# The New Deal for Young People: effect of the options on the labour market status of young men[*]

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**Abstract**: The New Deal for Young People was introduced throughout Great Britain in April 1998 as a key element of the government's welfare-to-work strategy.  Participants enter a period of intensive job search known as the 'Gateway' and then enter one of four options.  In this paper, the relative effectiveness of these options on unemployment exit and job entry is evaluated.  The evaluation uses a non-parametric matching approach and finds that the employment option dominates the other NDYP options.  Remaining on the Gateway appears more effective than entering the remaining options.  The relative strength of these effects changes over time.

**Keywords**: unemployment, evaluation, propensity score, matching, treatment effects.

**JEL classification**: C14, C40, H43, J64, J68

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**1. Introduction**

The New Deal for Young People (NDYP) was introduced throughout Great Britain in April 1998 with the twin aims of helping the young unemployed into work and increasing their employability. It is a key element of the government's welfare-to-work strategy, being the largest of the New Deal programmes and the first to be implemented. The target group is *all* 18-24 year olds who have been claiming unemployment benefit (Jobseeker's Allowance – JSA) for a period of six months or more. Other 18-24 year olds with shorter unemployment spells who are deemed to face particular obstacles to employment are also eligible. The design of the programme is as follows. First, individuals enter a period of intensive jobsearch known as the 'Gateway'. Second, they enter one of four options – subsidised employment, full-time education and training, environmental task force or voluntary sector. After this, they enter a period known as 'follow-through' which is essentially the same as the Gateway.

In this paper, the relative effectiveness of these options in reducing unemployment and increasing employment among young men is evaluated. There are a number of reasons why this is of interest. From the policy viewpoint, the substantive results are important since NDYP is the flagship welfare-to-work programme in the UK. Clues as to the relative effectiveness of the options can help identify which elements of the NDYP are

working and which need to be changed. From a methodological viewpoint, this was the first major labour market programme within the UK to be evaluated using matching. This has the advantage that it avoids the functional form restrictions inherent in parametric techniques and also allows for heterogeneity of effects across individuals. Furthermore, matching is well-suited to the evaluation of multiple treatment programmes. The theory underpinning the use of matching for such evaluations has only recently been developed (Imbens, 2000, Lechner, 2001) and empirical examples of matching being used to assess multiple treatment programmes are extremely rare in any country; I am aware only of applications to France (Brodaty et al., 2001), Switzerland (Gerfin and Lechner, 2002) and Sweden (Larsson, 2000; Frölich et al., 2004). The approaches used in these studies differ and the results in this paper use yet another approach, covered in Lechner (2002). Consequently, the analysis contributes to the currently available evidence base on the evaluation of multiple treatment programmes using matching.

The analysis is also interesting in the regard that it is based on administrative rather than survey data. This is a reflection of the increasing availability of high quality administrative records at the population level and the growing potential for such data to be used in evaluating labour market programmes. The administrative data is sufficiently rich that it is credible to believe that the key influences on both selection into NDYP options as well as labour market outcomes are observed and that consequently the identifying assumption underpinning matching is satisfied. The basis for this assertion is considered more fully in Section 4. A limitation of administrative unemployment records is that destination on unemployment exit is often poorly recorded, resulting in difficulties

when using such data to consider transitions into employment.  However, in this application the problem is less severe since, with the young client group in question, economic inactivity is relatively rare (Bonjour et al., 2001).  This means that exits from unemployment can be more directly interpreted as movements into employment.  Nonetheless, given the policy importance of the employment effect of programmes, the issue is examined directly in the analysis that follows by making use of information on destinations upon JSA exit and imposing assumptions where the information is missing.  This is described in more detail below.  The strength of the data is that it does not suffer from problems of non-response, attrition or recall error.  It also allows recording of organisational details that are not observable from survey data.  Such information is important in this analysis.

The reason for focusing on men is that three-quarters of NDYP participants were male.  It is important to be clear that this is not an evaluation of the programme in its entirety.  In particular, there is no consideration of the effectiveness of the Gateway, nor is any attention given to how participants in the options would have fared had they not participated in NDYP at all.  More broadly, the macroeconomic effects are not considered.  Rather, the focus is on the effectiveness of one option compared to another, with the intention of identifying the most successful elements of NDYP.  Since all individuals participating in an option will have been unsuccessful in finding work during the Gateway, they are likely to be particularly disadvantaged in terms of labour market characteristics.  This lends particular importance to the analysis.

The format for the remainder of this paper is as follows. In the next section, the NDYP programme is described and set in the context of comparable programmes from other countries. This is followed in section 3 by a description of the methodology. After describing the data in section 4, the results are given in section 5. This is followed by an assessment of the robustness of these results in section 6. The substantive findings are discussed in section 7, which also concludes.

## 2. The structure of NDYP

As noted, there are three elements to NDYP. The first stage is the Gateway. This is a period of intensive job search with help and guidance provided by a New Deal Personal Adviser. The job seeker and the personal adviser meet at least once a fortnight. The original intention was that the Gateway would last for a maximum of four months. However, as will be shown later, this is often exceeded in practice.

Those not finding work during the Gateway enter one of four NDYP 'options':

1. subsidised employment (EMP) – full-time employment subsidised by a payment of £60 per week to the employer;
2. full-time education or training (FTET) – aimed at those lacking basic qualifications, it allows participants to study while remaining on benefits;

3. voluntary sector employment (VS) – participants work in a voluntary sector organisation with the aim of acquiring work experience. They remain on benefit but receive an additional £400 spread over six months;

4. employment in the Environmental Task Force (ETF) – similar to VS but working for an organisation with an environment-focused remit.

These options last up to six months, with the exception of FTET which can last up to one year. Furthermore, with all options other than FTET, employers are obliged to offer education or training at least one day a week and this should lead to the achievement of a formal qualification. To this end, they receive a payment of £750 for each NDYP placement.

The third element of the programme is Follow-through. The objective of Follow-through is 'to ensure that New Deal clients are helped throughout their participation on an option to progress towards the goal of finding and sustaining work, and are given further assistance if they return to unemployment' (Department for Education and Employment, 1998). In practice, Follow-through is often used to describe continuing advice and assistance once participants have been through an option but not left NDYP.

The design of the NDYP is unusual in that it allows participants to choose their route through the programme. That is, individuals choose which option to enter, although participation in NDYP remains compulsory. In fact, participants can only leave the programme by entering employment or ceasing to claim JSA. However, attempts to

avoid participation through short-term breaks in benefit claims are frustrated by the fact that individuals leaving NDYP for a period of less than 13 weeks remain subject to its requirements on resumption of JSA claim. Hence, to exit NDYP one must stop claiming unemployment benefits for more than 13 weeks either through working or some other means.

The options differ in their attractiveness and not all participants enter their preferred option. The remainder of this section draws on evidence from Bryson et al. (2000) and Hasluck (2000) to derive an account of the selection into options.

On balance, levels of satisfaction among participants in EMP are higher than for any other option and dissatisfied participants in other options are most likely to say that they would prefer to be in EMP. The key obstacle to entering EMP may stem either from a shortage of vacancies or a reluctance of employers to accept potential participants, even with the incentive of a subsidy. There is evidence that employers are seeking basic ability and work readiness rather than specific job-related skills and experience. Members of minority ethnic groups and those with health problems are less likely to enter EMP. Administrative records show that ethnic minority participants were put forward for job vacancies as frequently as other participants and this suggests that their low representation may be due partly to discriminatory employment practices.

The largest option is FTET which is intended to address longer-term barriers to employment arising from lack of qualifications. Despite this, there is little difference in

the level of qualifications held by those in FTET compared to participants in other options. It is particularly popular among members of minority ethnic groups, though whether the extent to which this is a choice constrained by inability to enter EMP is not certain.

The remaining two options, VS and ETF, are characterised by a significant proportion of reluctant participants and lower levels of satisfaction. Many of those in VS would prefer instead to be in EMP or FTET. However, it is among ETF participants that satisfaction is lowest. There is evidence that this option caters especially for those New Deal participants who are difficult to place. The type of work involved in the two options differs, with ETF focusing chiefly on manual work, while VS involves mainly retail and service jobs. It is worth noting that for both VS and ETF there was a proportion who were committed to the work they were involved in and viewed it as making a positive contribution to the community.

Finally, there are those who remain on the Gateway for an extended period. Personal advisers may allow this where individuals have particular job search problems. However, there is also evidence that overstayers are unwilling participants. Indeed, they are more likely to be subject to benefit penalties (for reasons of non-compliance) and to state that New Deal pushes people into things they do not want to do.

**2.1 European evidence**

It is useful to place this analysis in the broader context of programmes in other countries that are comparable to some extent to NDYP or elements within it. In doing this, I draw heavily on Knight and White (2002) who compare the effect of NDYP with other interventions in Europe and the US. Most relevant are the European results since they tend to be more recent; programmes similar to NDYP have not been the main focus of evaluation attention in the US for some time. Below, the NDYP options are considered in turn.

The employment option can be compared with wage subsidy programmes in other countries. These typically identify a positive effect on employment. Most relevant (in terms of comparability of programme and reliability of analysis) are Sweden (Larsson, 2000), Switzerland (Gerfin and Lechner, 2002) and East Germany (Eichler and Lechner, 2002); the estimated impact on the employment rate ranges from 6-14 percentage points.

The VS and ETF options are essentially job creation programmes in the public sector. These are typically less successful in increasing employment and in fact have been found in other countries to have a negative effect. The most relevant analyses are those carried out in Switzerland (Gerfin and Lechner, 2002), East Germany (Bergemann et al., 2000) and France (Brodaty et al, 1999). These give a wide range of estimates of the effect on employment; from –30 to –7 percentage points.

Most mixed are the results for full-time training programmes; the FTET equivalent.
Focusing on those recent analyses that used a similar approach to that used in this paper,
the range of estimates suggests a poorer performance than that seen for wage subsidies
but a better performance than that for job creation programmes.  Analyses in France
(Brodaty et al, 1999), East Germany, Switzerland and Sweden provide estimates ranging
from a gain in employment of 8 percentage points to a loss of 15 percentage points.

## 3. Propensity score matching

*3.1 Theory*

When considering a single treatment programme, the mean effect of treatment on the
treated[1] can be written

$$\theta(x) = E(Y^1 \mid S = 1, X=x) - E(Y^0 \mid S = 1, X=x) \tag{1}$$

where $Y^1$ and $Y^0$ are the potential outcomes for participants ($S=1$) and non-participants
($S=0$) respectively and $X$ is a set of conditioning variables.[2]  The term $E(Y^0 \mid S = 1, X=x)$
is the mean of the counterfactual which, since it is unobservable, must be identified and
estimated.

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[1] For a detailed survey of evaluation techniques and parameters of interest see, for example, Heckman et al.
(1999).
[2] The notational convention followed in this paper is to denote population quantities by capital letters and
sample quantities by lower-case letters.

Using matching to estimate $\theta(x)$ is possible if, conditional on observable characteristics, non-treatment (potential) outcomes are independent of participation. Formally, the conditional independence assumption (CIA) can be written:

$$Y^0 \amalg S \mid X = x, \ \forall x \in \chi \tag{2}$$

where $\amalg$ denotes independence and $\chi$ denotes the part of the attribute space for which the treatment effect is defined.[3] Rosenbaum and Rubin (1983) showed that if the CIA holds, it is sufficient to match on the probability of participation; the propensity score. This reduces the dimensionality of the match to one; rather than matching on a vector of characteristics, it is possible to match on just the propensity score. Participating individuals can only be matched if there exist individuals with similar $x$ among the non-participants (i.e. there is common support in $x$). Subject to this requirement, the CIA allows non-participants' outcomes to be used to infer participants' counterfactual outcomes, therefore allowing $\theta(x)$ to be estimated.

The assumption required to estimate the effects of multiple treatment programmes is an intuitive generalisation of the single treatment case.[4] Now the outcome that would result from treatment is assumed to be independent of treatment group, after controlling for differences in individual characteristics.

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[3] This form of the CIA is sufficient for the treatment on the treated parameter, Identification of the average treatment effect requires that $Y^0$, $Y^1 \amalg S \mid X = x, \ \forall x \in \chi$.

[4] See Imbens (2000) and Lechner (2001).

