

Health Impacts of New Deal for Young People

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Policy Studies Institute

UNIVERSITY OF WESTMINSTER



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Abbreviations and Acronyms

BMRB	British Market Research Bureau
CAPI	Computer Assisted Personal Interviewing
DDA	Disability Discrimination Act 1995
DH	Department of Health
EGW	Extended Gateway
EMP	Employment Option
ES	Employment Service
ETF	Environment Task Force
FTET	Full Time Education and Training (Education) Option
LASER	London and South East Region
LFS	Labour Force Survey
GHS	General Household Survey
HSE	Health Survey for England
JSA	Jobseekers Allowance
M	matching estimator
ND	New Deal
NDED	New Deal Evaluation Database
NDPA	New Deal Personal Adviser
NDYP	New Deal for Young People
PSI	Policy Studies Institute
S	smoothing estimator
SCELI	Social Change and Economic Life Initiative
SOC	Standard Occupational Classification
TTWA	Travel to Work Area
UoD	unit of delivery
VS	Voluntary Sector
W1	wave one
W2	wave two

Executive Summary

The government has made a general commitment to assessing the health impact of major new policy initiatives. This study, commissioned by the Department of Health, set out to examine whether the New Deal for Young People had any impact on the health of those who took part. We were interested in whether New Deal had a direct impact on participants' health, through the opportunities and activities that were open to them whilst on the programme, and also in whether it had more indirect impacts on health, through accelerating progress towards favourable labour market outcomes.

Unemployed people tend to have worse health than those who are employed. This raises the question of whether policies designed to stimulate employment might also have benefits for health. The New Deal programmes have formed the centrepiece of the current government's attempts to move more people from welfare into work. Through employment-based placements and education courses, the New Deal for Young People has also attempted to provide some of the developmental opportunities that young unemployed people tend to miss out on, such as opportunities to develop skills and to engage in regular purposeful activity which gains them respect from others.

This report is based on national surveys of 6010 entrants to New Deal for Young People (NDYP) that took place approximately six months and 18 months after participants had entered the programme. The surveys were originally commissioned by the Employment Service, as part of its overall evaluation of New Deal. The Department of Health commissioned the addition of extra health questions in order to facilitate this research on health impacts.

Since the New Deal is compulsory for 18 to 24-year-olds with six months of continuous unemployment, it was not possible to

compare the health outcomes of all programme participants with those of similar individuals who did not take part. Our analyses assess the relative effectiveness of each of the New Deal Options (Employment, Education, Voluntary Sector and Environment Task Force (ETF)) in relation to various health outcomes. The primary evaluation approach adopted is known as the method of matching. This technique makes use of the wide range of background information on respondents that was collected from the surveys, supplemented by information from the New Deal Evaluation Database (NDED), to construct a comparison group for the sample who entered each Option that consisted of people with very similar characteristics.

The data set provided a range of health measures including some which related to health and disability before entry to New Deal, some which related to current health at the wave one and wave two surveys, and some which spanned periods before and after entry to New Deal. Measures of pre-New Deal health were used to control for health selection into the various New Deal Options. Measures of current health at the two surveys provided the information on health outcomes. They included indicators of self-assessed general health, mental health, self-efficacy and experiential deprivation.

Matched comparisons of health outcomes for participants in the various Options showed that, 18 months after New Deal entry:

- participation in the New Deal Options appeared to have a beneficial impact on general health, compared with remaining on an extended version of Gateway;
- Education Option participants appeared to be doing relatively well in terms of mental health;
- the Employment Option had the most beneficial impacts on self-efficacy; and
- participants on the Employment and Education Options had the lowest levels of experiential deprivation, suggesting that these Options might be good sources of experiences such as taking responsibility, engaging in purposeful activity, meeting a range of people, and feeling respected by people.

Comparisons with models of health outcomes at the wave one interview, five to six months after New Deal entry, were used to

test the robustness of differences in health outcomes. These comparisons suggested that the models of Option entry might not have controlled fully for differences in mental health. However, most of the effects described above increased from the wave one to the wave two interviews, increasing our confidence that they were not merely selection effects.

Matching results do not show whether health impacts resulted from the activities undertaken on Options themselves, or whether they were indirect results of the Options' effectiveness in moving people towards more rewarding labour market outcomes. Smoothing regression techniques were used to examine the extent of indirect effects on health by including variables, such as labour market outcomes, determined after New Deal entry.

These models provided some evidence of indirect impacts of New Deal on general health, via labour market outcomes. Results for mental health were less clear.

In conclusion, the research provided some evidence that labour market programmes do have the potential for beneficial impacts on participants' health. Some of the health benefits of particular New Deal Options appeared to result from accelerated progression into employment, but it also seemed likely that benefits could accrue in more direct ways, through the activities that people were engaged in whilst on Options.

However, the research was limited by the lack of explicit information on general health, mental health and self-efficacy prior to programme entry. This meant that we could not be confident that the models controlled fully for initial differences in health between entrants to the various New Deal Options. More research, with better indicators of pre-programme health, is needed to establish the precise extent of health impacts generated by different types of labour market support.

Introduction

This report is concerned with the relationship between health and unemployment among young people, and with the ways in which labour market programmes might make an impact on this relationship.

Young people are often assumed to have few health problems, yet recent research has shown that both physical and psychological problems are prevalent among 16 to 24-year-olds. The Health Survey for England (HSE) found that around a quarter of English 16 to 24-year-olds suffered from some kind of long-standing illness, and a similar proportion were taking some kind of prescribed medicine (Prescott-Clarke and Primatesta, 1998). Research among Scottish young people found that more than a third had symptoms of minor psychological ill-health (West, 1997).

The health of unemployed young people tends to be worse than that of young people in education or employment (West and Sweeting, 1996; Montgomery and Schoon, 1997). In particular, unemployed young people are more likely to be anxious and depressed, more likely to have suicidal thoughts, and more likely to make suicide attempts (Platt and Kreitman, 1985; West and Sweeting, 1996).

The strong association between poor health and unemployment raises questions about the extent to which labour market programmes, aimed at increasing opportunities for employment, might also have the potential for beneficial impacts on health. There is increasing recognition from within the policy

community that interventions designed to have an impact on one area might also impact in unplanned ways on other policy areas. In the recent White Paper, *Saving Lives: Our Healthier Nation* (Department of Health, 1999), the government committed itself to assessing the health impact of major new policy initiatives.

The research reported here, commissioned by the Department of Health, was one of the first British health impact assessments of a labour market policy. It examines the health impact of the New Deal programme for 18 to 24-year olds, using data from a longitudinal survey of 6010 participants (see page 6 for more details of the survey).

THE NEW DEAL

The New Deal programmes for unemployed people have formed a key part of the government's welfare to work strategy. They aim to increase the employability of participants as well as to help them move more quickly into jobs. More broadly, the government's *Operational Vision* for New Deal for Young People (NDYP) states that the programme aims to tackle individual and social consequences of unemployment among young people, giving them 'a greater chance to take control of their lives, recognising that work is the foundation for independence and a sense of self-worth' (Employment Service, 1997: 2-3).

NDYP was the first of several New Deal programmes to be announced. It was rolled out nationally in April 1998 following a three-month trial in 12 Pathfinder areas. The programme was targeted at young people aged 18 to 24 years who had been claiming unemployment benefits for at least six months, or who had particular vulnerabilities which meant that they might benefit from early entry to its provision. Participation in NDYP is compulsory for the target group, who face benefit sanctions if they do not comply with the programme's requirements.

There are three main phases to NDYP, shown in Figure 1.1. After an initial interview with a New Deal Personal Adviser (NDPA), participants enter the first, Gateway, phase of the programme. Gateway is designed to last for up to four months and, during this phase, New Deal participants should receive intensive advice, help and counselling about job search, job

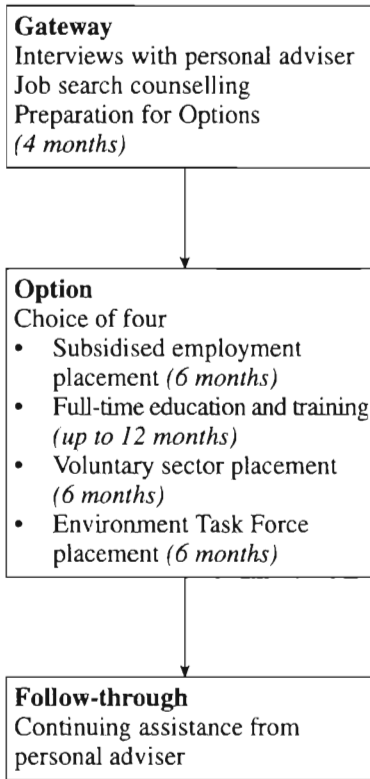


Figure 1.1: *The New Deal Programme*

matching services, and opportunities to discuss and prepare for the various Options available during the next stage of the programme.

The four Options available to NDYP participants are subsidised employment (or help to set up as self-employed), full-time education and training, work in the voluntary sector, or work with the Environment Task Force. Options generally last for six months, with the exception of full-time education and training, which may last for up to a year.

After completing their Options, New Deal participants progress to a follow-through phase where they are given further help to find suitable employment.

POTENTIAL HEALTH IMPACTS OF NEW DEAL FOR YOUNG PEOPLE

In order to think constructively about the ways in which a programme such as NDYP might impact on the health of young people, it is necessary first to look in more detail at what is known about the relationship between health and unemployment. In doing so, we will draw on the literature cited in the first report from this research project (Lakey, 2001).

The relationship between poor health and unemployment is a complex one, which embodies both direct and indirect effects operating in different directions. The literature on this subject tends to distinguish between *selection* effects (poor health making it harder to find or keep work) and *causation* or *exposure* effects (unemployment having a detrimental impact on people's health) (Bartley, 1994; Hammarstrom and Janlert, 1997).

Direct selection effects occur when people lose or quit their jobs because of poor health, or when poor health prevents them from finding a job. The relationship between health and job search is likely to vary between individuals. While we might expect health problems to reduce the efficiency of job search in some cases, people with certain types of health problems, such as psychological problems, may find it particularly difficult to be without a job, and may, as a result, be more flexible in their job search (Mastekaasa, 1996). Indirect selection effects may also operate, when factors such as childhood disadvantage put people at increased risk of both unemployment and poor health (Montgomery et al, 1996).

The strength of both direct and indirect selection effects is likely to be related to the labour market context. Research suggests that people with health problems experience particular difficulties in finding employment during periods of economic recession (Bartley and Owen, 1996).

Exposure to unemployment may also have an adverse impact on the health of young people, sapping their confidence and increasing the risk of depression (Banks and Ullah, 1988; Theodossiou, 1998). Such causation effects are frequently explained with reference to theories of deprivation. Poor health among the unemployed is thought to result from a combination of

material deprivation, or poverty, and deprivation of experiences and opportunities (Jahoda, 1982; Warr, 1987). For example, unemployed people may miss out on opportunities to practise their skills, may become more isolated from the wider society, and may be deprived of valued social roles. Negative attitudes towards unemployed people may create a 'shaming environment' which affects the mental health and self-esteem of those who are unemployed (Eales, 1989; Rantakeisu et al, 1997). The frustration of agency, the individual's ability to achieve goals and ambitions, is also expected to take a toll on the mental health of unemployed people (Fryer, 1997).

Selection and causation effects combined may produce a downward spiral of worsening health and long-term unemployment. Individuals who become unemployed as a result of health problems risk having those health problems exacerbated or compounded by the material and other deprivations associated with unemployment. The resulting health problems, in their turn, make it increasingly difficult for them to compete successfully in the job market (Acheson, 1998; Fryer, 1997). Recent research from the Netherlands suggests that such downward spirals may be particularly disadvantageous when they occur during youth (van de Mheen et al, 1998).

Where unemployment has a detrimental effect on health, or employment has a beneficial effect, policies which move people from welfare into work might indirectly benefit health. However, not all employment will necessarily have benefits for health. Research suggests that employment in poor conditions can be detrimental to health (Burchell, 1994; Winefield et al, 1991) and that underemployment may, like unemployment, be detrimental to the self-esteem of young people (Prause and Dooley, 1997). Health benefits may be contingent upon the quality of the jobs found after participation in programmes such as New Deal.

In order to measure the overall health impact of any labour market programme we would need to consider whether it was actually creating new jobs for unemployed people, or simply altering the distribution of unemployment among individuals. Achieving a positive impact on health would depend on improving the employment prospects of the group which was most adversely affected by unemployment. Labour market programmes which helped the more job-ready access jobs while

leaving a residue of those who were more difficult to help might have a limited impact on health as a result.

We should also be aware that some labour market interventions might have adverse effects on health (Fryer, 1999). For example, research suggests that young people who search intensively for a job, only to be confronted with repeated failure to obtain work, tend to have poorer mental health than those who search less intensively (Banks and Ullah, 1988; Leana and Feldman, 1992). Depression and lowered self-efficacy as a result of failed job search may lead to a decrease in motivation and effectiveness in finding a job, and a reduction in the capacity to persist with job search activity in the face of setbacks or adversity (Creed, 1998).

As well as having the potential to improve health prospects by moving people into employment, labour market programmes might also impact directly on the health of their participants, by providing a substitute source of the benefits and opportunities more generally associated with employment. The thinking behind NDYP (Employment Service, 1997) suggests a number of ways in which the programme might provide opportunities that could lead to improvements in psycho-social health. They include:

- provision of regular activity for young people (utilising their talents and energy) to prevent them drifting into passivity or delinquency and to influence their work attitudes in a positive way;
- provision of opportunities to undertake work which is seen as useful by the young person and the wider community, in order to bolster feelings of self-worth; and
- provision of opportunities to acquire and practise new skills, to increase the social capital and resources of participants, and thus improve their chances of finding work.

SURVEYS OF NEW DEAL PARTICIPANTS

The Employment Service commissioned an extensive programme of evaluation to establish whether NDYP benefits participants and to establish the programme's impact on the wider labour market (Hall and Reid, 1998). As a part of this programme, the Policy

Studies Institute and BMRB (British Market Research Bureau) Social Research were commissioned by the Employment Service to carry out a large-scale survey of NDYP participants, in two stages. The Department of Health research, on the health impacts of New Deal, was commissioned when plans for the evaluation of the programme were already far advanced. The design of the health impact assessment must therefore be seen as pragmatic rather than ideal. Its most important limitation is the lack of any measures of individuals' general or mental health prior to New Deal entry. The compulsory nature of the New Deal programme meant that there was no comparison group of non-participants available to allow estimation of the overall impact of the programme on health. This study focuses instead on the associations between different types of New Deal experience and changing health.

A random sample of 11,197 young people was selected from the September to November 1998 cohort of New Deal entrants. The two-stage survey was carried out face-to-face in respondents' homes, using Computer Assisted Personal Interviewing (CAPI). At the first stage, 6010 interviews were carried out between February and July 1999, comprising 54 per cent of all sampled cases, and 67 per cent of those where a correct address could be found. For the second stage, 3391 interviews were conducted between February and June 2000. This means that 56 per cent of those interviewed at stage one were re-interviewed at stage two. A number of respondents had moved address between the two interviews. If we exclude stage one respondents for whom no correct address was available at stage two, 66 per cent were re-interviewed. Weighting was carried out to account for the effects of reduced sample sizes as a result of survey non-response and sample attrition. Details of the weighting process are provided in Appendix 2. Tests showed that there was no serious attrition with respect to health outcomes between the two waves of the survey.

Young people were defined as New Deal entrants if it was recorded on Employment Service (ES) administrative systems that they had been invited to take part in New Deal. However, around 7 per cent of respondents had no recall of participation in New Deal. For the most part, absence of recall appeared to result from a very brief participation involving little contact with the programme (Bryson et al, 2000).

Stage one of the survey was designed to capture participants' early experiences of the programme. Six out of ten New Deal entrants interviewed at wave one were still on the programme (Bonjour et al, 2001). Stage two, comprising repeat interviews with the same participants, was designed to obtain information on their experiences and attitudes after finishing the programme. Seven out of ten were no longer on New Deal when interviewed for the wave two survey.

Both waves of the survey included questions on long-term disabilities and health problems and their effect on work, and a question asking whether an individual's own health had affected his or her ability to find or keep work over the past year. They also included a series of questions on attitudes to work that were designed to measure psycho-social concepts such as self-efficacy. In addition, the second survey included a series of questions on the work histories of respondents, which identified whether they had spent time 'not seeking work due to sickness or ill health' during any of the five years before New Deal entry.

The Department of Health commissioned the Policy Studies Institute and the BMRB to add a number of other health questions to the two stages of the survey. These questions measured general health, mental health and a number of health-related factors such as deprivation of the experiential opportunities described in the previous section, experience of negative attitudes towards the unemployed, and support received from family and others.

