups thereby reducing the potential bias caused by differences in characteristics between the two groups. It has been assumed that there is a constant distribution of unobservable characteristics conditional on the observed characteristics. If, however, this assumption fails, matching may not completely remove the bias because unobservable differences in characteristics persist. For instance, it has not been possible to control for qualifications because this information is only available for just under half of NDYP participants and almost none of the comparison group.

Another key characteristic that cannot be controlled for by matching is age. Members of the comparison group are always older than the treatment group because of the choice of counterfactual. To remove the effect of age on the estimated effect of NDYP a difference-in-differences approach has been used. The difference in the proportion of time claiming ALMBs between the two groups has been calculated before and after the introduction of NDYP. Taking the difference of these differences provides an estimate of the impact of NDYP. The effect of age can be removed if it is assumed that the effect of age remains constant before and after the introduction of NDYP.[25] This is the fourth problem: if the difference between the two groups is not constant then the estimates may be unreliable.

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[24] See Appendix A for further information on propensity score matching. The two groups have been matched using the following characteristics: disability status, ethnicity, the population density of their LAD, the claimant rate in their LAD when their claim reached six months duration and the number of days they spent in employment during the 12 months prior to their claim becoming six months duration. Missing values have been included as a valid category in the analysis.

[25] The length of time on JSA prior to the introduction of NDYP has been calculated for those aged 18 to 24 and 25 to 30 when their claim reached six months duration. If someone has a number of JSA spells that meet the criteria for inclusion in this analysis, only their first has been included.

It has not been possible to match the cohorts of younger and older groups pre-NDYP because there are not sufficient appropriate variables contained in the JUVOS five per cent sample. The JUVOS five per cent sample was merged with the Client Extract to gain information on clients' characteristics, but only ten per cent of records on the JUVOS database between 1990 and 1996 had matching records. Clients with matching records are also unlikely to be a random sample of all clients because the client extract only contains records for clients since May 1998. Clients with matching records are likely to be amongst the most disadvantaged because they claimed JSA between 1990 and 1996, and they have made a claim since May 1998.

The difference in the proportion of time claiming JSA for the older and younger group has been calculated using annual cohorts of clients whose claims reached six months duration between July 1990 and June 1993.

The difference-in-differences approach will also remove the effect of other differences between the two groups that are constant before and after the introduction of NDYP. For instance, if the difference in the proportion of time in prison between those aged 18 to 24 and those aged 25 to 30 remains constant over time, then the difference-in-differences approach will remove this systematic difference. To reduce the potential bias from changes over time, the pre-NDYP period has been restricted to the years 1990 to 1993 because these years are likely to be most similar to 1999 (see Section 5.2).

There is another key problem with adopting a difference-in-differences approach. The approach requires both the comparison and treatment groups to have received the counterfactual during the pre-programme period. This is not the case since both groups received support in addition to the counterfactual JSA regime. For instance, between 1994 and 1998 JSA clients aged 18 to 24 whose claim reached 52 weeks received 'Workwise' and '1-2-1' which provided training and jobsearch assistance. These programmes may have reduced the time the younger group spent claiming ALMBs compared to the older group.[26] If they did then the difference in the proportion of time claiming ALMBs pre-NDYP will be biased: had both groups received the counterfactual pre-NDYP the younger group would have spent longer claiming ALMBs than it actually did. It has not been possible to remove this problem.

4.2   Which of the New Deal for Young People's options is the best?

A cohort of NDYP participants who started their first NDYP option between July 1999 and June 2000 has been used. The cohort has been followed for four years from the start of their option to test whether those taking one option spent more time in employment than those taking another option.

4.2.1   Outcome measure

The proportion of time spent in employment after starting an NDYP option has been used as the outcome measure in this analysis. It has been possible to use time in employment because a difference-in-differences approach is not necessary to control for age effects: option participants are drawn from the same age group. As discussed in Chapter 3 there are a number of problems using employment data from the WPLS but employment is the outcome of interest and using time off ALMBs requires strong assumptions to be made.

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[26] There was also a set of programmes for the older group.

There are three principal problems with employment data in the WPLS: (1) employment spells for those not earning enough to pay tax are not included, (2) employment spells for the self-employed are not included[27] and (3) some spells have missing start and/or end dates. Each of these problems could bias the estimated effect of one option compared to another if they affect those who have taken one option more than those who took another.

In order to use employment data it has been assumed that, on average, the gaps in the WPLS affect all pairwise option comparisons equally. Under this assumption the difference in the proportion of time spent in employment between two options cohorts can be used to estimate the effect of taking one option instead of another. This assumption may be strong, but, it can be partly tested using spells with missing start and/or end dates and it is a more reasonable assumption than assuming that clients, on average, spent a constant proportion of their time in 'other states'.

This assumption has been tested by examining the number of records excluded because they have missing start and/or end dates. The proportion of records lost will not provide a conclusive answer to whether the sample is biased because it is not possible to test the effect of the other two problems. It will however provide an indication of whether the findings are likely to be biased.[28]

The analysis has also been undertaken using time off ALMBs as the success measure (See Section 5.4 for the results).[29]

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[27] Data for the self-employed and those claiming tax credits will be available from late 2005.

[28] See Section 5.3.2 for further information.

[29] The analysis for time claiming active labour market benefits has been undertaken for males only because females are more likely to leave active labour market benefits for inactive benefits.

4.2.2 Method

The option cohorts have been matched according to their propensity score[30] so the effect of differences in the characteristics of options participants can be removed. The use of matching is especially important for this analysis because NDYP participants can choose the option they take (with assistance from their personal advisor) whereas in the net impact analysis clients were either eligible for NDYP or not.

4.3 Who performs best on the New Deal for Young People?

A cohort of male and female leavers from NDYP between July 1999 and June 2000 has been followed for four years using the WPLS. A cohort of leavers has been used because post-programme success is the variable of interest. The analysis of the two other questions used cohorts of starters because it is important to include the effect of participants remaining unemployed for longer to remain on the programme. The length of time that the participant spent on NDYP is not important for this analysis.

The leavers have been grouped according to their post-programme benefit and employment experiences. Logistic regression has then been used to determine the relative likelihood of clients with certain characteristics having successful post-programme longer-term outcomes.

The findings of this analysis refer to gross outcomes and not net impacts.

4.3.1 Grouping clients

For each client the length of time in employment, on JSA, on other benefits[31] and on New Deals or Employment Zones has been obtained. The proportion of time subsequently spent in each state, the average length of spell in each state and the number of spells in each state have been calculated for all clients. These provide 12 variables with which to group participants.

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[30] The cohorts have been matched using the following characteristics: whether they were an early entrant to NDYP, disability status, ethnicity, gender, employment during the 12 months prior to starting the option, population density in the client's LAD, the claimant rate in the client's LAD, the clients age when they joined NDYP and highest qualification. Missing values have been included as a valid category in the analysis.

Early entrants have been defined as those starting NDYP during the first 167 days of their JSA claim.

Highest qualification has been included as a characteristic for matching because it has been assumed that all participants are equally likely to have had their qualifications recorded.

[31] Other benefits included are IS, IB, Widows Benefit (WB), Bereavement Benefit (BB), Severe Disability Allowance (SDA) and Invalid Care Allowance (ICA).

Grouping was carried out using clustering techniques that identify centres of groups of observations and place observations into the cluster whose centre is closest. The distance used relates to the distance from the cluster centre on all variables used for the clustering.[32] The final clusters were obtained by varying the number of clusters and selecting the number of clusters that produced the tightest grouping.

4.3.2　Probability of success

Logistic regression has been used to determine the relative likelihood of clients with certain characteristics having successful longer-term outcomes after controlling for other factors.[33] The variables included in the regression are: highest qualification level, NDYP leaving stage, length of previous JSA claims, age on joining NDYP, unemployment rate and population density in the participant's LAD, disability status, ethnicity and gender.

Clusters have been used as the basis for the logistic regression because they allow participants to be split into two groups: those with successful longer-term outcomes and those without.

Observations with missing values for any variable have been dropped from the analysis so the results may be biased if some groups of participants are systematically more likely to have missing values for any of the variables. This is a particular concern with the inclusion of qualifications because this caused 54 per cent of observations to be dropped. A total of 87,040 leavers were included in the analysis.

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[32] The FASTCLUS procedure performs a disjointed cluster analysis on the basis of distances computed from one or more quantitative variables. The observations are divided into clusters such that every observation belongs to one and only one cluster.

The FASTCLUS procedure uses Euclidean distances, so the cluster centres are based on least-squares estimation. This kind of clustering method is often called a k-means model, since the cluster centres are the means of the observations assigned to each cluster when the algorithm is run to complete convergence. Each iteration reduces the least-squares criterion until convergence is achieved.

Source: SAS OnlineDoc v. 8, SAS Institute Inc. (1999).

[33] A stepwise regression approach was used in which the variables with the largest explanatory power were included in the model first. All variables were tested at the five per cent significance level.

5 Results

5.1 What is the impact of the New Deal for Young People?

5.1.1 Match quality

There are no specific tests for match quality but the following three measures provide an indication of how good the match is: the sample size, the number of times members of the matching group have been included in the match and the distribution of those excluded from the match. The results of these measures are summarised in Appendix B.

There are two ways that a matched group can be obtained in this analysis: the NDYP group can be matched to the older group (NDYP-older match) and the older group can be matched to the NDYP group (older-NDYP match). Replacement of observations has been allowed because there is a large difference in the number of observations for each group.

The two matches differ in the type of characteristics that they excluded from the match. The NDYP-older match excluded a high proportion of each cohort compared to the older-NDYP match and it also disproportionately excluded clients with specific characteristics from the matched cohort. For instance, the NDYP-older matched cohort from March 2000 excluded 95 per cent of the NDYP group and 89 per cent of the older group from the North-West Jobcentre Plus (JCPlus) region. Cohort members from London were also disproportionately excluded from the NDYP-older match.[34] In contrast, the older-NDYP match included nearly all individuals from all regions and other characteristic groupings such as ethnicity, qualifications and disability status. The older-NDYP match has been used in this analysis because it includes a better distribution and higher percentage of the original cohorts.

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[34] See Appendix B for the regional distribution of those included and those excluded from the match for the March 2000 cohorts.

Regional distribution is a good indicator of match quality because regional dummies were not included in the logistic regression used to calculate the propensity score.

The sensitivity of the results to the match has been tested by using a different method for matching. The results, given in Section 5.2, indicate that the findings are robust because the results are similar using either type of matching.

5.1.2 Time on Jobseeker's Allowance pre-New Deal for Young People

Figure 5.1 shows the difference in the proportion of time that the younger and the older groups spent claiming JSA before the introduction of NDYP. A positive number, such as three percentage points, indicates that the older group spent, on average, three percentage points longer claiming JSA than the NDYP group. This interpretation of the difference in the proportion of time claiming JSA or ALMBs has been used throughout the paper.

Annual cohorts have been used because the JUVOS data prior to 1999 only contains information on five per cent of clients so using monthly cohorts would contain too few observations to be reliable.

It has not been possible to include the years after 1993 because the fourth year after 1994 includes observations for clients who have participated on NDYP. Including these observations would bias the estimated pre-NDYP difference between the younger and older groups.

Figure 5.1   Difference in the proportion of time on JSA between the younger and older groups
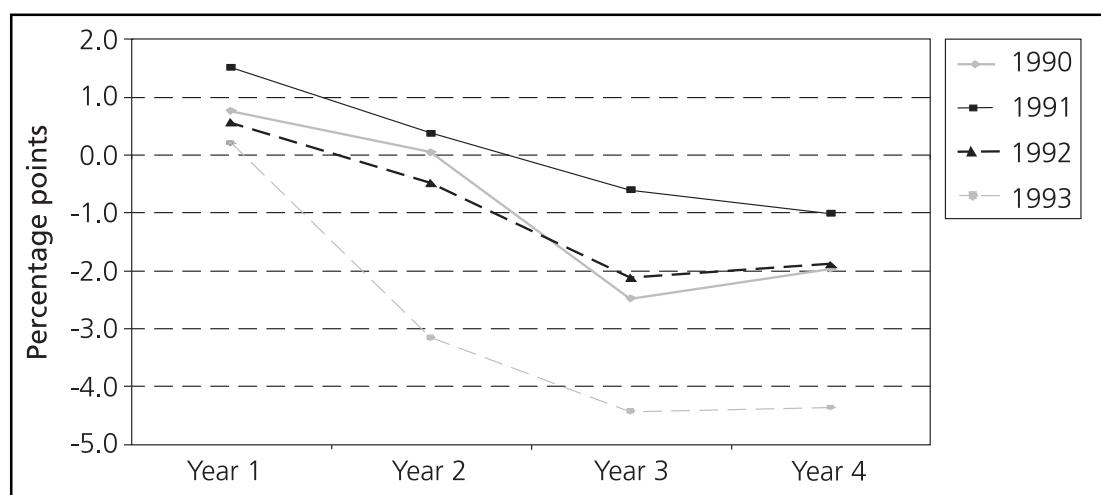
Although JUVOS data is available from October 1982, the estimates have been restricted to 1990 to 1993. The selection of 1990 is to some extent arbitrary but it has been chosen because it is not too far into the past that the labour market was significantly different to the labour market in 1999. Section 5.2 includes a sensitivity using all information available from the JUVOS five per cent sample to estimate the difference in the proportion of time claiming JSA pre-NDYP.

5.1.3 Net impact of the New Deal for Young People

Table 5.1 and Table 5.2 show the estimated net impact of NDYP on participants starting the programme between July 1999 and June 2000 over a four year period. The table shows the proportion of time the older and NDYP groups spent claiming ALMBs (columns 1 and 2), the difference in the proportion of time claiming ALMBs post-NDYP (column 3), the difference in the proportion of time claiming JSA pre-NDYP (column 4) and the estimated net impact of NDYP (column 5). A positive net impact, say six percentage points, indicates that the older group spent six percentage points longer claiming ALMBs than NDYP participants.

Table 5.1   Estimated impact of the New Deal for Young People, time on ALMBs[35]

	Older group (1) %	NDYP group (2) %	Difference on ALMBs (3) %	Difference pre-NDYP[1] (4) %	Net impact[2] (5) %
Year 1	59.6	52.0	7.6	1.1	6.6
Year 2	35.9	31.6	4.3	-0.2	4.5
Year 3	27.9	25.6	2.3	-1.6	3.9
Year 4	23.4	22.3	1.1	-1.6	2.7
Overall	36.7	32.9	3.8	-0.6	4.4

[1] This has been calculated as a weighted average of the annual cohorts where the weights are the number of observations in each year.

[2] Net impact equals the difference on ALMBs minus the difference pre-NDYP.

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[35] The statistical significance of the results has not been tested because the results presented are for the population of NDYP participants and members of the comparison group. Some observations have been dropped as a result of the matching and other data issues, but the results do represent as close to a census as possible. For this reason the statistical significance of the results in Section 5.3 have also not been tested.

Table 5.2   Estimated impact of the New Deal for Young People in days, time on ALMBs[36]

	Older group	NDYP group	Difference on ALMBs	Difference pre-NDYP	Net impact
Year 1	218	190	28	4	24
Year 2	131	115	16	-1	16
Year 3	102	94	8	-6	14
Year 4	86	82	4	-6	10
Overall	537	481	56	-8	64

There are three interesting results contained in Table 5.1 and Table 5.2. First, the proportion of time spent claiming ALMBs falls for both groups over the four year period. This suggests that both groups spent more time in employment (assuming both groups spent a fixed proportion of their time in 'other states'). The NDYP group, however, continued to spend less time claiming ALMBs than the older group and, over the four year period, spent 64 fewer days claiming ALMBs than the older group. Second, the difference in the proportion of time claiming ALMBs falls quite quickly from 7.6 percentage points to only 1.1 percentage points in year four. Finally, applying the difference-in-differences estimates, the net impact of NDYP falls from 6.6 percentage points in year one to 2.7 percentage points in year four.