With $M+1$ treatments, only one of the potential outcomes $\{ Y^0, Y^1, ..., Y^M \}$ will be observed, the other $M$ being counterfactuals. The mean effect of treatment $m$ relative to treatment $l$ for those receiving treatment $m$ can be written:

$$\theta^{m,l}(x) = E(Y^m \mid S = m, X=x) - E(Y^l \mid S = m, X=x) \tag{3}$$

where $S \in \{ 0, 1, .., M \}$ denotes the type of treatment.

In the multiple treatment case, the CIA becomes:

$$Y^l \coprod S \mid X=x, \quad S \in \{m,l\}, \quad \forall x \in \chi \qquad . \tag{4}$$

From this it follows that only the subsample of participants in treatments $m$ or $l$ is needed to estimate $\theta^{m,l}(x)$. As with the single treatment case, rather than conditioning on $x$, it is possible to condition instead on the propensity score $P^{l|ml}(X) = P(S=l \mid S \in \{m,l\}, X=x)$:

$$Y^l \coprod S \mid P^{l|ml}(X) = P^{l|ml}(x), S \in \{m,l\} \quad 0 < P^{j|ml}(x) < 1, \ \forall x \in \chi, \ \forall j = m,l. \tag{5}$$

Hence, the propensity score conditional on being in treatment $m$ or $l$ can be used to estimate $\theta^{m,l}(x)$ in the multiple treatment case.

If the CIA is satisfied, matching offers an attractive means of carrying out programme evaluations since it is not dependent on any functional form assumptions, it allows for

heterogeneity of effects across individuals and it can correct for important biases

associated with evaluation (Heckman et al., 1997, 1998).[5]


*3.2 Approach in this evaluation*


The approach taken in this evaluation consists of four main stages. First, the propensity

scores are estimated. These may be estimated either simultaneously or as a series of

dichotomous models. The latter approach has been followed here (a series of logit

models). Theoretically, this is preferable since it avoids the restrictions associated with

simultaneous models. For example, it is well known that the multinomial logit has the

Independence of Irrelevant Alternatives (IIA) property and this can produce unreliable

results when the categories modelled are substitutes. With NDYP, it is plausible that

some of the options may be substitutes for each other (for example, VS and ETF) so the

appeal of the multinomial logit is reduced. The other obvious candidate is the

multinomial probit. This does not have the IIA property. However, in practice, further

restrictions on the covariance matrix may be required, as may exclusion restrictions

(Lechner, 2002).

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[5] An alternative to the cross-sectional matching considered here is to combine matching with difference-in-differences. This requires observations both before and after participation and is not possible in this case due to the structure of the available data. The identifying assumption for this estimator is different from the CIA; namely, that the conditional selection bias is the same in the before and after periods. This is not a weaker assumption than CIA but may be more or less plausible than CIA in any given application. However, if CIA holds in the after period (the premise of this paper) a difference-in-differences matching estimator would require the additional assumption that the CIA also hold in the before period. The empirical evidence on the relative success of cross-sectional and difference-in-differences matching estimators in recovering estimates close to those provided by experiments is mixed. As an illustration of this, Heckman and Smith (1999) show that with enough covariates to make the CIA plausible, they are about the same in terms of performance.

At a practical level, using a series of binary models is more robust to errors since a mis-specification when modelling one pair of treatments will have less extensive consequences than in the simultaneous case. Arguments in favour of the multinomial approach are also at the practical level. Namely, there is less output to consider and it is more straightforward to observe the factors driving the overall selection process. This becomes an important consideration as the number of options increases.[6]

The result of estimating the series of logit models is a series of conditional probabilities for participating in a given option; $P^{l|ml}(X)$ in the notation of equation 5. These are the propensity scores that are used for matching. However, before that, the second stage is to ensure that, in each pairwise comparison of 'treatment' option with 'comparison' option, there is support for those individuals in the treatment option. This is operationalised by discarding those treated individuals whose propensity score lies outside the range bounded by the extrema of the corresponding propensity score among individuals in the comparison option. Consequently, effects are only estimated in regions of the attribute space where two observations from two options could be observed having a similar probability of participation. In this paper, when considering a particular option, those treated individuals who are unsupported in the pairwise comparisons with any of the other options are discarded. This means that $\theta^{m,l}(x)$ and $\theta^{m,k}(x)$ are estimated for a consistent group of individuals and thereby removes the possibility that differences

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[6] Lechner (2002) is the only paper I am aware of that compares matching estimates for a multiple treatment programme using propensity scores resulting from a multinomial model with estimates using propensity scores resulting from a series of binomial models. The comparison finds little difference in either the match quality or the resulting estimates. However, it is not possible to generalise from this single empirical result.

between these two estimated effects may be due to compositional differences in the

option *m* participants on which each effect is calculated.[7]

Having enforced the support requirement, the third stage is carry out the matching for all

pairwise combinations. This is done using the single nearest-neighbour technique which

involves finding for each treated individual that comparison individual with the most

similar propensity score (and, consequently, most similar characteristics).[8] This

procedure is usually implemented with replacement; each treated individual has one

match but a comparison individual may be matched to more than one treated individual.

Dehejia and Wahba (2002) find that allowing for comparison 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 comparison

group, the pool of comparators able to provide a close match would become exhausted

were matching carried out without replacement. The drawback to matching with

replacement is that the standard errors are more complicated to calculate and the variance

will be higher because fewer observations are being used for the implicit comparison

group.

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[7] See Lechner (2000) for a discussion of the common support problem.
[8] There are a number of possible alternative approaches to carrying out the matching (see, for example, Heckman et al., 1999). However, numerous investigations show the choice of approach to make little difference to the resulting estimates (see, for a recent example, Smith and Todd, 2002). To investigate sensitivity to type of matching, effects were also estimated using kernel methods. These are reported in section 6.

The fourth and final stage is to estimate the effects of each option relative to every other option. For those in option $m$, the mean effect of option $m$ rather than option $l$ is estimated as the mean difference in the outcome variable between individuals in option $m$ and the matched comparators in option $l$. The two main outcomes of interest are whether the individual is unemployed[9] and whether he is employed.

## 4. The data

The analysis uses administrative data for all males entering NDYP between September and November 1998, a total of 33,672 individuals. The administrative database is based on benefit payments records together with information entered by the personal adviser in the course of interviews with the job seeker and subsequent monitoring of that individual's progress through NDYP. This is the first time a British labour market evaluation has been based on population administrative data and it offers significant advantages compared with the use of sample data.[10] First, dealing with a population rather than a sample bypasses all considerations of non-random sampling and biases due to sample non-response and attrition. This both simplifies the analysis and increases the confidence we can have that the estimated effects are true population effects. Second, estimates are based on a greater number of observations than when working with samples. This means that effects can be more precisely estimated.

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[9] For the purposes of this analysis, individuals on NDYP are regarded as unemployed.
[10] For an evaluation of NDYP based on survey data see Bonjour et al. (2001).

Table 4.1 provides some simple descriptive statistics on the members of the options. The variation across options is considered more fully in section 5.1. However, it is evident from Table 4.1 that descriptive statistics are presented for five options rather than the four options discussed so far. This fifth option[11] arises from the fact that, in practice, many individuals in NDYP remained on the Gateway for longer than the maximum intended duration of four months and never entered one of the options. To provide some illustration of this, Figure 4.1 shows the number of days spent on the Gateway. While there is a marked spike in the distribution at the four month mark, many individuals clearly remain on the Gateway for longer than this, and in some cases considerably longer.

This tendency to overstay on the Gateway raises the interesting analytical possibility of considering, in addition to the relative effects of the NDYP options, the effects of these options relative to prolonged Gateway treatment. It is important to note that this brings us no nearer the situation of being able to capture the separate effect of Gateway since everybody considered will have had at least some experience of the Gateway. In fact, the effects of each option considered below are perhaps best viewed as being the combined effects of Gateway followed by the option. In the case of the Gateway overstayers, their treatment can be viewed as Gateway followed by more Gateway. Seen in this way, including the extended Gateway (EGW) as an additional option allows us to consider the effect of continuing in the Gateway rather than entering one of the options.

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[11] More correctly, this is a pseudo-option. However, for simplicity, it will be referred to as an option in the remainder of this paper.

The usefulness of this group is that its members can be viewed with some qualification as receiving no treatment at the option stage. Such evidence as is available suggests that the intensity of Gateway diminishes with time such 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 action starts and referrals grows initially with gateway duration but levels off after about 20 weeks. The approach taken in this analysis is to regard anybody staying in Gateway for more than 5 months (150 days) and not proceeding onto an option as a Gateway overstayer. This allows for some slippage (up to a month) in the intended four month duration and avoids dividing the sample at the peak evident in Figure 4.1. Hence, the Extended Gateway will comprise those individuals who do not enter an option but who overstay on the Gateway.[12]

It is interesting to consider how subsequent levels of unemployment vary by option. Table 4.2 shows on the diagonal the levels of unemployment (in May 2001[13]) by option and the unadjusted differences in the off-diagonal cells.[14] These are the differences one would expect were there no selection effect. They provide a useful context against which to consider the later results. From this we can see that 29.5 per cent of men in EMP were unemployed compared with 45.0 per cent in the FTET. The difference of 15.5 percentage points indicates that, in the absence of selection effects, EMP substantially reduced the chances of being unemployed in May 2001 compared with FTET.

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[12] This does not preclude the possibility of those in an option having stayed on the Gateway for more than five months.

[13] At the time of writing, the week commencing 28 May 2001 was the latest date for which unemployment information was available.

## 4.1 Characteristics influencing participation and outcomes

Matching estimates rely on the CIA so it is important to consider whether it is valid to make such an assumption. In this analysis, very rich data are available on the characteristics of the participant, the area in which he lives and the job centre he attends. This increases the confidence that all those factors affecting both participation and outcomes are observed. This is discussed below is in the context of the results of estimating option participation. These results are presented in Appendix Table 5.1, where each column gives the results of estimating the probability of being in one option for those who were in that option or in a specific one of the others. For example, the first column considers the probability of being in the employment option for all those in either EMP or FTET, the second considers the same probability for those in either EMP or VS, and so on. With five options to be considered, ten models must be estimated.[15]

Given the large number of variables and the multiple treatment nature of the programme, these results are extensive. As a general comment, it is apparent that there are a number of significant variables in each model. This indicates that there were important differences in the composition of the options and highlights the potential for matching to be effective. To gain a more detailed insight, the results of estimating option entry for those either in that option or in EGW[16] are considered. Hence, the discussion that follows concentrates on the differences across the true NDYP options (i.e. not EGW).

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[14] Note that the table is symmetric about the diagonal, albeit with reversed sign.
[15] The probability of being in option A for those in option A or B is equal to 1 minus the probability of being in option B for these same people. Hence, ten rather than twenty models are required.
[16] EGW is chosen as it is as close to a no-treatment group as possible.

These results are grouped by type of variable and there is particular emphasis on why it may be that a particular variable should affect outcomes. This follows from the fact that the results of the propensity score model show which variables are significantly associated with participation but not which influence outcomes. Because of this, for each variable it is important to think through whether it is likely to have an influence on outcomes. Furthermore, the factors associated with entry to EMP are likely to be similar to those associated with finding work after NDYP. This is due to the fact that participation in EMP is contingent on an employer's willingness to take on an NDYP applicant, albeit with a subsidy. Hence, considering whether a factor influences outcomes provides an insight into why that factor may also influence participation. It should be noted that all variables considered here are either constant over time (such as ethnicity), evolve in a way unaffected by participation (such as age) or are measured at a time predating participation (such as qualifications at time of Opportunity start).[17]

*Personal characteristics*

Since only men are considered in this analysis, the influence of gender on labour market outcomes is controlled for. Bonjour et al. (2001) found that there were variations in the gender mix across the options. Coupled with the observation that, for fertility or other reasons, labour market outcomes typically vary by gender, it is apparent that it is important to balance this characteristic across participants and non-participants. The population is also relatively homogenous with regard to age since NDYP only applies to

those aged between 18 and 24. Those closer to the upper end of this range may be more attractive to employers since they are more likely to have some useful work experience. In support of this, the data shows a definite age gradient in the type of work sought. Older workers are more likely to be seeking managerial, professional or technical jobs; these are occupations that are appropriate to those with previous employment experience. Hence, age is likely to be an important factor in finding work. The same reasoning applies when considering entry into EMP although, as noted in section 2, it may be general experience of working that is in demand rather than job-specific skills. Accordingly, the results show that younger participants were less likely to be in EMP (and were most likely to be in ETF).

Those with partners were more likely to be in FTET or ETF. It is not obvious why having a partner should be associated with participation in a particular option. However, with regard to outcomes, partnership status is relevant since family labour supply choices may differ from those of individuals (see, for example, Blundell and MaCurdy, 1983).