Data from the New Deal surveys were matched with information from administrative records on the survey sample held on the Employment Service's New Deal Evaluation Database (NDED). This allowed identification of people who were recorded by the Employment Service as disabled, at the point of entry to New Deal. More detailed information on the various measures of health is provided in Chapter 2.

THE EVALUATION APPROACH

There are a number of possible ways of evaluating programme effects and the choice of best approach is determined in large part by practicalities, including the nature of the programme and the quality of data available for analysis. Both of these were important

in selecting the approach used for the evaluation of the health impacts of New Deal. Since no comparison group of non-participants was available, the survey did not aim to estimate the *overall* effect of the New Deal on health. Rather, the aim was to consider the *relative* effects of the New Deal Options. These effects are estimated in respect of a range of health outcomes. The evaluation approach adopted is known as the method of matching. The methods of analysis used are described fully in Appendix 1.

CONTENTS OF THE REPORT

Chapter 2 of this report describes the various measures of general, mental and long-term health of NDYP participants that were available from the survey and administrative data. It examines associations between these health variables and other individual characteristics. Chapters 3 and 4 present the results of matching analyses which were undertaken to estimate the impact of various New Deal Options upon health. Chapter 5 concludes the report with a discussion of what may be learned from the results presented.

The Health of New Deal Participants

This chapter provides a descriptive overview of the range of information on health and disability that is available from the surveys of NDYP entrants and matched administrative data. These sources together provide information on self-perceived general health, long-term health problems and disabilities and their effect on work, mental health, and psycho-social aspects of health such as self-efficacy, experiential deprivation and unemployment shame.

Survey estimates of the health of New Deal entrants are compared with results from other surveys of young people in the general population, and associations between different health measures are examined. The chapter also provides some descriptive information about variation in the main health outcomes by gender, ethnicity and educational attainment.

Key findings of this chapter are as follows:

- More than eight out of ten NDYP entrants described their general health as good, very good or excellent.
- Fewer than one in 20 described their general health as poor.
- The average mental health score of NDYP entrants was slightly lower than that of adults in the general population.
- Slightly fewer than one in five respondents reported having a long-term health problem or disability.
- Male NDYP entrants reported somewhat better mental health than female ones.
- On the other hand, males were more likely to have experienced drug or alcohol problems, more likely to have

suffered as a result of missing out on the range of experiences generally provided by employment, and more likely to have experienced shaming attitudes to their unemployment.

- Ethnic minority NDYP entrants had better general health than white ones, and were less likely to have a work-limiting long-term illness or disability.
- Ethnic minority respondents were less likely than whites to have experienced shaming attitudes to their unemployment.
- NDYP entrants with no qualifications had poorer health than did those with qualifications, whatever the health measure used.

SELF-ASSESSED GENERAL HEALTH

The New Deal surveys each contained a single question asking respondents to assess their own current general health as excellent, very good, good, fair or poor. Such questions on self-perceived health status are routinely included in many national health surveys, including the British General Household Survey (GHS) and the Health Survey for England (HSE), although there are differences in the response scales used. Like the New Deal surveys, the HSE uses a five-point scale, but the categories (very good, good, fair, bad and very bad) are different. The GHS uses a three-point scale. Despite the subjective nature of the question, this type of measure has been found to be a useful indicator of health status which shows the expected associations with other measures of health status, and which is also an independent predictor of future health problems (Jee and Or, 1999). However, while questions on perceived health have been found to discriminate well within culturally homogeneous populations, they may not be effective in distinguishing health differences between populations which do not share common standards and perceptions of good health (Mathers, 1996). This might have implications for the comparison of self-perceived general health status between young people from different ethnic groups.

Distributions on self-reported general health questions have been found to be generally skewed in the direction of good health and they have been criticised for insensitivity in discriminating between the broadly 'good' levels of health enjoyed by the

Table 2.1: *Current general health, in New Deal surveys (1999) and the HSE (1995–97)*

<i>Current general health</i>	<i>NDYP</i>		<i>HSE 1995–97 (a)</i>	<i>Percentages</i>	
	<i>Wave 1</i>	<i>Wave 2</i>		<i>Men</i>	<i>Women</i>
Excellent	29	26	Very good	41	36
Very good	33	32	Good	46	49
Good	24	24	Fair	12	14
Fair	10	13	Bad	1	2
Poor	4	4	Very bad	0	0
<i>Weighted base</i>	<i>6003</i>	<i>3390</i>		<i>2862</i>	<i>3331</i>
<i>Unweighted base</i>	<i>6002</i>	<i>3388</i>		<i>2364</i>	<i>2761</i>

(a) Source: Prescott-Clarke and Primatesta, 1998

majority of the population (Sturgis et al, 2001). Table 2.1 shows that more than eight out of ten New Deal survey respondents described their own current general health as good, very good or excellent, and around 86 per cent of HSE respondents described their health as either good or very good (Prescott-Clarke and Primatesta, 1998).

MENTAL HEALTH

Although studies of mental health and labour market status have traditionally tended to use the GHQ12 indicator of mental health (Lakey, 2001), the New Deal surveys used the mental health indicator from the Medical Outcomes Study 36-Item Short-Form Health Survey (SF-36) (Ware and Sherbourne, 1992). The SF-36 is increasingly being included in national surveys of population health and provides a useful means of comparison between the health statuses of different populations. It was included in the 1996 HSE, allowing us to make some comparisons between the mental health of New Deal entrants and that of 16 to 24-year-olds in the general population of England. Whereas the GHQ12 was designed as an instrument for detecting cases of psychiatric disorder, the eight dimensions of the SF-36, of which mental

health is one, were designed to produce profiles of general health. A recent review of key health measures recommended the mental health dimension of the SF-36 as being a valid and sensitive measure of mental health (Sturgis et al, 2001).

Each health dimension covered by the SF-36 is measured by a multiple item scale. The use of multiple items makes this indicator more sensitive than the self-reported indicator of general health described above and less subject to floor and ceiling effects. For each dimension, item scores are generally coded, summed and transformed onto a scale from 0 (worst) to 100 (best).

The mental health scale has five items. Respondents are asked to say for how much of the time during the past four weeks they have:

- been a very nervous person;
- felt so down in the dumps that nothing could cheer them up;
- felt calm and peaceful;
- felt down-hearted and low; and
- been a very happy person.

Response categories are all of the time, most of the time, a good bit of the time, some of the time, a little of the time, and none of the time. Factor and reliability analyses of the New Deal survey data confirmed that the five items could be effectively combined to give a scale of mental health, with items three and five reversed.¹

For each item, a score of one indicated that the respondent had experienced that particular type of mental health problem (anxiety, depression, sadness) all of the time during the past four weeks. A score of six indicated that they had not experienced that problem at all during the past four weeks. Individual scores on each item were added together and divided by five to produce a generalised scale of mental health, which was then transformed on to a scale of 0 to 100. Mean mental health scores were used

¹ Principal component analysis identified this as a single factor with an Eigenvalue of 3.14. The Kaiser-Meyer-Olkin measure of sampling adequacy was 0.83 and the Bartlett's test was significant. Reliability analysis (alpha=0.85) confirmed that the group 'mental health' provides reliable estimates of all the five items.

Table 2.2: *Current mental health (SF-36), in New Deal surveys and the HSE (1996)*

	<i>NDYP Wave 1</i>	<i>NDYP Wave 2</i>	<i>HSE 1996 (a)</i>
Mean mental health score (scale of 0 to 100)	74.7	74.3	75.4
<i>Weighted base</i>	5952	3373	—
<i>Unweighted base</i>	5942	3369	—

(a) Source: Sturgis et al, 2001

in our analyses of health outcomes. Table 2.2 shows that the mean level of mental health among New Deal entrants (74.7 at wave one, and 74.3 at wave two) was slightly lower than that of adults in the general population (75.4), as reported in the 1996 HSE. Analysis has shown that levels of mental health as measured by the SF-36 do not vary significantly by age (Sturgis et al, 2001).

LONG-STANDING ILLNESS AND DISABILITY

Indicators of long-standing illness and disability are included in many national and international surveys, including the GHS, the HSE, the Labour Force Survey (LFS) and the Census. Like the general health measure described above, indicators of long-term illness rely on self-reports and their main appeal is their simplicity and transparency. Many surveys also include questions on whether the long-term illness limits the activities or work that the respondent is able to undertake.

The precise wording of the long-term illness question tends to vary from one source to another. Respondents to the GHS and the HSE are asked whether they have any long-standing illness, disability or infirmity which has troubled them for some time, or which is likely to affect them over a period of time. In contrast, the New Deal surveys, like the LFS, asked respondents more specifically whether they had any health problems or disabilities that they expected to last for more than a year.

Some surveys, such as the GHS, ask respondents about the effects of their long-term health problem or disability on normal daily activities. Others, such as the Labour Force Survey, ask about limitations on the kind or amount of work that the respondent is able to do.

New Deal survey respondents were asked whether they had any long-standing health problems or disabilities, and, if so, whether these affected the kind or amount of paid work that they could do. Eighteen per cent of respondents said that they did have long-term health problems or disabilities (Table 2.3). This was lower than the 23 per cent of male and 27 per cent of female 16 to 24-year-olds identified by the HSE as having a long-standing illness, disability or infirmity (Prescott-Clarke and Primatesta, 1998), a difference which probably relates to the way that the questions were asked in the two surveys. Research suggests that responses to limiting long-term illness questions are sensitive to quite small differences in question wording (Sturgis et al, 2001).

Among New Deal entrants who did refer to long-term health problems or disabilities, 30 per cent said that these had no effect on their work capacity, and 22 per cent said that they affected the kind of work that they could do but not the amount (Table 2.3). Forty-six per cent said that their problems affected both the kind and the amount of work that they could do, and 2 per cent said that they affected the amount of work only. Overall, 12.5 per cent of New Deal entrants reported a work-limiting disability or health problem.

The data on New Deal survey respondents also included an indicator of whether they had been assessed as disabled by the Employment Service before entry to New Deal. This is one of only two indicators of health status prior to New Deal entry and is therefore of particular importance for our analysis of health outcomes. The indicator is derived from administrative procedures in which prospective Jobseekers Allowance (JSA) claimants are asked whether they have any health problems that affect the work they can do. Those who mention long-term work-limiting health problems are then asked a series of questions to ascertain whether they would also be defined as disabled by the Disability Discrimination Act 1995 (DDA). The DDA definition of disability is based on a number of questions about limitations to 'normal day-to-day activities' as a result of illness or impairment.

Table 2.3: Long-term health problems and disability, reported at the wave one and wave two New Deal surveys

	<i>Percentages</i>	
	<i>Wave 1</i>	<i>Wave 2</i>
Long-term health problem/disability		
No	82	80
Yes	18	20
Work-limiting health problem/disability		
No	87.5	85
Yes	12.5	15
<i>Weighted base</i>	<i>5945</i>	<i>3350</i>
<i>Unweighted base</i>	<i>5993</i>	<i>3351</i>
Whether long-term health problems/ disabilities limited work capacity		
No limit on work capacity	30	23
Limited kind of work	22	26
Limited amount and kind of work	46	49
Limited amount of work	2	2
<i>Weighted base</i>	<i>1077</i>	<i>672</i>
<i>Unweighted base</i>	<i>1048</i>	<i>662</i>

Since 1997, the LFS has also included further questions on limiting long-term illness and disability to reflect the provisions of the Disability Discrimination Act 1995 (DDA). Those with health problems or disabilities are asked whether these ‘substantially limit [their] ability to carry out normal day-to-day activities’. If this is the case, or if they have one of a number of progressive illnesses, they are defined as having a ‘current disability covered by the DDA’ (Sly et al, 1999).

Thirteen per cent of New Deal survey respondents were defined as having a work-limiting disability covered by the DDA, according to the ES administrative data, similar to the 12.5 per cent of respondents who had a work-limiting disability according to the survey questions.

NOT SEEKING WORK DUE TO SICKNESS, 1993 TO 1997

The other health variable relating to a period prior to New Deal entry was derived from a series of questions on the respondent's work history, included in the wave two survey. For each year between leaving full-time education and 1997 (the year prior to New Deal entry), respondents were asked whether they had spent any time out of employment. If they had spent any time out of employment they were asked whether, among other reasons, this was because they were 'not seeking work due to sickness or ill health'. Combining the five years for which this question was asked provided some limited information on health problems prior to New Deal that were sufficiently serious to prevent respondents from looking for work. Only 2.5 per cent of wave two respondents reported any such periods of sickness during the five years prior to New Deal. Despite the limited nature of this variable (it provides no information about health problems that occurred during periods of employment or full-time education), it was included in the models of New Deal participation because it provided some information on health prior to New Deal, additional to that provided by the ES disability indicator.

HEALTH PROBLEMS AFFECTING ABILITY TO FIND OR KEEP A JOB

Respondents to each wave of the New Deal survey were asked to say whether any of a number of different issues had adversely affected their ability to find or keep a job during the past year. These issues included their own ill health, and problems with drugs or alcohol.

Seventeen per cent of respondents to the first wave survey said that their own ill-health or disability had made it difficult to find or keep a job during the past year, higher than the 13 per cent recorded as disabled by the ES, and also higher than the proportion who reported in the surveys that they had a long-term work-limiting disability or health problem. Three per cent said that problems with drugs or alcohol had made it difficult for them to find or keep a job. Similar proportions also reported experiencing these problems at the second wave survey.

SELF-EFFICACY

Self-efficacy is a key concept in the psychology of motivation and may be seen as one indicator of the psycho-social health of an individual. It is distinguished from the more general concept of self-esteem by its concern with particular behaviours and outcomes. Where self-esteem tells us how young people felt about themselves overall, self-efficacy variables are related to specific goals, telling us, for example, how young people felt about their ability to get a job and their ability to execute successfully the actions necessary to obtain a job. Theories of self-efficacy suggest that people are more likely to attempt to achieve particular goals where they are confident of success, and that experience of success in achieving goals reinforces confidence and encourages further attempts at achievement (Bandura, 1997). Some labour market programmes in the United States have specifically attempted to boost the self-efficacy of unemployed people by helping them develop strategies to cope with setbacks and enhance their persistence in job search (van Ryn and Vinokur, 1992). The New Deal does not set out to improve self-efficacy in such a targeted way, but some of the services that it provides, such as support with job search and confidence building, would seem to be compatible with this aim.

The New Deal surveys included a five-item scale of 'job search self-efficacy', measuring the extent to which respondents felt confident of their abilities to look for work, and to make a positive contribution as employees. Respondents were asked to say how far they agreed with the following statements (the figures in brackets show the percentage of wave two respondents who gave a positive response to each):

- I know the best ways to apply for the kind of work I want (74%).
- I know how to write a good application letter (72%).
- I do well at job interviews when I get them (67%).
- I have lots of experience relevant to work (63%).
- I have many work-related skills that make me a good employee (76%).

Responses to each item were scored from one to five, with higher scores indicative of greater confidence in capacity to carry out that aspect of job search. Responses to all the items were positively correlated, indicative of an underlying construct, and this was also confirmed by factor analysis with a wider set of attitudinal variables, where the five self-efficacy items emerged as a distinct factor.² Among those who responded to both surveys, mean self-efficacy scores on a scale of one to five were 3.81 at wave one and 3.95 at wave two.

EXPERIENTIAL DEPRIVATION

Employment is believed to have the potential to fulfil a number of functions which are beneficial for mental health, through offering a range of opportunities that are not open to those who are unemployed (Jahoda, 1982; Warr, 1987). These include opportunities for exercising control, for skill use, and for interpersonal contact, and for experiencing a degree of certainty about the future, externally generated goals, variety, and valued social position (Warr, 1987).

Research using data from the British Social Change and Economic Life Initiative (SCELI) (Gershuny, 1994) concluded that these types of opportunity were strongly associated with paid employment, for adults aged 20 to 60 years, and that access to them correlated significantly with levels of psychological adjustment. The same analysis also found that the effect of access or lack of access to these opportunities was more important for unemployed than employed people.

Our research replicated the categories used in the SCELI study. Respondents at the wave two interview were asked to say how far they agreed with the following five statements, relating to their experience in the last week. There were five possible responses, ranging from strongly agree through to strongly

² Principal component analysis identified this as a single factor with an Eigenvalue of 2.59. The Kaiser-Meyer-Olkin measure of sampling adequacy was 0.78 and the Bartlett's test was significant. Reliability analysis ($\alpha=0.77$) confirmed that the group 'work-related self-efficacy' provides reliable estimates of all the five items.

disagree. Figures in brackets show the percentage who strongly or slightly agreed (or disagreed) with each statement:

- I had time on my hands that I did not know what to do with (agree – 42%).
- Most days I met quite a range of people (disagree – 26%).
- I was doing things that were useful for other people (disagree – 20%).
- I had certain responsibilities at particular times most days of the week (disagree – 18%).
- I felt respected by the people I met (disagree – 11%).