The results suggest that NDYP has a persistent impact for at least four years following participation. The NDYP group spent 481 days claiming ALMBs compared to 537 days for the older group over the four year period (without controlling for the pre-NDYP difference). The impact of NDYP however diminishes over the four years.

The aggregate figures in Table 5.1 and Table 5.2 mask a high degree of variation across the 12 monthly cohorts. Figure 5.2 shows the difference in the proportion of time claiming ALMBs for the 12 monthly cohorts. The lines join together estimates for each cohort in their first, second, third and fourth years since their JSA claims reached six months duration.

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[36] Rounded to the nearest day.

Figure 5.2   Net impact of the New Deal for Young People for 12 monthly cohorts
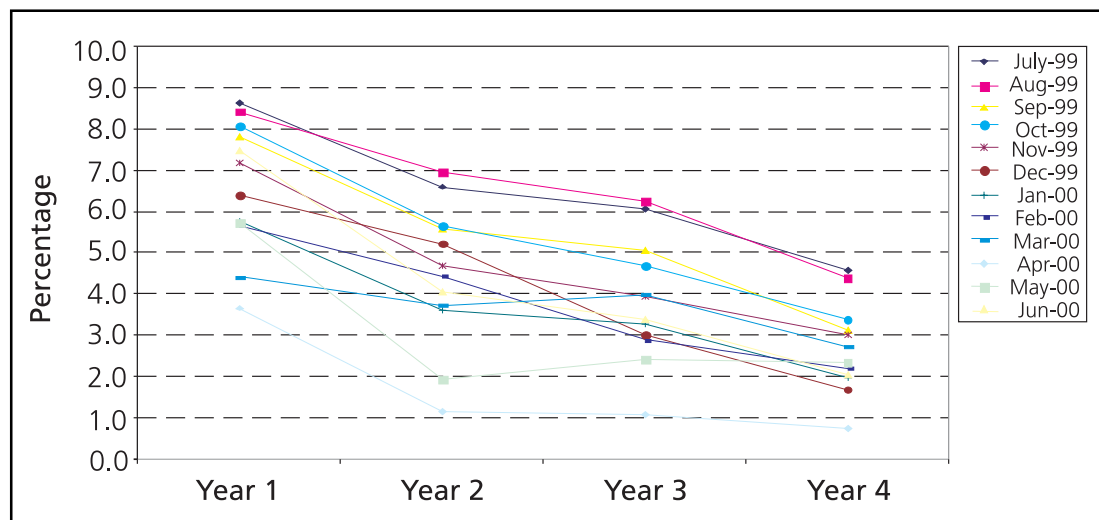
Figure 5.2 shows that NDYP participants spent a lower proportion of time claiming ALMBs than the older group and that the difference fell over the four year period for the 12 monthly cohorts. The net impact estimates in the first year range from just over eight percentage points to slightly under four percentage points. This difference is not insubstantial, but it is not possible to explain the difference in performance. Section 5.2 extends the analysis to include 24 monthly cohorts over three years to test whether there is greater variation in subsequent monthly cohorts.

5.2   Sensitivity analysis

5.2.1   Time on inactive benefits

One of the key priorities in the DWP's Five Year Strategy is to activate the inactive. The other side of this policy goal is to reduce the flows on to inactive benefits. The analysis above has been expanded to test whether participation on NDYP reduces the proportion of time the treatment group spent claiming both active and inactive benefits. The same matched cohorts and methodology from the main analysis have been used with ALMBs expanded to include IS and IB.

Before proceeding to the results a word of caution should be provided. It has not been possible to use a difference-in-differences approach to control for structural differences in the proportion of time claiming both active and inactive benefits between the comparison and treatment groups. This is because data is not available on IS and IB spells prior to the introduction of NDYP. Hence, the results may be biased if the systematic difference in the proportion of time claiming both active and inactive benefits is different to the systematic difference between the proportion of time spent claiming ALMBs.

Table 5.3 and Figure 5.3 show the difference in the proportion of time that the matched NDYP and older groups spent claiming IS and IB over the four year period. Over the four year period both groups spent an increasing proportion of their time claiming inactive benefits but the increase was greater for the older group.

Figure 5.3   Difference in time claiming IS and IB between the NDYP and older groups
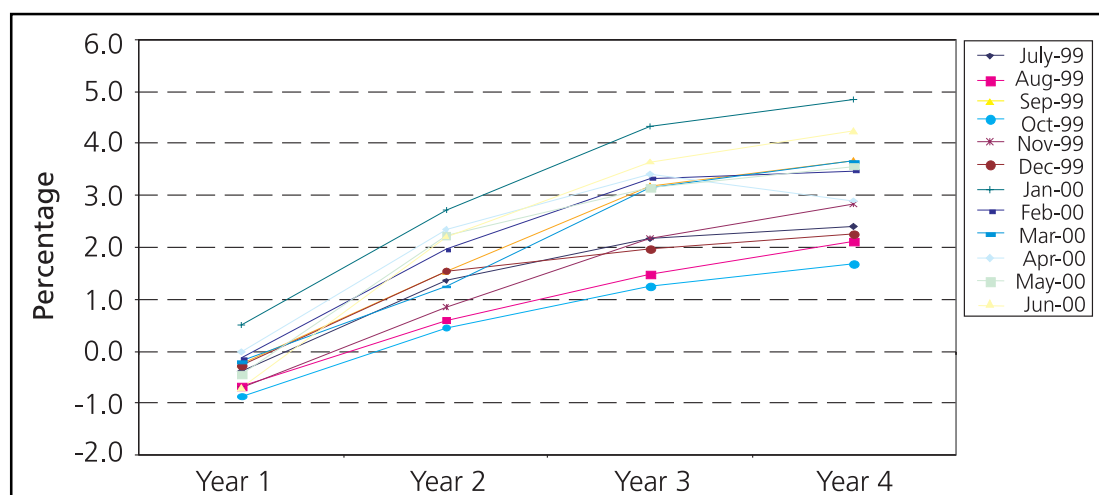
Table 5.3   Proportion and number of days claiming IS and IB[37]

	Percentages			Days[1]		
	Older group %	NDYP group %	Difference %	Older group	NDYP group	Difference
Year 1	5.7	6.1	-0.4	21	22	-1
Year 2	11.2	9.7	1.6	41	35	6
Year 3	13.8	11.1	2.7	51	41	10
Year 4	15.5	12.3	3.1	57	45	11
Overall	11.6	9.8	1.8	169	143	26

[1] Rounded to the nearest day.

Table 5.4 brings together the estimated net impact of NDYP and the difference in the proportion of time claiming inactive benefits. The results suggest that, when inactive benefits are included, the difference between the time claiming benefits for the NDYP and older groups is increased. The NDYP participants subsequently spent around two percentage points less time claiming inactive benefits, which equates to 26 days over the four year period. The NDYP group was spending 12 per cent of its time on inactive benefits in year four.

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[37] A positive number indicates that the older group spent a greater proportion of its time claiming IS and IB than the NDYP group.

Table 5.4   Net impact of the New Deal for Young People and the difference in the proportion of time claiming IS and IB

	Net impact of NDYP %	Percentage point difference in time claiming IS and IB %	Total percentage point difference %	Total days[1]
Year 1	6.6	-0.4	6.2	23
Year 2	4.5	1.6	6.0	22
Year 3	3.9	2.7	6.6	24
Year 4	2.7	3.1	5.9	21
Overall	4.4	1.8	6.2	90

[1] Rounded to the nearest day.

5.2.2   Are the results stable?

There is some variation in the net impact estimates over the 12 monthly cohorts but they are closely bunched together over the four year period. To test whether the net impact estimated for the 12 monthly cohorts is typical of a wider set of NDYP cohorts the analysis has been repeated using 24 monthly cohorts followed for three years. Figure 5.4 and Table 5.5 show the results.

Figure 5.4 shows the estimated net impact for each of the 24 monthly cohorts in the first, second and third years after they started NDYP.

Figure 5.4   Net impact of the New Deal for Young People over three years for cohorts from July 1999 to June 2001
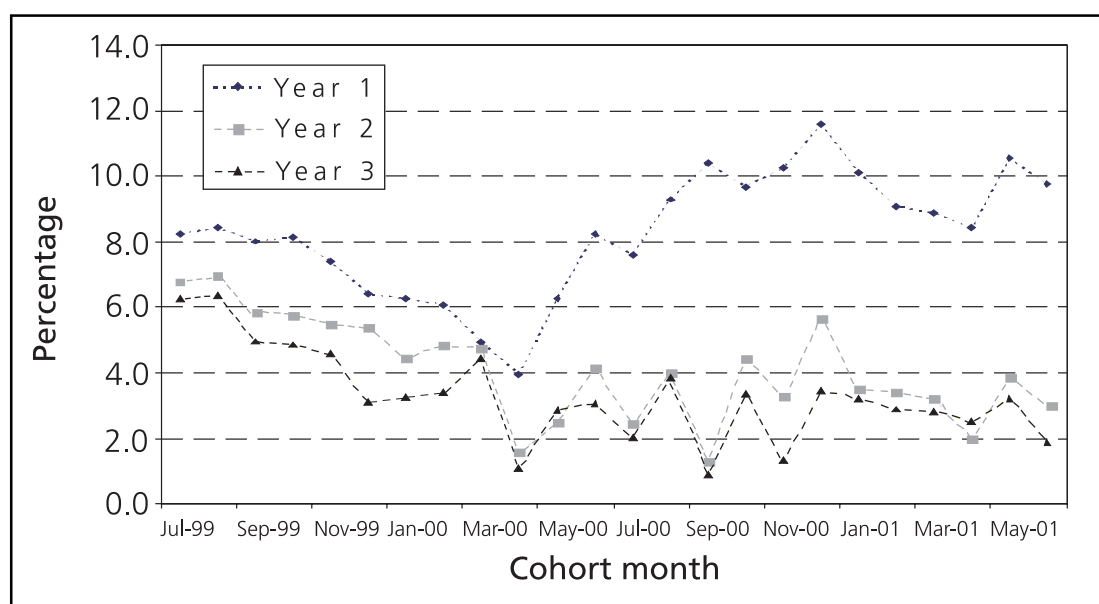
Table 5.5   Net impact of the New Deal for Young People examining 24 monthly cohorts for three years, time on ALMBs[38]

	Older group %	NDYP group %	Difference on ALMBs %	Difference pre-NDYP %	Net impact %
Year 1	58.4	49.6	8.7	0.7	8.0
Year 2	34.7	31.1	3.5	-0.6	4.1
Year 3	27.0	25.4	1.7	-1.8	3.4
Overall	40.0	35.4	4.6	-0.5	5.2

Figure 5.4 shows that the net impact estimates for the cohorts before April 2000 followed a similar downward pattern for years one, two and three. The cohorts after April 2000 however exhibit a divergence in the net impact estimates between year one and years two and three. This suggests that something changed in the labour market to cause the divergence.

One possible explanation of the divergence is the re-engineering of ND25 Plus in April 2001.[39] The timing of the re-engineering of New Deal Twenty Five Plus (ND25 Plus) corresponds to the divergence because the JSA claims of the older April 2000 cohort members became six months in duration during April 2000. Hence, if their claim was continuous, by April 2001 (year two for the April 2000 cohort) the clients would have been eligible for the re-engineered ND25 Plus. The divergence between years one and years two and three corresponds to the introduction of ND25 Plus.

There are a couple of reasons to suggest that the divergence may be caused by ND25 Plus. First, the fall corresponds to the time of the re-engineering of ND25 Plus. Second, the fall of the net impact in year two for the cohorts after April 2000 was not caused by the NDYP group spending longer claiming ALMBs, but rather the older group spending less time claiming benefit. Table 5.6 shows that the proportion of time claiming ALMBs fell for both the NDYP and older groups after the April 2000 cohort but that the fall was greater for the older group. Finally, the fall between years one and two for the post-April 2000 cohorts is smaller than the fall between years two and three for the pre-April 2000 cohorts. This is consistent with ND25 Plus causing the divergence because more clients and more 'job ready' clients were

[38] The difference in the proportion of time spent claiming ALMBs pre NDYP has been calculated for 1990 to 1994. It has been possible to include 1994 because the cohorts have been followed for only three years. Including 1994 reduces the pre-NDYP differences indicating that the younger group worsened relative to the older group in 1994 compared to the previous years.

[39] The re-engineering of ND25 Plus brought the programme into line with NDYP by introducing a mandatory Intensive Activity Period and reducing the length of claim necessary for eligibility. For a full discussion of the changes to ND25 Plus see Wilkinson (2003B).

eligible for ND25 Plus for the post-April 2000 cohorts than the pre-April 2000 cohorts.[40]

Table 5.6   Average proportion of time claiming ALMBs before and after the April 2000 cohort

	NDYP			Older		
	Year 1 %	Year 2 %	Year 3 %	Year 1 %	Year 2 %	Year 3 %
Before April 2000 cohort	52.8	31.8	25.7	60.8	36.7	28.4
After April 2000 cohort	47.2	30.7	25.2	56.7	33.2	26.0
Difference	5.7	1.1	0.5	4.1	3.5	2.4

The divergence between years one and two for the post-April 2000 cohorts is large. The average fall between year one and two for the pre-April 2000 cohorts was 1.7 percentage points and the fall for the post-April 2000 cohorts was 5.7 percentage points. This suggests that if the divergence was caused by the introduction of ND25 Plus then the effect of ND25 Plus was in the region of 4.0 percentage points. This is a large fall to be attributed to widening the eligibility criteria for ND25 Plus and making intensive activity periods mandatory. If ND25 Plus is driving the divergence then it indicates that the impact estimated may be understated.

5.2.3   Increasing the number of years used to calculate the pre-New Deal for Young People difference

To test whether the estimated net impact is sensitive to the number of years used to estimate the pre-NDYP differences between the two groups, the pre-NDYP period has been extended to include the years 1983 to 1989. Figure 5.5 shows the pre-NDYP difference in the proportions of time claiming JSA between the younger and older groups.

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[40] It has been assumed that clients with shorter JSA claims are more 'job ready' than clients with longer JSA claims. The results in Table 5.19 confirm this expectation.

Figure 5.5   Difference-in-differences estimates: 1983 to 1993[41]
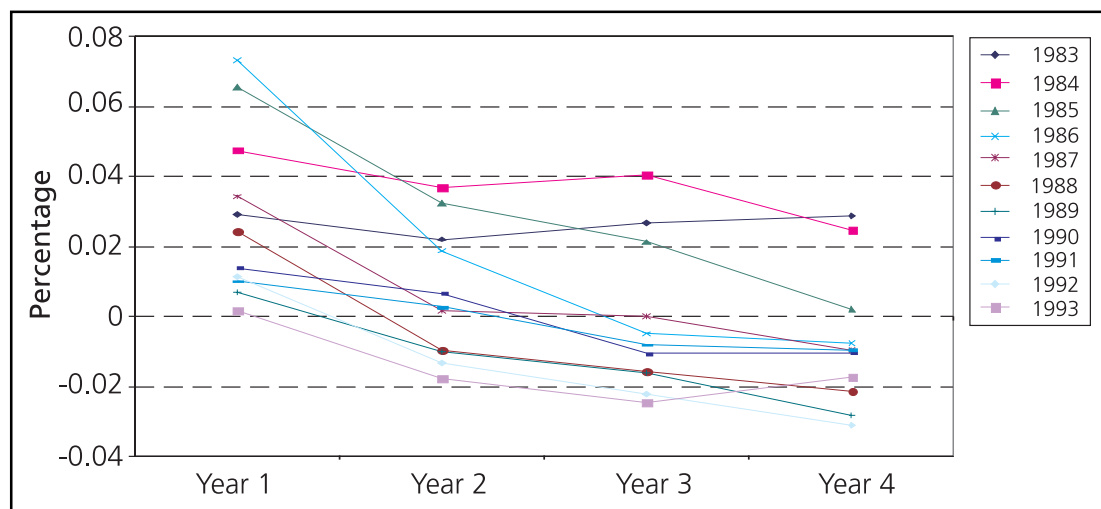
Figure 5.5 shows that the position of the younger group relative to the older group worsened between 1983 and 1993. Across nearly all the cohorts the percentage difference between the two groups fell over the four year period showing that the younger group was spending more time on JSA relative to the older group. The annual cohorts also exhibit a downward trend with a high percentage difference between the two groups during the years 1983 to 1986 and a lower percentage difference during the later years of 1990 to 1993. This supports the decision to exclude 1983 to 1989 from the analysis because earlier years have a clearly different pattern to the later years.