The data also records whether participants have a disability. This is based on the individual's own assessment that he has a physical or mental impairment that has a substantial and long-term effect on his ability to carry out normal day-to-day activites. Those with a such disability may be at a significant disadvantage in the labour market. If the disability affects the type or amount of work possible, individuals will necessarily be searching from a smaller pool of vacancies. Furthermore, discrimination by employers is

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[17] It is important to avoid balancing any characteristics that change as a result of participation, or that the individual might alter in anticipation of, or in order to change the probability of, entering a particular option

possible.  The results of the participation model show that disabled men were less likely to be in EMP and more likely to be in VS or ETF.

Employer discrimination may also be relevant when considering the labour market outcomes of ethnic minority participants.  Again, men from ethnic minorities were less likely to be in EMP.  They were most likely to select into FTET.  Interestingly, they were very unlikely to participate in ETF.

*Human capital*

Empirical studies routinely find education to be an important determinant of labour market success (see Arulampalam et al. (2000) for a recent British example).  Information on education is poorly recorded in the data; in 56 per cent of cases it is missing.  However, information is available on preferred occupation and this is closely related to education and qualifications.  To see this, note that the recorded occupational preferences are agreed with the personal adviser as being commensurate with what is achievable with regard to qualifications and experience.  In this way, preferred occupation provides a means of controlling for differences across the options in educational attainment and job-related experience.

*Early entrants*

As mentioned in section 1, some participants qualify for early entry to NDYP on the grounds of facing particular obstacles to employment.  However, this is relatively rare; only ten per cent of participants enter before the six-month stage of their unemployment

since this could bias estimates.

spell. The two main categories are those who qualify on the grounds of disability and those who qualify on the grounds of being an ex-offender. Variables were included in the participation models to control for this additional source of heterogeneity.

*Labour market experience*

There is reason to believe that the experience of unemployment is scarring in the sense that it adversely affects later success in the labour market (see, for example, Gregg, 2001). Furthermore, it is well established (Heckman et al., 1999) that controlling for individual labour market histories plays an extremely important role in capturing individual heterogeneity. In this evaluation, perfectly recorded unemployment histories extending back as far as January 1995 are available. These are used to derive summary measures of unemployment over the three and three-quarter years preceding NDYP entry. Such summary measures are very informative of individuals' overall disposition to the labour market and may reflect persistent attitudinal characteristics or some other long-standing barrier to employment.

They perform particularly strongly in the participation models. The results show participation in EMP to be associated with having a more favourable unemployment history; fewer claims, fewer days spent unemployed and a shorter qualifying spell. This is unsurprising given the role of employers in the selection into EMP. Conversely, ETF was associated with having the least favourable unemployment history. The situation for FTET and VS is more mixed.

*Geographic information*

The economy of Britain is characterised by considerable regional variation. In 2001 regional GDP ranged from £27.7bn in the North East to £140.3bn in Greater London (Office for National Statistics, 2003). There are also differences between cities and rural areas in the availability and nature of employment opportunities. Variables indicating region and rurality are included in the participation models to capture heterogeneity across geographical areas. This variation was found to be significant in the participation models, although the pattern revealed is not straightforward to interpret. There is also considerable variation within regions. To focus explicitly on economic heterogeneity, local unemployment rates at the time of NDYP entry were included in the models. These ranged from 1-14 per cent, and the estimation results show a greater tendency for those in areas of higher unemployment to enter an option rather than remain on the Gateway. While it is only possible to speculate, this may reflect a feeling among those in higher-unemployment areas that they require additional help to find employment and are unlikely to be successful if they rely on their own job search efforts.

*Delivery characteristics*

A particular characteristic of NDYP is that it aims at substantial local flexibility in delivery.[18] This is captured through a variable that measures the extent to which a particular 'Unit of Delivery' (UoD) allows those on NDYP to trial options and another variable that measures the job search intensity of the Gateway for that UoD. In addition, dummy variables are included to capture the effect of different partnership arrangements

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[18] It should be noted that the delivery model is exogenous to the individual jobseeker who has no choice over the partnership arrangements in place at his local office nor any other organisational practice.

for delivering NDYP at the local level.  The existence of such partnerships is a key organisational feature of NDYP.  Four types of delivery model exist:

1. a contract between the Employment Service (ES) and an individual provider (who may represent a number of other organizations)

2. a contract between the ES and a consortium whereby the consortium is responsible for most of the New Deal delivery

3. a Joint Partnership, in which all partners, including the ES, are equal

4. a contract between the ES and a private sector organisation, which leads the delivery of New Deal.

To the extent that this flexibility was designed to assess the most efficient means of delivering NDYP, it is expected that there would be some variations across delivery models in the effect on labour market outcomes.   The effects on participation are evident.  For all options, an increased tendency in the UoD to trial options was associated with participating in an option rather than remaining on the Gateway.  The results for the index of Gateway intensity are more mixed; it appears to be associated with entry to EMP and to ETF.  It is not clear how to interpret this result.  This is also true when considering the type of delivery model, despite a number of significant associations being found.

There are also variations across the UoDs with regard to the nature of the NDYP client group they serve.  Variables are included in the participation models that indicate the proportion of the NDYP clients who are from an ethnic minority,  the proportion who are

disabled and the average length of unemployment. High ethnic minority representation among the UoD clients was associated with reduced likelihood of an individual entering FTET or ETF while high levels of disability were associated with increased likelihood of entering EMP. Those individuals attending UoDs where the average claim lengths were longer were less likely to enter EMP, VS and, especially, ETF.

*Time of NDYP entry*

The final set of variables included in the participation models controls for the time of entering NDYP. Since there is a correlation between time of entering NDYP and time of leaving option, the inclusion of this variable partially controls for variations in outcomes over the business cycle. The pattern when examining the model results is clearest for FTET; participation in FTET becomes increasingly unlikely as time goes on. This may be due to the course starts being concentrated at certain points in the academic year. Using information on the mean delay between NDYP entry and option entry from Table 4.1, the dates considered in the model would map roughly onto the period mid-March to June. It is unsurprising that enrolments would decline approaching the summer months.

*Overall*

As shown above, a wealth of information is available at the level of the individual, the local area and the UoD. The analysis in this paper proceeds on the premise that, after controlling for the variables included in the participation models, there is little remaining unobserved heterogeneity that is systematically correlated with participation and

outcomes.  However, as with any identifying assumption, the CIA is ultimately

untestable.


**5. Results**


**5.1 Examining the performance of the match**


Figures 5.1 to 5.5 show the distributions of the propensity scores for each of the pairwise

comparisons.  The vertical lines in the charts show the region of common support.  From

this it is appears that few observations will be dropped after enforcing the support

requirement.  Table 5.1 shows that, in fact, less than 1 per cent of all observations are

dropped.  The level of rejections is not evenly distributed across the options.  Hardly any

of those in FTET are discarded for reasons of support, compared with 2.7 per cent of

those in EMP.


This level of rejections is very low and is therefore unlikely to compromise the

representativeness of the results.[19]  To demonstrate this, Table 5.2 shows the effect on the

unadjusted outcome levels of dropping these observations.   This follows an identical

format to Table 4.2 and differs only to the extent that the unadjusted levels are calculated

on the reduced sample.  The levels of unemployment are very similar to those shown in

Table 4.2.  As an overall comment the changes are sufficiently small as to be ignorable.

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[19] It should be noted that other evaluations of multiple treatment programmes have discarded much higher
proportions.  For example, Gerfin and Lechner (2002) rejected over 14 per cent of their sample, while
Frölich et al (2000) rejected over 27 per cent.  This is due, at least in part, to a more demanding
enforcement of the support requirement.

One consideration with single nearest neighbour matching with replacement is the possibility that some observations in the comparison group may be heavily used despite other similar observations being available. This may result in increased variance which could be avoided with only minimal adverse consequences for achieved balance. An insight into this is possible by considering the distribution of the matching weights. A summary measure is given in Table 5.3. Each cell relates to a difference pairwise match and shows the share of the largest 10 per cent of weights relative to the sum of the weights in the respective comparison group. As such, it indicates the extent to which the matched comparison group members are concentrated in a smaller number of individuals. There is limited empirical evidence against which to judge the results. However, the highest concentration of 35 per cent is comfortably lower than that given in Lechner (2002), Gerfin and Lechner (2002) or Frölich et al (2004).

The most important aspect of the performance of the match is the extent to which it manages to achieve balance in those variables included in the propensity score model. The extent to which the matching was successful in this regard is shown in Table 5.4. Here, cell (i, j) summarises the degree of similarity between those participating in option i and those in option j identified through matching as a comparison group. This measure of similarity is calculated over all variables included in the propensity score model following the method of Rosenbaum and Rubin (1985).[20] There is no available metric against which to judge the performance of the match but comparing the results to other

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[20] Precise details of the calculation of this diagnostic measure are provided in the footnote to Table 5.4.

studies (for example, Lechner, 2002, Gerfin and Lechner, 2002 or Frölich et al., 2004) suggests an adequate performance of the match.

**5.2 Unemployment effects**

Table 5.5 presents the estimated effects on unemployment. The outcome variable is whether the individual was unemployed at the time of the week commencing 28 May 2001. The figures emboldened on the leading diagonal in each of the panels in the table give the percentage in each option who were unemployed at this time. This was lowest among those in EMP. The entries in the off-diagonal cells show, for those in the option indicated by the row, how participating in that option affected their level of claiming compared to what it would have been had they participated in the option indicated by the column. These differences are given as percentage points and a positive entry indicates that those in the row option are more likely to be unemployed than they would be had they instead been in the column option.

To illustrate, the rate of unemployment among those in EMP was lower by about 12 percentage points than if they had participated instead in FTET. The results show that EMP dominates the other options. That is, members of any other option would have fared better had they instead been in EMP, and members of EMP would have fared worse had they not participated in that option. However, the comparison with EGW shows that, while EMP participants were less likely to be unemployed than had they remained on the Gateway, the benefit to those on EGW of participating instead in EMP was not

statistically significant.  Overall, EGW can be seen to dominate the options other than

EMP.  With regard to the three other options, there is little difference between FTET and

VS, but both of these options dominate ETF.

The results presented in Table 5.5 relate to the week commencing 28 May 2001.  It is

also interesting to consider the evolution of the effects over time and these are presented

in Figures 5.6 to 5.10.  All these graphs follow the same format which can be explained

by considering the first graph.  Here, participants in EMP are considered and the points

plotted give the relative effect of the other options estimated for each week from January

1999 onwards.  The y-axis gives the size of the effect in percentage points.  Obviously,

this is zero in the case of EMP, so this appears as a horizontal line.  The rightmost points

represent the week commencing 28 May 2001 and therefore correspond to the effects

presented in Table 5.5.  Only effects significant at the 95 per cent level are plotted.  The

difference between the graphs is that each one corresponds to the members of a different

option.

Taking Figures 5.6 to 5.10 as a whole, a number of points are evident.  First, EGW

increased the chances of exiting JSA in the short-run but the dominance of EMP was

apparent from April 2000 onwards.  The dominance of EMP over FTET, VS and ETF

grew until near the end of 1999, presumably as many employers chose to retain

subsidised workers beyond the six months for which the subsidy was available.  A period

of adjustment then ensued until the start of 2001, after which point the relative effects

remained stable and reflected the hierarchy evident from the snapshot results already

considered. EGW retained secondary dominance. Second, the differences between

FTET, VS and ETF are only ever quite small, although ETF emerges as the most poorly-

performing option. Third, although those in EMP benefited from that option, it appears

that those who remained on the Gateway would not have fared any better under EMP.

While this is the same result shown in Table 5.5, the consistency of this result over time

is striking.

Since the individuals considered entered NDYP between September and November 1998,

their status in May 2001 is some 18-20 months after this point. However, it is interesting

to consider their status relative to the time of option entry, in acknowledgement of the

fact that the timing of option entry varies across individuals. Table 5.6 shows the

estimated relative effects of the options on the probability of being unemployed one year

after entering the option. It should be noted that the distribution of option start dates

means that this is the longest period over which observations can be considered for the

full cohort. At this stage, levels of unemployment were higher across all options than

when observed in May 2001. EMP appears even more dominant and the secondary

dominance of EGW is likewise increased. However, no statistically significant

differences between FTET, VS and EGW are evident. It is plausible to believe that since

FTET can last for up to a year, its effect has had insufficient time at this stage to reveal

itself.