Item one was reversed and a reliability analysis conducted to see whether the five items could be combined into one scale. Responses to all items were positively correlated, indicative of an underlying construct.³ The items were summed and divided by five to give a scale of experiential deprivation from one to five, with higher scores indicating more deprivation. The mean score on this scale, for all wave two respondents who provided answers to these questions, was 2.34.

UNEMPLOYMENT SHAME

Research suggests that unemployment may negatively affect the mental health of those who experience it, as a result of the negative moral judgements which the wider society is felt to make about those who are unemployed (Eales, 1989; Rantakeisu et al, 1997). Some unemployed people may internalise these judgements to an extent which makes them question their own worth, leading to feelings of shame and, potentially, to reduced mental health.

Rantakeisu and colleagues (1997) operationalised the concept of shame by measuring the extent to which young unemployed people had experienced six types of negative attitudes from others. Respondents were asked if they felt that other people had been annoyed with them, spoken about them in disparaging terms, avoided them, regarded them as less competent, regarded them as

³ Cronbach's alpha=0.74

Table 2.4: *Feelings of shame due to unemployment, wave two
New Deal survey respondents*

	<i>Percentages</i>				
	<i>Many times</i>	<i>Couple of times</i>	<i>Once in a while</i>	<i>Never</i>	<i>Mean score</i>
People were:					
Annoyed	23	24	16	37	2.7
Spoke badly	11	17	23	14	3.2
Regarded as lazy	20	23	16	13	2.8
Took no notice	9	14	13	64	3.3
Unemployment shame (a)	–	–	–	–	2.01

(a) On a scale from 1 (low) to 4 (high)

lazy, or taken no notice of them, because they were unemployed. Our research with New Deal entrants partially replicated their work, using four out of the six statements (annoyed with you, spoken about you badly, regarded you as lazy, took no notice of you). Respondents were asked how frequently they found that people reacted to them in each of these ways during their most recent experience of unemployment. The four response categories were many times, a couple of times, once in a while and never. Responses to the four items were found to be highly correlated with each other, indicating the reliability of an underlying construct,⁴ which we have termed unemployment shame. Responses to the four items were summed and divided by four to give a scale of unemployment shame from one to four. The scale was then reversed so that higher values indicated stronger feelings of shame when unemployed. Responses are shown in Table 2.4.

ASSOCIATIONS BETWEEN HEALTH MEASURES

Measures of general health are known to be highly correlated with measures of mental health (Prescott-Clark and Primatesta, 1998),

⁴ Cronbach's alpha=0.84

suggesting that those in poor general health tend to experience a mixture of physical and psychological problems. Our survey results also showed a significant correlation between general health and mental health among New Deal entrants (Table 2.5). Respondents who assessed their own general health as poor at the wave two survey scored an average of only 55.5 on the SF-36 mental health scale, whereas those who reported that their general health was excellent scored an average of 81.2 on this scale of mental health.

Table 2.5 also shows a strong association between self-assessed general health and self-reports of limiting long-term health problems. Among those who assessed their own general health as poor, 70 per cent said that they had a long-term health problem or disability that limited the kind or amount of work that they could do, while among those who assessed their own general health as excellent, only 3 per cent reported having a work-limiting disability or health problem.

Table 2.5 shows that the survey indicator of work-limiting disability was much more closely related to general health than the administrative indicator was. Only around a quarter of those who said they were in poor health were marked as having a long-term work-limiting disability or health problem on the ES's administrative database, compared with the 70 per cent who reported a long-term work-limiting disability or health problem at the survey. At the other end of the scale, 10 per cent of those who said they were in excellent health were marked as work-disabled on the ES's administrative database compared with just 3 per cent who reported a work-limiting health problem in the survey. The difference may reflect the fact that the disability indicator related to a period before New Deal, whereas the survey information on long-term illness was collected at the same time as self-reports of general health. It may also relate to differences in the definition of work-limiting health problems and disability and to differences in the contexts in which these measures were used.

The disability indicator is a particularly important variable because, as a measure of pre-New Deal health, it is included in the models of propensity to enter the various New Deal Options. However, the relatively low level of association between the disability indicator and general health does raise some questions about its likely effectiveness as a control for the health of respondents on entry to New Deal.

Table 2.5: Associations between general health and other health indicators

<i>General health:</i>	<i>Cell percentages and mean scores</i>				
	<i>Excellent</i>	<i>Very good</i>	<i>Good</i>	<i>Fair</i>	<i>Poor</i>
Percentage with work-limiting long-term illness or disability	3	5	16	43	70
Percentage with disability indicator (NDED)	10	12	18	29	26
Percentage reporting some time not looking for work due to sickness, 1993 to 1997	1	1	3	4	15
Percentage with problems affecting ability to find or keep a job in past year:					
Own ill health	5	7	19	45	74
Drug or alcohol problems	1	1	3	6	3
Mean mental health score (a)	81.2	77.3	72.3	63.8	55.5
Mean self-efficacy score (b)	4.23	4.01	3.72	3.62	3.52
Mean experiential deprivation score (c)	2.12	2.22	2.46	2.68	2.76
Mean unemployment shame score (d)	1.86	1.95	2.07	2.25	2.23
<i>Weighted base</i>	<i>878</i>	<i>1089</i>	<i>826</i>	<i>446</i>	<i>151</i>
<i>Unweighted base</i>	<i>879</i>	<i>1079</i>	<i>836</i>	<i>445</i>	<i>148</i>

(a) Score from 0 to 100, with higher scores representing better mental health.

(b) Score from 1 to 5 with higher scores representing higher self-efficacy.

(c) Score from 1 to 5 with higher scores representing more experiential deprivation.

(d) Score from 1 to 4 with higher scores representing more unemployment shame.

Table 2.6: *Associations between mental health and other health indicators*

<i>Mental health score:</i>	<i>Cell percentages and mean scores</i>			
	<i>80 or more</i>	<i>65<80</i>	<i>50<65</i>	<i><50</i>
Percentage with work-limiting long-term illness or disability	8	14	25	36
Percentage with disability indicator (NDED)	13	18	21	20
Percentage reporting some time not looking for work due to sickness, 1993 to 1997	1	2	4	7
Percentage with problems affecting ability to find or keep a job in past year:				
Own ill health	10	15	27	40
Drug or alcohol problems	1	1	5	5
Mean self-efficacy (a)	4.13	3.85	3.66	3.54
Mean experiential deprivation (b)	2.06	2.42	2.65	2.97
Mean unemployment shame (c)	1.77	2.06	2.26	2.58
<i>Weighted base</i>	<i>1703</i>	<i>721</i>	<i>580</i>	<i>368</i>
<i>Unweighted base</i>	<i>1606</i>	<i>763</i>	<i>600</i>	<i>399</i>

(a) Score from 1 to 5 with higher scores representing higher self-efficacy.

(b) Score from 1 to 5 with higher scores representing more experiential deprivation.

(c) Score from 1 to 4 with higher scores representing more unemployment shame.

The other variable included in the propensity models, the indicator of having spent some time unemployed but not looking for work due to sickness between 1993 and 1997, was strongly associated with current general health, but Table 2.5 suggests that it accounted for only a small proportion of people with health problems. Only 15 per cent of people who reported poor health at

the second wave survey were picked up by this variable, compared with 26 per cent who were picked up by the disability indicator and 70 per cent who reported having long-term work-limiting health problems in the survey. Again, this casts doubt on the likely effectiveness of the variable in controlling fully for health selection into New Deal Options.

Among the other health variables, all were associated with general health. The better an individual's self-assessed general health, the more likely they were to have high self-efficacy, and the less likely they were to suffer from experiential deprivation or feelings of shame associated with unemployment.

Table 2.6 shows the association of categorical mental health with the other health variables available from the data set on New Deal entrants. A comparison of the results in Tables 2.5 and 2.6 shows that mental health was, like general health, strongly associated with most of the other health variables. It was less strongly associated with work-limiting long-term health problems than general health was, but was more strongly associated with the psycho-social variables of experiential deprivation and unemployment shame.

HEALTH AND INDIVIDUAL CHARACTERISTICS

Health is known to be associated with a wide range of demographic variables and levels of health also vary significantly by region (Sturgis et al, 2001). Since our models of the propensity to enter the different New Deal routes control for differences in a broad range of individual and local area characteristics, we assume that this improves the extent to which they control for variations in health. It is not possible for this report to show all the ways in which health varied by the individual characteristics of respondents. However, Table 2.7 provides an idea of the extent of differences between groups by showing how the health measures varied by three key characteristics: sex, ethnicity and whether the respondent had any qualifications.

The table shows that there was no significant difference in the general or long-term health of male and female New Deal entrants, but that females tended to have lower scores on the mental health variable. This reflects a general tendency for young

Table 2.7: Measures of health by sex, ethnicity and whether respondent had any qualifications

	Male	Female	White	Minority	Some qualifications	No qualifications
General health score (a)	3.63	3.60	3.59**	3.78	3.67***	3.44
Mental health score (a)	74.9**	73.1	74.4	74.0	75.5***	70.2
Percentage with work-limiting long-term illness or disability	15	16	16***	9	14***	20
Percentage with disability indicator (NDED)	16	16	17***	9	15**	18
Percentage reporting some time not looking for work due to sickness, 1993 to 1997	2.4	2.7	2.7	1.1	2.2	3.3
Percentage with problems affecting ability to find or keep a job in past year:						
Own ill health	18	16	19***	10	16***	22
Drug or alcohol problems	2.8***	0.8	2.5**	0.4	1.7***	3.7
Self-efficacy (b)	3.92	3.94	3.92	3.95	4.05***	3.50
Experiential deprivation (c)	2.39***	2.20	2.35**	2.25	2.25***	2.62
Unemployment shame (d)	2.07***	1.87	2.02**	1.93	1.98***	2.10
<i>Weighted base</i>	2386	1004	2916	469	2607	783
<i>Unweighted base</i>	2382	1008	2714	666	2510	880

(a) Score from 1 (poor) to 5 (excellent).

(b) Score from 0 to 100, with higher scores representing better mental health.

(c) Score from 1 to 5 with higher scores representing higher self-efficacy.

(d) Score from 1 to 5 with higher scores representing more experiential deprivation.

(e) Score from 1 to 4 with higher scores representing more unemployment shame.

Note: Comparisons are significant at the following levels using Pearson chi-squared test for percentage differences and t-tests for comparisons of mean scores: *** 1%, ** 5%.

women to report more mental health problems than young men (see, for example, Prescott-Clarke and Primatesta, 1998). Young women were significantly less likely than young men to have experienced drug or alcohol problems that affected their ability to find or keep a job. Levels of self-efficacy were similar for men and women, but men tended to experience higher levels of both experiential deprivation and unemployment shame than women did.

New Deal entrants from ethnic minority groups had significantly better levels of self-assessed general health compared with white New Deal entrants, and they were less likely to report work-limiting health problems. They were also significantly less likely to say that problems with drugs or alcohol had affected their ability to find or keep a job over the past year. New Deal entrants from ethnic minorities reported lower levels of experiential deprivation and unemployment shame than white New Deal entrants did. However, the mental health and self-efficacy scores of white and ethnic minority New Deal entrants were similar.

The substantial differences in levels of general health and limiting long-term illness between white and ethnic minority New Deal entrants could suggest that longer-term unemployed young people from ethnic minorities tend to be in better health than their white counterparts. Research suggests that it is more difficult for ethnic minority young people to get jobs even when they are not suffering additional disadvantages such as poor health (Modood et al, 1997). To the extent that this is the case, healthy young people from ethnic minorities would be more likely than their counterparts in the white population to be unemployed. On the other hand, differences in self-reported health status between white and ethnic groups could also reflect cultural differences in health expectations and, therefore, ways of responding to these questions (see page 11).

A quarter of New Deal entrants had no academic or vocational qualifications. Table 2.7 shows that those without qualifications had significantly lower levels of health on all the measures used.

Health Impacts of New Deal Options

As we saw in Chapter 1 (see Figure 1.1), all NDYP entrants experience similar types of help during the first, Gateway, phase of the programme. Experiences diverge at the second stage, when participants choose one of four Options: a subsidised employment placement, full-time education and training, a voluntary sector placement or an Environmental Task Force placement. The next two chapters examine the question of the impacts of various New Deal Options on the health of New Deal participants, using the matching approach described in detail in Appendix 1. We were interested in finding out whether the experience of participating in the New Deal Options themselves was beneficial for health, and whether any of the New Deal Options had indirect effects on health as a result of their impact on the labour market position of respondents.

The health outcomes considered in this chapter are general health, mental health, self-efficacy, and experiential deprivation. All of these provided measures of current health at the wave two interview and there were also wave one measures available for all of these variables except experiential deprivation. The other health indicators described in Chapter 2 were excluded from the analysis of health impacts because they related to problems that might have occurred before New Deal entry. Indicators of work-limiting illness or disability tended to refer to problems experienced during the past year. The indicator of unemployment shame related to the last period of unemployment that the respondent had experienced, which, for some, was before entry to New Deal.

Key findings of this chapter are as follows:

- Participation in the New Deal Options did appear to have benefits for general health, compared with remaining on Gateway for an extended period.
- The full-time education and training Option performed relatively well in terms of the mental health of its participants.
- The Employment Option appeared to be the most beneficial for the self-efficacy of its participants (their confidence in their own ability to find and keep a job), with the Environment Task Force Option also appearing to be beneficial in this respect.
- Participants on the Employment and full-time education and training Options tended to show the lowest levels of experiential deprivation.

Table 3.1 shows differences in the various measures of health at the wave two survey between participants in the six New Deal routes (Employment Option, Education Option, Voluntary Sector Option, Environment Task Force Option, Extended Gateway and normal Gateway),⁵ prior to the application of the matching procedure.

The table shows that New Deal entrants who remained on Gateway for five months or more without entering an Option (Extended Gateway participants) had the lowest mean general health score at wave two: their score was 3.45 on a scale from one (poor health) to five (excellent health). The mean general health score for Extended Gateway participants was significantly lower than that of participants on the Employment and Education Options.

Voluntary Sector Option participants had the lowest mean score for mental health. Their mean mental health score at wave two was 67.2 on a scale of 0 to 100. This was significantly lower than the mean mental health scores of participants on the Employment, Education and Environment Task Force Options (which ranged from 71.8 to 73.9), and was also significantly lower than that of individuals who left New Deal during the first four months of Gateway. After Voluntary Sector Option participants,

⁵ See page 2 for more detailed definitions of these New Deal routes.

Extended Gateway participants had the next lowest score for mean mental health. Their mental health score of 70.4 was significantly lower than that of Employment Option participants, who scored an average of 73.9 on this scale.

Employment Option participants had the highest mean score for self-efficacy and their average level of self-efficacy was significantly higher than that of participants on all the other New Deal routes. There were no significant differences in the levels of experiential deprivation reported by participants on the different New Deal routes.

It is important to stress that the mean differences shown in Table 3.1 do not provide indications of the impact of New Deal, because they do not control for initial health differences between participants in the six New Deal routes. A low health score for a particular route could mean that it was less effective in improving health, but it could also indicate that the route was felt to be particularly appropriate for people with poor health, so that more such individuals were directed towards it. For example, Voluntary Sector Option participants had the lowest mean mental health score. Voluntary sector placements may have been seen as helpful for people with mental health problems because they provided opportunities to work in a less pressurised environment with more support available. Since we know nothing about the mental health of participants before they entered New Deal, we are unable to say from these tables how far the wave two scores of voluntary sector participants represented an improvement on their previous mental health.

In order to estimate the impact of New Deal Options on health, we used a matched comparison methodology, which controlled for initial differences in the characteristics of participants when they entered the programme. As we explain in Appendix 1, explicit prior measures of health were limited to the disability indicator and information on whether the respondent had any periods of not seeking work due to ill health between 1993 and 1997. Although we expect that the inclusion in the participation models of a broad range of other variables known to be associated with health will effectively control for additional health variation, it is not possible to be totally confident that matching results are unaffected by health selection factors. Therefore, results from the following analyses of health impacts need to be interpreted with caution.