Table 5.7   Net impact of the New Deal for Young People, time on ALMBs: difference-in-differences estimates from 1983 to 1993

	Older group %	NDYP group %	Difference on ALMBs %	Difference pre-NDYP %	Net impact %
Year 1	59.6	52.0	7.6	3.1	4.5
Year 2	35.9	31.6	4.3	1.2	3.0
Year 3	27.9	25.6	2.3	0.4	1.9
Year 4	23.4	22.3	1.1	-0.2	1.4
Overall	36.7	32.9	3.8	1.1	2.7

Table 5.7 shows the net impact estimated using cohorts from 1983 to 1993 to estimate the pre-NDYP difference between the older and younger groups. Including all the cohorts reduces the estimated net impact. The relative performance of NDYP participants may have improved to the same levels as the 1980's without the

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[41] For the figures see Appendix C.

introduction of NDYP, but this is unlikely so these estimates underestimate the impact of NDYP.

5.2.4 Are the results sensitive to the method of matching?

To test whether the net impact estimates are sensitive to the method used for matching, the analysis has been run using exact matching instead of propensity score matching. The monthly cohorts have been matched (with replacement) on the following characteristics: JCPlus region, JCPlus district, employment history during the 12 months prior to NDYP[42], ethnicity[43] and disability status. Matches have been obtained where JCPlus region is the same and at least three of the other four characteristics are the same.[44]

Table 5.8 **Estimated impact of the New Deal for Young People estimated using exact matching, time on ALMBs**

	Older group %	NDYP group %	Difference on ALMBs %	Difference pre-NDYP %	Net impact %
Year 1	59.8	51.9	7.9	1.1	6.8
Year 2	36.1	31.5	4.5	-0.2	4.7
Year 3	28.0	25.6	2.4	-1.6	4.0
Year 4	23.7	22.3	1.4	-1.6	3.0
Overall	36.9	32.8	4.1	-0.6	4.7

The results in Table 5.8 show that the effect of modifying the form of matching is small. The estimated net impact is slightly higher in each year culminating in an estimate 0.3 percentage points higher than the estimate using propensity score matching. The small difference between the two forms of matching suggests that the estimates are robust to changes in the method of matching.

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[42] Time in employment was categorised as none, less than three months, three to six months, six to nine months and nine to 12 months.

[43] Categorised as White, Black, Asian and Other.

[44] Where an individual can be matched together with more than one person the match included has been randomly selected. Matches where all five characteristics match have been selected over matches where only four of the five characteristics match.

5.3   Which of the New Deal for Young People's options is the best?

5.3.1   Match quality

There are no specific tests for match quality, but the following measures provide an indication of the effectiveness of the match: the sample size, the number of times members of the matching group were included in the match and the propensity score distributions for each pairwise comparison. The results of these measures are summarised in Appendix D.

On each of these measures the matched cohorts VS-EO, EO-FTET and VS-FTET comparisons are satisfactory since the participants on each option have similar propensity score distributions. The distributions for ETF-VS, ETF-EO and ETF-FTET are less similar because there are fewer ETF participants with lower propensity scores than for VS, EO and FTET. This is not a problem when ETF is matched to the other options because ETF participants can be included more than once, but it does suggest that a group of VS, EO and FTET participants are dropped when these options are matched to ETF. Analysis of the distributions suggests that when VS, EO and FTET participants are matched to ETF participants, females are predominantly dropped from the sample. This result is not unexpected as far fewer females take the ETF option than the other options so their propensity to be in the ETF group is lower. Figure 5.6 and Figure 5.7 show the propensity score distributions for ETF and VS participants including and excluding females.[45]

Figure 5.6   Propensity score distribution for ETF-VS, males and females

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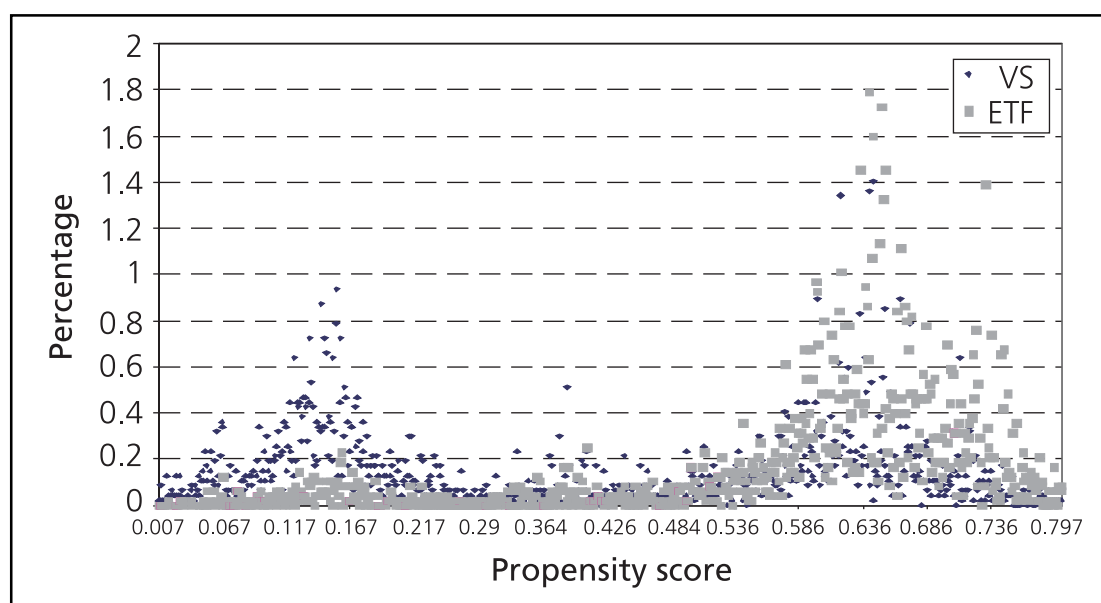
[45] See Appendix D for further information.

Figure 5.7   Propensity score distribution for ETF-VS, males only
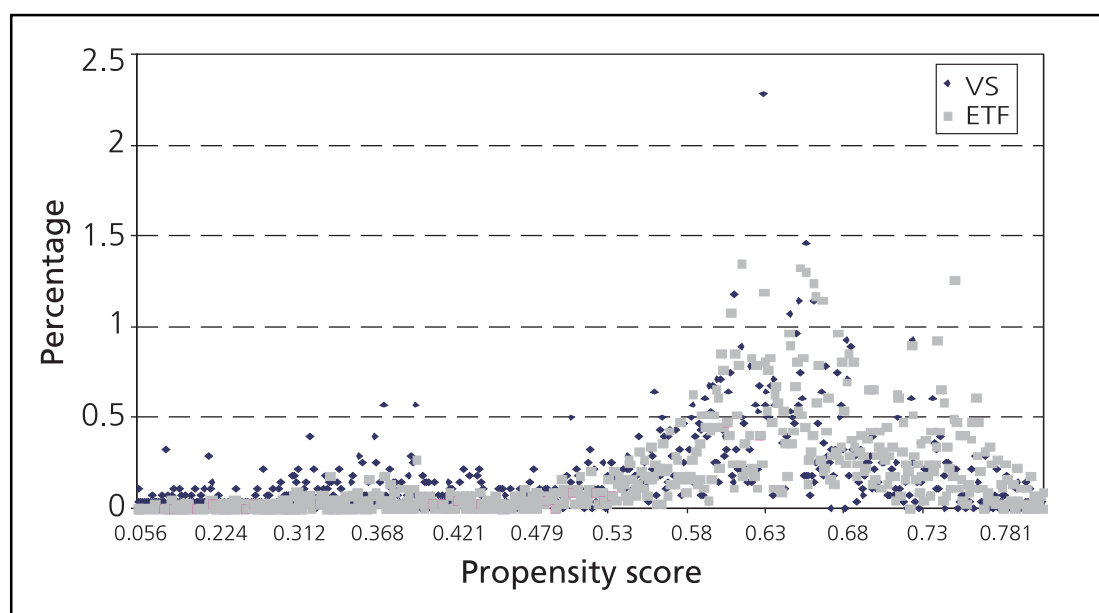
There are fewer participants with lower propensity scores for VS if females are excluded from the sample. The matching process will remove VS participants with low propensity scores when VS is matched to ETF because only a few ETF participants have low propensity scores. Figure 5.7 suggests that predominantly females have been removed from the cohort of VS matched to ETF because although some male ETF participants have low propensity scores, more females have lower scores. This finding is confirmed by Table 5.9 which shows the number and percentage of clients included in the match broken down by gender.

Table 5.9   Unmatched and matched VS participants, VS matched to ETF

	Unmatched	Matched
Female	1,608	300
Per cent	84.28	15.72
Male	506	4,336
Per cent	10.45	89.55

The exclusion of females is not necessarily a problem because the matching process results in a cohort that share similar characteristics: the matched cohorts can be used to estimate the effect of taking one option compared to another. The cohort of VS matched to ETF is not representative of females taking VS, but this is not necessary to estimate the effect of the option.

All the matched cohorts have been included in this analysis.

5.3.2 Missing employment records

Table 5.10 shows the proportion of records included in the WPLS for the matched cohorts that have missing start and/or end dates. The cell with ETF as treatment and VS as comparison shows that 36.7 per cent of the VS cohort's and 37.1 per cent of the ETF cohort's employment records had missing start or end dates on the WPLS when VS participants were matched to ETF participants.

To calculate the proportion of records with missing start or end dates, records have been weighted for individuals being included in the matched cohort more than once.

Table 5.10   Percentage of records with missing start and end dates in the matched cohorts

Treatment	Comparison ETF	VS	EO	FTET
ETF	-	36.7 / 37.1	33.0 / 37.2	37.6 / 37.3
VS	37.5 / 37.1	-	33.0 / 36.8	37.3 / 37.0
EO	37.1 / 33.1	36.9 / 32.9	-	37.3 / 33.0
FTET	37.6 / 37.5	37.0 / 37.7	33.0 / 37.5	-

Table 5.10 shows that the proportion of time in employment is unlikely to be biased for the pairwise comparisons between ETF, VS and FTET because the proportions of records with missing dates are similar. The comparisons including EO are more likely to be biased because the proportion of records with missing dates is around four percentage points lower for EO than the other options. This potential bias should be considered when examining the impact of EO compared to ETF, VS and FTET.

5.3.3 Difference in the proportion of time in employment

Table 5.11 and Table 5.12 show how much longer those taking the comparison option spent in employment compared to those taking the treatment option for the matched[46] and unmatched cohorts. For instance, if ETF is the treatment and FTET is the comparison, those taking FTET spent on average 1.6 percentage points longer in employment over the four year period than those taking ETF.

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[46] The matched cohorts have been generated by matching comparison participants to the treatment participants with replacement.

Table 5.11   Difference in the proportion of time in employment between the matched cohorts[47]

Treatment	Comparison			
	ETF %	VS %	EO %	FTET %
ETF	-	1.3	12.2	1.6
VS	-1.3	-	11.4	-0.3
EO	-13.1	-11.2	-	-10.8
FTET	-1.8	-0.2	10.9	-

Table 5.12   Difference in the proportion of time in employment between the unmatched cohorts

Treatment	Comparison			
	ETF %	VS %	EO %	FTET %
ETF	-	2.5	14.3	2.8
VS	-2.5	-	11.8	0.3
EO	-14.3	-11.8	-	-11.5
FTET	-2.8	-0.3	11.5	-

The difference between the estimated effects in Table 5.11 and Table 5.12 indicate the impact of differences in characteristics on the employment outcomes of those taking each option. For instance, the effect of EO compared to ETF is slightly larger for the whole cohort than for the matched cohort. This suggests that the characteristics of those taking EO make them slightly more disposed towards spending time in employment than those taking ETF.

EO is clearly the best option in terms of the subsequent time in employment, but there are three concerns that suggest the estimated impact is biased upwards. First, there may be unobservable characteristics, systematically correlated with participant's choice of option that have not been controlled for. For instance, it has not been possible to control for a participant's motivation for finding employment. If this characteristic is correlated with the option choice then the estimated effects will still be biased (although less than the unmatched cohorts). Second, the proportion of employment spells with missing dates is lower for EO than for the other options so the estimates may be biased. Finally, time spent on EO has been included as time in employment. Including time on EO as valid employment spells increases the estimated net impact of EO compared to the other options during year one (see Appendix F).

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[47] See Appendix F for more information.

There are two methods that can be used to remove the effect of EO being treated as an employment spell: (1) subtract time spent on EO from the proportion of time in employment for all options, or (2) exclude the estimated effects during year one and assume that all pairwise comparison groups are equally likely to participate on EO during years two, three and four. The second approach has been adopted because the former underestimates the proportion of time that EO participants spend in employment excluding time on EO.

Some EO spells do not have matching valid employment spells on the WPLS so the total length of time on EO is greater than the total recorded length of time in employment whilst on EO. Hence, subtracting total EO spells from total recorded employment spells whilst on EO is negative and will cause the proportion of time EO participants spend in employment to be underestimated. Table 5.13 shows the results using the second approach.

The assumption that all pairwise comparison groups spent a similar proportion of time on EO during years two, three and four appears reasonable. Appendix G shows the proportion of time that all cohorts spent participating on EO during the four year period.

Table 5.13   Difference in the proportion of time in employment between the matched cohorts excluding year one

| | Comparison | | | |
Treatment	ETF %	VS %	EO %	FTET %
ETF	-	2.1	9.4	3.0
VS	-1.2	-	8.9	1.1
EO	-10.4	-8.4	-	-6.6
FTET	-3.1	-1.5	7.0	-

Excluding year one reduces the effect of EO and increases the effect of FTET. EO however remains the best option for increasing the proportion of time in employment in the longer-term followed by FTET, VS and ETF at a distance. Those taking FTET spent less time in employment during year one than those taking VS or ETF, but they subsequently spent longer in employment.

The estimates in Table 5.13 are also consistent. There is some variation but for all matches one option clearly dominates the other. For instance, when VS was matched to EO, VS participants spent less time in employment and when EO was matched to VS, EO participants spent more time in employment.

The results in Table 5.13 are in agreement with Greenberg et al. (2004): the 'work first' approach of EO dominates the human capital approach of FTET. This is not however surprising because the length of FTET is similar to the training programmes in the USA. The median number of days spent on FTET is low at 130 for starters in

1999, 124 for starters in 2000 and 103 for starters in 2001. The average lengths are slightly longer for those who completed their spells and went on to follow-through (145, 141 and 138 days respectively) but the length of training remains short.[48]

To place the percentage point differences in their context, Table 5.14 and Table 5.15 show the equivalent number of days in employment following option participation and the total number of days that each matched cohort spent in employment over the four year period (excluding year one).