Some insight into the dynamics is possible by inspecting the changing effects over time.

Figures 5.11 to 5.15 show how the effect has developed since the time of option entry,

and extends the observation period beyond a year post-entry.[21] The design of NDYP is

strikingly evident from these graphs. In particular, there is a consistent discontinuity at

the six months point.[22] For those in EMP, Figure 5.11 shows that being in any of the

other options, but especially EGW, is associated with lower unemployment over the first

six months. However, as soon as this point is passed, the benefits of EMP are evident in

that any other option is associated with higher levels of unemployment. The second best

option appears to be EGW, although the three other options close the gap quite

considerably. A similar overall pattern can be seen in Figure 5.12 for those in FTET.

However, the dominance of FTET over ETF appears towards the end of the observation

period (about a year and a half after option entry). Figure 5.13 shows no difference

between FTET, VS and ETF for those in VS. As with FTET, it appears that those in ETF

may have fared better had they entered FTET (Figure 5.14). Interestingly, while those in

ETF would be less likely to be unemployed had they entered EMP or EGW, there appears

to be little to choose between these alternatives. Finally, Figure 5.15 shows that for those

in EGW, this may have been as good an option as EMP.


**5.3 Employment effects**

In addition to considering movements away from unemployment, the effect of the options

on employment is also of great policy relevance. However, this is complicated by the

fact that employment status is not observed in the available data. All that it is recorded is

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[21] A greater lag since option entry was not considered in Table 5.6 since not all individuals had been observed more than a year post-entry.
[22] This discontinuity is not evident in Figures 5.6-5.10 since the differences in start dates combined with the differing durations of the Gateway result in the discontinuity being smoothed.

whether an individual is claiming JSA and, for those who are no longer, what their destination was on leaving JSA. This destination information states whether they have entered work or entered some type of economic inactivity.[23] The problem arises since, as mentioned in the Introduction, this destination information is only partially recorded; in fact, in about a fifth of cases the destination on JSA exit is unknown.[24] Clearly, the effect on employment requires a variable indicating whether the individual is employed and this in turn requires an assumption to be made in those cases where destination is unobserved. The approach taken here is to construct variables indicating employment status under two extreme assumptions. In the first instance, it is assumed that *none* of those individuals who exited JSA to an unknown destination is in work. That is, a dummy variable was generated taking the value 1 when an individual is recorded as having left JSA to enter work, and 0 otherwise. In the alternative case, *all* those individuals who exited JSA to an unknown destination are assumed to be in work. That is, a dummy variable was generated taking the value 0 when an individual is claiming JSA or is recorded as having left JSA to some type of economic inactivity, and 1 otherwise. Bounding the analysis in such a way permits the identification of a range of estimates between which the true effect should lie.

Table 5.7 presents the resulting estimates of the effect of the options on employment as at 28 May 2001. The upper panel gives those results consistent with the assumption that no

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[23] That is, an activity other than employment or claiming JSA.
[24] It should be noted that even if an individual is recorded as entering employment on JSA exit, it is only possible to regard the individual as being in work in later periods if it is assumed that he has not become economically inactive in the intervening period. The converse applies for those recorded as leaving JSA to economic inactivity – they may have subsequently entered work. It should be further noted that this ambiguity relates only to movement between employment and inactivity – movement from employment to unemployment or from inactivity to unemployment are captured in the data.

exits are accounted for by employment unless explicitly stated, while the lower panel gives estimates based on the converse assumption. In some respects, the results are similar to those for the effect on unemployment. EMP emerges as the dominant option and EGW has secondary dominance. Of the remaining three options, there is nothing to choose between FTET and ETF. However, VS appears to dominate ETF (a difference in employment rates of about six percentage points) and also may slightly dominate FTET (a difference of between zero and three percentage points). Table 5.8 presents analogous results for employment one year after option entry. A qualitatively similar picture emerges, although the relative effects are generally larger in size.

While the effects on employment are similar to those for unemployment, a notable difference is evident in the comparison of those on EGW with those on EMP; although those in EGW were no more likely to have left unemployment had they instead entered EMP, the results show that participating in EMP would have increased their chances of finding work.

## 6. Assessing the results

### 6.1 Sensitivity of the results to the definition of EGW

Given the secondary dominance of EGW, it is worthwhile investigating the sensitivity of the results to the precise definition of this pseudo-option. As noted, the Gateway was intended to have a maximum duration of four months so, to allow for some slippage,

EGW was defined as comprising those option non-participants who remained on the Gateway for more than five months. To examine the robustness of the findings to the precise definition of EGW, a version of EGW was constructed using a six-month rather than a five-month threshold. As Table 6.1 shows, under this definition, the performance of EMP improves relative to EGW.[25]

## 6.2 Sensitivity of the results to choice of matching approach

To explore the sensitivity of the results to the type of matching, the effects were also estimated using kernel matching (see, for example, Heckman et al., 1997). Under this approach, a match is constructed for each participant using a weighted average over multiple non-participants where the weights are determined by the type of kernel and the choice of bandwidth. Following Black and Smith (Journal of Econometrics, 2004) who in turn follow Racine and Li (Journal of Econometrics, 2004), leave-one-out validation was used to select the bandwidth. This operates by taking a single non-participant and constructing a match from the remaining non-participants using kernel matching with a given bandwidth. This can provide an 'out-of-sample' forecast for the non-participant in question and, repeating this across all non-participants, allows the mean square error (MSE) for a particular outcome to be calculated. The bandwidth that minimises the MSE can therefore be identified and used for the later matching estimates. The results are shown in Table 6.2.

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[25] This improvement in the relative performance of EMP is not surprising since the change in definition of EGW effectively removed those Gateway 'overstayers' who were on the margin of exiting unemployment

Table 6.3 presents the unemployment effects estimated by kernel matching using an Epanechnikov kernel and the bandwidths given in Table 6.2. As an overall comment, the results are similar to those in Table 5.5. This provides some reassurance as to the robustness of the findings. The most notable difference is that those participating in EGW were significantly more likely to be unemployed than had they instead participated in EMP. The results under nearest neighbour matching, while of the same sign, were not significant. This higher level of precision under kernel matching is to be expected since a larger number of observations are used in computing the counterfactual. It can be seen that the t-statistics in Table 6.3 are consistently greater than in Table 5.5.

## 6.3 Sensitivity of the results to treatment of labour market history

An alternative method of controlling for heterogeneity in the labour market history of participants in different options is to construct sub-samples of individuals with similar labour market histories and to match within each of these subsamples. To do this, the sample was divided into five equal sized groups on the basis of their total time spent in unemployment since 1995. Within each quintile, matching was performed and the resulting matching weights were then used to derive new estimates of the relative effects of the options. The results are shown in the Table 6.4. The same qualitative picture as in Table 5.5 is evident. However, there are some differences. Notably, those participating in EGW were significantly more likely to be unemployed than had they instead participated in EMP.

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at the five-month point.

## 6.4 Sensitivity of the results to choice of matching variables

The variables used for matching are those which are felt to satisfy the CIA. However, it may be felt that the justification for including certain variables is questionable. To explore the sensitivity of the results to the choice of matching variables, the effects were estimated using a smaller set of matching variables. Two alternatives were considered.[26] First, those variables identifying the type of local delivery model were excluded. Second, those variables indicating week of entry were excluded. The results are presented in Tables 6.5 and 6.6 respectively. Once again, the same overall pattern emerges. Furthermore, in Table 6.5, those remaining on the Gateway would have been significantly less likely to be unemployed had they instead been in EMP.

## 6.5 Relative effects

It is instructive to consider what is driving the change over time in estimated effects. Since the effects are relative, any change could be caused by an improvement in the performance of one option or a worsening of the other. For example, does the declining relative performance of EMP over time indicate that the jobs secured by this means are short-term in nature or does it suggest that the other options are closing the gap? To gain some insight into this, it is revealing to examine the unadjusted levels of unemployment by option as depicted in Figure 6.1. This suggests that the narrowing of the differences between EMP and all other options may be explained by a combination of the two

reasons. The proportion unemployed among those in EMP drops sharply to begin with (due to those completing the six-month option being retained by their employers) and then appears to stabilise. By contrast, FTET, VS and ETF show a steady decline in the proportions unemployed. Those in EGW also show a decline in unemployment but this is initially at a much faster rate than for those in the other non-EMP options.

## 6.6 Comparing the matching results to simple differences

The results presented indicate that EMP is dominant and that EGW dominates the three other options. However, this is also the conclusion that would have been reached through a cursory inspection of the relative mean levels of the unadjusted outcome variables (Table 5.2 and Figure 6.1). In view of this coincidence of findings, it is interesting to consider the extent to which the estimated effects derived through matching differ from the simple differences.

In Table 6.7, the entry in each cell represents the difference between the unadjusted estimate of programme effect on unemployment as at 28 May 2001 (taken from Table 5.2) and the estimate derived through matching. Thus, for example, the effect of being in EMP compared to FTET was estimated simplistically at −15.2 percentage points. The matching estimate (for those in the employment option) was −12.2 percentage points. Hence, the overall effect of carrying out the matching was to reduce the apparent effect by 3 percentage points. In other words, once account is taken of the differences between

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[26] Clearly, sensitivity to the omission of other variables could be examined. However, the case for including the variables considered in this section is arguably more marginal than for some of the other

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the employment and education option participants, the estimated relative effect falls. This provides some support for the approach adopted here since some of these differences are substantial, most notably when considering ETF. In most cases, matching reduces the estimated absolute effect of treatment on the treated.

## 7. Summary and conclusion

The message that emerges from this analysis is that a period of subsidised employment is the most effective means of exiting unemployment and securing unsubsidised employment. Also, remaining on the Gateway is more effective than the other options of education, voluntary sector or environmental work. The differences between these last three options are more subtle. When considering unemployment at a point in time, there is little difference between FTET and VS but ETF performs least well in terms of encouraging unemployment exits. Hence, entrants to NDYP who enter the option stage are likely to remain unemployed longest if they enter ETF. Comparing outcomes relative to the time of option entry, there is little to choose between the three. However, the dominance of FTET over ETF is eventually established. It is not unexpected that the human capital investment of the education option takes some time to appear. Since FTET can last for up to a year, it will be very interesting to consider longer-term trends as later data becomes available. With regard to job entry, there is no difference between FTET and ETF. VS, on the other hand, certainly out-performs ETF and may also improve the chances of finding a job relative to FTET.

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variables, hence their relevance in this sensitivity analysis.

Consideration of the dynamics of the effects showed that the relative advantage of the employment option and the extended Gateway declined over time, but gravitated towards a stable level. There are two possible reasons for this. First, the decline in the relative advantage of these two options may be due to some of the unsubsidised jobs being only short-term in nature. Second, the decline may be due to a relative improvement in the outcomes of those in the education, voluntary sector and ETF options. While a cursory examination of the trends suggests an equilibrium may have been reached, it is conceivable that this may be disrupted if it is due to the first reason, and the second reason has yet to take effect. Hence, a payback to the education option may be observed at some point in the longer-term.

While it is perhaps unsurprising that a period of subsidised employment is the most effective means of achieving unsupported work (Atkinson and Meager (1994) found a similar result for the UK), the relative effectiveness of the extended Gateway is less expected. There are a number of possible reasons for the relative success of EGW. For example, participating in an option may reduce job search for the duration of the option, thus making it less likely to find work. Furthermore, the effects of (non-employment) option participation may be delayed, as already mentioned. It is also important to remember that the extended Gateway is not, in fact, a no-treatment group. Individuals who remain for an extended period on the Gateway may still participate in supervised job search and this may have positive employment effects (White and Lakey, 1992). However, the importance of considering this group is evident. Were the advantages of the employment option over the extended Gateway marginal, remaining on the Gateway

would be a more cost-effective option in view of its relative cheapness.  Indeed, the

results suggest that the benefits of EMP relative to EGW vary across option participants.

For example, participating in ETF reduced the chances of having exited unemployment

one year later relative to EMP or EGW by the same amount. This suggests that the most

cost-effective way of encouraging those in ETF to exit unemployment may be to allow

them to remain on the Gateway.