Table 3.1: *Measures of health, self-efficacy and experiential deprivation at wave two, by New Deal Option*

	<i>Mean scores</i>					
	<i>Employment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>	<i>Gateway</i>
General health (a)	3.73	3.70	3.62	3.64	3.45	3.59
Mental health (b)	73.9	71.8	67.2	72.0	70.4	72.0
Self-efficacy (c)	4.20	3.88	3.90	3.83	3.89	3.94
Experiential deprivation (d)	2.18	2.33	2.45	2.54	2.36	2.27
<i>Weighted base</i>	275	847	348	354	437	1113
<i>Unweighted base</i>	263	852	366	377	473	1037

(a) Scale from 1 (poor) to 5 (excellent). The following differences are significant (significance levels in brackets): between employment and extended gateway (2%), and education and extended gateway (1%).

(b) Scale from 0 to 100 with higher scores indicating better health. The following differences are significant: between Employment and VS (1%), Employment and Extended Gateway (4%), Education and VS (1%), VS and ETF (1%), VS and Gateway (1%).

(c) Scale from 1 to 5 with higher scores indicative of higher job search self-efficacy. The following differences are significant: between Employment and each of the other routes (1%), between ETF and Extended Gateway (2%).

(d) Scale from 1 to 5 with higher scores indicating more deprivation. None of the differences were significant at the 5% level or above.

MATCHING RESULTS FOR WAVE TWO HEALTH OUTCOMES

The first stage in the matching process was to model the probability of participating in one Option rather than another (see Appendix 1 for more detailed information on the methods used). New Deal entrants who left the programme in the first four months of Gateway were excluded from the matching analysis, because they were not felt to have received a treatment

comparable to that of Option participants. Those who remained on Gateway for five months or longer without entering an Option (known as the Extended Gateway participants) were retained in the analysis along with participants in the four Options.

We estimated models for the probability of participating in the four Options and Extended Gateway. From the models, propensity scores were calculated as the estimated probability of participating in one Option rather than another. These propensity scores were used to perform matched comparisons between similar individuals participating on different Options. For each New Deal route we constructed four comparison groups, one for each of the other routes. This allowed us to conduct pair-wise comparisons of all routes, resulting in a total of 20 comparisons.

General health

Table 3.2 shows the matching estimates of the impact of New Deal Options on general health, taking the general health score at the wave two survey as the outcome measure. In this table, each figure represents the difference in mean general health scores (on a scale of one to five) between the participants who went along the route shown in the row and their matched counterparts in the route shown in the column heading. For example, the number 0.326, shown in the fifth column of row one, means that those in subsidised employment had mean general health scores that were 0.326 points higher than those of their matched counterparts on the Extended Gateway, on a scale of one to five. A number with a minus sign means that those on the route denoted by that row had lower mean general health scores than their counterparts in the column route. Significant differences are shown in bold.

The other bold figures in rows two and three of column five tell us that participants on the Education Option and the Voluntary Sector Option also had higher mean general health scores than their respective matched counterparts on Extended Gateway. The general health score for Education Option participants was 0.413 points higher than that of matched Extended Gateway participants, and that of Voluntary Sector Option participants was 0.305 points higher than that of their matched Extended Gateway participants.

Table 3.2: General health score at the wave two survey (results from matching analysis)

Differences in mean general health scores for each row route, relative to comparators in the routes shown in the column headings.

	Employ- ment	Education	VS	ETF	Extended Gateway
Employment		0.083	-0.064	-0.064	0.326**
Education	-0.109		0.106	0.132	0.413***
VS	0.214	-0.008		0.396*	0.305**
ETF	-0.100	-0.110	0.163		0.181
Extended Gateway	-0.219	-0.320***	-0.387***	-0.305*	

Note: General health was measured on a scale from 1 (poor) to 5 (excellent). Bold differences are statistically significant at the following significance levels: *** 1%, ** 5%, and * 10%.

Matched comparisons between routes are not symmetric because the comparator group in each case is determined by the composition of participants on the route in question, with the closest match being taken from the comparator group for each individual case. For this reason, figures in the last row of Table 3.2, showing the general health outcomes of Extended Gateway participants had they been on the other Options, are *not* an exact mirror image of figures in the last column of the same table.

The last row of Table 3.2 shows that people on Extended Gateway would have done better in terms of their general health had they been on the Education, Voluntary Sector or Environment Task Force Options. The comparison with the Employment Option points in the same direction but is not significant. Overall, the models provide quite consistent evidence that Extended Gateway does less well than the New Deal Options with respect to general health impacts.

There is one more significant difference. The bold figure of 0.396 in the third row of column four shows that participants on the Voluntary Sector Option had mean general health scores that were nearly 0.4 points higher than they could have expected if they had been on the Environment Task Force Option instead.

Differences in mean general health estimated using the propensity score matching technique were larger than the

respective raw differences shown in Table 3.1. The biggest change was in the comparison between the Voluntary Sector and Environment Task Force Options where the raw general health scores were 3.62 and 3.64 respectively, giving a difference of just 0.02 compared to the 'matched' difference of 0.396.

Mental health

Table 3.3 shows the results for the matched analysis of mental health scores at wave two, on a scale of 0 to 100. There were four significant differences. Young people on the Employment Option had a mean mental health score about 3.5 points higher than that of their counterparts who had participated in the Voluntary Sector Option (this result was only significant at the 10 per cent level). Participants on the Education Option had a mental health score which was 4.2 points higher than that of their counterparts on Extended Gateway. Participants on the Voluntary Sector Option and those who remained on the Extended Gateway both had mean mental health scores that were 3 to 4 points lower than those of their respective matched counterparts on the Education Option. With regard to mental health, then, the Education Option appeared to perform relatively well. While the raw mental health scores in Table 3.1 showed lower mean mental health for participants in the Voluntary Sector Option and Extended Gateway, compared to other routes, the positive impact of the Education Option only became clear once we had controlled for selection into the various Options.

Self-efficacy

Table 3.4 shows the matching estimates of the impact of New Deal Options on the self-efficacy of respondents,⁶ with the mean self-efficacy score at wave two as the outcome measure. The table shows that participants in all Options except the Voluntary Sector Option had higher self-efficacy scores than their counterparts on

⁶ The discussion of self-efficacy outcomes draws on pp 49–50 of the Employment Service Research and Development Report, *New Deal for Young People: National Survey of Participants: Stage 2* (Bonjour et al, 2001).

Table 3.3: Mental health score at the wave two survey (results from matching analysis)

Differences in mean mental health scores for each row route, relative to comparators in the routes shown in the column headings.

	<i>Employment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>
Employment		1.984	3.488*	1.952	2.208
Education	1.440		1.792	3.040	4.240**
VS	-0.096	-3.728**		-0.576	-2.272
ETF	-1.632	-1.936	1.008		-1.264
Extended Gateway	0.576	-3.088**	2.960	-0.864	

Note: Mental health is measured on a scale from 0 (worst) to 100 (best). Bold differences are statistically significant at the following significance levels:

*** 1%, ** 5%, and * 10%.

Extended Gateway, but this difference was significant at the 5 per cent level or higher only in the case of the Employment Option. Those who had remained on the Extended Gateway could have expected to have higher self-efficacy scores had they been on any Option, significantly so in the case of the Employment and ETF Options. Among participants in the four Options, those who had undertaken subsidised employment placements had the highest self-efficacy scores, with significant differences between Employment and all routes except the ETF Option. The overall picture was one of subsidised employment having the most beneficial impacts on self-efficacy.

Experiential deprivation

Table 3.5 gives the matching results for experiential deprivation at the wave two survey. The experiential deprivation variable was measured on a scale of one to five and higher scores indicated more deprivation. The results show that Employment Option participants had mean scores for experiential deprivation that were approximately 0.25 points lower than those of their matched counterparts on the Education Option and 0.3 points lower than those of their counterparts on the ETF Option. Participants on the

Table 3.4: *Self-efficacy score at the wave two survey (results from matching analysis)*

Differences in mean self-efficacy scores for each row route, relative to comparators in the routes shown in the column headings.

	<i>Employ- ment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>
Employment		0.164**	0.212**	0.163	0.237**
Education	-0.335***		0.100	-0.003	0.148
VS	-0.167	0.027		-0.051	-0.011
ETF	-0.351***	-0.063	0.226**		0.037
Extended Gateway	-0.368***	-0.066	-0.133	-0.227***	

Note: Self-efficacy is measured on a scale from 1 (worst) to 5 (best).

Bold differences are statistically significant at the following significance levels:

*** 1%, ** 5%, and * 10%.

Voluntary Sector Option, ETF Option and Extended Gateway all experienced significantly more deprivation than their respective counterparts on the Employment Option, and ETF Option participants experienced more deprivation than their counterparts on the Education Option, with differences in a similar range. The results suggest that Employment and Education Options provided the best source of the experiences measured by this indicator, such as taking responsibility, engaging in purposeful activity, meeting a range of people and feeling respected by people in general. It is not apparent from these results whether these Options were achieving this by the types of opportunities that they themselves offered participants or by their effectiveness in moving participants towards more rewarding types of labour market activity.

COMPARISON WITH RESULTS FOR WAVE ONE HEALTH OUTCOMES

As we said earlier, results from the matching models of health outcomes should be treated with some caution because of the limited information on health prior to New Deal entry. One way of

Table 3.5: *Experiential deprivation score at the wave two survey (results from matching analysis)*

Differences in mean experiential deprivation scores for each row route, relative to comparators in the routes shown in the column headings.

	<i>Employment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>
Employment		-0.251**	-0.065	-0.308**	-0.122
Education	0.163		-0.021	-0.143	-0.055
VS	0.292**	0.091		-0.079	0.020
ETF	0.274**	0.202*	0.073		0.083
Extended Gateway	0.225*	0.124	0.007	0.116	

Note: Experiential deprivation was measured on a scale of 1 (least deprivation) to 5 (most deprivation). Bold differences are statistically significant at the following significance levels: *** 1%, ** 5%, and * 10%.

testing the stability of the outcomes we have measured is through comparison of the wave two results with matching results for health outcomes at wave one. The wave one interviews were carried out approximately six months after entry to New Deal, when most Option participants were still on the programme. Thus, the wave one matching results provide a possible indication of early programme effects. Like the wave two results, they could also show the effects of selection into different New Deal routes according to health. Where the matched results show similar impacts of Options at wave one and wave two, or impacts that diminish from wave one to wave two, we cannot discount the possibility that health selection was partly responsible. However, if the strength of the Option impact appears to increase between wave one and wave two we may be more confident that the Option itself was, either directly or indirectly, having an impact on health.

Table 3.6 shows a comparison of the matching results for general health at the wave one and wave two interviews. The last column of this table shows that the positive effects of participating in the Employment Option, the Education Option and the Voluntary Sector Option all increased from wave one to wave two, compared with remaining on the Extended Gateway. The last two rows of the table show that the negative effect of remaining on Gateway also increased from wave one to wave two, compared

with participation in any of the Options. The negative effects of remaining on Gateway were significant at wave two when Extended Gateway participants were compared with their matched counterparts on the Education, Voluntary Sector and ETF Options. These results provide additional evidence that participation in the New Deal Options seemed to have a positive effect on general health, and that this effect was not primarily due to a tendency for people with poorer general health to remain on Extended Gateway.

The positive effect of participation in the Voluntary Sector Option, compared with ETF Option participation, also became apparent only at wave two.

Table 3.6 also shows that, at wave one, Employment Option participants had better general health than their matched counterparts on the Education Option and, conversely, Education Option participants had worse general health than their counterparts on the Employment Option. Both of these effects were reduced and became non-significant by wave two. They may have reflected some initial selection of people with better general health into the Employment rather than the Education Option. Another possibility is that they indicated some early advantage for general health, gained from participation in the Employment rather than the Education Option, that was not sustained in the longer term.

Table 3.7 compares the mental health differences between participants on each New Deal route and their matched counterparts on other routes at the wave one and wave two interviews. The results contrast with those of the previous table, which showed that the positive effects of Option participation on general health increased at wave two. The last column of Table 3.7 shows that participation in each of the Options had a positive effect on mental health, compared with remaining on Gateway, at wave one, although the comparison was not statistically significant for voluntary sector participants. Conversely the penultimate row of the table shows that those who remained on Extended Gateway tended to have lower mental health scores than their counterparts on Options, at wave one, although the result was only statistically significant in the comparison with Education Option participants. By wave two, the positive effect of participation in each Option, on mental health, was reduced

Table 3.6: Comparison of general health differences at the wave one and wave two surveys (results from matching analysis)

Differences in mean general health scores for each row route, relative to comparators in the routes shown in the column headings.

		Employ- ment	Education	VS	ETF	Extended Gateway
Employment	W1	–	0.19*	0.42	0	0.21*
	W2	–	0.08	–0.06	–0.06	0.33**
Education	W1	–0.23*	–	0.14	0	0.19
	W2	–0.11	–	0.11	0.13	0.41***
VS	W1	0.05	0.05	–	0.04	0.10
	W2	0.21	–0.01	–	0.40*	0.31**
ETF	W1	0.02	0.06	–0.07	–	0.17
	W2	–0.10	–0.11	0.16	–	0.18
Extended Gateway	W1	–0.08	–0.11	–0.06	–0.11	–
	W2	–0.22	–0.32***	–0.39***	–0.31*	–

Note: General health was measured on a scale of 1 (poor) to 5 (excellent).

Bold differences are statistically significant at the following significance levels:

*** 1%, ** 5%, and * 10%.

compared with remaining on Gateway. Only Education Option participants continued to have significantly better levels of mental health than Extended Gateway participants at wave two. Extended Gateway participants also continued to have significantly lower levels of mental health than their counterparts on the Education Option.

The table also shows that participants on the Education Option tended to have better mental health than their matched counterparts on other routes. By wave two, participants on the Voluntary Sector Option had significantly lower levels of mental health than their matched counterparts on the Education Option.

These results suggest that there may have been some uncontrolled selection of people with better mental health onto New Deal Options, while those whose mental health was poorer were more likely to remain on Gateway. Another plausible explanation is that participation in the New Deal Options had a positive effect on mental health initially, when people were

Table 3.7: *Comparison of mental health differences at the wave one and wave two surveys (results from matching analysis)*

Differences in mean mental health scores for each row route, relative to comparators in the routes shown in the column headings.

		<i>Employ- ment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>
Employment	W1	–	1.28	1.81	1.23	4.85**
	W2	–	1.98	3.49*	1.95	2.21
Education	W1	0.14	–	0.10	5.94**	6.48***
	W2	1.44	–	1.79	3.04	4.24**
VS	W1	0.38	0.26	–	3.30	3.31
	W2	–0.10	–3.73**	–	–0.58	–2.27
ETF	W1	–0.21	–1.94	–1.28	–	4.46**
	W2	–1.63	–1.94	1.01	–	–1.26
Extended Gateway	W1	–1.6	–3.09**	–1.50	–1.81	–
	W2	0.58	–3.09**	2.96	–0.86	–

Note: Mental health (SF–36) was measured on a scale of 0 (worst) to 100 (best). Bold differences are statistically significant at the following significance levels: *** 1%, ** 5%, and * 10%.

actually participating in the placements, which tended to wear off after the placements were completed. Qualitative work with New Deal participants suggests that employment-based Option placements, including those in the Voluntary Sector and with the ETF, could lead to disappointment for young people when they did not result in longer-term employment opportunities, sometimes with adverse consequences for their mental health (Lakey et al, 2001). Participants in the Education Option may have been on their Options for longer (Education Options could last for up to a year, whereas other Options lasted for a maximum of six months) and would have been less exposed to such risks of disappointment than young people on employment-based Options were.

Table 3.8 compares the differences in levels of self-efficacy between participants on each New Deal route and their matched counterparts on other routes at the wave one and wave two interviews. Results in the first two rows of the table show that participation in the Employment Option tended to have a positive

Table 3.8: Comparison of self-efficacy differences at the wave one and wave two surveys (results from matching analysis)

Differences in mean self-efficacy scores for each row route, relative to comparators in the routes shown in the column headings.

		Employ- ment	Education	VS	ETF	Extended Gateway
Employment	W1	–	0.09	0.25**	0.24**	0.20**
	W2	–	0.16**	0.21**	0.16	0.24**
Education	W1	–0.17	–	0.02	0.13	0.14
	W2	–0.34***	–	0.10	0	0.15
VS	W1	–0.18	–0.11	–	0.02	–0.13
	W2	–0.17	0.03	–	–0.05	–0.01
ETF	W1	–0.24**	–0.13	0	–	0.11
	W2	–0.35***	–0.06	0.23**	–	0.04
Extended Gateway	W1	–0.05	–0.03	–0.09	–0.03	–
	W2	–0.37***	–0.07	–0.13	–0.23***	–

Note: Self-efficacy was measured on a scale of 1 (low) to 5 (high).