Table 5.14   Difference in the number of days in employment between the matched cohorts excluding year one[49]

| | Comparison | | | |
| | ETF % | VS % | EO % | FTET % |
Treatment				
ETF	-	23	103	33
VS	-13	-	98	13
EO	-114	-92	-	-73
FTET	-34	-16	76	-

Table 5.15   Number of days in employment for the matched cohorts excluding year one[50]

| | Comparison | | | |
Treatment	ETF	VS	EO	FTET
ETF	-	363 / 340	444 / 341	374 / 341
VS	352 / 365	-	466 / 366	383 / 366
EO	352 / 466	374 / 466	-	393 / 466
FTET	351 / 383	366 / 382	460 / 381	-

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[48] The mean number of days on FTET for option starters was 153 in 1999, 147 in 2000 and 127 in 2001. The mean number of days on FTET for those going on to follow-through was 166 in 1999, 161 in 2000 and 150 in 2001.

[49] Rounded to the nearest day.

[50] Rounded to the nearest day.

Table 5.14 shows that the 10.4 percentage point difference between the EO cohort matched to the ETF cohort equates to the EO cohort spending 114 days longer in employment than the ETF cohort (EO matched to ETF). Table 5.15 shows that for the pairwise matched comparison, on average, the ETF cohort spent 352 days in employment and the EO cohort spent 466 days in employment during years two, three and four. The 10.4 percentage point difference is equivalent to the EO cohort spending just over a third longer in employment than the ETF cohort.

5.4   Sensitivity analysis

5.4.1   Comparing estimated impact using time off ALMBs and time in employment

This analysis has been undertaken for males only because time off ALMBs is likely to be better correlated with time in employment for males than females. The strength of the relationship between net impact estimates using time off ALMBs and time in employment has been tested using simple regression analysis. Table 5.16 shows the regression results including all four years of data and Table 5.17 shows the results including data from only years two, three and four.

Table 5.16   Estimated impact using employment regressed on estimated employment using time off ALMBs, all data

	Dependent variable: estimated net impact using time in employment		
Independent variables	Coefficient	Standard error	t statistic
Constant	-0.00	0.01	-0.06
Estimated net impact using time on ALMBs	-0.89	0.13	-6.96
R^2 = 0.51, N=48			

Table 5.17   Estimated impact using employment regressed on estimated employment using time off ALMBs, data from years two, three and four

	Dependent variable: estimated net impact using time in employment		
Independent variables	Coefficient	Standard error	t statistic
Constant	-0.00	0.00	-0.43
Estimated net impact using time on ALMBs	-0.75	0.20	-38.16
R^2 = 0.98, N=36			

In both regressions the intercept is insignificant and the coefficient on the estimated net benefit using time off ALMBs is highly significant. More variation is explained by

the second regression than the first which can partly be explained by EO spells being included as employment spells.[51]

Both regressions suggest that using time off ALMBs produces a higher estimate of impact compared to using time in employment. This result is not directly comparable with the net impact estimates of NDYP in Section 5.1 because participants leaving during Gateway are not included in the option cohorts. The results are, however, indicative, suggesting that estimating the impact of NDYP using time off ALMBs may overestimate NDYP's impact on getting participants into employment.

5.5 Who performs best on the New Deal for Young People?

5.5.1 Clusters

Using clustering techniques, male and female NDYP participants have been split into five clusters. Table 5.18 shows how each of the clusters score against the 12 variables used for grouping.

Table 5.18 **Average scores for each cluster on each clustering variable**[52]

	Off benefits	Long JSA	Other benefits	Employment	JSA and New Deal
Proportion of time on JSA	3%	36%	7%	18%	47%
Average JSA spell length	33	212	60	84	166
Number of JSA spells	0.7	3.4	1.0	3.2	5.1
Proportion of time on other benefits	2%	9%	73%	4%	6%
Average other benefit spell length	26	105	903	49	73
Number of other benefit spells	0.2	0.9	2.7	0.5	0.8
					Continued

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[51] Appendix E shows the regression results excluding only impact estimates for year one where EO is included in the match.

[52] The proportion of time for each client does not necessarily add to 100 per cent. It is possible for a client to be in more than one state at a time, for example, on New Deal and JSA. It is also possible that a client spent time in a state for which there was no record on the WPLS leading to less than 100 per cent of time being accounted for.

Table 5.18   Continued

	Off benefits	Long JSA	Other benefits	Employment	JSA and New Deal
Proportion of time on New Deal	0%	7%	1%	2%	37%
Average New Deal spell length	5	88	17	27	383
Number of New Deal spells	0.1	1.0	0.2	0.3	1.6
Proportion of time in employment	41%	13%	9%	52%	16%
Average employment spell length	437	116	97	251	139
Number of employment spells	1.6	1.5	0.9	6.4	2.1
Frequency	72,430	38,090	30,320	26,120	22,600

The most populous cluster, 'Off Benefits', contains clients who spent approximately two-fifths of their time in employment, but only around five per cent of their time claiming benefits. This means that these clients spent more than half of their time in unknown states, which may be employment that is not included in the WPLS or complete withdrawal from the labour market.

The 'Long JSA' cluster consists of clients who spent a significant amount of time on JSA. The JSA claims for this cluster lasted the longest of all the clusters. These clients spent approximately half their time in unknown states, which may reflect labour market success or exits from the labour market.

The 'Employment' cluster contains clients who spent, on average, more than half their time in employment. They also spent a significant amount of time on JSA.

The 'JSA and New Deal' cluster contains clients who spent almost half their time on JSA together with significant periods on New Deal programmes.

Clients in the 'Other Benefits' cluster spent almost three-quarters of their time on inactive benefits. These clients spent most of their time out of the active labour market.

5.5.2   Likelihood of success

Clients have been deemed to have had successful long-term outcomes after leaving NDYP if they were included in the 'Off Benefits' or 'Employment' clusters. These clusters have the highest proportion of time in confirmed employment and the least time on benefits.

The relative likelihoods of being in the successful clusters for certain characteristics are shown in Table 5.19. It shows the likelihood of a successful outcome for the characteristic in the 'Group' column compared to the characteristic in the 'Comparison' column. For example, participants from Asian ethnic groups are 64 per cent more

likely to achieve successful outcomes than participants from Black ethnic groups once other factors have been controlled for.

Table 5.19   Factors affecting the likelihood of a successful post-New Deal for Young People outcome

	Group	Comparison	Likelihood
Disability	Non-PWD	PWD[1]	35% more likely
Gender	Males	Females	7% more likely
Ethnicity	Asian groups	Black groups	64% more likely
	White	Black groups	29% more likely
Leaving stage	Pre-Gateway	Follow-Through	78% more likely
	Gateway	Follow-Through	51% more likely
	EO	Follow-Through	317% more likely
	FTET	Follow-Through	98% more likely
	ETF	Follow-Through	38% more likely
	VS	Follow-Through	63% more likely
Joining age	19	18	37% more likely
	20	18	70% more likely
	21	18	110% more likely
	22	18	144% more likely
	23	18	161% more likely
	24	18	170% more likely
Previous claim	<8 months	>24 months	280% more likely
	8–16 months	>24 months	157% more likely
	16–24 months	>24 months	67% more likely
Qualifications	Other	None	81% more likely
	Foundation	None	32% more likely
	NVQ level 1	None	51% more likely
	NVQ level 2	None	100% more likely
	NVQ level 3	None	160% more likely
	NVQ level 4+	None	308% more likely

[1] Person With a Disability.

Table 5.19 shows that there are large differences in the likelihood of success for the different leaving stages. Leavers from EO are the most likely to achieve successful outcomes followed by leavers from FTET, Pre-Gateway, VS, Gateway, ETF and Follow-Through.[53] This is in agreement with the findings in Section 5.3: EO participants are the most likely to have successful outcomes from NDYP in the longer-term.

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[53] As discussed in Section 5.3 some spells on the EO may also be recorded as employment spells. The effect of this should not be large, because a cohort of leavers has been used so only subsequent EO spells could possibly be included as employment spells.

The probability of success decreases as age decreases and as the length of claims prior to NDYP increases. Clients with high qualifications are more likely to achieve successful outcomes (clients with qualifications at level 4 and above were 308 per cent more likely to have a successful outcome than those with no qualifications). Low or no qualifications may however be a proxy for other forms of disadvantage not associated with qualifications. The result should not be interpreted as showing that increasing qualifications will necessarily improve longer-term outcomes from NDYP.

There are also differences between different ethnic groupings. White groups are 29 per cent more likely to be in the successful clusters than Black groups. Asian groups are however 64 per cent more likely to be in the successful clusters than Black groups.

The claimant count and population density in the client's LAD were also included as variables in the logistic regression. The likelihood of success decreased significantly as each of these increased.

The results presented are based on gross outcomes and not net impacts. For instance, there are two possible explanations for older participants performing better than younger participants. First, older participants may be more job ready than younger participants so they would have achieved better outcomes even without NDYP. The second explanation is that there may be no difference between participants of different ages when they start the programme, but the programme has a greater impact on older participants than younger participants. From the results in Table 5.19 it is not possible to identify which explanation dominates.

### 5.5.3	Are longer-term and immediate outcomes linked?

Figure 5.8 shows that the immediate destinations of NDYP participants are a good guide to their longer-term outcomes. Leavers to unsubsidised employment are most likely to have spent their time in employment or off benefits, leavers to other benefits are likely to have remained on other benefits and leavers to JSA are likely to have spent most of their time on JSA or New Deals.

**Figure 5.8   Proportion of New Deal for Young People leavers to
each immediate destination in each cluster**
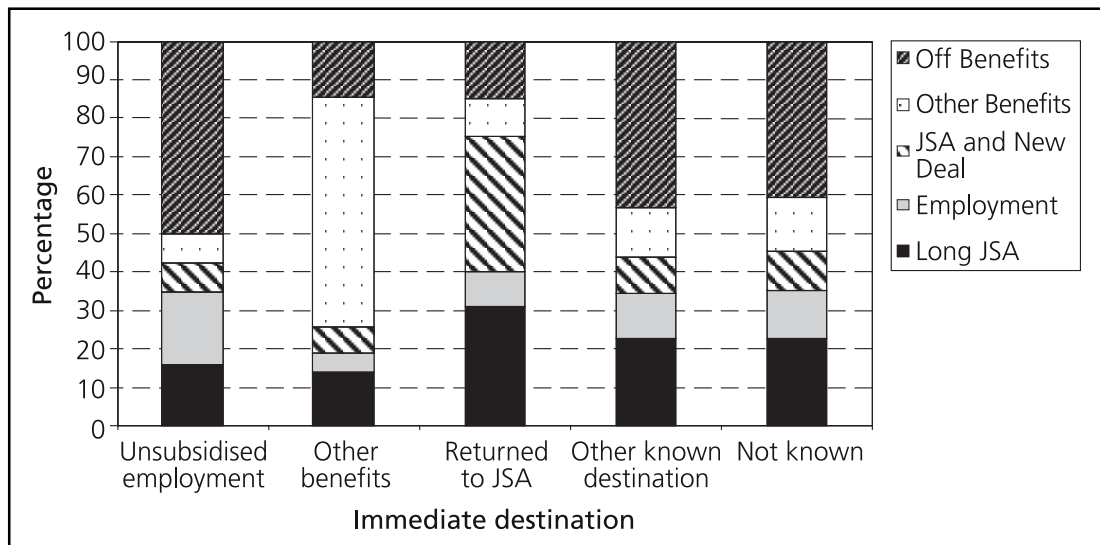
6 Conclusions

6.1   What is the impact of the New Deal for Young People?

NDYP participants spent 64 fewer days over a four year period claiming ALMBs than a group of JSA clients slightly too old to be eligible for NDYP. The estimated impact of NDYP gradually fell over the four year period from 24 days in the first year to ten days in the fourth year, but the impact of NDYP remained positive over the entire four year period.

This estimate only includes the direct impact of NDYP on participants and not the indirect effects of the programme on non-participants. Riley *et al.* (2000) found that by the first half of 2000, NDYP had generated about 25,000 jobs (including those in subsidised employment) of which 10,000 went to people outside the NDYP age group. This suggests that our analysis has not included a significant part of the programme's benefit: NDYP's impact on non-participants.

There are a number of uncertainties surrounding the impact estimates. First, the comparison group were eligible for ND25 Plus so the estimated impact of NDYP will be underestimated, particularly during years three and four. Second, time off ALMBs has been used as the outcome measure but time off these benefits does not necessarily correspond to a positive outcome. Third, time off ALMBs has been calculated and not time off all benefits. If IS and IBs are included then NDYP participants spent, on average, 90 fewer days claiming benefits than the comparison group over a four year period. This may be due to NDYP having a direct or indirect effect on inactive benefit claims as well as ALMBs claims. Finally, the impact of NDYP has been estimated using a difference-in-differences approach. The difference in the proportion of time claiming JSA before NDYP was introduced has been estimated using the years 1990 to 1993. The selection of these years is, to an extent, arbitrary. Including earlier years reduces the estimated net impact whereas restricting the sample further increases the estimated impact.

Including active and inactive benefits suggests that NDYP reduced the proportion of time participants subsequently spent claiming benefits by 90 days over the four year

period. The NDYP group was spending 12 per cent of its time on inactive benefits in the fourth year.

6.2   Which of the New Deal for Young People's options is the best?

The findings support the results in Bradley (2004): the Employment Option performs the best followed by the Full-time Education and Training Option, the Voluntary Option and the Environment Task Force Option. These findings also suggest that the timing of the NDYP evaluation by Bonjour et al. (2001) was too early to allow the effect of the Full-Time Education and Training Option to be realised.

The use of matching is central to the results because clients choose which option to take. If there are characteristics systematically correlated with the choice of option that have not been included in the matching then the results presented in this paper may be biased. A number of client characteristics have been controlled for, but it has not been possible to control for all relevant characteristics. Clients who are highly motivated may be more likely to take the Employment Option. Since it has not been possible to control for this factor, the results may overstate the effect of the Employment Option compared to the other options. The size of the difference between the Employment Option and the other options suggests that, even after accounting for potential bias, those taking the Employment Option have the most successful longer-term outcomes.

The difference-in-differences approach was unnecessary for this analysis because the comparison and treatment groups could be matched by their age. For this reason it was possible to use time in employment instead of time off ALMBs. Comparing the results obtained using time in employment and time claiming ALMBs suggests that there is a strong correlation between these two outcome measures and that time off ALMBs overestimates the impact of each option compared to using time in employment. This indicates that the net impact of NDYP may have been overestimated if time in employment is the outcome of interest and the results from options are comparable with the net impact estimates.

6.3   Who performs best on the New Deal for Young People?

It has been possible to group the leavers according to what they have done since leaving. The characteristics of those in the different clusters provide an insight into who is most likely to achieve a successful longer-term outcome.

Leavers from the Full Time Education and Training Option and the Employment Option are the most likely to have successful long-term outcomes from NDYP. Those who leave to other benefits are likely to spend the majority of their time on other benefits and those returning to JSA are likely to spend significant time on JSA or New Deals.

There are large differences in the rates of success for participants with different characteristics. Older participants, those with higher qualifications and those with shorter JSA claims prior to NDYP are far more likely to have successful outcomes in the long-term after NDYP. Geography also plays an important role with local labour market conditions having a large effect on the likelihood of success in the longer-term.

These results are however based on gross outcomes and not net impacts. Hence, participants with higher qualifications may be more likely to have successful outcomes but it is not possible to determine whether this is because they are more able or because the programme helps them more than participants without qualifications. No or low qualifications may also be acting as a proxy for other forms of disadvantage that it has not been possible to include in the logistic regression.

An interesting finding is that gender does not seem to play an important role in determining long-term success after other factors have been controlled for. Females are likely to spend longer on other benefits, but these claims are offset by males being more likely to spend longer claiming JSA.

Appendix A
Propensity score matching

The central problem for programme evaluation is missing data. If someone has participated on NDYP then it is possible to follow their post-programme labour market experiences, but it is not possible to know what would have happened to them if they had not participated on the programme.