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**Table 4.1**: Descriptive statistics

| | EMP | FTET | VS | ETF | EGW | ALL |
|---|---|---|---|---|---|---|
| *Personal characteristics:* | | | | | | |
| Age on NDYP entry | 20.8 | 20.8 | 20.9 | 20.8 | 21.1 | 20.9 |
| | (2.0) | (2.0) | (2.0) | (2.0) | (2.0) | (2.0) |
| Has partner - % | 7.3 | 7.7 | 5.8 | 8.9 | 7.0 | 7.5 |
| Disability indicator - % | 13.2 | 15.2 | 15.9 | 13.6 | 12.2 | 14.0 |
| From ethnic minority - % | 11.3 | 21.2 | 20.7 | 8.4 | 22.5 | 18.1 |
| | | | | | | |
| *Unemployment history:* | | | | | | |
| Number of JSA claims since January 1995 | 4.3 | 4.7 | 4.8 | 5.4 | 4.9 | 4.9 |
| | (2.4) | (2.4) | (2.4) | (2.6) | (2.7) | (2.6) |
| Total days unemployed before NDYP | 597.4 | 681.3 | 733.7 | 772.4 | 749.7 | 713.1 |
| | (440.6) | (478.6) | (495.5) | (489.0) | (496.8) | (486.3) |
| Length of qualifying JSA claim (days) | 244.5 | 286.5 | 307.9 | 305.8 | 302.6 | 292.2 |
| | (229.7) | (270.6) | (293.9) | (278.9) | (295.8) | (278.4) |
| | | | | | | |
| *NDYP history:* | | | | | | |
| Days from NDYP entry to GW | 18.5 | 19.7 | 23.4 | 23.6 | 26.0 | 22.4 |
| | (30.2) | (30.2) | (35.2) | (35.8) | (34.3) | (33.1) |
| Days from GW to Option | 94.9 | 118.7 | 170.0 | 169.6 | | 136.1 |
| | (83.1) | (97.6) | (98.3) | (97.6) | | 100.0 |
| | | | | | | |
| *Region:* | | | | | | |
| Scotland - % | 16.5 | 11.3 | 13.9 | 14.9 | 10.8 | 12.7 |
| Northern  - % | 6.8 | 11.5 | 6.4 | 7.3 | 5.3 | 7.9 |
| North west  - % | 18.5 | 15.5 | 11.5 | 19.0 | 16.0 | 16.1 |
| Yorkshire/humberside  - % | 10.2 | 11.6 | 12.5 | 14.0 | 9.5 | 11.4 |
| Wales - % | 9.4 | 5.2 | 6.2 | 9.4 | 4.6 | 6.4 |
| West midlands  - % | 7.0 | 7.1 | 5.7 | 6.8 | 9.2 | 7.4 |
| East midlands & eastern  - % | 8.5 | 9.4 | 9.4 | 9.2 | 9.0 | 9.2 |
| South west  - % | 5.2 | 4.5 | 5.1 | 3.8 | 4.3 | 4.5 |
| London and south east - % | 17.9 | 24.0 | 29.3 | 15.6 | 31.2 | 24.4 |
| | | | | | | |
| *Area  characteristics:* | | | | | | |
| Rural area - % | 3.6 | 2.9 | 1.8 | 2.7 | 2.3 | 2.7 |
| Local unemployment rate at ND entry | 5.8 | 5.7 | 5.3 | 5.7 | 5.2 | 5.5 |
| | (2.1) | (2.0) | (1.8) | (2.0) | (1.9) | (2.0) |
| Number of observations | 2323 | 6559 | 2483 | 3762 | 5534 | 20661 |

Note: standard deviations in parentheses for continuous variables.  The local unemployment rate is at the level of the travel-to-work area.

**Table 4.2**: Levels of unemployment in week commencing 28 May 2001 and differences between options.

| Option | EMP | FTET | VS | ETF | EGW |
|--------|-----|------|-----|-----|-----|
| EMP | **29.5** | -15.5 | -17.2 | -22.4 | -9.1 |
| FTET | | **45.0** | -1.7 | -6.9 | 6.4 |
| VS | | | **46.7** | -5.2 | 8.1 |
| ETF | | | | **51.9** | 13.3 |
| EGW | | | | | **38.6** |

Note: the entries on the leading diagonal show the percentage of participants in each option who were claiming JSA in May 2001. The entries in the off-diagonal cells show differences between the options in these unemployment rates, expressed as percentage point differences: cell(i,j) = unemployment rate for option i less unemployment rate for option j.

**Table 5.1**: Observations dropped due to common support requirement

| | EMP | FTET | VS | ETF | EGW | Total |
|--------|-----|------|-----|-----|-----|-------|
| Observations before | 2317 | 6550 | 2483 | 3761 | 5528 | 20639 |
| Observations dropped | 62 | 19 | 43 | 16 | 46 | 186 |
| Percentage dropped | 2.7 | 0.3 | 1.7 | 0.4 | 0.8 | 0.9 |

**Table 5.2**: Levels of unemployment in week commencing 28 May 2001 and differences between options after enforcing support

| Option | EMP | FTET | VS | ETF | EGW |
|--------|-----|------|-----|-----|-----|
| EMP | **29.8** | -15.2 | -16.7 | -22.1 | -8.9 |
| FTET | | **45.0** | -1.5 | -6.9 | 6.3 |
| VS | | | **46.6** | -5.3 | 7.9 |
| ETF | | | | **51.9** | 13.2 |
| EGW | | | | | **38.7** |

See note to Table 4.2

**Table 5.3**: Concentration of the weights due to matching with replacement (%)

| Option | EMP | FTET | VS | ETF | EGW |
|--------|-----|------|-----|-----|-----|
| EMP | | 22 | 30 | 29 | 24 |
| FTET | 31 | | 31 | 34 | 28 |
| VS | 30 | 21 | | 28 | 23 |
| ETF | 31 | 25 | 31 | | 26 |
| EGW | 35 | 26 | 29 | 32 | |

Note: cell(i,j) relates to the match of individuals in treatment group i to those in comparison group j. The entry in each cell is a concentration ratio: the share of the total sum of weights in the respective comparison group accounted for by the largest decile of matching weights in that comparison group. This is expressed as a percentage.

**Table 5.4**: Covariate imbalance in the matched samples: mean standardised differences

|  | Comparison group: | | | | |
|---|---|---|---|---|---|
|  | EMP | FTET | VS | ETF | EGW |
| Treatment group: | | | | | |
| EMP |  | 1.9 | 3.0 | 2.2 | 1.9 |
| FTET | 2.2 |  | 1.8 | 1.9 | 1.6 |
| VS | 2.5 | 2.1 |  | 2.5 | 2.4 |
| ETF | 2.3 | 1.9 | 2.2 |  | 1.8 |
| EGW | 2.1 | 1.4 | 1.8 | 2.3 |  |

Note: the entry in each cell gives the mean standardised difference across those in the treatment group and those in the matched comparison group for all the variables included in the estimation of the propensity score. As such, it provides a summary measure of the degree of balance across the two groups - the difference expressed as a percentage of standard error. For each variable, the absolute difference in means across the two groups is divided by the square root of the average of the two associated variances and multiplied by 100. Averaging across all variables yields the entry in each cell.

**Table 5.5:** Estimated effects on unemployment in week commencing 28 May 2001

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **29.84** | -12.23** | -15.03** | -16.90** | -7.76** |
|  | - | (7.32) | (6.84) | (8.58) | (4.51) |
|  | - | [6.18] | [6.26] | [7.19] | [3.82] |
| FTET | 11.24** | **45.04** | -3.00 | -3.55* | 7.26** |
|  | (6.73) | - | (1.80) | (2.21) | (5.65) |
|  | [6.41] | - | [1.64] | [2.08] | [5.26] |
| VS | 14.04** | -0.16 | **46.58** | -4.33* | 9.35** |
|  | (6.75) | (0.10) | - | (2.17) | (5.59) |
|  | [6.22] | [0.08] | - | [1.99] | [4.65] |
| ETF | 15.94** | 5.18** | 4.00* | **51.91** | 10.74** |
|  | (8.16) | (3.48) | (2.05) | - | (7.05) |
|  | [7.58] | [2.87] | [1.84] | - | [6.31] |
| EGW | 3.45 | -7.92** | -8.64** | -9.96** | **38.72** |
|  | (1.76) | (6.13) | (5.20) | (6.23) | - |
|  | [1.81] | [6.02] | [5.02] | [5.55] | - |

Note: the entries in bold on the leading diagonal show the percentage of participants in each option who were claiming JSA in May 2001. The main entries in the off-diagonal cells give the matching estimates of the average effect of treatment on the treated. That is, for options i and j, cell(i,j) gives the percentage point mean difference in unemployment rates between participants in i and their matched comparators in j. Approximated t-statistics appear in parentheses below the estimated effects. These were computed using the variance approximations given in Lechner (2001). The entries in brackets are the t-statistics implied by the bootstrapped standard errors (250 replications). In view of the close similarity between these approximations and the bootstrapped results, only the approximations are given in later Tables.
* denotes significant at 5% level (2-sided test); ** denotes significant at 1% level.

**Table 5.6:** Estimated effects on unemployment one year after option entry

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **34.82** | -21.17** | -19.60** | -18.00** | -4.60** |
|  | - | (12.08) | (8.44) | (8.64) | (2.62) |
| FTET | 20.86** | **59.56** | -1.12 | -0.61 | 16.23** |
|  | (12.13) | - | (0.65) | (0.37) | (12.33) |
| VS | 18.66** | -1.06 | **58.55** | -2.33 | 15.59** |
|  | (8.46) | (0.62) | - | (1.12) | (9.01) |
| ETF | 15.96** | 0.83 | 0.66 | **62.27** | 15.88** |
|  | (7.82) | (0.56) | (0.33) | - | (10.29) |
| EGW | 2.75 | -17.37** | -15.37** | -14.10** | **43.43** |
|  | (1.40) | (13.27) | (8.84) | (8.32) | - |

See note to Table 5.5 for format of table. The outcome variable in this case is whether the individual was claiming JSA one year after entering his option.

**Table 5.7:** Estimated effects on employment in week commencing 28 May 2001

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| *Assuming no unknown exits are to employment:* | | | | | |
| EMP | **47.14** | 26.72** | 21.91** | 25.90** | 20.40** |
|  | - | (17.54) | (10.63) | (14.72) | (11.95) |
| FTET | -23.51** | **20.99** | -3.08* | 0.54 | -4.25** |
|  | (14.06) | - | (2.17) | (0.41) | (3.72) |
| VS | -19.05** | 3.49** | **23.86** | 5.76** | -0.66 |
|  | (9.20) | (2.58) | - | (3.58) | (0.46) |
| ETF | -22.18** | -1.13 | -5.71** | **18.78** | -4.24** |
|  | (11.91) | (0.98) | (3.47) | - | (3.40) |
| EGW | -18.20** | 4.03** | -0.21 | 4.49** | **24.06** |
|  | (9.55) | (3.78) | (0.14) | (3.55) | - |
| *Assuming all unknown exits are to employment:* | | | | | |
| EMP | **57.16** | 28.53** | 23.68** | 27.85** | 14.28** |
|  | - | (17.64) | (10.95) | (14.75) | (7.91) |
| FTET | -23.78** | **30.41** | -1.39 | 1.84 | -13.17** |
|  | (13.95) | - | (0.89) | (1.25) | (10.28) |
| VS | -23.22** | 1.68 | **31.86** | 6.36** | -9.14** |
|  | (10.88) | (1.10) | - | (3.56) | (5.57) |
| ETF | -23.96** | -1.48 | -5.58** | **26.77** | -12.62** |
|  | (12.33) | (1.13) | (3.10) | - | (8.71) |
| EGW | -11.69** | 12.04** | 9.37** | 12.48** | **42.02** |
|  | (5.92) | (9.89) | (5.86) | (8.01) | - |

See note to Table 5.5 for format of table. The outcome variable in this case is whether the individual was employed in May 2001. To address the issue of unknown destinations on JSA exit, results are presented under two assumptions. In the upper panel an individual is regarded as employed in May 2001 if he is not claiming JSA and it is recorded that he left JSA to enter work. In the lower panel, an individual is regarded is regarded as employed in May 2001 if he is not claiming JSA and either it is recorded that he entered work on leaving JSA or his destination on leaving JSA was not recorded. Computationally, the outcome variable is straightforwardly constructed to accord with these rules.