Bold differences are statistically significant at the following significance levels: *** 1%, ** 5%, and * 10%.

impact on self-efficacy (or confidence in one’s own ability to get a job) at both the wave one and wave two interviews, compared with participation on other New Deal routes. The positive effects of the Employment Option on self-efficacy were increased at wave two, when Employment Option participants were compared with their matched counterparts on the Education Option and Extended Gateway. On the other hand, the differences in self-efficacy scores between Employment Option participants and their counterparts on the Voluntary Sector and ETF Options were reduced at wave two, suggesting that these other employment-based Options might also have started to have a positive impact on self-efficacy. However, the first column of Table 3.8 shows that the negative effect on self-efficacy of being on the Education and ETF Options and the Extended Gateway was increased at wave two relative to matched counterparts on the Employment Option. Voluntary Sector Option participants did not have significantly lower self-efficacy scores than their counterparts on the Employment Option.

Other significant figures in Table 3.8 suggest that participation in the ETF Option might also have had some positive impact on the self-efficacy of young people. For example, the bold figure of 0.23 in column three, row eight shows that ETF Option participants had significantly higher self-efficacy scores than their counterparts on Voluntary Sector Options at the wave two interview, whereas there was no difference at wave one. The figure of -0.23 in the last row of column four suggests that Extended Gateway participants had significantly lower self-efficacy scores than those of their counterparts on the ETF Option. Again, this level of difference was not apparent at wave one.

In summary, matched comparison models of health outcomes at the wave two survey, showed that:

- participation in the New Deal Options appeared to have benefits for general health, compared with remaining on Gateway;
- the Education Option tended to perform relatively well in terms of the mental health of participants;
- Employment Option participation had the most benefits for self-efficacy, but there was some evidence that ETF Option participation was also beneficial in this respect; and
- participants on the Employment and Education Options tended to show the lowest levels of experiential deprivation.

Questions on experiential deprivation were not asked at wave one, but comparisons of wave one and wave two results for the other health outcomes allowed us to test the robustness of these results. The comparisons provided evidence that the results described above were not merely the consequence of health selection. They also showed that participants in all the New Deal Options had higher levels of mental health at wave one, compared with their matched counterparts on Extended Gateway. In the case of the Employment, Voluntary Sector and ETF Options, this was likely to be either a selection effect, or a transitory benefit, as the differences were substantially reduced at wave two.

Direct and Indirect Health Impacts

From the evaluation of New Deal for Young People with respect to labour market outcomes (Bonjour et al, 2001) we know that the probability of getting a job after New Deal varied systematically by New Deal route, with the Employment Option appearing to be particularly effective at helping young people find work. As there is a tendency for employed people to have better health than unemployed people, we might expect that any Options that increased the probability of employment for their participants might also have raised the overall level of participants' health. However, the existence of health benefits might also depend on the quality of employment obtained.

Using smoothing regression techniques (see Appendix 1 for more details), this chapter examines the extent to which the health benefits of NDYP Options resulted from the effectiveness of these Options at moving their participants more quickly into employment.

Key findings of this chapter are as follows:

- Some of the general health benefits of NDYP Options were likely to have accrued via the success of these Options in helping participants find jobs.
- However, participation in the full-time education and training Option also appeared to have some direct benefits for mental health, which were not the result of faster movement into jobs.

Table 4.1 shows the association between health and labour market status at wave two. Those who were employed at wave two had

Table 4.1: *General and mental health scores at the wave two survey, by labour market status at wave two*

	<i>Employed</i>	<i>Government programme (a)</i>	<i>Unemployed</i>	<i>Non-participating</i>
General health (b)	3.85	3.67	3.54	2.65
Mental health (c)	79.7	73.5	71.1	62.7
<i>Weighted base</i>	<i>1358</i>	<i>820</i>	<i>874</i>	<i>296</i>
<i>Unweighted base</i>	<i>1301</i>	<i>904</i>	<i>849</i>	<i>291</i>

(a) Including New Deal.

(b) On a scale from 1 (poor) to 5 (excellent)

(c) On a scale from 0 (worst) to 100 (best)

Note: All pair-wise differences are significantly different from zero at the 1% level apart from the difference between government programme and unemployment, which is significant at the 10% level for general health and at the 5% level for mental health.

the highest levels of both general and mental health, and those who had left the labour market had the lowest levels of health.

The matching models presented in the previous chapter controlled for a broad range of characteristics that helped to determine which New Deal Options participants entered, but they did not control for the effects of events that happened after New Deal entry. In this chapter, we use smoothing regression techniques, described in some detail in Appendix 1, to control for the effects of labour market progress after New Deal entry.

The first stage of the smoothing regression approach involved the estimation of ordered logit models of general health and regression models of mental health. The models estimated underlying health scores as a linear function of a range of explanatory variables.

Twenty separate ordered logit models were estimated for general health outcomes at wave two, and a further twenty regression models were estimated for mental health outcomes at wave two,⁷ each one pooling observations for a particular Option

⁷ Results not shown here but available on request.

and its matched comparison group (as used in the previous analysis). Each model included a number of explanatory variables plus a dummy variable indicating whether the observation came from the Option (treatment) or the comparison group. In the case of the ordered logit models, the coefficient of the treatment dummy was converted into the treatment effect on the general health score,⁸ enabling us to make direct comparisons between results generated using the smoothing approach and those generated from the matching approach described in the previous section.

Where the matching process controlled for differences in the characteristics of the Option and comparison groups these characteristics were not included in the estimated models. However, the models did include variables relating to characteristics that were determined (or may have been determined) after programme entry. The most important of these were labour market status at the wave two interview, health at the wave one interview and a number of indicators of disadvantage, such as whether the respondent ever spent time in local authority care, homeless, sleeping rough or in a young offenders' institution.

The dummy variables for labour market status and previous health at wave one were significant in nearly all of the ordered logit models for health. In the regression models for mental health, the mental health score at wave one was significant in all cases. Labour market status was again important, although it was significant in only a small number of estimations and generally at a lower level of significance. The mental health score at wave one was an important determinant of mental health at wave two. The inclusion of wave one health variables in the model of health

8 Applying the matching approach we get the difference in the health score between the treatment and the control group whereas the dummy in the ordered logit model measures the change in the probability of being in a higher health category, as a result of being treated. To make the two estimates comparable, the probability effect has to be translated into a health score effect. This was done using the following procedure. First, the expected probabilities for each health category were calculated both for the treatment and the control groups. Then, these expected probabilities were translated into a mean score for each of the two groups by multiplying the probability of each category by its value (ie expected probability of being in category 'excellent' times six, plus the expected probability of being in category 'very good' times five plus ... and so on up to and including expected probability of being in category 'poor' times one).

outcomes at wave two may be seen as a way of controlling for previously uncontrolled health selection. However, it is important to point out that these variables may also control for the early effects of New Deal on health. For this reason, the smoothing models should be seen as providing an indication of the longer-term impacts of New Deal Options on health.

Table 4.2 presents a comparison of treatment effects estimated for general health at wave two using the smoothing approach (S), with treatment effects estimated using the matching approach (M), as reported in the previous chapter. The comparison provides a general indication of the extent to which the treatment effects described in Chapter 3 were influenced by a combination of labour market outcomes from the various New Deal routes, uncontrolled health selection into these routes, and early effects of participation in the various Options.

Results in the last column of Table 4.2 show that participants in the New Deal Options tended to have better general health than their counterparts on Extended Gateway, whether a simple matching or a smoothing approach was used. Controlling for labour market outcomes at wave two and health at wave one reduced the apparent difference in general health for participants on the Education Option compared with their counterparts on Extended Gateway, and the difference became non-significant in the comparison between Employment Option participants and their counterparts on Extended Gateway. The last two rows of the table show that Extended Gateway participants continued to show worse general health than their counterparts on the New Deal Options, when the smoothing approach was used. However, the significant differences, between Extended Gateway participants and their counterparts on the Education, Voluntary Sector and ETF Options, were all reduced in size when the smoothing approach was used. There was also a reduction in the apparent benefit of Voluntary Sector participation, compared with participation on the ETF Option.

These results, together with the fact that labour market status was significant in most of the ordered logit models of general health, provide some evidence for indirect impacts of New Deal Options on general health, via their effects on employment. Health at wave one was also significant in the logit models, suggesting that some part of the treatment effect shown in results of the

Table 4.2: Comparison of general health differences at wave two using the basic matching (M) and smoothing approaches (S)

Differences in mean general health scores for each row route, relative to comparators in the routes shown in the column headings.

		Employ- ment	Education	VS	ETF	Extended Gateway
Employment	M	–	0.08	–0.06	–0.06	0.33**
	S	–	0.04	–0.08*	0	0.32
Education	M	–0.11	–	0.11	0.13	0.41***
	S	–0.12	–	0.03	0.10	0.32***
VS	M	0.21	–0.01	–	0.40*	0.31**
	S	0.15**	–0.03	–	0.10**	0.36**
ETF	M	–0.10	–0.11	0.16	–	0.18
	S	–0.12	–0.13	0.22	–	0.16
Extended Gateway	M	–0.22	–0.32***	–0.39***	–0.31*	–
	S	–0.26	–0.30*	–0.32***	–0.18	–

Note: General health was measured on a scale of 1 (poor) to 5 (excellent).

Bold differences are statistically significant at the following significance levels:

*** 1%, ** 5%, and * 10%.

simple matching model was due to selection of young people with different levels of health into different Options, or to early effects of the New Deal programme.

Table 4.3 compares the differences in mental health between participants on the five New Deal routes and their matched comparison groups on other routes, generated using the simple matching (M) and the smoothing approaches (S). The last column of this table shows that the mental health advantage of Education Option participants over their counterparts on Extended Gateway decreased and became non-significant when the smoothing approach was used. Conversely, the last two rows of column two show us that the mental health disadvantage of those who remained on Extended Gateway compared with their matched counterparts on the Education Option also decreased and became non-significant when labour market outcomes were controlled.

The figure of 2.67 in row four of the first column shows that Education Option participants had higher mental health scores

Table 4.3: *Comparison of mental health differences at wave two using the basic matching (M) and smoothing approaches (S)*

Differences in mean mental health scores for each row route, relative to comparators in the routes shown in the column headings.

		<i>Employ- ment</i>	<i>Education</i>	<i>VS</i>	<i>ETF</i>	<i>Extended Gateway</i>
Employment	M	–	1.98	3.49*	1.95	2.21
	S	–	0.46	2.00	1.34	–1.38
Education	M	1.44	–	1.79	3.04	4.24**
	S	2.67*	–	2.08	0.59	0.91
VS	M	–0.10	–3.73**	–	–0.58	–2.27
	S	1.73	–3.78**	–	–2.78	–3.98**
ETF	M	–1.63	–1.94	1.01	–	–1.26
	S	–0.58	–1.57	1.26	–	–4.05**
Extended Gateway	M	0.58	–3.09**	2.96	–0.86	–
	S	3.49*	–1.30	4.74***	1.46	–

Note: Mental health (SF-36) was measured on a scale of 0 (worst) to 100 (best). Bold differences are statistically significant at the following significance levels: *** 1%, ** 5%, and * 10%.

than their counterparts on the Employment Option when the smoothing approach was used. The figure of –3.78 in row six of column two shows that participants in the Voluntary Sector Option continued to show lower mental health scores than their counterparts on the Education Option when the smoothing approach was used. The persistence of certain benefits of Education Option participation, after labour market outcomes at wave two and health at wave one were controlled, provides some evidence that participation in this Option may, in itself, have been beneficial for mental health.

Other figures in the last column of Table 4.3 show that Employment Option, Voluntary Sector Option and ETF Option participants all had lower mental health scores than their counterparts on Extended Gateway, when the smoothing approach was used. The difference was statistically significant in the case of comparisons between the Voluntary Sector and ETF participants and their matched counterparts on Extended Gateway.

Conversely, the last row of the table shows that, when the smoothing approach was used, those who remained on Extended Gateway had better mental health scores than those of their matched counterparts on the Employment, Voluntary Sector and ETF Options, significantly so in the case of comparisons with the Employment and Voluntary Sector Options. These results suggest that these New Deal Options had little direct benefit for mental health compared with remaining on the Extended Gateway.

Conclusions

Although there is a great deal of research documenting the association between poor health and unemployment, there is very little research on the ways in which labour market programmes might impact on this relationship. This research aimed to make some contribution towards filling this gap, by examining the impact of New Deal on the health of young people in Britain.

As we said in the introduction, the scope of the research was limited by having to fit in with pre-existing arrangements for delivering and evaluating New Deal. The compulsory nature of New Deal meant that no control group of non-participants was available, so this study compared the health impacts of various routes through the programme. More seriously, the timing of the surveys used to evaluate New Deal, to which health questions were added for this research, meant that we were unable to collect information on general and mental health prior to programme entry.

Despite these limitations, the research was able to produce some indications of the impact of various New Deal Options on health, using propensity score matching techniques to control for selection into the various programme routes. The matched comparison analysis of health outcomes presented in Chapter 3 suggested that:

- participation in the New Deal Options appeared to have benefits for general health, compared with remaining on Gateway;

- the Education Option tended to perform relatively well in terms of the mental health of participants;
- Employment Option participation had the most benefits for self-efficacy, but there was some evidence that ETF Option participation was also beneficial in this respect; and
- participants on the Employment and Education Options tended to show the lowest levels of experiential deprivation.

Comparisons with wave one outcomes suggested that these results were not merely reflections of health selection. However, the comparisons with wave one outcomes did suggest that the propensity models might not have fully controlled for selection factors related to mental health.

The analysis using smoothing regression techniques, presented in Chapter 4, provided evidence that some of the general health benefits of New Deal Options were likely to have accrued via their success at helping participants into employment. The picture was less clear with regard to mental health, but it appeared that participation in the Education Option might have had some direct mental health benefits.

Overall, the results of this research suggest that labour market programmes do have the potential to make beneficial impacts on participants' health. Some of the health benefits appeared to come from accelerated progression into employment, but it seems likely that benefits also accrued in other ways, through the activities that people were engaged in whilst participating in the Options themselves.

However, more research is needed to establish the precise extent of health impacts generated by different labour market programmes. In particular, there is a need for studies that collect reliable information on health prior to programme entry, as well as post-programme, so that health-related selection can be adequately controlled, and more definitive conclusions reached.

Appendix 1

Methods of Analysis⁹

THE METHOD OF MATCHING

The essential difficulty of programme evaluation is one of missing data. We can observe choices that individuals make or influences they are exposed to, and can also observe outcomes. The problem is that we cannot observe the outcome that would have resulted had the individual made an alternative choice or been subject to an alternative influence. This hypothetical outcome is known as the counterfactual.

Simple inspection of the differences in outcomes between those participating and those not participating in a programme is likely to be misleading since no account is taken of selection into that programme. In other words, there may be systematic differences in characteristics across participants and non-participants that one might expect to influence outcomes. In order to isolate the programme effect from the effect of individual characteristics, these selection effects must be accounted for. As noted earlier, there are a number of methods available to do this.

Generally viewed as the most defensible approach is random assignment. This operates by creating a control group of individuals who are randomly denied access to a programme. The

⁹ This and other methodological appendices draw heavily on *New Deal for Young People: National Survey of Participants: Stage 2*, by Dorothe Bonjour, Richard Dorsett, Genevieve Knight, Stephen Lissenburgh, Arpita Mukherjee, Joan Payne, Martin Range, Peter Urwin and Michael White, Employment Service Research and Development Report, ESR67, March 2001.

outcomes of those participating in the programme relative to those in the control group provide an indication of the programme effect (subject to some provisos – see Heckman et al, 1999). This approach was not an option for the evaluation of the New Deal since the programme was introduced nationally and universally for the eligible population.

The approach used instead was the method of matching (more fully, propensity score matching). This is described below. In doing so, the single treatment¹⁰ case is considered first. While the range of Options in the New Deal makes for a more complicated structure, concentrating on the single treatment case allows us to focus on the main principles behind matching (which are common to both the multiple treatment and the single treatment case) before considering the additional refinements necessitated by the multiple treatment nature of the New Deal.

The essence of the approach is that, for each treated person, a non-treated individual is identified who is, in some sense, similar. In effect, this non-treated person becomes the counterfactual for the treated person. That is to say, the outcome of the identified non-treated person can be regarded as the outcome that would have resulted had the treated person not received treatment. Comparing the average outcome of those in the treated group with their matched counterparts provides an indication of the effect of the treatment (in a similar way to the random assignment case).

Implicit in this approach is the key identifying assumption of matching; namely, that if one can control for differences in characteristics between the treated and non-treated group, the outcome that would result in the absence of treatment is the same in both cases. Clearly, this outcome is observed for the group that receives no treatment, but this assumption allows the counterfactual outcome for the treatment group to be inferred, and therefore for any differences between the participants and non-participants to be attributed to the effect of the programme. With all relevant differences between the treatment and comparator groups accounted for, the matching technique can be viewed as the non-experimental analogue of a random assignment approach.