The most defensible method for programme evaluation is random assignment. This randomly generates a comparison group from the population eligible for the programme that can be used as a counterfactual. This method could not be used in the net impact analysis because NDYP is a mandatory programme for all JSA clients aged 18 to 24 who have been claiming JSA for at least six months. There is no subset of clients who meet these criteria and who have not participated on NDYP.

To overcome the missing data problem this analysis has taken a similar group of people as the comparison group. The raw difference between the treatment and the comparison groups will however be a biased estimate of the programme effect if there are systematic differences in how the comparison and treatment groups would have responded to programme participation. The aim of matching is to pair comparison and treatment participants that have similar observable characteristics and may respond in a similar way to programme participation. The difference between the matched comparison and treatment groups will then provide an estimate of the programme effect.

There are two key problems with matching. First, is there sufficient information to match the comparison and treatment groups, and second, as the number of matching characteristics increases, the likelihood of finding two individuals who match decreases. Rosenbaum *et al.* (1983) have argued that comparison and treatment groups can be matched using a function of characteristics, the crudest of

which is the propensity score. The propensity score is the probability that someone in either the comparison or treatment group belongs to the comparison group. The propensity score has been calculated using logistic regression.[54]

A matched pair has been included in the matched cohort if their propensity scores match to three decimal places. The matches have been made with replacement so that if the comparison group is matched to the treatment group, observations from the treatment group are included once but observations from the comparison group can be included more than once if necessary.[55]

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[54] A stepwise approach has been used so that variables are included in the regression in the order of their explanatory power. The level of significance used was the five per cent level.

[55] Suppose two members of the comparison group (A, B) are being matched to three members of the treatment group (C, D, E) and that they all have the same propensity score. Under the matching methodology used person A has been matched with person C and person B has been matched to the next different person with the same propensity score i.e. person D. If however two members of the comparison group (X, Y) are being matched to one member of the treatment group (Z) with the same propensity score then X has been matched to Z and then, because there is no-one different with the same propensity score, Y has been matched to Z.

Appendix B
Quality of the matching of New Deal for Young People participants and the older group

Table B.1   Older cohorts matched to NDYP cohorts, all cohorts

	NDYP group	Older group
Original sample size	149,788	97,100
Sample size after Client Extract and LAD data	148,526	94,162
Sample size after females and duplicates removed	102,553	64,750
Observations included in match	101,812	60,829
Median number of times observation matched	-	2
Percentage of observations matched up to three times	-	99%
Largest number of times observation matched	-	13

Table B.2   Matched and unmatched by JCPlus region, older matched to NDYP, March 2000 cohort

JCPlus region		NDYP cohort		Older cohort	
		Unmatched	Matched	Unmatched	Matched
Scotland	Fr	2	1,198	0	726
	%	0.17	99.83	0	100.00
North East	Fr	1	839	0	470
	%	0.12	99.88	0	100.00
North West	Fr	112	1,434	0	2,325
	%	7.24	92.76	0	100.00
Yorkshire & the Humber	Fr	3	1,225	0	807
	%	0.24	99.76	0	100.00
Wales	Fr	5	695	0	542
	%	0.71	99.29	0	100.00
West Midlands	Fr	1	999	0	699
	%	0.10	99.90	0	100.00
East Midlands	Fr	3	595	0	502
	%	0.50	99.50	0	100.00
East of England	Fr	0	490	0	506
	%	0.00	100.00	0	100.00
South East	Fr	1	576	0	781
	%	0.17	99.83	0	100.00
London	Fr	2	1,433	0	2,226
	%	0.14	99.86	0	100.00
South West	Fr	1	518	0	418
	%	0.19	99.81	0	100.00

Table B.3   NDYP cohorts matched to older cohorts, all cohorts

	NDYP group	Older group
Original sample size	149,788	97,100
Sample size after Client Extract and LAD data	148,526	94,162
Sample size after females and duplicates removed	102,553	64,750
Observations included in match	60,829	63,623
Median number of times observation matched	1	-
Percentage of observations matched up to three times	100%	-
Largest number of times observation matched	11	-

Table B.4   Matched and unmatched by JCPlus region, NDYP matched to older, March 2000 cohort

JCPlus region		NDYP cohort		Older cohort	
		Unmatched	Matched	Unmatched	Matched
Scotland	Fr	12	1,188	4	714
	%	1.00	99.00	0.56	99.44
North East	Fr	3	837	8	452
	%	0.36	99.64	1.74	98.26
North West	Fr	1471	75	884	111
	%	95.15	4.85	88.84	11.16
Yorkshire & the Humber	Fr	27	1,201	22	743
	%	2.20	97.80	2.88	97.12
Wales	Fr	462	238	78	315
	%	66.00	34.00	19.85	80.15
West Midlands	Fr	13	987	6	679
	%	1.30	98.70	0.88	99.12
East Midlands	Fr	34	564	10	463
	%	5.69	94.31	2.11	97.89
East of England	Fr	261	229	27	338
	%	53.27	46.73	7.40	92.60
South East	Fr	489	88	49	457
	%	84.75	15.25	9.68	90.32
London	Fr	1383	52	191	1,013
	%	96.38	3.62	15.86	84.14
South West	Fr	306	213	5	387
	%	58.96	41.04	1.28	98.72

Appendix C
Difference-in-differences estimates: 1983 to 1993

	Older %	Younger %	Difference %
Year 1	67.1	64.0	3.1
Year 2	46.9	45.7	1.2
Year 3	39.1	38.7	0.4
Year 4	33.4	33.6	-0.2
Total	46.6	45.5	1.1

The cohorts for the years 1990 to 1993 differ slightly from the cohorts included in Table 5.1 because once someone has been included in an annual cohort they have been excluded from all subsequent cohorts.

Appendix D
Quality of the matching of options cohorts

D.1   Measures of match quality

Table D.1   Treatment: ETF

	ETF	VS	EO	FTET
Original sample size	4,740	4,701	4,753	9,329
Observations included in match	4,636	2,589	-	-
	4,653	-	3,345	-
	4,700	-	-	4,295
Median number of times observation matched	-	2	1	1
Percentage of observations matched up to three times	-	94%	97%	100%
Largest number of times observation matched	-	22	13	7

Table D.2   Treatment: VS

	ETF	VS	EO	FTET
Original sample size	4,740	4,701	4,753	9,329
Observations included in match	2,589	4,114	-	-
	-	4,513	3,344	-
	-	4,660	-	4,475
Median number of times observation matched	1	-	1	1
Percentage of observations matched up to 3 times	93%	-	98%	100%
Largest number of times observation matched	25	-	19	6

Table D.3   Treatment: EO

	ETF	VS	EO	FTET
Original sample size	4,740	4,701	4,753	9,329
Observations included in match	3,345	-	4,514	-
	-	3,344	4,658	-
	-	-	4,712	4,552
Median number of times observation matched	1	1	-	1
Percentage of observations matched up to three times	97%	98%	-	100%
Largest number of times observation matched	16	12	-	7

Table D.4   Treatment: FTET

	ETF	VS	EO	FTET
Original sample size	4,740	4,701	4,753	9,329
Observations included in match	4,295	-	-	8,899
	-	4,475	-	9,258
	-	-	4,552	9,063
Median number of times observation matched	2	2	2	-
Percentage of observations matched up to three times	90%	92%	95%	-
Largest number of times observation matched	31	16	16	-

D.2   Propensity score distributions

The following figures show the propensity score distributions for participants of each option. The first figure shows the probability that someone in the ETF or VS cohorts were in the ETF cohort. Note the probability that someone in the EFT or VS cohorts taking VS is one minus the probability that they took ETF.

Figure D.1   ETF and VS cohorts
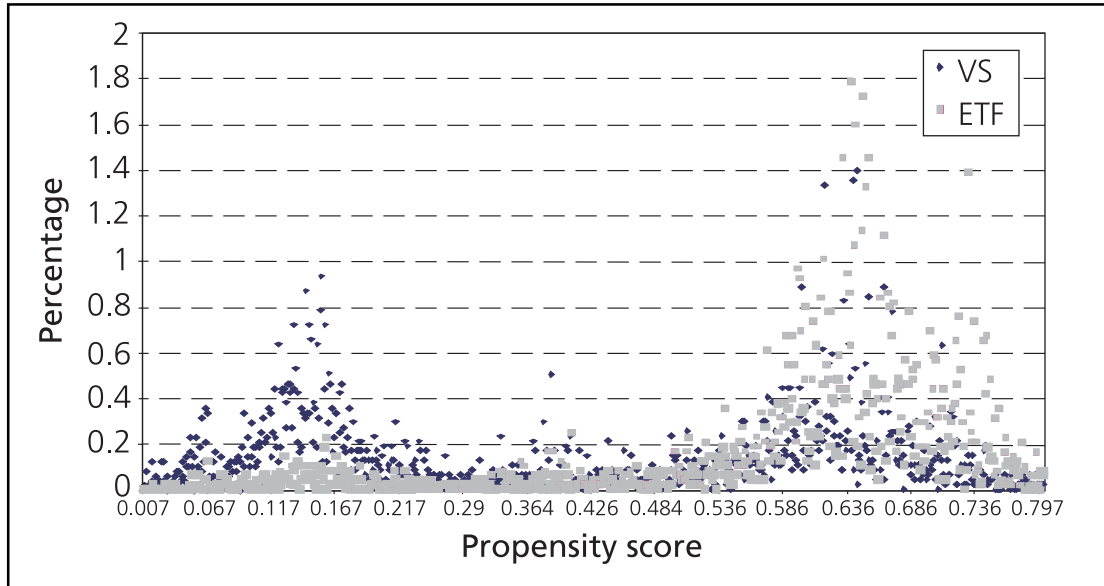
Figure D.2   ETF and EO cohorts
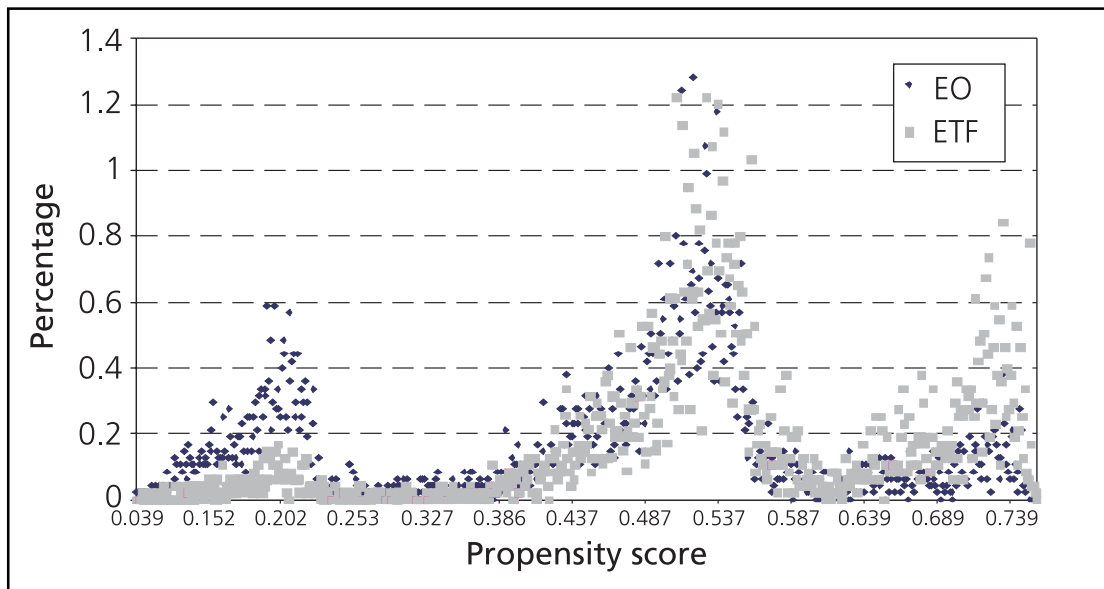
Figure D.3   ETF and FTET cohorts
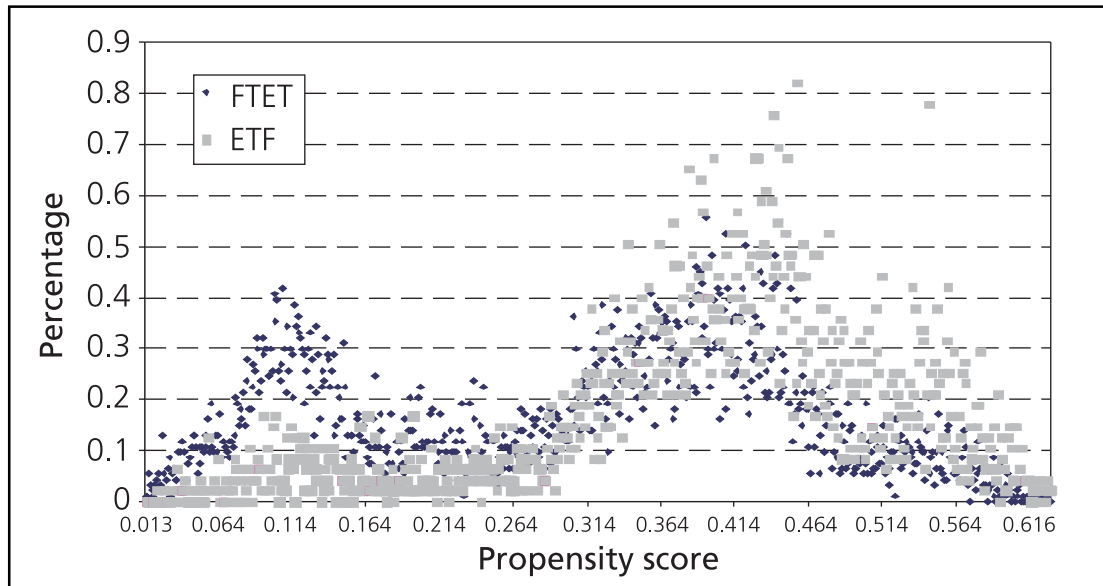
Figure D.4   VS and EO cohorts
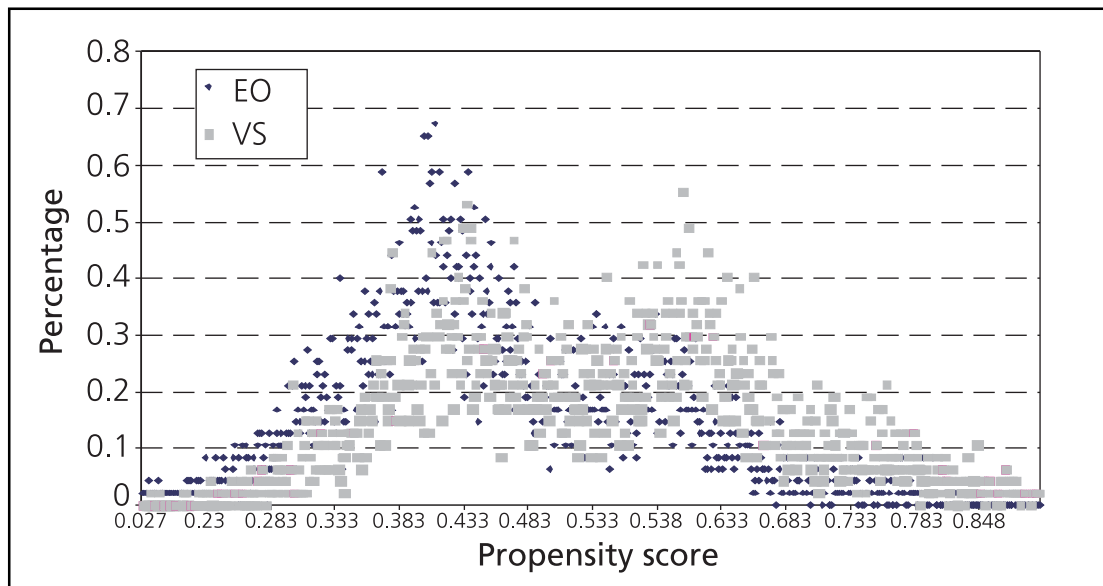
Figure D.5   VS and FTET cohorts
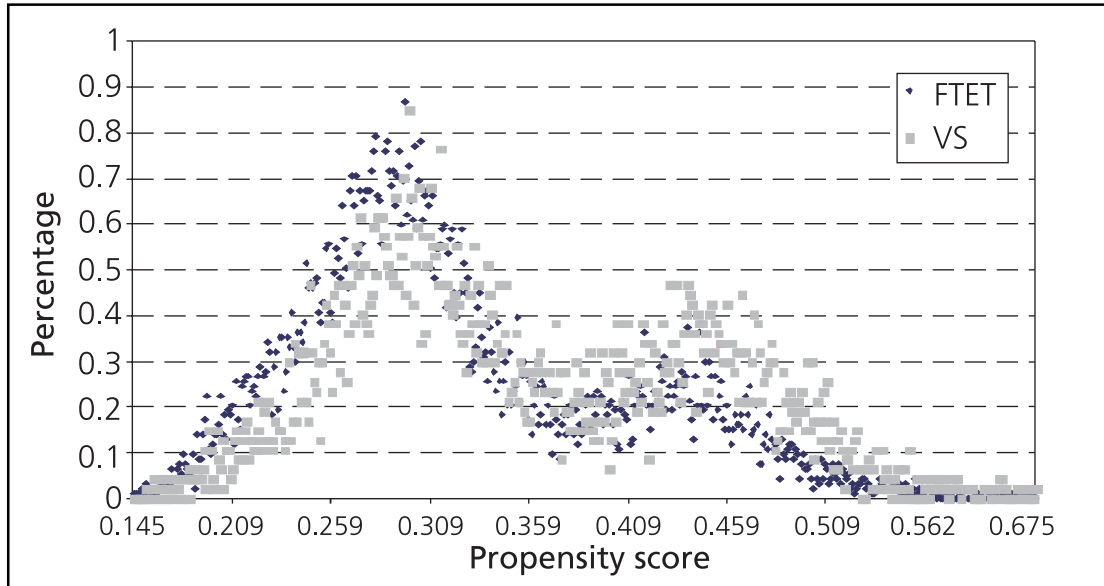
Figure D.6   EO and FTET cohorts
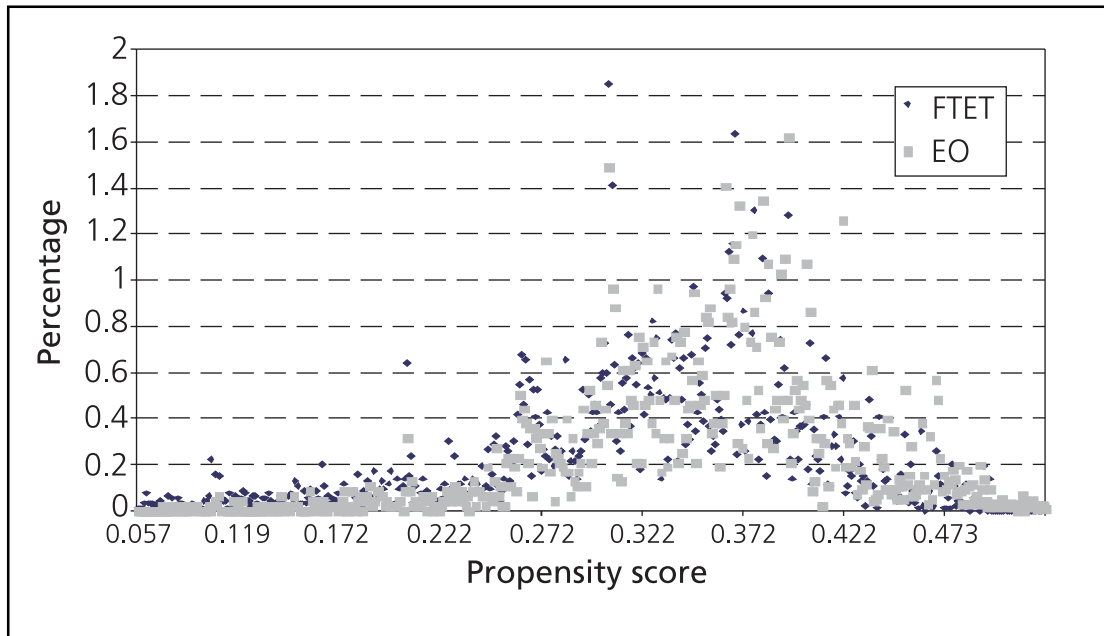
D.3 Propensity score distributions excluding females