**Table 5.8:** Estimated effects on employment one year after option entry

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| *Assuming no unknown exits are to employment:* | | | | | |
| EMP | **47.72** | 33.78** | 28.03** | 33.26** | 22.75** |
| | - | (23.85) | (14.19) | (20.44) | (13.40) |
| FTET | -30.78** | **13.38** | -3.05* | -0.12 | -9.55** |
| | (18.76) | - | (2.50) | (0.11) | (8.83) |
| VS | -24.67** | 4.35** | **16.85** | 5.48** | -5.29** |
| | (12.26) | (3.78) | - | (3.95) | (3.91) |
| ETF | -25.65** | 0.10 | -3.03* | **12.34** | -8.73** |
| | (14.46) | (0.11) | (2.22) | - | (7.37) |
| EGW | -20.75** | 10.33** | 5.07** | 8.79** | **22.13** |
| | (10.82) | (11.61) | (3.94) | (7.66) | - |
| *Assuming all unknown exits are to employment:* | | | | | |
| EMP | **55.08** | 36.70** | 28.91** | 32.64** | 13.92** |
| | - | (24.57) | (13.89) | (17.83) | (7.70) |
| FTET | -32.72** | **19.03** | -3.32* | -0.01 | -21.08** |
| | (19.49) | - | (2.41) | (0.00) | (17.10) |
| VS | -27.61** | 4.63** | **23.17** | 7.03** | -14.56** |
| | (13.13) | (3.50) | - | (4.50) | (9.22) |
| ETF | -26.45** | 0.60 | -3.41* | **18.41** | -18.09** |
| | (14.10) | (0.54) | (2.14) | - | (12.97) |
| EGW | -11.45** | 21.24** | 15.81** | 18.65** | **39.30** |
| | (5.81) | (19.75) | (10.74) | (12.71) | - |

See note to Table 5.7 for format of table.  The outcome variable in this case is whether the individual was employed one year after entering his option.

**Table 6.1:** Estimated effects on unemployment in week commencing 28 May 2001 and unemployment one year after option entry using a 6 month threshold for the definition of EGW

| $\theta$ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| *Unemployed 28 May 2001* | | | | | |
| EMP | **29.83** | -12.23** | -15.12** | -16.89** | -9.26** |
|  | - | (7.31) | (6.85) | (8.58) | (4.92) |
| FTET | 11.25** | **45.04** | -3.00 | -3.56* | 6.72** |
|  | (6.75) | - | (1.80) | (2.22) | (4.92) |
| VS | 13.96** | -0.25 | **46.55** | -4.33* | 8.10** |
|  | (6.71) | (0.15) | - | (2.17) | (4.57) |
| ETF | 15.94** | 5.26** | 4.00* | **51.91** | 11.00** |
|  | (8.16) | (3.54) | (2.05) | - | (6.92) |
| EGW | 6.79** | -5.29** | -7.75** | -9.63** | **39.78** |
|  | (3.43) | (3.92) | (4.43) | (5.71) | - |
| *Unemployed 1 year post option entry* | | | | | |
| EMP | **34.80** | -21.18** | -19.69** | -17.99** | -7.80** |
|  | - | (12.09) | (8.45) | (8.64) | (4.00) |
| FTET | 20.80** | **59.55** | -1.12 | -0.62 | 13.95** |
|  | (12.10) | - | (0.65) | (0.37) | (9.89) |
| VS | 18.58** | -1.14 | **58.54** | -2.33 | 13.21** |
|  | (8.43) | (0.67) | - | (1.12) | (7.21) |
| ETF | 15.96** | 0.79 | 0.66 | **62.27** | 15.29** |
|  | (7.82) | (0.53) | (0.33) | - | (9.42) |
| EGW | 4.97* | -15.15** | -13.88** | -14.12** | **45.60** |
|  | (2.33) | (11.02) | (7.59) | (7.97) | - |

See note to Table 5.7 for format of table.

**Table 6.2:** Bandwidth identified through leave-one-out validation

| Option | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP |  | 0.1 | 0.2 | 0.1 | 0.1 |
| FTET | 0.1 |  | 1 | 0.3 | 0.3 |
| VS | 0.2 | 0.1 |  | 0.1 | 0.1 |
| ETF | 0.2 | 0.2 | 0.3 |  | 0.1 |
| EGW | 0.2 | 0.2 | 0.2 | 0.2 |  |

Note: the entries in each cell show the bandwidth that was found to minimise mean square error in the leave-one-out validation process. The bandwidth in cell(i,j) was used in the kernel matching estimates (presented in Table 6.3) of the effect of participating in option i rather than option j for those in option i.

**Table 6.3:** Estimated effects on unemployment in week commencing 28 May 2001 - kernel matching

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **29.77** | -12.92** | -14.30** | -18.25** | -6.69** |
|  | - | (11.60) | (10.00) | (12.50) | (5.68) |
| FTET | 12.47** | **45.06** | -1.61 | -5.73** | 6.48** |
|  | (9.09) | - | (1.33) | (4.79) | (6.68) |
| VS | 13.89** | 0.79 | **46.66** | -3.75** | 8.54** |
|  | (8.48) | (0.67) | - | (2.68) | (6.86) |
| ETF | 17.25** | 4.79** | 3.85** | **51.92** | 11.48** |
|  | (12.41) | (4.80) | (2.81) | - | (9.33) |
| EGW | 5.98** | -6.97** | -8.64** | -11.63** | **38.69** |
|  | (4.66) | (7.07) | (7.05) | (9.77) | - |

See note to Table 5.5 for format of table. Estimates used the Epanechnikov kernel with the bandwidth chosen by leave-one-out validation as given in Table 6.2 The t-statistics implied by the bootstrapped standard errors (200 replications) appear in parentheses below the estimated effects.

**Table 6.4:** Estimated effects on unemployment in week commencing 28 May 2001 - propensity score matching within quintile of total unemployment experience

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **29.52** | -14.14** | -12.51** | -18.31** | -4.26* |
|  | - | (8.47) | (5.84) | (9.35) | (2.45) |
| FTET | 11.02** | **45.00** | -4.64* | -3.22 | 8.32** |
|  | (6.48) | - | (2.56) | (1.87) | (5.86) |
| VS | 12.76** | 0.26 | **46.70** | -2.22 | 8.98** |
|  | (6.13) | (0.14) | - | (1.04) | (4.88) |
| ETF | 16.29** | 5.17** | 2.98 | **51.88** | 11.06** |
|  | (8.36) | (3.15) | (1.38) | - | (6.74) |
| EGW | 6.26** | -8.97** | -8.48** | -10.48** | **38.59** |
|  | (3.35) | (6.35) | (4.56) | (6.02) | - |

See note to Table 5.5 for format of table. Heterogeneity of unemployment experience was accounted for in this case by matching separately within each quintile of total number of days spent unemployed prior to NDYP entry and then accumulating the resulting weights to allow an effect across all quintiles to be estimated.

**Table 6.5:** Estimated effects on unemployment in week commencing 28 May 2001, omitting indicators of local delivery model from matching variables

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **29.77** | -12.73** | -14.13** | -16.41** | -5.80** |
|  | - | (7.72) | (6.60) | (8.47) | (3.36) |
| FTET | 12.46** | **45.06** | -3.30 | -3.91* | 6.61** |
|  | (7.50) | - | (1.95) | (2.36) | (5.20) |
| VS | 12.40** | -0.77 | **46.66** | -2.51 | 10.41** |
|  | (5.77) | (0.47) | - | (1.26) | (6.29) |
| ETF | 16.85** | 2.85 | 4.71* | **51.92** | 10.18** |
|  | (8.74) | (1.91) | (2.38) | - | (6.74) |
| EGW | 4.46* | -7.72** | -9.63** | -11.38** | **38.69** |
|  | (2.35) | (6.06) | (5.80) | (7.10) | - |

See note to Table 5.5. The results in this table are based on a reducing set of matching variables – those relating to the delivery model are omitted.

**Table 6.6:** Estimated effects on unemployment in week commencing 28 May 2001, omitting dummy variables for week of entry

| θ | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP | **29.88** | -12.08** | -13.78** | -17.82** | -6.39** |
|  | - | (7.33) | (6.33) | (9.06) | (3.67) |
| FTET | 13.83** | **45.03** | -0.89 | -5.68** | 6.08** |
|  | (8.48) | - | (0.55) | (3.59) | (4.82) |
| VS | 12.08** | -1.29 | **46.61** | -5.11** | 8.47** |
|  | (5.70) | (0.78) | - | (2.60) | (5.10) |
| ETF | 18.65** | 4.39** | 4.41* | **51.87** | 12.03** |
|  | (9.81) | (2.95) | (2.24) | - | (7.90) |
| EGW | 2.38 | -6.47** | -8.80** | -11.32** | **38.70** |
|  | (1.22) | (5.07) | (5.29) | (7.14) | - |

See note to Table 5.5. The results in this table are based on a reducing set of matching variables – those relating to the week of entry are omitted.

**Table 6.7:** How the unadjusted differences differ from the matching results (unemployment in week commencing 28 May 2001)

|  | EMP | FTET | VS | ETF | EGW |
|---|---|---|---|---|---|
| EMP |  | -3.0 | -1.7 | -5.2 | -1.1 |
| FTET | 4.0 |  | 1.5 | -3.3 | -0.9 |
| VS | 2.7 | 1.7 |  | -1.0 | -1.5 |
| ETF | 6.2 | 1.7 | 1.3 |  | 2.5 |
| EGW | 5.4 | 1.6 | 0.7 | -3.2 |  |

Note: for option i and option j, cell(i,j) in this table gives the difference between cell(i,j) in Table 5.2 and the estimated effect in cell(i,j) of Table 5.5. Entries in grey correspond to those comparisons for which the respective effect in Table 5.5 was not significant at the 5 per cent level.