¹⁰ Note that in the evaluation literature the term 'treatment' is used to denote experience of a labour market programme. The same convention is followed here. The different Options of the New Deal represent separate treatments.

For this assumption to be plausible, one must be able to control for all characteristics that will affect both participation and outcomes jointly. This requires very informative data. In the case of the New Deal, such data were available and it therefore appears defensible to apply the matching approach. As will be seen later, rich information (drawn from administrative records and survey responses) was available on the labour market and other characteristics at both the individual and local area level.

A practical difficulty that arises when attempting to match individuals is that, as the number of characteristics to be matched increases, the probability of not finding a match increases. In other words, the chances of finding a 'similar' person fall as one becomes more specific as to what this person should be like. Where a suitable match can be found for an individual in the treatment group, there is said to be *support* in the comparator group for that member of the treatment group. Rosenbaum and Rubin (1983) show that if the identifying assumption for matching holds, it will also hold for certain functions of the controlling variables. One such function is the propensity score; the probability of belonging to the treatment group. Propensity score matching involves judging similarity between individuals purely on the basis of their propensity score. Matching using a single number in this way can prove less demanding in terms of support than matching a large number of characteristics directly. An additional requirement, however, is that the propensity score in the comparator group must be greater than zero but less than one for all values of the propensity score in the treatment group. If this condition does not hold, the remedy is to discard those observations in the treated group that are causing the problem. This ensures there is support for the treated group among the comparator group. While it does not cause any real problems when only a small proportion of the sample is discarded in this way, should a more sizeable number of observations be rejected, the representativeness of the estimated effect may be compromised.

There are a number of possible ways of identifying the comparator group. The single nearest-neighbour technique, used in the present research, involves finding for each treated individual that non-treated individual with the most similar propensity score (and, consequently, most similar characteristics).

This procedure is usually implemented with replacement: each treated individual has one match but a non-treated individual may be matched to more than one treated individual. Dehejia and Wahba (1999) find that allowing for non-treatment group members to be used more than once as comparators improves the performance of the match. Furthermore, matching with replacement in this way is less demanding in terms of the support requirement since individuals in the comparator group who would provide the closest match to a number of treated individuals remain available. Should a certain type of individual be common in the treatment group but relatively uncommon in the comparator group, the pool of comparators able to provide a close match would become exhausted were matching carried out without replacement. Allowing replacement in the matching process overcomes this difficulty.

NDYP, of course, is a multiple treatment programme with individuals able to move through a number of Options. The methodology can be easily adapted to suit this. The assumption required to identify effects generalises in an intuitive way. Identification is now possible so long as the outcome that would result from treatment is independent of treatment group, after controlling for differences in individual characteristics. The theory underpinning matching as an evaluation technique for multiple treatment programmes is set out in Imbens (2000) and Lechner (1999).

A major practical consideration when evaluating multiple treatment programmes using matching is that of support. As before, this is the requirement for 'similar' individuals to those in the treatment group to exist in the comparator group. Matching in this context requires comparing each type of treatment with each other type of treatment. It follows that if programme effects are to be estimated across a common group of individuals then there must be support in each type of treatment for every other type of treatment. This can prove too severe a restriction in that it can result in too many people being excluded from the analysis and hence the resulting estimate of the treatment effect not being representative of the whole treated population. The alternative is to not insist upon common support among each type of treatment for every other type of treatment, but instead just to ensure that in all comparisons between two treatments, there is support among the comparator

treatment. This has the major practical advantage of keeping the number of excluded people observations to a minimum, but suffers from the drawback that the results are not as general across the Options. For example, comparing the effect of being in the Employment Option rather than the full-time Education Option may be based on a different group of Employment Option participants than when comparing the effect of being in the Employment Option rather than the Voluntary Sector Option. Hence, one must be more circumspect about assuming that the effects are consistent across the choice of comparison Option.

APPLYING THE MATCHING METHOD TO ANALYSIS OF HEALTH OUTCOMES

In order to illustrate the way matching techniques were applied, we consider the effect of programme participation on the probability of having poor general health at a subsequent point in time.

The obvious way of doing this is to compare the proportion with poor health among those who participated and those who did not participate. However, the results of such comparisons will be misleading if certain characteristics of participants differ substantially from those of non-participants. More specifically, if participants had better health than non-participants did before entering the programme, one would expect them to have had better health afterwards even if they had not taken part in the programme.

In order to identify the effect of the programme on health, one must therefore take account of the participants' likely health prospects had they not participated. Subtracting this from the actual result yields an estimate of programme effect. However, the difficulty arises from the fact that only actual health is observed rather than hypothetical health prospects that would have resulted from participants not participating. What is needed in order to estimate the programme effect is an indication of the health prospects for those who participated *had they not participated*.

The results provided by the simplistic approach of comparing the proportion in poor health among participants and non-participants implicitly assume that the health prospects of

non-participants provide a reasonable estimate of the health prospects of participants had they not participated. The method of matching improves upon this by considering only that subset of non-participants who are in some sense similar to participants. The comparison of like with like means that the health prospects of this subset can be regarded as a better indicator of the health prospects of participants had they not participated.

In the case of the New Deal, a number of Options are considered simultaneously. Hence, an estimate is needed for each Option of how individuals in that Option would have fared had they instead entered a different Option. This means that, when considering the health effects of that Option for its participants, a set of 'similar' individuals from each of the other Options must be identified. This makes it possible to derive an estimate of the health effect of the Option relative to each of the other Options.

It is useful to provide an outline of the process involved in carrying out the matching. There are essentially two stages. First, models of Option participation are estimated. Second, individuals are matched using the resulting estimates of probability of Option participation. When considering the effect of a given Option (for example, the Employment Option) compared to another Option (say, Environment Task Force (ETF)), the starting point is to estimate the probability of participating in the Employment Option for those who participated in either the Employment Option or the ETF. Next, each individual in the Employment Option is matched with that individual in ETF with the most similar probability of being in the Employment Option. Comparing the proportion of Employment Option participants in poor health with the proportion in poor health among this matched group allows us to estimate the health effect of the Employment Option relevant to the ETF, providing that any effects of health on selection into these Options have been adequately controlled. This process can be carried out for all combinations of Options to provide information on the relative effects of all Options.

STRUCTURE OF THE ANALYSIS

The aim of the analysis is to estimate the relative effect of the New Deal Options on a range of health outcomes. For example,

there is a need to know how an individual in one Option would have fared in one of the other Options. Given the design of New Deal, those participating in an Option will have previously participated in the Gateway. Hence, the overall effect of the New Deal for those in a given Option is not just the effect of that Option, but the combined effect of the Option and the earlier Gateway.

There is no attempt to capture the separate effect of Gateway. A comparison of those people who participated in the Gateway but did not enter an Option with those who did proceed to an Option is complicated by the fact that the matching approach controls for factors that led people into different Options. Those who did not enter an Option cannot be considered because they did not reach the 'starting line' for the analysis. However, analysis revealed that the intended Gateway maximum duration of four months was being exceeded in a substantial number of cases.¹¹ This is depicted in Figure A1.1. There is a noticeable increase in exits at about four months after Gateway entry (approximately 122 days). Clearly, a significant minority remain on Gateway for longer than four months – some for much longer. This presents a possibility for analysis. Specifically, those individuals who did not enter an Option but who remained on Gateway for longer than intended can be regarded as a reference group against which the other Options can be compared. The characteristics of this group of people will be considered more fully below, but it is conceivable to regard them as a group of untreated individuals. Whereas those participating in an Option can be regarded as having received a treatment of Gateway plus Option, those who simply remain on the Gateway can be regarded as receiving a treatment of Gateway plus more Gateway.

Such evidence as is available suggests that the intensity of Gateway diminishes with time, so that those on an Extended Gateway can be regarded as receiving little additional attention beyond their initial Gateway experience. For example, analysis of administrative data reveals that the number of starts on and referrals to specific types of provision grows initially with gateway duration but levels off after about 20 weeks.

¹¹ This calculation of time on the Gateway excludes interruptions in claims.

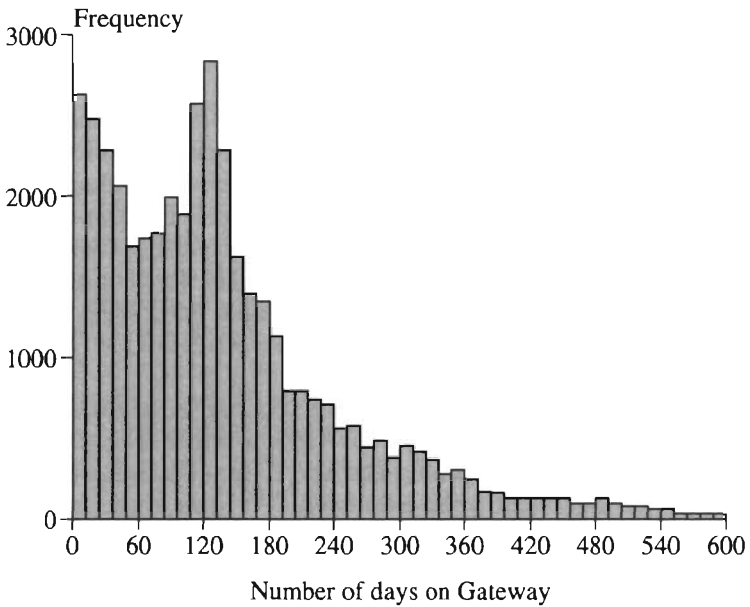


Figure A1.1: *Number of days spent on the Gateway (from administrative data)*

Hence, five routes through New Deal (combinations of treatments) are considered in the analysis. The first four routes correspond to the Gateway followed by one of the New Deal Options. The fifth route corresponds to the Gateway followed by remaining on the Gateway. It is the closest available approximation to a no-treatment group (at the Option stage). Thus, the analysis will allow examination of the effect of remaining in Gateway rather than moving to an Option, but will not allow quantification of the Gateway effect for those who leave Gateway in the intended timeframe. For the purpose of this analysis, the Extended Gateway group is defined as those individuals who did not enter an Option and who remained on Gateway for longer than five months (150 days).¹²

¹² Five months was chosen rather than four in order to exclude those individuals who remain on the Gateway only slightly longer than intended and to avoid splitting the sample at the peak Gateway duration.

MATCHING RESULTS

Matching analysis was based on those members of the September–November 1998 cohort who responded to both the first interview and the follow-up interview. There was substantial depletion of the sample since only 30 per cent of individuals who were sampled initially responded in both waves. In order to take some account of this, and to lessen the consequent biases in the estimates of programme effects, sample weights were constructed. These were used in the estimation of Option participation (the first stage of the matching process). Full details are provided in Appendix 2.

The first stage in the matching process is to model the probability of participating in one Option rather than another. Since there are four Options and the Extended Gateway, ten probability models must be estimated.^{13,14} From these models, ‘propensity scores’ are calculated as the estimated probability of participating in one Option rather than another. It is these scores that are used to perform the match, as noted earlier.

It is only necessary to include those variables that influence both participation and outcomes. If a variable influences participation but not outcome, there is no need to control for the differences between the treatment and the control groups for this variable since the outcome variable of interest is unaffected. Alternatively, if the variable influences outcome but not participation, there is no need to control for it since it will be insignificantly different in the treatment and comparison groups.¹⁵ Variables that affect neither participation nor outcome are clearly unimportant, so the only remaining type of variable is the kind that influences both participation and outcome – and these are the

13 The probability of entering Option A compared with Option B is one minus the probability of entering Option B compared with Option A, hence the probability model need only be estimated once for each pair of possible Options.

14 An alternative would be to estimate the probabilities of entering each Option simultaneously using, for example, a multinomial probit model. Appendix 3 explains briefly why the pairwise approach is preferred.

15 An example of a variable that might affect outcomes but not participation would be disability. This was found to be unrelated to participation in Options but has been shown by a number of studies to reduce the chances of employment entry.

ones needed to match. To include additional variables may increase the probability that there is no close match for a treated individual. However, as shown later, the proportion of the sample eventually discarded due to lack of support was quite small, so the approach adopted was to estimate all participation models using a common set of explanatory variables.

The outcomes under consideration when the propensity score models were first estimated were those relating to labour market effects, such as employability, and the probability of finding unsubsidised employment. However, the broad range of explanatory variables included in these models encompasses factors that are also known to be associated with health. All available information on health prior to New Deal entry was also used in these original models, so it was considered justifiable to use them as the starting point for our analysis of health outcomes.

In view of the central importance of the propensity scores in deriving an adequate match, it is instructive to consider the estimation results. These are given in Appendix 3. Each column in Appendix 3 gives the results of estimating participation in a given Option for those who are in that Option or in a given one of the other Options. Hence, the first four columns show the results of modelling the probability of being in subsidised employment for those who were either in subsidised employment or the full-time education and training Option, the Voluntary Sector Option, the ETF Option or the Extended Gateway, respectively. Column one, for example, looks at the probability of being on the subsidised Employment Option for those who were on either the Employment or the Education Option. It shows that people from ethnic minority groups, those with literacy and numeracy problems, those who had experienced more JSA claims, and those who had been claiming for longer, were less likely to be placed on the Employment Option. Those with a driving licence and those who had spent a greater proportion of their time in employment were more likely to be on the Employment Option.

The next three columns show the results of modelling the probability of being in the full-time education and training Option for those who were either in the full-time education and training Option or the Voluntary Sector Option, the ETF Option or the Extended Gateway, respectively. The remaining columns have an analogous interpretation.

The full range of factors influencing participation in each Option has been described in detail elsewhere (Bonjour et al, 2001). Focusing on the two health-related variables (whether the respondent was classified as disabled by the Employment Service, and whether the respondent had any periods out of work and not looking for work due to sickness), neither was significantly associated with participation in particular Options. However, this cannot be taken to mean that the type of Option entered was unrelated to health. These variables provide only very limited information on health prior to New Deal entry (see page 21), which means that selection into particular Options on health grounds may not be fully controlled for in the participation models.

Having estimated the probability models and associated propensity scores, the next step in the matching process is to identify the counterfactual for each treated person. This was done by finding, for each individual in a given Option, that individual in each other Option with the most similar propensity score. Since there are five Options being considered, this results in 20 pairwise matchings. These matchings are not symmetric. That is to say, identifying a comparator group for those in the Employment Option from among those in the ETF, for example, is a separate exercise from identifying a comparator group for those in the ETF from among those in the Employment Option. This is because the comparator group in each case is determined by the composition of the Option in question.

The most general results when using a matching approach are obtained when all effects are calculated across a common group of individuals. In this case, the effects associated with one Option relate to exactly the same group of individuals regardless of which other Option is being used to generate the counterfactuals. This allows general statements to be made about the relative effects of each of the Options without having to qualify such assertions with provisos about the composition of the primary Option being different in each case. Furthermore, such an approach allows one to calculate not just the effect of 'treatment on the treated' (that is, the effect of participating in an Option for those who did participate in it) but also the effect of the Option for somebody drawn randomly from any of the Options. However, this requires common support across all Options for every other Option. In the

case of our analysis, this proved to be too severe a restriction since half of the sample was being discarded for non-support reasons. This would seriously compromise the representativeness of the results. Hence, the approach taken was to ensure that there was support in the comparison group for the Option being considered, but not to enforce a common support across all Options. While this means that the results cannot be viewed as being as general as those based on a common support, they have the advantage of being more representative of the Option in question.

Having discarded unsupported individuals, of whom there were very few, the matching process concludes by finding, for each Option participant, a counterfactual person from the comparison Option.¹⁶ This may result in individuals being used as comparators more than once. Should this happen, such individuals receive a weight that corresponds to the number of times they serve as comparators.¹⁷ Hence, the sum of weights in the comparator Option is equal to the number of observations in the treatment Option.

ASSESSMENT

The results of the modelling process produced apparently plausible models of Option participation which included a number of significant variables in all cases. This, together with the rich variable set used in the modelling, provides some reassurance that the matching approach is successfully controlling for those characteristics which might be expected to result in differences in outcomes between participants in the different Options. The information on the resulting matching weights and the remaining differences between the treatment and comparison groups in each case appear acceptable. In view of this, it is considered appropriate to examine the effect of the different Options by simply comparing mean differences.

16 The percentage of unsupported individuals that had to be discarded ranged from 0 to 6.5. In more than half of all the pair-wise comparisons less than 2 per cent were discarded and in only one case was it more than 5 per cent.

17 Using individuals more than once as comparators can affect the estimated standard errors. In future research bootstrapping techniques will be applied to control for this.