Figure D.7 ETF and VS cohorts
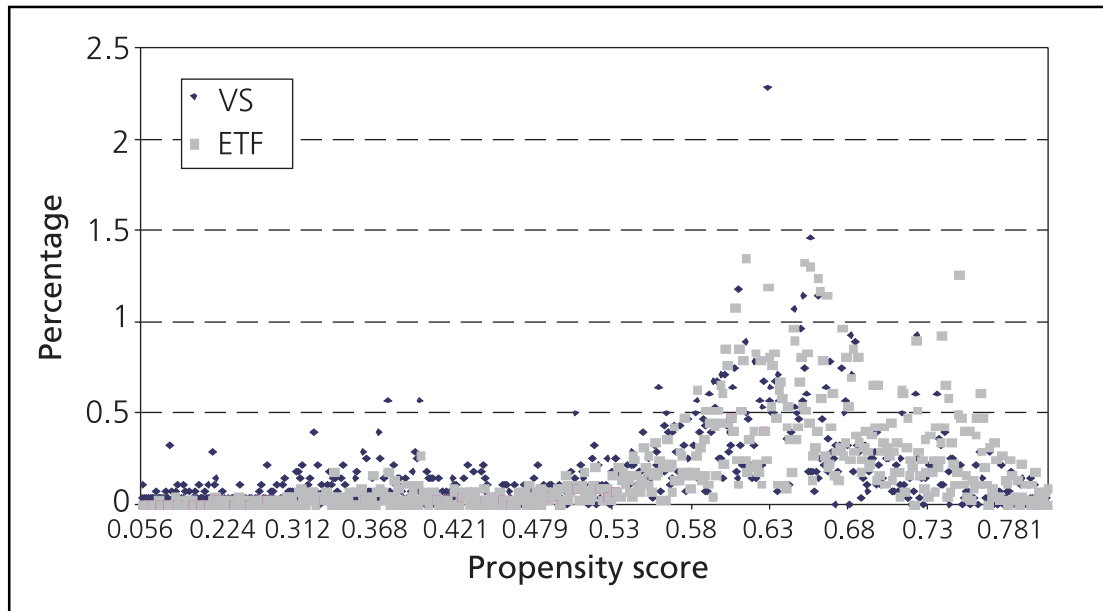
Figure D.8 ETF and EO cohorts
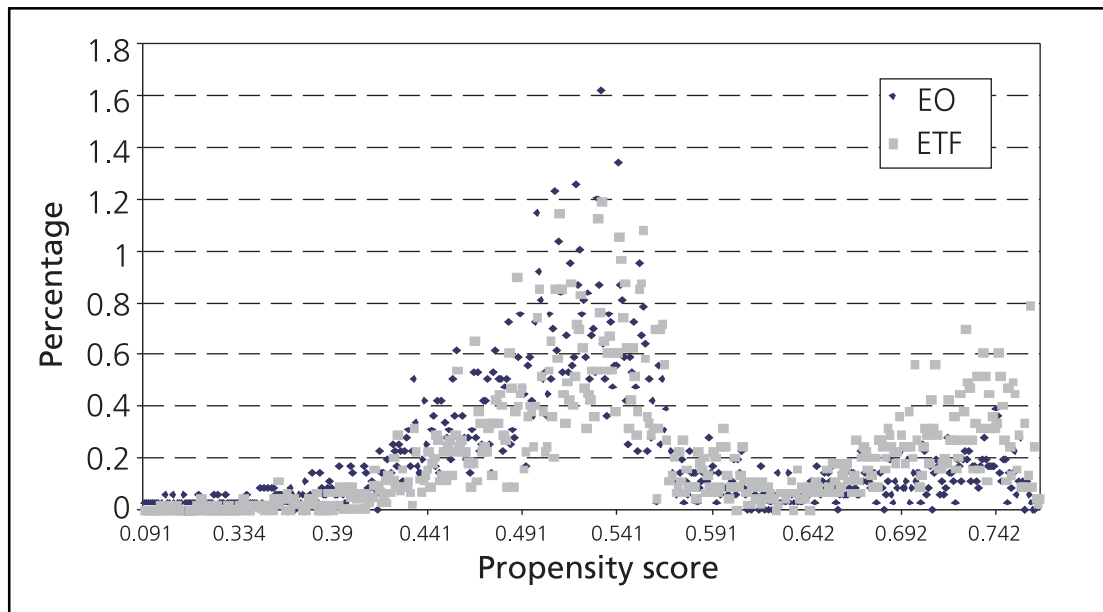
Figure D.9   ETF and FTET cohorts
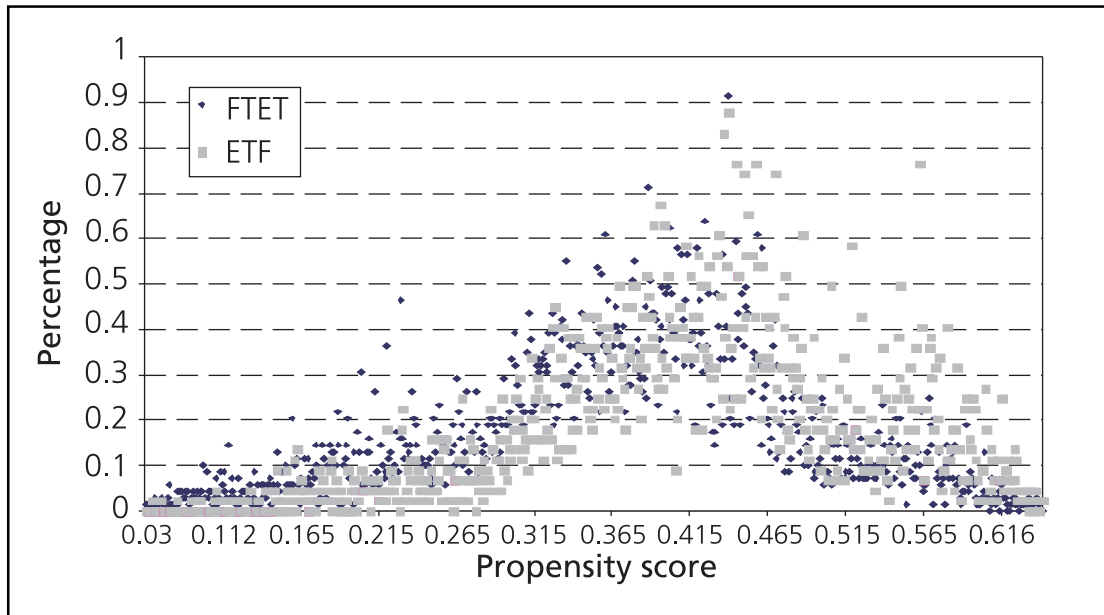
Figure D.10   VS and EO cohorts
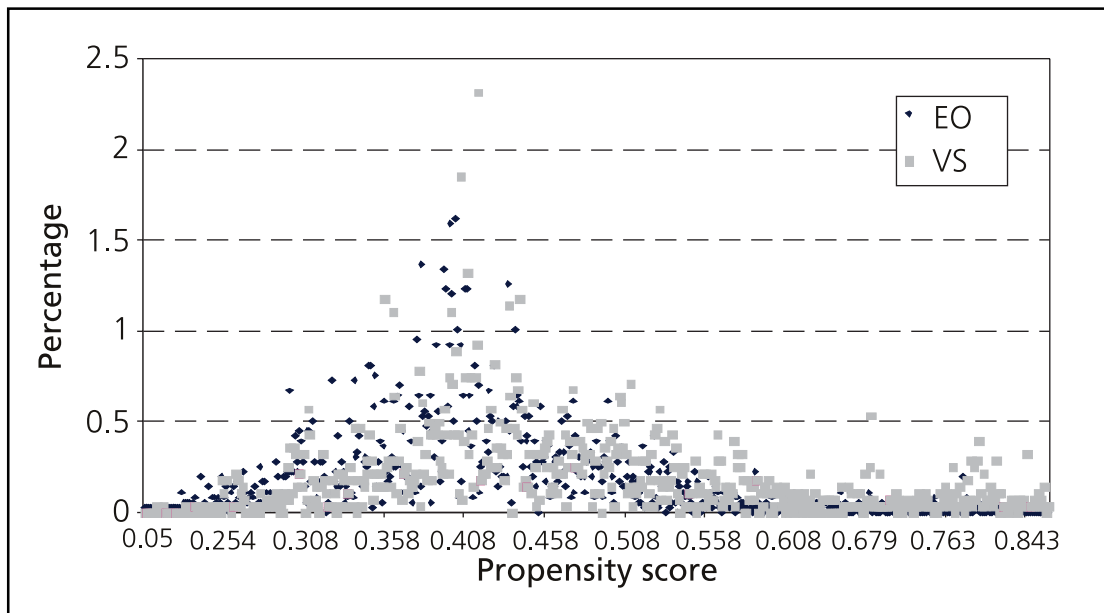
Figure D.11   VS and FTET cohorts
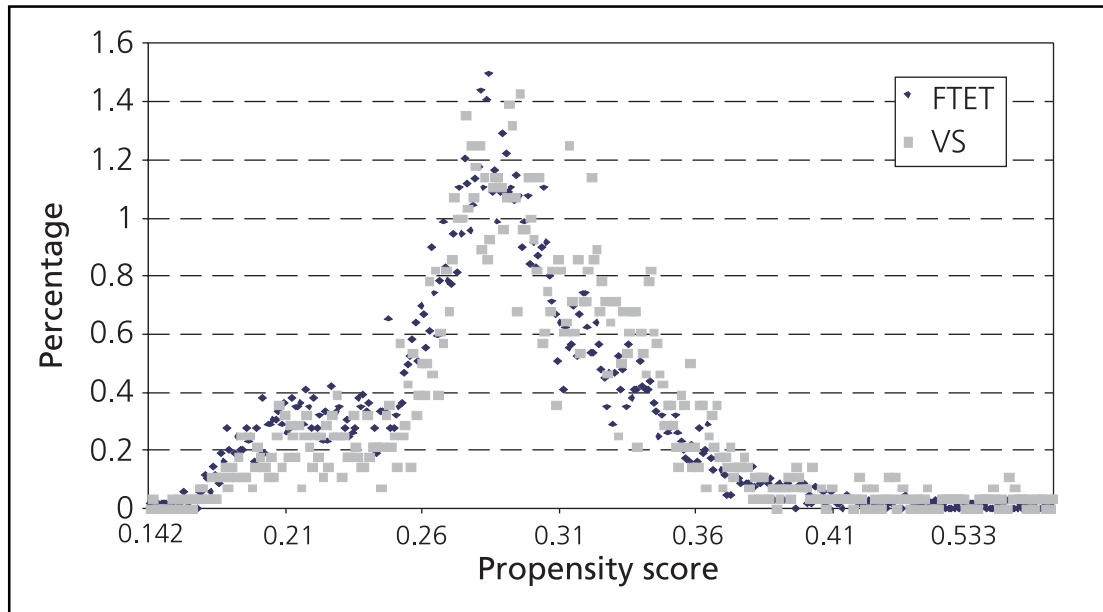
Figure D.12   EO and FTET cohorts
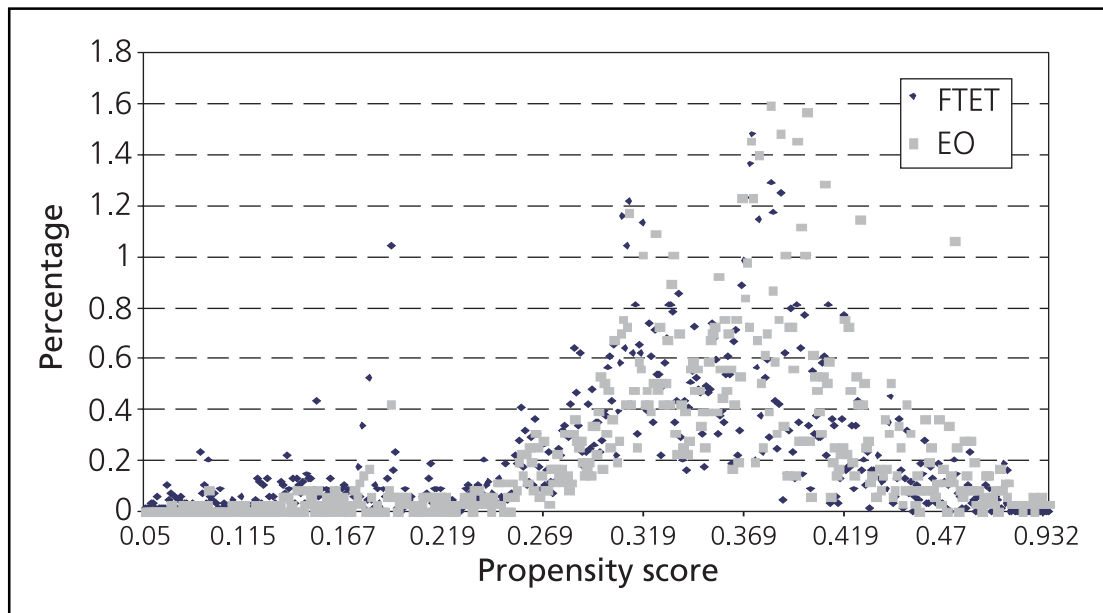
Appendix E
Estimated impact using employment regressed on estimated employment using time off benefit, data excluding year one for all EO comparisons