**Appendix Table 5.1**: Results of estimating participation in option i for those participants in option i or option j

| | Prob of EMP for those in EMP or: | | | | Prob FTET for FTET or: | | | Prob VS, VS or: | | Pr ETF: |
|---|---|---|---|---|---|---|---|---|---|---|
| | FTET | VS | ETF | EGW | VS | ETF | EGW | ETF | EGW | EGW |
| *Personal characteristics:* | | | | | | | | | | |
| Aged 19 years at ND entry | 0.164 | 0.379 | 0.323 | 0.039 | 0.212 | 0.152 | -0.097 | -0.055 | -0.275 | -0.225 |
| | (1.80) | (3.31) | (3.19) | (0.40) | (2.38) | (1.93) | (1.36) | (0.54) | (2.97) | (2.76) |
| Aged 20 years at ND entry | 0.287 | 0.619 | 0.712 | 0.411 | 0.311 | 0.359 | 0.101 | 0.075 | -0.176 | -0.213 |
| | (3.03) | (5.18) | (6.66) | (4.01) | (3.35) | (4.32) | (1.34) | (0.70) | (1.80) | (2.43) |
| Aged 21 years at ND entry | 0.304 | 0.539 | 0.751 | 0.279 | 0.295 | 0.428 | 0.041 | 0.131 | -0.213 | -0.341 |
| | (2.90) | (4.14) | (6.36) | (2.50) | (2.95) | (4.71) | (0.51) | (1.14) | (2.04) | (3.64) |
| Aged 22 years at ND entry | 0.572 | 0.770 | 1.177 | 0.540 | 0.228 | 0.555 | 0.002 | 0.338 | -0.253 | -0.525 |
| | (5.36) | (5.81) | (9.51) | (4.78) | (2.19) | (5.69) | (0.02) | (2.77) | (2.35) | (5.26) |
| Aged 23 years at ND entry | 0.557 | 0.805 | 1.188 | 0.295 | 0.274 | 0.558 | -0.201 | 0.381 | -0.406 | -0.750 |
| | (4.97) | (5.69) | (9.18) | (2.53) | (2.49) | (5.47) | (2.34) | (2.98) | (3.62) | (7.34) |
| Aged 24 years or over at ND entry | 0.475 | 0.757 | 1.126 | 0.274 | 0.258 | 0.631 | -0.213 | 0.389 | -0.430 | -0.799 |
| | (4.18) | (5.28) | (8.51) | (2.31) | (2.30) | (6.02) | (2.44) | (2.94) | (3.74) | (7.54) |
| Partnered | -0.154 | 0.172 | -0.293 | 0.010 | 0.305 | -0.056 | 0.176 | -0.417 | -0.150 | 0.297 |
| | (1.58) | (1.34) | (2.71) | (0.10) | (3.02) | (0.70) | (2.39) | (3.73) | (1.44) | (3.56) |
| Disability indicator | -0.381 | -0.485 | -0.415 | -0.335 | -0.130 | -0.016 | 0.111 | 0.112 | 0.248 | 0.154 |
| | (4.54) | (4.79) | (4.40) | (3.70) | (1.81) | (0.24) | (1.85) | (1.36) | (3.29) | (2.22) |
| Ethnic minority | -0.522 | -0.225 | 0.213 | -0.387 | 0.259 | 0.683 | 0.159 | 0.511 | -0.106 | -0.535 |
| | (6.00) | (2.14) | (1.98) | (4.43) | (3.47) | (8.64) | (2.86) | (5.29) | (1.44) | (6.89) |
| *Human capital - preferred occupation:* | | | | | | | | | | |
| Managers & administrators | -0.046 | -0.404 | 1.253 | 0.153 | -0.448 | 1.306 | 0.150 | 1.780 | 0.548 | -1.204 |
| | (0.17) | (1.28) | (2.99) | (0.56) | (1.78) | (3.51) | (0.74) | (4.41) | (2.20) | (3.23) |
| Professional | -0.612 | -1.038 | 0.323 | 0.362 | -0.386 | 0.791 | 0.750 | 1.166 | 1.124 | 0.103 |
| | (2.30) | (3.35) | (0.93) | (1.19) | (1.74) | (2.98) | (3.68) | (3.90) | (4.59) | (0.35) |
| Associate prof & technical | -0.376 | -0.584 | 0.577 | 0.236 | -0.197 | 0.988 | 0.512 | 1.143 | 0.648 | -0.390 |
| | (3.16) | (3.99) | (3.65) | (1.82) | (1.77) | (7.94) | (5.72) | (7.77) | (5.55) | (2.96) |
| Clerical & secretarial | -0.233 | -0.569 | 0.393 | 0.242 | -0.324 | 0.599 | 0.401 | 0.891 | 0.727 | -0.167 |
| | (3.03) | (6.09) | (4.47) | (2.97) | (4.51) | (9.12) | (7.01) | (11.01) | (9.88) | (2.44) |
| Craft & related | -0.001 | 0.283 | 0.164 | 0.270 | 0.270 | 0.185 | 0.238 | -0.114 | -0.054 | 0.056 |
| | (0.01) | (2.77) | (2.00) | (3.42) | (3.16) | (2.93) | (4.06) | (1.27) | (0.62) | (0.87) |
| Personal & protective services | -0.351 | -0.631 | 0.524 | 0.110 | -0.259 | 0.820 | 0.386 | 1.073 | 0.637 | -0.370 |
| | (3.10) | (4.58) | (3.84) | (0.91) | (2.57) | (7.98) | (4.73) | (8.68) | (6.08) | (3.42) |
| Sales | -0.428 | -0.763 | 0.487 | 0.216 | -0.354 | 0.838 | 0.554 | 1.180 | 0.897 | -0.216 |
| | (4.62) | (7.01) | (4.41) | (2.15) | (4.46) | (10.22) | (8.34) | (12.16) | (10.71) | (2.46) |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Plant & machine operators | 0.198 | 0.048 | 0.310 | 0.307 | -0.178 | 0.140 | 0.116 | 0.277 | 0.238 | -0.022 |
| | (2.20) | (0.42) | (3.21) | (3.31) | (1.86) | (1.81) | (1.61) | (2.76) | (2.48) | (0.28) |
| *Early entrants - type of NDYP eligibility:* | | | | | | | | | | |
| Disability | 0.207 | 0.769 | 1.175 | 1.089 | 0.492 | 0.930 | 0.831 | 0.314 | 0.258 | -0.053 |
| | (1.51) | (4.05) | (6.02) | (6.30) | (3.12) | (5.55) | (5.93) | (1.47) | (1.38) | (0.27) |
| Literacy/ numeracy problem | 0.026 | 0.233 | 0.969 | 0.797 | 0.254 | 1.056 | 0.845 | 0.676 | 0.536 | -0.100 |
| | (0.12) | (0.81) | (3.18) | (2.98) | (1.09) | (3.95) | (3.94) | (2.06) | (1.95) | (0.33) |
| English $2^{nd}$ language | -0.390 | 2.284 | 1.193 | 1.422 | 2.115 | 1.523 | 1.476 | -0.784 | -0.157 | 0.249 |
| | (0.69) | (1.95) | (1.02) | (1.89) | (2.05) | (1.47) | (2.67) | (0.54) | (0.14) | (0.22) |
| Ex-regular | -0.259 | 1.045 | 0.541 | 0.122 | 1.144 | 1.006 | 0.153 | -0.759 | -1.113 | -0.715 |
| | (0.73) | (1.50) | (1.02) | (0.31) | (1.86) | (2.14) | (0.51) | (0.99) | (1.77) | (1.49) |
| Ex-offender | -0.068 | 0.654 | 0.399 | 0.389 | 0.644 | 0.423 | 0.438 | -0.232 | -0.309 | 0.042 |
| | (0.49) | (3.20) | (2.42) | (2.55) | (3.75) | (3.24) | (3.76) | (1.18) | (1.70) | (0.30) |
| Labour market returner | 0.905 | 1.520 | 0.523 | -0.257 | 0.228 | -0.489 | -0.825 | -0.321 | -0.961 | -0.597 |
| | (1.17) | (1.24) | (0.53) | (0.39) | (0.20) | (0.54) | (1.29) | (0.25) | (0.88) | (0.73) |
| Large scale redundancy | -0.020 | | 0.846 | -0.087 | | 1.219 | -0.131 | | | -1.681 |
| | (0.03) | | (0.76) | (0.14) | | (1.11) | (0.24) | | | (1.52) |
| Local authority care | -0.074 | 0.889 | 1.479 | 1.214 | 0.847 | 1.463 | 1.020 | 0.863 | 0.231 | -0.303 |
| | (0.16) | (1.27) | (2.06) | (2.08) | (1.52) | (2.28) | (2.14) | (1.07) | (0.35) | (0.42) |
| 28-day rule | 0.565 | 0.397 | 0.795 | 1.067 | -0.137 | 0.243 | 0.618 | 0.444 | 0.694 | 0.338 |
| | (2.15) | (1.21) | (2.58) | (3.28) | (0.47) | (0.87) | (2.15) | (1.32) | (2.02) | (1.04) |
| 6 month + stock | -0.080 | -0.078 | -0.005 | 0.550 | 0.097 | 0.124 | 0.610 | 0.055 | 0.432 | 0.577 |
| | (0.31) | (0.26) | (0.02) | (1.88) | (0.45) | (0.64) | (3.09) | (0.22) | (1.78) | (2.51) |
| ES discretion | -0.227 | 0.351 | 0.410 | 0.481 | 0.841 | 0.825 | 0.770 | 0.065 | -0.086 | 0.005 |
| | (1.01) | (1.06) | (1.40) | (1.81) | (3.13) | (3.66) | (3.88) | (0.20) | (0.28) | (0.02) |
| *Labour market experience & NDYP history:* | | | | | | | | | | |
| No. JSA claims from Jan 95 to ND entry | -0.101 | -0.115 | -0.176 | -0.114 | -0.028 | -0.079 | -0.025 | -0.064 | -0.001 | 0.055 |
| | (8.16) | (7.63) | (12.99) | (9.30) | (2.52) | (8.23) | (2.94) | (4.97) | (0.06) | (5.91) |
| Total days unemployed before ND§ | -0.039 | -0.060 | -0.089 | -0.053 | -0.021 | -0.042 | -0.013 | -0.024 | 0.008 | 0.030 |
| | (4.78) | (6.04) | (9.63) | (6.35) | (2.77) | (6.07) | (2.25) | (2.76) | (1.11) | (4.37) |
| Duration of current claim at ND entry§ | -0.054 | -0.075 | -0.089 | -0.056 | -0.027 | -0.034 | -0.005 | -0.011 | 0.013 | 0.027 |
| | (3.93) | (4.65) | (5.93) | (4.06) | (2.59) | (3.50) | (0.66) | (0.93) | (1.21) | (2.88) |
| Time from ND entry to Gateway§ | -0.018 | -0.229 | -0.278 | -0.592 | -0.254 | -0.335 | -0.489 | 0.000 | -0.170 | -0.201 |
| | (0.21) | (2.22) | (2.87) | (5.59) | (3.52) | (4.91) | (7.73) | (0.00) | (2.17) | (2.92) |
| *Goegraphic information:* | | | | | | | | | | |
| Scotland | 0.220 | 0.033 | 0.286 | 0.408 | 0.023 | 0.123 | 0.225 | 0.101 | 0.240 | 0.129 |
| | (1.98) | (0.24) | (2.28) | (3.53) | (0.21) | (1.25) | (2.59) | (0.83) | (2.16) | (1.31) |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Northern | -0.698 | -0.354 | -0.022 | -0.006 | 0.538 | 0.792 | 0.757 | 0.223 | 0.271 | 0.072 |
| | (5.18) | (2.03) | (0.14) | (0.04) | (4.08) | (6.95) | (7.39) | (1.46) | (1.92) | (0.60) |
| North west | 0.076 | 0.398 | -0.020 | 0.136 | 0.404 | -0.094 | 0.020 | -0.553 | -0.319 | 0.237 |
| | (0.71) | (3.01) | (0.17) | (1.28) | (3.81) | (1.02) | (0.26) | (4.70) | (3.06) | (2.67) |
| Yorkshire & Humberside | -0.291 | -0.345 | -0.304 | -0.024 | 0.009 | 0.040 | 0.200 | -0.047 | 0.231 | 0.323 |
| | (2.27) | (2.23) | (2.09) | (0.18) | (0.08) | (0.38) | (2.13) | (0.36) | (1.93) | (3.01) |
| Wales | 0.335 | 0.230 | 0.042 | 0.656 | -0.053 | -0.302 | 0.259 | -0.324 | 0.322 | 0.664 |
| | (2.75) | (1.54) | (0.31) | (5.11) | (0.42) | (2.82) | (2.50) | (2.44) | (2.51) | (6.13) |
| West midlands | 0.255 | 0.218 | -0.042 | 0.031 | 0.190 | -0.168 | -0.152 | -0.441 | -0.307 | 0.199 |
| | (1.94) | (1.31) | (0.28) | (0.23) | (1.45) | (1.47) | (1.55) | (2.94) | (2.26) | (1.75) |
| East midlands & eastern | -0.107 | 0.012 | 0.039 | 0.218 | 0.269 | 0.200 | 0.311 | -0.060 | 0.051 | 0.217 |
| | (0.92) | (0.08) | (0.29) | (1.84) | (2.52) | (2.03) | (3.74) | (0.49) | (0.47) | (2.22) |
| South west | -0.207 | -0.044 | 0.296 | 0.147 | 0.192 | 0.542 | 0.387 | 0.244 | 0.194 | -0.002 |
| | (1.41) | (0.25) | (1.74) | (0.97) | (1.37) | (4.02) | (3.41) | (1.51) | (1.37) | (0.02) |
| Rural area | 0.197 | 0.680 | 0.523 | 0.231 | 0.470 | 0.333 | 0.075 | -0.170 | -0.403 | -0.233 |
| | (1.34) | (3.21) | (3.03) | (1.45) | (2.64) | (2.43) | (0.61) | (0.86) | (2.15) | (1.60) |
| Local unemployment rate at ND entry | 0.061 | 0.127 | 0.033 | 0.156 | 0.067 | -0.032 | 0.107 | -0.102 | 0.044 | 0.133 |
| | (3.37) | (5.49) | (1.68) | (8.27) | (3.51) | (1.99) | (7.21) | (4.78) | (2.28) | (8.05) |
| *Delivery characteristics at UoD level:* | | | | | | | | | | |
| Index of ethnicity in ND inflow | 0.028 | -0.069 | 0.024 | -0.077 | -0.048 | 0.018 | -0.078 | 0.058 | -0.011 | -0.123 |
| | (0.62) | (1.23) | (0.45) | (1.64) | (1.15) | (0.45) | (2.39) | (1.13) | (0.25) | (3.04) |
| Index of disability in ND inflow | 0.223 | 0.197 | 0.119 | 0.186 | 0.040 | -0.095 | -0.030 | -0.103 | -0.057 | 0.033 |
| | (4.35) | (3.07) | (2.03) | (3.44) | (0.77) | (2.04) | (0.75) | (1.73) | (1.08) | (0.69) |
| Index of unemployment length in ND inflow | -0.136 | 0.026 | 0.131 | -0.092 | 0.167 | 0.272 | 0.025 | 0.095 | -0.154 | -0.257 |
| | (3.59) | (0.54) | (3.05) | (2.27) | (4.52) | (8.04) | (0.87) | (2.20) | (3.99) | (7.34) |
| Index of option 'trialling' | 0.036 | 0.046 | -0.039 | 0.098 | -0.018 | -0.064 | 0.070 | -0.060 | 0.119 | 0.102 |
| | (0.86) | (0.89) | (0.85) | (2.27) | (0.46) | (1.81) | (2.19) | (1.35) | (2.87) | (2.87) |
| Index of Gateway 'intensity' | 0.117 | 0.154 | 0.022 | 0.161 | 0.013 | -0.112 | 0.032 | -0.104 | 0.023 | 0.125 |
| | (2.60) | (2.81) | (0.44) | (3.46) | (0.32) | (2.97) | (0.99) | (2.22) | (0.55) | (3.30) |
| Delivery model: ES joint partnership | -0.239 | 0.122 | 0.030 | 0.078 | 0.270 | 0.197 | 0.276 | -0.016 | 0.064 | 0.049 |
| | (2.26) | (0.90) | (0.25) | (0.67) | (2.63) | (2.21) | (3.34) | (0.13) | (0.57) | (0.51) |
| Delivery model: ES consortium contract | -0.313 | -0.218 | -0.044 | -0.267 | 0.137 | 0.304 | 0.117 | 0.207 | -0.005 | -0.219 |
| | (2.62) | (1.47) | (0.32) | (2.16) | (1.23) | (2.97) | (1.39) | (1.59) | (0.04) | (2.16) |
| Delivery model: private sector led | -0.242 | -0.426 | -0.281 | -0.437 | -0.244 | -0.132 | -0.182 | 0.212 | 0.097 | -0.128 |
| | (1.71) | (2.66) | (1.78) | (2.98) | (2.11) | (1.15) | (1.90) | (1.56) | (0.82) | (1.12) |
| *Time of NDYP entry:* | | | | | | | | | | |
| 31 August 1998 | -0.148 | 0.027 | 0.073 | -0.019 | 0.139 | 0.134 | 0.085 | 0.069 | -0.069 | -0.105 |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1.21) | (0.18) | (0.52) | (0.14) | (1.21) | (1.29) | (0.96) | (0.51) | (0.57) | (0.96) |
| 14 September, 1998 | 0.161 | 0.138 | 0.296 | 0.207 | 0.033 | 0.087 | 0.090 | 0.080 | 0.040 | -0.040 |
| | (1.44) | (0.97) | (2.27) | (1.71) | (0.29) | (0.87) | (1.04) | (0.61) | (0.35) | (0.38) |
| 21 September, 1998 | 0.082 | -0.027 | 0.038 | 0.055 | -0.135 | -0.068 | 0.019 | 0.122 | 0.149 | -0.007 |
| | (0.68) | (0.18) | (0.27) | (0.42) | (1.19) | (0.65) | (0.21) | (0.92) | (1.26) | (0.07) |
| 28 September, 1998 | 0.064 | 0.008 | -0.009 | 0.012 | -0.083 | -0.138 | -0.017 | 0.001 | 0.064 | 0.037 |
| | (0.57) | (0.06) | (0.07) | (0.10) | (0.77) | (1.44) | (0.20) | (0.01) | (0.57) | (0.38) |
| 5 October, 1998 | 0.220 | 0.193 | 0.021 | 0.006 | 0.014 | -0.162 | -0.126 | -0.141 | -0.180 | -0.060 |
| | (1.85) | (1.28) | (0.16) | (0.05) | (0.12) | (1.58) | (1.43) | (1.05) | (1.52) | (0.58) |
| 12 October, 1998 | 0.177 | 0.074 | -0.118 | 0.060 | -0.090 | -0.300 | -0.093 | -0.270 | -0.053 | 0.148 |
| | (1.47) | (0.49) | (0.88) | (0.47) | (0.77) | (2.96) | (1.04) | (2.05) | (0.44) | (1.42) |
| 19 October, 1998 | 0.211 | 0.173 | 0.007 | 0.085 | -0.021 | -0.177 | -0.075 | -0.124 | -0.073 | 0.027 |
| | (1.68) | (1.08) | (0.05) | (0.64) | (0.17) | (1.63) | (0.80) | (0.87) | (0.58) | (0.24) |
| 26 October, 1998 | 0.201 | -0.120 | -0.052 | 0.186 | -0.263 | -0.208 | 0.007 | 0.103 | 0.245 | 0.210 |
| | (1.50) | (0.73) | (0.34) | (1.31) | (2.09) | (1.82) | (0.07) | (0.71) | (1.89) | (1.76) |
| 2 November, 1998 | 0.222 | -0.104 | -0.099 | 0.014 | -0.343 | -0.265 | -0.202 | 0.101 | 0.090 | 0.019 |
| | (1.87) | (0.71) | (0.74) | (0.11) | (3.11) | (2.62) | (2.31) | (0.79) | (0.80) | (0.18) |
| 9 November, 1998 | 0.271 | 0.158 | 0.067 | 0.069 | -0.091 | -0.228 | -0.166 | -0.063 | -0.095 | -0.046 |
| | (2.23) | (1.03) | (0.49) | (0.54) | (0.76) | (2.15) | (1.82) | (0.46) | (0.78) | (0.42) |
| 16 November, 1998 | 0.345 | 0.173 | 0.076 | 0.339 | -0.175 | -0.277 | -0.013 | -0.106 | 0.109 | 0.136 |
| | (2.70) | (1.07) | (0.53) | (2.50) | (1.38) | (2.45) | (0.13) | (0.73) | (0.83) | (1.16) |
| 23 November, 1998 | 0.218 | -0.040 | -0.064 | -0.128 | -0.202 | -0.176 | -0.284 | 0.066 | -0.034 | -0.147 |
| | (1.57) | (0.23) | (0.40) | (0.89) | (1.53) | (1.45) | (2.78) | (0.43) | (0.26) | (1.21) |
| *Dummy indicators for missing values:* | | | | | | | | | | |
| Missing value for partnership | -0.090 | -0.129 | -0.393 | -0.427 | 0.000 | -0.306 | -0.316 | -0.326 | -0.351 | -0.103 |
| | (0.44) | (0.52) | (1.78) | (2.11) | (0.00) | (1.83) | (2.26) | (1.55) | (1.89) | (0.65) |
| Missing value for duration of current claim | 0.444 | 0.524 | 0.768 | 0.425 | 0.048 | 0.418 | -0.101 | 0.550 | -0.126 | -0.392 |
| | (0.96) | (0.86) | (1.45) | (0.89) | (0.09) | (0.87) | (0.25) | (0.92) | (0.24) | (0.83) |
| | | | | | | | | | | |
| Constant | -0.560 | 0.129 | 0.088 | -1.033 | 0.696 | 0.671 | -0.510 | 0.060 | -1.212 | -1.228 |
| | (3.30) | (0.62) | (0.46) | (5.98) | (4.25) | (4.55) | (4.03) | (0.32) | (7.39) | (8.33) |
| Observations | 8882 | 4800 | 6085 | 7857 | 9033 | 10321 | 12093 | 6244 | 8011 | 9296 |