However, it should be emphasised that this assessment of the plausibility of the models applies to their effectiveness in estimating labour market outcomes. The assumption made is that the same factors that influence employment in the absence of the programme would also influence health in the absence of the programme. Some limited measures of prior health were included in the matching procedures, and to some extent it could be argued that the breadth of the measures used in matching proxies or 'covers'¹⁸ the missing variables which might have provided more detailed information on prior health. Although it appears reasonably plausible to assume that the broad range of variables included in the models will cover for missing health variables, this assumption is a strong one. It is possible that the participation models may not fully control for associations between the New Deal route taken and the health characteristics of individuals prior to New Deal entry. Such differences may be partly responsible for differences in health outcomes between participants on the various Options. As a result, comparisons of mean differences in health will need to be interpreted with caution.

THE SMOOTHING APPROACH

The matched comparison method is used to estimate the impacts of various New Deal Options on health, but results from this method do not show whether any benefits accrued directly, as a result of participation in the programme, or indirectly, as a result of accelerated entry into beneficial employment. In order to address this issue, we need to take account of variables relating to experiences after the start of New Deal, and their effects on health.

In order to take these selection factors into account, a combination of the matched comparison method with ordered logit estimations of general health and regression models of mental health was used. These regression techniques are known as 'smoothing' because they attempt to smooth out that part of the

¹⁸ The idea of 'cover' in multivariate analysis is explained in Mosteller and Tukey (1977). In outline, the explanatory power of a variable can often be captured by a set of other variables correlated with it and with the outcome variable. These other variables 'cover' the effect of the missing variable.

difference in outcomes that can be explained by variables determined after programme entry, such as employment status. Ordered logit models are used to estimate the categorical outcomes for general health as a function of explanatory variables. The underlying score to be estimated is a linear function of the explanatory variables and cut-off points.

For each Option, we used the same matched comparison group that was used in the previous matching analysis. We then ran an ordered logit model (general health) or a regression model (mental health) pooling the observations of the Option and its comparison group, including a dummy variable that indicated whether an observation came from the Option (treatment) or the comparison group. The coefficient of the dummy provided a measure of the direct treatment effect. This treatment effect was net of selection into programme option and net of indirect programme effects.

The matching process controlled for differences in the characteristics of the Option and comparison groups and we did not include these characteristics in the estimated models. However, we did include characteristics that were determined after programme entry. The most important of these were labour market status, previous general and mental health (wave one) and a number of specific indicators of disadvantage such as having spent time in prison, homeless, or in local authority care. The health and specific disadvantage variables helped to control for selection effects that could not be included in the original matching because they were or might have been determined after programme entry. By including a variable for labour market status we were able to control for the indirect effect of the labour market outcomes of New Deal on the health of participants.

Appendix 2

Weighting to Account for Sample Non-response and Attrition

When considering the survey data, it is important to take account of reduced sample sizes due to both survey non-response and sample attrition. Out of the 11,159 individuals identified as the sampling frame, only 5999¹⁹ responded. This amounts to a response rate of 54 per cent. Furthermore, of these respondents, only 3373 (56 per cent) responded in the second wave.²⁰ Hence, only 30 per cent of individuals sampled responded in both waves. This depletion of the sample may lead to biases in the estimates of programme effects.

In order to address this, sample weights were constructed. Two types of weight were derived, reflecting the two means of sample reduction noted above. Both were calculated using probabilistic models. The first weight attempts to correct for non-response. To do this, a probit model of survey response was estimated across all individuals in the sampling frame. The inverse of the estimated probabilities can then be used to weight back to the sampling frame (and thereby to the cohort population from which the sampling frame was randomly drawn). The second weight attempts to account for sample attrition and proceeds in a similar way. A probit model of response to the second wave was

19 This is slightly higher than the number responding when considering the administrative data (5978).

20 Note that 12 people responded in wave two but not in wave one. They have been dropped from the analysis.

estimated across all wave one survey respondents. Now, the inverse of the estimated probabilities can be used to weight back to the sample of wave one survey respondents. Applying the product of these two weights will allow the sample of wave two respondents to be regarded as representative of the cohort population.

It would be possible to derive a single weight based on the probability of an individual in the sampling frame responding to the wave two (and therefore also the wave one) survey. However, there are two reasons why it is helpful to take explicit account of the two stages involved in sample reduction. The first is that the factors affecting non-response are likely to be different from those determining attrition. For example, non-response may be partly explained by individuals being 'anti-survey', yet attrition cannot be explained in this way. Second, estimating the non-response weight relies on administrative data, whereas both survey and administrative data can be used to estimate the attrition weight. Were a single weight capturing both non-response and attrition to be estimated, this would have to be based solely on the administrative data, thereby disallowing the use of the additional variables available in the survey data.

The results of estimating the two response models are presented in the following table. Column 1 is estimated over the 11,045 eligible members of the sampling frame.²¹ Several variables appear significant in determining response. Being older, having a higher number of JSA claims, having a longer time to Gateway entry, and living in the South East are all associated with lower response rates. Positive factors include being female, having a partner, being disabled and living in a rural area or an area of high local unemployment. There were also a range of effects associated with preferred occupation and New Deal eligibility. All area 'clusters' are more likely to respond than the high unemployment, inner-city cluster. The model correctly predicted the response of 60 per cent of the sampling frame.²² This indicates some explanatory power, although there is still a

21 For 114 people, the measured qualifying spell of unemployment was 0 days – they have been dropped from the analysis.

22 A fitted probability exceeding 0.5 was taken to indicate a predicted response to the survey.

substantial amount of unexplained variation. However, this is only a summary measure of the fit and does not take account of the continuous nature of the predicted probability.

Column 2 in the table is estimated over the respondents to the wave one survey. As a generalisation, many of the variables had effects similar in direction to those found when examining response at wave one, although there were differences in size and sometimes in significance. However, additional variables drawn from the wave one survey information were included in the analysis.²³ This showed an increased tendency towards response at wave two for those who remained in education longer.

Paradoxically, the effect of having academic qualifications was negative, albeit insignificant. Having vocational qualifications increased the response. In terms of housing tenure, being an owner-occupier predisposes individuals to responding relative to living in social rented accommodation. Conversely, renting privately was associated with non-response. Those in the Gateway or on an Option at the time of wave one sampling were more likely to respond than those who stated at the wave one interview that they were not on the New Deal. Finally, those who were not sure how useful the New Deal had been were less likely to respond than those who thought it very useful. Overall, the model correctly predicted response for 62 per cent of wave one respondents.

As a check on the performance of these weights, a number of variables present in both the probability models are considered in the table below. Column 1 shows the profile of the sampling frame (which is assumed to be the same as that of the population since it was identified through random sampling). Column 2 shows the profile of those responding in wave one. The characteristics outlined above as important determinants of response can be seen to exert their influence, particularly the low response rate associated with living in London and the South East. Applying the weights from the first probability model yields column 3, which has restored the profile of the population. In column 4, the characteristics of the wave two respondents are presented. Applying the weights derived from the second probability model

²³ Although wave one survey data were available on preferred occupation, administrative data was used since it has fewer missing values.

	(1) <i>Response to wave 1 interview</i>	(2) <i>Response to wave 2 interview</i>
Age at entry to first New Deal spell	-0.043 (6.70)**	-0.034 (3.76)**
Gender	0.062 (2.08)*	
Partner	0.253 (5.06)**	
Disability indicator	0.082 (2.06)*	0.198 (3.70)**
Number of JSA claims since Jan 1995	-0.024 (4.43)**	
Rural area	0.233 (2.43)*	0.242 (1.98)*
TTWA unemployment rate at ND entry	0.022 (2.51)*	0.031 (2.59)**
Time from ND entry to Gateway	-0.001 (2.74)**	
SOC: managers and administrators	-0.057 (0.47)	0.440 (2.35)*
SOC: professional	0.013 (0.10)	0.139 (0.77)
SOC: associate professional and technical	0.016 (0.27)	0.015 (0.17)
SOC: clerical and secretarial	0.131 (3.42)**	0.041 (0.78)
SOC: craft and related	-0.012 (0.28)	-0.102 (1.66)
SOC: personal and protective services	0.004 (0.09)	0.069 (1.12)
SOC: sales	0.058 (1.42)	0.118 (2.12)*
SOC: plant and machine operators	0.055 (1.13)	0.071 (1.08)
NDYP eligibility: later Restart flows	-0.007 (0.27)	0.052 (1.45)
NDYP eligibility: disabled	0.224 (2.34)*	-0.204 (1.76)
NDYP eligibility: literacy/numeracy	0.320 (1.94)	-0.116 (0.59)

	(1) <i>Response to wave 1 interview</i>	(2) <i>Response to wave 2 interview</i>
NDYP eligibility: English 2nd language	0.098 (0.39)	0.356 (1.00)
NDYP eligibility: ex-regular	-0.527 (2.34)*	-0.355 (0.96)
NDYP eligibility: ex-offender	-0.250 (2.66)**	-0.107 (0.76)
NDYP eligibility: lone parent	0.410 (1.31)	0.829 (2.15)*
NDYP eligibility: labour market returner	-0.194 (0.58)	0.665 (1.36)
NDYP eligibility: large-scale redundancy	0.650 (1.13)	-0.029 (0.05)
NDYP eligibility: local authority care	-0.307 (0.93)	-0.031 (0.06)
NDYP eligibility: 28-day rule	0.314 (1.41)	-0.638 (2.35)*
NDYP eligibility: 6-month+ stock	-0.176 (1.65)	-0.088 (0.54)
NDYP eligibility: ES discretion	-0.122 (0.92)	0.038 (0.20)
ES region: Scotland	0.444 (8.13)**	0.443 (6.07)**
ES region: Northern	0.261 (4.31)**	0.280 (3.40)**
ES region: North West	0.241 (5.83)**	0.276 (4.58)**
ES region: Yorks/Humb	0.314 (6.35)**	0.280 (4.10)**
ES region: Wales	0.269 (4.02)**	-0.035 (0.39)
ES region: West Midlands	0.324 (6.31)**	0.306 (4.29)**
ES region: East Midlands and Eastern	0.284 (5.55)**	0.315 (4.49)**
ES region: South West	0.354 (3.24)**	0.085 (0.59)
Cluster A	0.162 (1.58)	0.292 (2.13)*

	(1) <i>Response to wave 1 interview</i>	(2) <i>Response to wave 2 interview</i>
Cluster B	0.317 (5.18)**	0.586 (7.09)**
Cluster C	0.181 (3.55)**	0.261 (3.64)**
Cluster D	0.236 (4.52)**	0.337 (4.80)**
Cluster E	0.133 (2.93)**	0.205 (3.22)**
Cluster F	0.145 (3.65)**	0.199 (3.59)**
Any academic qualifications		-0.079 (2.01)*
Any vocational qualifications		0.085 (2.33)*
First left FT education at age 15		0.016 (0.22)
First left FT education at age 17		0.057 (1.22)
First left FT education at age 18		0.031 (0.57)
First left FT education at age 19		0.089 (1.38)
First left FT education at age 20		0.137 (1.54)
First left FT education at age <15		-0.101 (1.10)
First left FT education at age >20		0.190 (2.26)*
Housing tenure: other		-0.170 (2.29)*
Housing tenure: owner-occupier		0.128 (3.13)**
Housing tenure: private renter		-0.239 (4.60)**
NDYP status at wave one: follow-through		0.108 (1.55)
NDYP status at wave one: Gateway		0.104 (2.32)*

	(1) <i>Response to wave 1 interview</i>	(2) <i>Response to wave 2 interview</i>
NDYP status at wave one: option		0.240 (5.31)**
Perception of NDYP: cannot recall		-0.042 (0.52)
Perception of NDYP: fairly useful		-0.002 (0.05)
Perception of NDYP: not at all useful		-0.082 (1.36)
Perception of NDYP: not sure how useful		-0.260 (2.19)*
Perception of NDYP: not very useful		-0.031 (0.55)
Missing value: SOC	-0.001 (0.00)	-0.441 (1.19)
Missing value: rural	-1.019 (2.85)**	-0.451 (0.86)
Missing value: TTWA	0.196 (0.62)	-0.024 (0.06)
Missing value: partner	-0.022 (0.25)	
Missing value: cluster	0.216 (0.23)	
Constant	0.577 (3.94)**	0.106 (0.49)
Observations	11045	5947

Absolute value of z-statistics in parentheses

* significant at 5% level; ** significant at 1% level

For the categorical variables, the bases are as follows: SOC – ‘other’; NDYP eligibility – ‘6-month flow’; ES region – ‘LASER’; area clusters – ‘G: inner cities, high unemployment’; first left FT education – ‘age 16’; housing tenure – ‘social housing’; NDYP status at wave 1 interview – ‘not on NDYP’; perception of NDYP – ‘very useful’.

achieves a profile similar to that of the wave one respondents given in column 2. Multiplying the weights from the two probability models and applying to the wave two respondents gives a profile quite similar to that in the population. This is given in column 6.

	<i>Population</i>	<i>W1 unadjusted</i>	<i>W1 adjusted</i>	<i>W2 unadjusted</i>	<i>W2 adjusted to W1</i>	<i>W2 adjusted to population</i>
	<i>(1)</i>	<i>(2)</i>	<i>(3)</i>	<i>(4)</i>	<i>(5)</i>	<i>(6)</i>
Age at ND entry	20.8	20.7	20.8	20.6	20.7	20.8
Gender (1=female)	27.8	29.2	28.0	29.8	29.9	28.8
Disability indicator	12.2	13.4	12.2	14.9	13.3	12.2
Non-white indicator	28.7	25.4	28.1	22.3	25.0	27.6
Rural area indicator	1.8	2.2	1.8	2.6	2.2	1.8
TTWA unemployment rate	5.7	5.8	5.7	5.9	5.8	5.7
ES region: Scotland	9.2	10.9	9.2	13.0	10.9	9.2
ES region: Northern	7.9	8.7	8.0	9.7	8.8	8.1
ES region: North West	16.6	16.8	16.6	17.3	16.7	16.6
ES region: Yorkshire/Humberside	13.3	14.8	13.3	16.0	14.8	13.2
ES region: Wales	4.9	5.5	4.9	5.5	5.4	4.8
ES region: West Midlands	7.2	7.5	7.2	7.5	7.5	7.2
ES region: East Midlands and Eastern	9.5	10.3	9.5	11.2	10.2	9.5
ES region: South West	1.7	1.8	1.7	1.7	1.8	1.7
ES region: Laser	29.8	23.8	29.6	18.2	23.9	29.8
Cluster A: rural, tight labour market	2.1	2.3	2.1	2.3	2.2	2.0
Cluster B: rural, high unemployment	6.1	7.2	6.1	8.7	7.2	6.0
Cluster C: rural/urban, tight labour market	9.9	9.9	10.0	9.6	9.9	10.1
Cluster D: rural/urban, high unemployment	10.6	12.0	10.6	13.8	12.1	10.7
Cluster E: urban, tight labour market	13.2	13.5	13.2	13.5	13.6	13.5
Cluster F: urban, high unemployment	24.7	26.4	24.8	27.8	26.6	25.1
Cluster G: inner-city, high unemployment	33.4	28.7	33.2	24.3	28.5	32.5

Appendix 3

Results of Estimating Option Participation

Each column in Appendix 3 gives the results of estimating participation in a given Option for those who were in that option or in a given one of the other options.