	Dependent variable: estimated net impact using time in employment		
Independent variables	**Coefficient**	**Standard error**	**t statistic**
Constant	-0.00	0.00	-0.17
Estimated net impact using time on benefits	-0.70	0.40	-17.34
$R^2 = 0.88$, N=42			

Appendix F
Difference in the proportion of time spent in employment

Table F.1   Treatment: ETF

Matched cohort

Year	VS %	ETF %	Difference %
1	40.2	41.2	-1.0
2	30.4	27.6	2.8
3	33.0	31.5	1.5
4	35.9	33.9	2.0
Overall	34.9	33.6	1.3

Matched cohort

Year	EO %	ETF %	Difference %
1	61.7	41.1	20.5
2	38.8	27.7	11.2
3	40.6	31.6	9.1
4	42.0	34.1	7.9
Overall	45.8	33.6	12.2

Matched cohort

Year	FTET %	ETF %	Difference %
1	38.5	41.2	-2.7
2	30.2	27.7	2.5
3	34.9	31.6	3.3
4	37.1	34.0	3.1
Overall	35.2	33.6	1.6

Whole cohort

Year	VS %	ETF %	Difference %
1	44.0	41.1	2.8
2	30.8	27.6	3.2
3	33.5	31.5	1.9
4	35.9	34.0	1.9
Overall	36.0	33.6	2.5

Whole cohort

Year	EO %	ETF %	Difference %
1	63.8	41.1	22.7
2	41.3	27.6	13.7
3	42.7	31.5	11.2
4	43.5	34.0	9.4
Overall	47.8	33.6	14.3

Whole cohort

Year	FTET %	ETF %	Difference %
1	40.6	41.1	-0.5
2	31.5	27.6	3.9
3	35.4	31.5	3.8
4	38.0	34.0	4.0
Overall	36.4	33.6	2.8

Table F.2   Treatment: VS

Matched cohort

Year	ETF %	VS %	Difference %
1	41.7	43.4	-1.7
2	28.6	30.6	-2.0
3	32.9	33.4	-0.4
4	34.8	35.9	-1.2
Overall	34.5	35.8	-1.3

Matched cohort

Year	EO %	VS %	Difference %
1	62.6	43.9	18.8
2	41.2	30.7	10.5
3	42.9	33.5	9.4
4	42.9	36.0	6.9
Overall	47.4	36.0	11.4

Matched cohort

Year	FTET %	VS %	Difference %
1	39.2	44.0	-4.8
2	31.6	30.7	0.8
3	34.9	33.5	1.4
4	37.2	35.9	1.3
Overall	35.7	36.0	-0.3

Whole cohort

Year	ETF %	VS %	Difference %
1	41.1	44.0	-2.8
2	27.6	30.8	-3.2
3	31.5	33.5	-1.9
4	34.0	35.9	-1.9
Overall	33.6	36.0	-2.5

Whole cohort

Year	EO %	VS %	Difference %
1	63.8	44.0	19.9
2	41.3	30.8	10.5
3	42.7	33.5	9.2
4	43.5	35.9	7.6
Overall	47.8	36.0	11.8

Whole cohort

Year	FTET %	VS %	Difference %
1	40.6	44.0	-3.4
2	31.5	30.8	0.7
3	35.4	33.5	1.9
4	38.0	35.9	2.1
Overall	36.4	36.0	0.3

Table F.3   Treatment: EO

Matched cohort

Year	ETF %	EO %	Difference %
1	42.4	63.7	-21.3
2	28.9	41.3	-12.4
3	32.6	42.9	-10.2
4	35.0	43.4	-8.4
Overall	34.7	47.8	-13.1

Year	VS %	EO %	Difference %
1	44.1	63.7	-19.6
2	30.7	41.4	-10.7
3	34.2	42.7	-8.5
4	37.4	43.5	-6.1
Overall	36.6	47.8	-11.2

Year	FTET %	EO %	Difference %
1	40.5	63.7	-23.2
2	32.0	41.4	-9.4
3	36.7	42.8	-6.1
4	39.0	43.5	-4.4
Overall	37.0	47.8	-10.8

Whole cohort

Year	ETF %	EO %	Difference %
1	41.1	63.8	-22.7
2	27.6	41.3	-13.7
3	31.5	42.7	-11.2
4	34.0	43.5	-9.4
Overall	33.6	47.8	-14.3

Year	VS %	EO %	Difference %
1	44.0	63.8	-19.9
2	30.8	41.3	-10.5
3	33.5	42.7	-9.2
4	35.9	43.5	-7.6
Overall	36.0	47.8	-11.8

Year	FTET %	EO %	Difference %
1	40.6	63.8	-23.2
2	31.5	41.3	-9.8
3	35.4	42.7	-7.4
4	38.0	43.5	-5.5
Overall	36.4	47.8	-11.5

Table F.4   Treatment: FTET

| | Matched cohort | | |
Year	ETF %	FTET %	Difference %
1	42.7	40.6	2.2
2	28.9	31.6	-2.7
3	32.3	35.5	-3.2
4	34.9	38.3	-3.4
Overall	34.7	36.5	-1.8

| | Whole cohort | | |
Year	ETF %	FTET %	Difference %
1	41.1	40.6	0.5
2	27.6	31.5	-3.9
3	31.5	35.4	-3.8
4	34.0	38.0	-4.0
Overall	33.6	36.4	-2.8

| | Matched cohort | | |
Year	VS %	FTET %	Difference %
1	43.9	40.5	3.5
2	30.7	31.4	-0.7
3	33.4	35.3	-1.8
4	36.0	37.9	-1.9
Overall	36.0	36.3	-0.2

| | Whole cohort | | |
Year	VS %	FTET %	Difference %
1	44.0	40.6	3.4
2	30.8	31.5	-0.7
3	33.5	35.4	-1.9
4	35.9	38.0	-2.1
Overall	36.0	36.4	-0.3

| | Matched cohort | | |
Year	EO %	FTET %	Difference %
1	63.2	40.4	22.8
2	40.9	31.6	9.3
3	42.3	35.4	6.9
4	42.7	38.1	4.7
Overall	47.3	36.4	10.9

| | Whole cohort | | |
Year	EO %	FTET %	Difference %
1	63.8	40.6	23.2
2	41.3	31.5	9.8
3	42.7	35.4	7.4
4	43.5	38.0	5.5
Overall	47.8	36.4	11.5

Appendix G
Difference in the proportion of time spent on EO

Table G.1   Treatment: ETF

	Matched cohort				Matched cohort				Matched cohort		
Year	VS %	ETF %	Difference %	Year	EO %	ETF %	Difference %	Year	FTET %	ETF %	Difference %
1	2.3	2.1	0.2	1	39.3	2.0	37.3	1	3.1	2.1	1.0
2	1.0	0.9	0.2	2	1.4	0.9	0.5	2	1.0	0.9	0.1
3	0.5	0.6	-0.2	3	0.6	0.6	-0.1	3	0.9	0.6	0.2
4	0.4	0.4	-0.1	4	0.4	0.4	-0.1	4	0.4	0.4	0.0
Overall	1.0	1.0	0.0	Overall	10.4	1.0	9.4	Overall	1.3	1.0	0.3

	Whole cohort				Whole cohort				Whole cohort		
Year	VS %	ETF %	Difference %	Year	EO %	ETF %	Difference %	Year	FTET %	ETF %	Difference %
1	3.6	2.1	1.5	1	39.8	2.1	37.7	1	6.1	2.1	4.0
2	0.9	0.9	0.0	2	1.3	0.9	0.4	2	0.9	0.9	0.0
3	0.4	0.6	-0.3	3	0.5	0.6	-0.1	3	0.6	0.6	0.0
4	0.3	0.4	-0.1	4	0.3	0.4	-0.1	4	0.3	0.4	-0.1
Overall	1.3	1.0	0.3	Overall	10.5	1.0	9.5	Overall	2.0	1.0	1.0

Table G.2 Treatment: VS

Matched cohort

Year	ETF %	VS %	Difference %
1	2.2	2.2	0.0
2	0.9	0.9	0.0
3	0.4	0.4	0.0
4	0.3	0.3	0.0
Overall	0.9	0.9	0.0

Year	EO %	VS %	Difference %
1	39.7	2.0	37.7
2	1.4	0.9	0.5
3	0.6	0.4	0.2
4	0.3	0.3	0.0
Overall	10.5	0.9	9.6

Year	FTET %	VS %	Difference %
1	2.8	2.1	0.8
2	0.9	0.9	0.0
3	0.5	0.4	0.2
4	0.3	0.3	0.0
Overall	1.1	0.9	0.2

Whole cohort

Year	ETF %	VS %	Difference %
1	3.7	2.3	1.3
2	0.9	0.9	0.0
3	0.6	0.4	0.3
4	0.4	0.3	0.1
Overall	1.4	1.0	0.4

Year	EO %	VS %	Difference %
1	39.8	2.1	37.7
2	1.3	0.9	0.4
3	0.5	0.4	0.2
4	0.3	0.3	0.0
Overall	10.5	0.9	9.6

Year	FTET %	VS %	Difference %
1	5.9	2.1	3.8
2	0.9	0.9	0.0
3	0.6	0.4	0.3
4	0.3	0.3	0.1
Overall	1.9	0.9	1.0

Table G.3   Treatment: EO

Matched cohort

Year	ETF %	EO %	Difference %
1	2.3	39.8	-37.5
2	0.9	1.3	-0.4
3	0.6	0.5	0.0
4	0.3	0.3	0.0
Overall	1.0	10.5	-9.5

Whole cohort

Year	ETF %	EO %	Difference %
1	2.9	39.8	-36.9
2	0.9	1.3	-0.4
3	0.6	0.5	0.1
4	0.4	0.3	0.1
Overall	1.2	10.5	-9.3

Matched cohort

Year	VS %	EO %	Difference %
1	2.4	39.8	-37.3
2	0.9	1.3	-0.4
3	0.4	0.6	-0.2
4	0.3	0.3	0.0
Overall	1.0	10.5	-9.5

Whole cohort

Year	VS %	EO %	Difference %
1	2.8	39.8	-36.9
2	0.9	1.3	-0.4
3	0.4	0.5	-0.2
4	0.3	0.3	0.0
Overall	1.1	10.5	-9.4

Matched cohort

Year	FTET %	EO %	Difference %
1	3.3	39.8	-36.4
2	1.0	1.3	-0.3
3	0.7	0.5	0.2
4	0.3	0.3	0.0
Overall	1.3	10.5	-9.1

Whole cohort

Year	FTET %	EO %	Difference %
1	5.8	39.8	-34.0
2	0.9	1.3	-0.4
3	0.6	0.5	0.1
4	0.3	0.3	0.0
Overall	1.9	10.5	-8.6

Table G.4   Treatment: FTET

Matched cohort

Year	ETF %	FTET %	Difference %
1	2.1	3.1	-1.0
2	0.8	0.9	-0.1
3	0.5	0.7	-0.1
4	0.4	0.4	0.0
Overall	1.0	1.2	-0.3

Matched cohort

Year	VS %	FTET %	Difference %
1	2.2	3.0	-0.8
2	0.9	0.9	0.0
3	0.3	0.6	-0.3
4	0.3	0.3	-0.1
Overall	0.9	1.2	-0.3

Matched cohort

Year	EO %	FTET %	Difference %
1	39.9	3.0	36.9
2	1.4	0.9	0.5
3	0.6	0.7	0.0
4	0.4	0.3	0.0
Overall	10.6	1.2	9.3

Whole cohort

Year	ETF %	FTET %	Difference %
1	2.3	3.1	-0.8
2	0.9	0.9	0.0
3	0.6	0.6	0.0
4	0.4	0.3	0.1
Overall	1.1	1.3	-0.2

Whole cohort

Year	VS %	FTET %	Difference %
1	2.2	3.0	-0.9
2	0.9	0.9	0.0
3	0.4	0.6	-0.3
4	0.3	0.3	-0.1
Overall	0.9	1.2	-0.3

Whole cohort

Year	EO %	FTET %	Difference %
1	39.8	3.1	36.7
2	1.3	0.9	0.4
3	0.5	0.6	-0.1
4	0.3	0.3	0.0
Overall	10.5	1.2	9.3

Appendix H
Difference in the proportion of time spent in employment excluding year one

Table H.1   Treatment: ETF

Matched cohort

Year %	VS %	ETF %	Difference %
1	-	-	-
2	30.4	27.6	2.8
3	33.0	31.5	1.5
4	35.9	33.9	2.0
Overall	33.1	31.0	2.1

Matched cohort

Year %	EO %	ETF %	Difference %
1	-	-	-
2	38.8	27.7	11.2
3	40.6	31.6	9.1
4	42.0	34.1	7.9
Overall	40.5	31.1	9.4

Matched cohort

Year %	FTET %	ETF %	Difference
1	-	-	-
2	30.2	27.7	2.5
3	34.9	31.6	3.3
4	37.1	34.0	3.1
Overall	34.1	31.1	3.0

Whole cohort

Year %	VS %	ETF %	Difference %
1	-	-	-
2	30.8	27.6	3.2
3	33.5	31.5	1.9
4	35.9	34.0	1.9
Overall	33.4	31.1	2.3

Whole cohort

Year %	EO %	ETF %	Difference %
1	-	-	-
2	41.3	27.6	13.7
3	42.7	31.5	11.2
4	43.5	34.0	9.4
Overall	42.5	31.1	11.4

Whole cohort

Year %	FTET %	ETF %	Difference
1	-	-	-
2	31.5	27.6	3.9
3	35.4	31.5	3.8
4	38.0	34.0	4.0
Overall	35.0	31.1	3.9

Table H.2   Treatment: VS

	Matched cohort		
Year	ETF %	VS %	Difference %
1	-	-	-
2	28.6	30.6	-2.0
3	32.9	33.4	-0.4
4	34.8	35.9	-1.2
Overall	32.1	33.3	-1.2

	Whole cohort		
Year	ETF %	VS %	Difference %
1	-	-	-
2	27.6	30.8	-3.2
3	31.5	33.5	-1.9
4	34.0	35.9	-1.9
Overall	31.1	33.4	-2.3

	Matched cohort		
Year	EO %	VS %	Difference %
1	-	-	-
2	41.2	30.7	10.5
3	42.9	33.5	9.4
4	42.9	36.0	6.9
Overall	42.4	33.4	8.9

	Whole cohort		
Year	EO %	VS %	Difference %
1	-	-	-
2	41.3	30.8	10.5
3	42.7	33.5	9.2
4	43.5	35.9	7.6
Overall	42.5	33.4	9.1

	Matched cohort		
Year	FTET %	VS %	Difference %
1	-	-	-
2	31.6	30.7	0.8
3	34.9	33.5	1.4
4	37.2	35.9	1.3
Overall	34.5	33.4	1.1

	Whole cohort		
Year	FTET %	VS %	Difference %
1	-	-	-
2	31.5	30.8	0.7
3	35.4	33.5	1.9
4	38.0	35.9	2.1
Overall	35.0	33.4	1.6

Table H.3   Treatment: EO

Matched cohort

Year	ETF %	EO %	Difference %
1	-	-	-
2	28.9	41.3	-12.4
3	32.6	42.9	-10.2
4	35.0	43.4	-8.4
Overall	32.2	42.5	-10.4

Matched cohort

Year	VS %	EO %	Difference %
1	-	-	-
2	30.7	41.4	-10.7
3	34.2	42.7	-8.5
4	37.4	43.5	-6.1
Overall	34.1	42.5	-8.4

Matched cohort

Year	FTET %	EO %	Difference %
1	-	-	-
2	32.0	41.4	-9.4
3	36.7	42.8	-6.1
4	39.0	43.5	-4.4
Overall	35.9	42.5	-6.6

Whole cohort

Year	ETF %	EO %	Difference %
1	-	-	-
2	27.6	41.3	-13.7
3	31.5	42.7	-11.2
4	34.0	43.5	-9.4
Overall	31.1	42.5	-11.4

Whole cohort

Year	VS %	EO %	Difference %
1	-	-	-
2	30.8	41.3	-10.5
3	33.5	42.7	-9.2
4	35.9	43.5	-7.6
Overall	33.4	42.5	-9.1

Whole cohort

Year	FTET %	EO %	Difference %
1	-	-	-
2	31.5	41.3	-9.8
3	35.4	42.7	-7.4
4	38.0	43.5	-5.5
Overall	35.0	42.5	-7.5

Table H.4   Treatment: FTET

Matched cohort

Year	ETF %	FTET %	Difference %		VS %	FTET %	Difference %		EO %	FTET %	Difference %
1	-	-	-		-	-	-		-	-	-
2	28.9	31.6	-2.7		30.7	31.4	-0.7		40.9	31.6	9.3
3	32.3	35.5	-3.2		33.4	35.3	-1.8		42.3	35.4	6.9
4	34.9	38.3	-3.4		36.0	37.9	-1.9		42.7	38.1	4.7
Overall	32.0	35.1	-3.1		33.4	34.9	-1.5		42.0	35.0	7.0

Whole cohort

Year	ETF %	FTET %	Difference %		VS %	FTET %	Difference %		EO %	FTET %	Difference %
1	-	-	-		-	-	-		-	-	-
2	27.6	31.5	-3.9		30.8	31.5	-0.7		41.3	31.5	9.8
3	31.5	35.4	-3.8		33.5	35.4	-1.9		42.7	35.4	7.4
4	34.0	38.0	-4.0		35.9	38.0	-2.1		43.5	38.0	5.5
Overall	31.1	35.0	-3.9		33.4	35.0	-1.6		42.5	35.0	7.5

Appendix I
Difference in the proportion of time spent claiming ALMBs (males only)

Table I.1   Treatment: ETF

Matched cohort

Year	VS %	ETF %	Difference %
1	37.8	36.6	1.2
2	35.0	36.9	-2.0
3	29.5	30.9	-1.4
4	26.0	27.8	-1.7
Overall	32.1	33.0	-1.0

Year	EO %	ETF %	Difference %
1	30.0	36.5	-6.4
2	21.9	37.0	-15.1
3	17.8	30.7	-13.0
4	15.4	27.7	-12.3
Overall	21.3	33.0	-11.7

Year %	FTET %	ETF	Difference
1	32.1	36.5	-4.4
2	32.8	37.0	-4.1
3	25.7	30.8	-5.1
4	22.3	27.7	-5.4
Overall	28.2	33.0	-4.8

Whole cohort

Year	VS %	ETF %	Difference %
1	37.0	36.5	0.5
2	34.1	37.0	-2.9
3	29.0	30.8	-1.8
4	25.8	27.7	-1.9
Overall	31.5	33.0	-1.5

Year %	EO %	ETF %	Difference %
1	29.9	36.5	-6.6
2	21.5	37.0	-15.5
3	17.8	30.8	-13.0
4	15.2	27.7	-12.5
Overall	21.1	33.0	-11.9

Year %	FTET %	ETF	Difference
1	32.1	36.5	-4.4
2	33.0	37.0	-4.0
3	25.9	30.8	-4.9
4	22.5	27.7	-5.1
Overall	28.4	33.0	-4.6

Table I.2 Treatment: VS

Matched cohort

Year	ETF %	VS %	Difference %
1	36.2	37.2	-1.0
2	36.6	34.2	2.3
3	31.3	28.9	2.4
4	27.7	25.9	1.8
Overall	32.9	31.5	1.4

Year	EO %	VS %	Difference %
1	30.5	37.0	-6.5
2	21.4	34.1	-12.7
3	17.7	29.0	-11.3
4	15.2	25.8	-10.5
Overall	21.2	31.5	-10.3

Year	FTET %	VS %	Difference %
1	32.8	37.0	-4.2
2	33.3	34.2	-0.8
3	25.9	29.1	-3.1
4	22.2	25.8	-3.6
Overall	28.6	31.5	-2.9

Whole cohort

Year	ETF %	VS %	Difference %
1	36.5	37.0	-0.5
2	37.0	34.1	2.9
3	30.8	29.0	1.8
4	27.7	25.8	1.9
Overall	33.0	31.5	1.5

Year	EO %	VS %	Difference %
1	29.9	37.0	-7.0
2	21.5	34.1	-12.6
3	17.8	29.0	-11.3
4	15.2	25.8	-10.6
Overall	21.1	31.5	-10.4

Year	FTET %	VS %	Difference %
1	32.1	37.0	-4.9
2	33.0	34.1	-1.2
3	25.9	29.0	-3.1
4	22.5	25.8	-3.3
Overall	28.4	31.5	-3.1

Table I.3   Treatment: EO

Matched cohort

Year	ETF %	EO %	Difference %
1	36.4	29.9	6.5
2	36.6	21.5	15.2
3	30.0	17.8	12.2
4	27.4	15.2	12.2
Overall	32.6	21.1	11.5

Matched cohort

Year	VS %	EO %	Difference %
1	37.0	30.0	7.1
2	33.2	21.7	11.5
3	28.2	17.8	10.4
4	25.3	15.2	10.1
Overall	31.0	21.2	9.8

Matched cohort

Year %	FTET %	EO %	Difference %
1	31.6	29.9	1.7
2	33.0	21.6	11.5
3	26.0	17.8	8.2
4	22.3	15.3	7.0
Overall	28.2	21.1	7.1

Whole cohort

Year	ETF %	EO %	Difference %
1	36.5	29.9	6.6
2	37.0	21.5	15.5
3	30.8	17.8	13.0
4	27.7	15.2	12.5
Overall	33.0	21.1	11.9

Whole cohort

Year %	VS %	EO %	Difference %
1	37.0	29.9	7.0
2	34.1	21.5	12.6
3	29.0	17.8	11.3
4	25.8	15.2	10.6
Overall	31.5	21.1	10.4

Whole cohort

Year %	FTET %	EO %	Difference %
1	32.1	29.9	2.2
2	33.0	21.5	11.4
3	25.9	17.8	8.2
4	22.5	15.2	7.3
Overall	28.4	21.1	7.3

Table I.4   Treatment: FTET

Matched cohort

Year	ETF %	FTET %	Difference %
1	36.8	32.1	4.7
2	36.3	33.1	3.3
3	30.5	25.9	4.6
4	27.3	22.4	4.8
Overall	32.7	28.4	4.3

Whole cohort

Year	ETF %	FTET %	Difference %
1	36.5	32.1	4.4
2	37.0	33.0	4.0
3	30.8	25.9	4.9
4	27.7	22.5	5.1
Overall	33.0	28.4	4.6

Matched cohort

Year	VS %	FTET %	Difference %
1	37.0	32.1	4.9
2	33.8	33.0	0.8
3	29.0	26.0	3.1
4	25.5	22.6	2.9
Overall	31.3	28.4	2.9

Whole cohort

Year	VS %	FTET %	Difference %
1	37.0	32.1	4.9
2	34.1	33.0	1.2
3	29.0	25.9	3.1
4	25.8	22.5	3.3
Overall	31.5	28.4	3.1

Matched cohort

Year %	EO %	FTET	Difference
1	30.3	32.1	-1.7
2	21.9	33.0	-11.1
3	18.2	26.0	-7.8
4	15.5	22.5	-7.0
Overall	21.5	28.4	-6.9

Whole cohort

Year %	EO %	FTET	Difference
1	29.9	32.1	-2.2
2	21.5	33.0	-11.4
3	17.8	25.9	-8.2
4	15.2	22.5	-7.3
Overall	21.1	28.4	-7.3

Appendix J
Difference in the proportion of time spent in employment (males only)

Table J.1   Treatment: ETF

Matched cohort

Year	VS %	ETF %	Difference %
1	41.2	41.1	0.1
2	29.8	27.3	2.5
3	32.8	31.3	1.5
4	36.2	33.7	2.5
Overall	35.0	33.4	1.6

Matched cohort

Year %	EO %	ETF %	Difference %
1	61.8	41.0	20.8
2	39.9	27.5	12.4
3	42.3	31.6	10.8
4	42.9	34.0	9.0
Overall	46.7	33.5	13.3

Matched cohort

Year %	FTET %	ETF %	Difference %
1	39.4	41.0	-1.7
2	31.2	27.3	3.8
3	35.3	31.3	4.0
4	38.0	33.8	4.2
Overall	36.0	33.4	2.6

Whole cohort

Year	VS %	ETF %	Difference %
1	41.9	40.9	0.9
2	30.2	27.3	2.9
3	32.9	31.3	1.6
4	36.5	33.8	2.8
Overall	35.4	33.3	2.0

Whole cohort

Year %	EO %	ETF %	Difference %
1	62.6	40.9	21.7
2	40.2	27.3	12.8
3	41.9	31.3	10.6
4	42.9	33.8	9.2
Overall	46.9	33.3	13.6

Whole cohort

Year %	FTET %	ETF %	Difference %
1	39.9	40.9	-1.1
2	31.2	27.3	3.9
3	35.3	31.3	4.0
4	38.0	33.8	4.2
Overall	36.1	33.3	2.8

Table J.2   Treatment: VS

Matched cohort

Year	ETF %	VS %	Difference %
1	41.8	41.7	0.1
2	27.8	30.2	-2.4
3	31.9	32.9	-1.0
4	33.8	36.6	-2.8
Overall	33.8	35.4	-1.5

Matched cohort

Year	EO %	VS %	Difference %
1	62.0	41.7	20.3
2	39.0	30.6	8.4
3	41.2	33.5	7.7
4	42.0	37.2	4.8
Overall	46.0	35.7	10.3

Matched cohort

Year %	FTET %	VS %	Difference
1	39.1	41.7	-2.6
2	30.1	30.1	0.1
3	34.4	32.8	1.6
4	35.9	36.3	-0.4
Overall	34.9	35.2	-0.3

Whole cohort

Year	ETF %	VS %	Difference %
1	40.9	41.9	-0.9
2	27.3	30.2	-2.9
3	31.3	32.9	-1.6
4	33.8	36.5	-2.8
Overall	33.3	35.4	-2.0

Whole cohort

Year	EO %	VS %	Difference %
1	62.6	41.9	20.8
2	40.2	30.2	10.0
3	41.9	32.9	9.0
4	42.9	36.5	6.4
Overall	46.9	35.4	11.5

Whole cohort

Year %	FTET %	VS %	Difference
1	39.9	41.9	-2.0
2	31.2	30.2	1.0
3	35.3	32.9	2.4
4	38.0	36.5	1.5
Overall	36.1	35.4	0.7

Table J.3   Treatment: EO

Matched cohort

Year	ETF %	EO %	Difference %
1	41.6	62.6	-21.0
2	27.8	40.2	-12.4
3	32.1	42.0	-9.9
4	34.4	43.0	-8.6
Overall	34.0	46.9	-13.0

Matched cohort

Year	VS %	EO %	Difference %
1	42.0	62.5	-20.5
2	30.8	40.2	-9.4
3	33.2	41.8	-8.6
4	37.3	42.9	-5.6
Overall	35.8	46.8	-11.0

Matched cohort

Year	FTET %	EO %	Difference %
1	39.8	62.5	-22.7
2	31.6	40.1	-8.6
3	36.4	41.9	-5.5
4	38.6	42.9	-4.3
Overall	36.6	46.8	-10.3

Whole cohort

Year	ETF %	EO %	Difference %
1	40.9	62.6	-21.7
2	27.3	40.2	-12.8
3	31.3	41.9	-10.6
4	33.8	42.9	-9.2
Overall	33.3	46.9	-13.6

Whole cohort

Year	VS %	EO %	Difference %
1	41.9	62.6	-20.8
2	30.2	40.2	-10.0
3	32.9	41.9	-9.0
4	36.5	42.9	-6.4
Overall	35.4	46.9	-11.5

Whole cohort

Year	FTET %	EO %	Difference %
1	39.9	62.6	-22.7
2	31.2	40.2	-9.0
3	35.3	41.9	-6.6
4	38.0	42.9	-4.9
Overall	36.1	46.9	-10.8

Table J.4   Treatment: FTET

Matched cohort

Year	ETF %	FTET %	Difference %
1	41.7	39.6	2.1
2	27.6	31.1	-3.5
3	31.8	35.2	-3.3
4	34.9	37.9	-3.1
Overall	34.0	35.9	-1.9

Matched cohort

Year	VS %	FTET %	Difference %
1	42.4	39.7	2.7
2	30.4	31.1	-0.7
3	33.2	35.2	-2.0
4	36.6	38.0	-1.4
Overall	35.6	36.0	-0.4

Matched cohort

Year	EO %	FTET %	Difference %
1	62.6	39.6	23.0
2	39.7	31.2	8.5
3	41.8	35.4	6.4
4	42.3	38.1	4.2
Overall	46.6	36.1	10.5

Whole cohort

Year	ETF %	FTET %	Difference %
1	40.9	39.9	1.1
2	27.3	31.2	-3.9
3	31.3	35.3	-4.0
4	33.8	38.0	-4.2
Overall	33.3	36.1	-2.8

Whole cohort

Year	VS %	FTET %	Difference %
1	41.9	39.9	2.0
2	30.2	31.2	-1.0
3	32.9	35.3	-2.4
4	36.5	38.0	-1.5
Overall	35.4	36.1	-0.7

Whole cohort

Year	EO %	FTET %	Difference %
1	62.6	39.9	22.7
2	40.2	31.2	9.0
3	41.9	35.3	6.6
4	42.9	38.0	4.9
Overall	46.9	36.1	10.8

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