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
Age at ND entry	-0.102 (1.57)	-0.071 (0.89)	0.056 (0.68)	-0.013 (0.19)	0.035 (0.62)	0.217 (3.76)**	0.070 (1.41)	0.162 (2.01)*	0.063 (1.04)	-0.075 (1.14)
Female	0.268 (1.25)	-0.462 (1.75)	1.346 (4.14)**	0.115 (0.49)	-0.521 (2.95)**	1.220 (4.59)**	0.078 (0.47)	2.003 (5.39)**	0.564 (2.87)**	-1.433 (4.66)**
Partnered	0.280 (0.70)	0.002 (0.00)	-0.573 (1.17)	-0.287 (0.59)	0.078 (0.21)	-0.474 (1.44)	-0.062 (0.21)	-0.215 (0.49)	-0.127 (0.31)	0.111 (0.30)
Dependent children	0.112 (0.26)	0.235 (0.43)	0.560 (1.09)	-0.151 (0.30)	-0.055 (0.16)	0.105 (0.34)	-0.373 (1.34)	0.124 (0.32)	-0.383 (0.94)	-0.505 (1.46)
Ethnic minority	-0.905 (3.17)**	-0.590 (1.61)	0.301 (0.71)	-0.932 (2.81)**	0.157 (0.71)	0.890 (2.99)**	0.003 (0.02)	0.679 (1.62)	0.010 (0.04)	-1.081 (3.24)**
Housing tenure: owner-occupier	0.104 (0.53)	0.392 (1.50)	0.356 (1.43)	-0.147 (0.60)	0.343 (1.88)	0.210 (1.13)	-0.147 (0.91)	0.052 (0.21)	-0.544 (2.63)**	-0.539 (2.53)*
Housing tenure: private renter	-0.073 (0.21)	0.264 (0.70)	0.195 (0.45)	-0.442 (1.14)	0.389 (1.59)	0.053 (0.22)	-0.138 (0.62)	0.155 (0.45)	-0.370 (1.30)	-0.470 (1.57)
Housing tenure: other	-0.042 (0.10)	-0.609 (1.05)	-0.001 (0.00)	0.025 (0.05)	-0.472 (1.48)	0.007 (0.02)	-0.241 (0.78)	0.452 (0.89)	0.330 (0.91)	-0.453 (1.00)
First left FT education at age <15	0.361 (0.76)	0.830 (1.19)	0.565 (0.69)	0.209 (0.35)	0.217 (0.45)	0.291 (0.60)	-0.297 (0.82)	-0.182 (0.24)	-0.556 (1.12)	-0.509 (0.96)
First left FT education at age 15	0.160 (0.36)	-0.200 (0.42)	-0.338 (0.80)	0.130 (0.29)	-0.620 (1.86)	-0.410 (1.34)	-0.239 (0.83)	-0.115 (0.28)	0.160 (0.44)	0.674 (1.95)
First left FT education at age 17	-0.096 (0.40)	0.014 (0.05)	0.030 (0.10)	0.106 (0.41)	-0.210 (1.02)	0.192 (0.99)	0.104 (0.59)	0.148 (0.55)	0.165 (0.69)	-0.040 (0.17)

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
First left FT education at age 18	-0.108 (0.42)	-0.340 (0.99)	0.043 (0.13)	0.458 (1.47)	-0.068 (0.29)	0.473 (2.04)*	0.472 (2.10)*	0.644 (2.05)*	0.491 (1.68)	0.289 (0.99)
First left FT education at age 19	-0.026 (0.08)	-0.827 (2.31)*	-0.182 (0.41)	0.457 (1.20)	-0.751 (2.85)**	0.159 (0.47)	0.323 (1.21)	1.169 (2.61)**	0.922 (2.73)**	0.327 (0.74)
First left FT education at age 20	0.423 (0.92)	0.178 (0.24)	-0.218 (0.41)	0.608 (1.16)	-0.521 (1.22)	-0.568 (1.28)	-0.032 (0.09)	-0.247 (0.46)	0.206 (0.44)	0.662 (1.25)
First left FT education at age >20	0.525 (1.19)	-0.899 (1.59)	-0.308 (0.49)	0.151 (0.35)	-1.253 (3.13)**	-0.573 (1.06)	-1.068 (3.22)**	0.839 (1.61)	0.317 (0.84)	-0.020 (0.04)
Any academic qualifications	0.101 (0.47)	0.545 (1.98)*	0.476 (1.95)	0.493 (2.12)*	0.278 (1.52)	0.401 (2.28)*	0.274 (1.73)	0.100 (0.42)	-0.025 (0.12)	-0.138 (0.68)
Driver's licence	0.698 (2.07)*	0.854 (1.88)	0.343 (0.85)	0.535 (1.46)	-0.323 (1.08)	0.039 (0.14)	-0.269 (1.06)	0.278 (0.68)	0.416 (1.16)	-0.287 (0.87)
Driver's licence and car	0.444 (1.23)	0.530 (1.06)	1.308 (2.78)**	-0.083 (0.20)	0.653 (1.75)	0.029 (0.08)	-0.322 (1.07)	-0.408 (0.78)	-1.381 (3.26)**	-0.753 (1.81)
Literacy or numeracy problems	-0.523 (1.97)*	-0.488 (1.69)	-0.569 (1.90)	-0.127 (0.39)	0.137 (0.70)	-0.071 (0.37)	0.367 (2.16)*	-0.070 (0.28)	0.175 (0.81)	0.262 (1.09)
Prison record	-0.511 (0.87)	-0.493 (0.80)	-0.476 (0.79)	-0.444 (0.74)	-0.475 (0.98)	0.039 (0.09)	-0.085 (0.20)	0.231 (0.41)	0.305 (0.61)	-0.481 (1.13)
Disability indicator	0.202 (0.91)	0.174 (0.58)	0.170 (0.61)	0.334 (1.14)	0.094 (0.47)	-0.001 (0.00)	0.184 (0.96)	-0.077 (0.29)	0.088 (0.37)	0.047 (0.18)
Sickness prevented work, 93-97	-0.509 (0.89)	-0.081 (0.11)	1.419 (1.77)	-0.081 (0.14)	0.161 (0.32)	1.289 (2.15)*	-0.138 (0.33)	0.504 (0.76)	-0.196 (0.36)	-1.174 (1.58)

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
% time in employment 93–97	0.649 (2.13)*	0.897 (2.17)*	-0.088 (0.24)	0.322 (0.89)	0.340 (1.12)	-0.948 (3.34)**	-0.443 (1.75)	-1.087 (2.78)**	-0.484 (1.44)	0.375 (1.13)
No. JSA claims from Jan 95 to ND entry	-0.106 (2.31)*	-0.122 (2.01)*	-0.135 (2.32)*	-0.087 (1.71)	-0.060 (1.48)	-0.041 (1.05)	0.015 (0.40)	-0.027 (0.48)	0.028 (0.63)	0.049 (1.09)
Total days unemployed before ND	0.000 (0.48)	0.000 (0.46)	0.000 (1.18)	0.000 (1.04)	0.000 (0.23)	-0.001 (3.19)**	0.000 (1.52)	0.000 (0.85)	0.000 (0.91)	0.000 (0.59)
Duration of current claim at ND entry	-0.001 (2.45)*	-0.002 (2.44)*	-0.002 (2.24)*	-0.002 (2.92)**	0.000 (0.81)	0.000 (0.69)	0.000 (1.28)	0.000 (0.84)	0.000 (0.18)	0.000 (0.04)
Been on a government programme before ND	-0.027 (0.15)	-0.170 (0.74)	-0.179 (0.74)	0.464 (2.01)*	0.096 (0.55)	-0.075 (0.45)	0.438 (2.60)**	-0.302 (1.33)	0.394 (1.97)*	0.432 (2.12)*
SOC: managers and administrators	-1.383 (1.32)	-1.320 (1.17)	-0.693 (0.25)	-1.653 (1.71)	-0.470 (0.47)	1.494 (1.01)	-0.423 (0.56)	0.788 (0.31)	-0.700 (0.79)	-1.550 (1.09)
SOC: professional	-0.408 (0.47)	-1.009 (1.03)	0.916 (0.98)	1.398 (1.43)	-0.845 (1.32)	1.158 (1.53)	2.601 (2.07)*	0.603 (0.72)	3.102 (2.58)*	1.034 (1.01)
SOC: associate professional and technical	-0.482 (1.13)	-0.372 (0.71)	0.549 (0.87)	0.403 (0.81)	-0.190 (0.51)	1.162 (2.31)*	0.688 (2.02)*	0.956 (1.48)	0.466 (1.10)	0.388 (0.72)
SOC: clerical and secretarial	0.126 (0.53)	-0.290 (0.86)	0.434 (1.53)	0.813 (2.95)**	-0.748 (3.00)**	0.205 (0.92)	0.421 (2.03)*	1.009 (3.30)**	1.148 (4.08)**	0.327 (1.26)
SOC: craft and related	0.106 (0.35)	0.045 (0.10)	0.140 (0.40)	0.762 (1.93)	-0.149 (0.47)	0.003 (0.01)	0.390 (1.49)	0.198 (0.60)	0.511 (1.33)	0.680 (2.41)*

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
SOC: personal and protective services	-0.080 (0.23)	-0.334 (0.75)	0.659 (1.41)	0.348 (0.94)	-0.538 (1.88)	0.588 (1.88)	0.158 (0.65)	1.028 (2.58)*	0.574 (1.84)	-0.187 (0.57)
SOC: sales	0.050 (0.18)	-0.855 (2.26)*	0.677 (1.87)	0.251 (0.78)	-1.038 (4.12)**	0.496 (1.83)	0.102 (0.49)	1.406 (4.06)**	1.076 (3.73)**	-0.175 (0.54)
SOC: plant and machine operators	0.087 (0.26)	-0.209 (0.46)	-0.279 (0.66)	0.337 (0.83)	-0.283 (0.90)	-0.233 (0.90)	0.219 (0.83)	0.403 (1.05)	0.534 (1.41)	0.526 (1.62)
TTWA unemployment rate at ND entry	0.007 (0.09)	0.172 (1.76)	-0.069 (0.72)	0.215 (2.38)*	0.108 (1.56)	-0.001 (0.02)	0.170 (2.72)**	-0.111 (1.14)	0.128 (1.63)	0.242 (2.74)**
Delivery model:										
ES joint partnership	-0.061 (0.15)	-0.115 (0.24)	-0.043 (0.08)	0.064 (0.13)	-0.143 (0.43)	-0.292 (0.82)	0.020 (0.06)	0.375 (0.87)	0.507 (1.15)	0.445 (0.95)
Delivery model:										
ES consortium contract	0.144 (0.31)	0.499 (0.80)	-0.270 (0.43)	0.089 (0.17)	0.690 (1.51)	-0.407 (0.96)	0.056 (0.14)	-0.725 (1.31)	-0.252 (0.54)	0.081 (0.16)
Delivery model:										
private sector led	-0.901 (1.24)	-0.061 (0.08)	-0.763 (0.97)	-0.416 (0.57)	-0.166 (0.37)	0.149 (0.31)	-0.032 (0.08)	0.450 (0.71)	0.001 (0.00)	-0.076 (0.13)
Time from ND entry to Gateway	0.003 (1.01)	-0.002 (0.58)	-0.003 (0.89)	0.000 (0.02)	-0.003 (1.56)	-0.006 (2.03)*	-0.003 (1.07)	-0.002 (0.46)	0.002 (0.68)	0.003 (0.72)
Ethnic minority % in UoD inflow	0.009 (0.05)	0.094 (0.41)	0.058 (0.24)	-0.110 (0.49)	0.100 (0.66)	0.178 (1.07)	0.004 (0.03)	0.186 (0.86)	-0.080 (0.45)	-0.223 (1.25)
Disabled % in UoD inflow	0.184 (0.82)	0.392 (1.52)	-0.108 (0.42)	0.280 (1.23)	-0.071 (0.38)	-0.235 (1.26)	-0.117 (0.70)	-0.161 (0.70)	0.002 (0.01)	0.303 (1.50)

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
UoD mean length of claim at ND entry	-0.283 (1.52)	-0.086 (0.38)	-0.167 (0.67)	-0.489 (2.25)*	0.357 (2.40)*	0.189 (1.07)	-0.058 (0.41)	-0.277 (1.09)	-0.581 (3.22)**	-0.414 (1.86)
Option trialling intensity (factor score)	0.116 (0.73)	0.243 (1.34)	0.160 (0.72)	0.132 (0.75)	0.170 (1.24)	-0.029 (0.20)	0.041 (0.34)	-0.101 (0.56)	-0.013 (0.08)	-0.005 (0.03)
Gateway intensity (factor score)	-0.166 (0.83)	-0.262 (1.12)	-0.128 (0.49)	-0.080 (0.40)	-0.221 (1.35)	-0.045 (0.29)	-0.009 (0.06)	0.183 (0.81)	0.105 (0.64)	0.076 (0.45)
Rural area	0.348 (0.82)	1.981 (2.79)**	0.523 (0.94)	0.720 (1.33)	1.116 (1.92)	0.557 (1.29)	0.529 (1.15)	-0.953 (1.38)	-0.767 (1.00)	-0.004 (0.01)
ES region: Scotland	0.482 (1.06)	0.600 (1.17)	0.340 (0.63)	0.415 (0.77)	0.273 (0.72)	0.269 (0.70)	0.434 (1.20)	0.247 (0.52)	0.253 (0.57)	-0.028 (0.06)
ES region: Northern	-0.175 (0.33)	0.470 (0.78)	0.457 (0.73)	0.355 (0.60)	0.404 (0.98)	1.027 (2.50)*	0.859 (2.30)*	0.764 (1.40)	0.926 (1.94)	-0.027 (0.06)
ES region: North West	0.251 (0.51)	1.150 (2.08)*	-0.031 (0.05)	-0.010 (0.02)	0.999 (2.44)*	-0.170 (0.42)	0.095 (0.26)	-0.988 (1.90)	-0.777 (1.79)	0.241 (0.51)
ES region: Yorkshire/ Humberside	-0.590 (1.08)	-0.180 (0.30)	-0.717 (1.03)	-0.364 (0.60)	0.274 (0.66)	0.079 (0.18)	0.497 (1.22)	-0.390 (0.76)	0.294 (0.60)	0.370 (0.68)
ES region: Wales	0.147 (0.26)	0.310 (0.52)	0.012 (0.02)	0.214 (0.37)	-0.497 (1.18)	-0.330 (0.82)	-0.347 (0.87)	-0.524 (1.22)	0.301 (0.65)	0.148 (0.34)
ES region: West Midlands	0.320 (0.62)	0.960 (1.58)	-0.014 (0.02)	0.094 (0.16)	0.613 (1.48)	0.307 (0.61)	0.039 (0.10)	-0.597 (1.03)	-0.475 (0.99)	-0.099 (0.19)
ES region: East Midlands and Eastern	0.117 (0.26)	0.077 (0.16)	-0.159 (0.26)	0.419 (0.84)	-0.238 (0.71)	0.296 (0.84)	0.405 (1.36)	0.503 (1.07)	0.891 (2.33)*	0.417 (0.98)

	<i>Prob of EMP for those in EMP or:</i>				<i>Prob FTET for FTET or:</i>			<i>Prob VS, VS or:</i>		<i>Pr ETF:</i>
	<i>FTET</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>VS</i>	<i>ETF</i>	<i>EGW</i>	<i>ETF</i>	<i>EGW</i>	<i>EGW</i>
ES region: South West	0.273 (0.37)	1.749 (2.01)*	0.453 (0.49)	0.726 (0.90)	1.149 (1.60)	0.156 (0.21)	0.059 (0.11)	-1.270 (1.37)	-0.745 (1.06)	-0.299 (0.41)
Cluster A: rural, tight labour market	-1.400 (1.46)	-1.098 (0.73)	-1.122 (0.75)	-2.177 (1.87)	1.401 (1.96)	0.287 (0.40)	0.179 (0.31)	-1.040 (0.97)	-1.086 (1.43)	-0.526 (0.60)
Cluster B: rural, high unemployment	-0.437 (0.89)	-0.733 (0.92)	-0.572 (0.91)	-0.375 (0.62)	0.568 (1.19)	-0.072 (0.15)	0.804 (1.80)	-0.225 (0.29)	0.061 (0.11)	0.122 (0.18)
Cluster C: rural/urban, tight labour mkt.	-0.702 (1.29)	-0.565 (0.68)	-0.404 (0.56)	-1.210 (1.94)	1.375 (2.98)**	0.070 (0.15)	0.428 (0.98)	-0.617 (0.81)	-1.072 (1.99)*	-0.716 (1.10)
Cluster D: rural/urban, high unemp.	-0.163 (0.33)	0.451 (0.58)	-0.582 (0.87)	-0.498 (0.77)	1.261 (2.68)**	-0.557 (1.12)	0.132 (0.31)	-1.756 (2.24)*	-1.408 (2.41)*	-0.077 (0.12)
Cluster E: urban, tight labour market	0.170 (0.40)	-0.411 (0.64)	-0.023 (0.04)	-0.410 (0.78)	0.238 (0.64)	0.107 (0.24)	0.199 (0.57)	0.133 (0.19)	-0.402 (0.92)	-0.757 (1.25)
Cluster F: urban, high unemployment	0.003 (0.01)	-0.444 (0.71)	0.160 (0.33)	-0.432 (0.88)	0.319 (0.95)	0.226 (0.59)	0.090 (0.28)	0.055 (0.09)	-0.557 (1.31)	-0.576 (1.11)
Constant	1.398 (1.05)	1.073 (0.63)	-0.457 (0.27)	-1.019 (0.71)	-0.473 (0.38)	-3.465 (2.92)**	-2.367 (2.20)*	-2.945 (1.78)	-2.553 (1.95)	0.130 (0.09)
Observations	1121	633	645	740	1226	1238	1333	750	845	857

Absolute value of t-statistics in parentheses

* significant at 5% level; ** significant at 1% level

For the categorical variables, the bases are as follows: SOC – 'other'; ES region – 'LASER'; area clusters – 'G: inner cities, high unemployment'; first left FT education – 'age 16'; housing tenure – 'social housing'.

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