Each column gives the results of estimating participation in option i for those who participated in either option i or option j. For categorical variables, excluded categories are as follows: age – 18; SOC – "other"; eligibility – 6 month flow; ES region – London and the South East. Missing values were dealt with by setting them to the population mean and including dummy variables indicating whether the value was missing. Absolute value of z-statistics in parentheses.
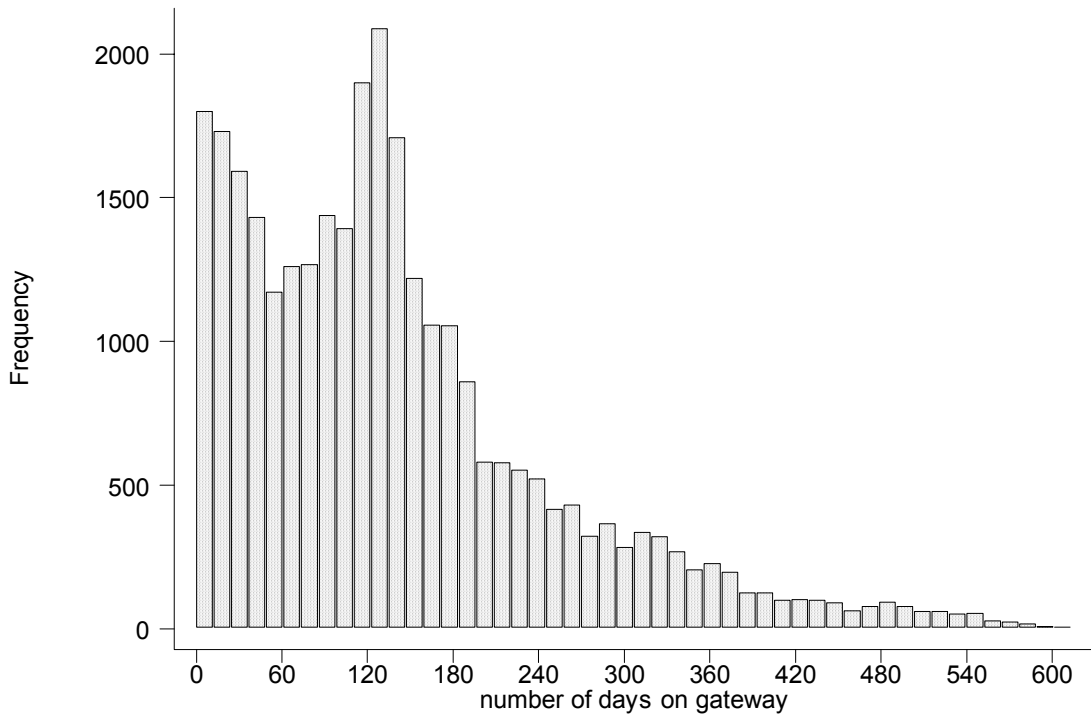§ The coefficients for these variables are multiplied by 100 for presentational convenience.

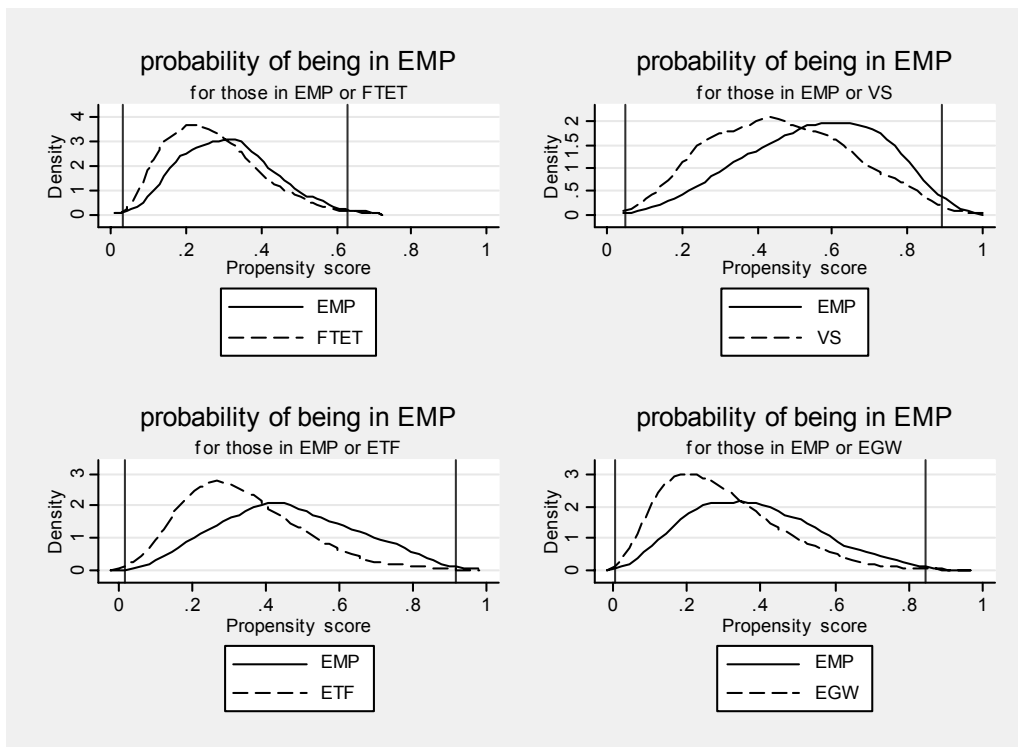**Figure 4.1**: Number of days spent on the Gateway



**Figure 5.1**: The probability of participating in the employment option – comparing the distribution of estimated propensity scores for the other options.
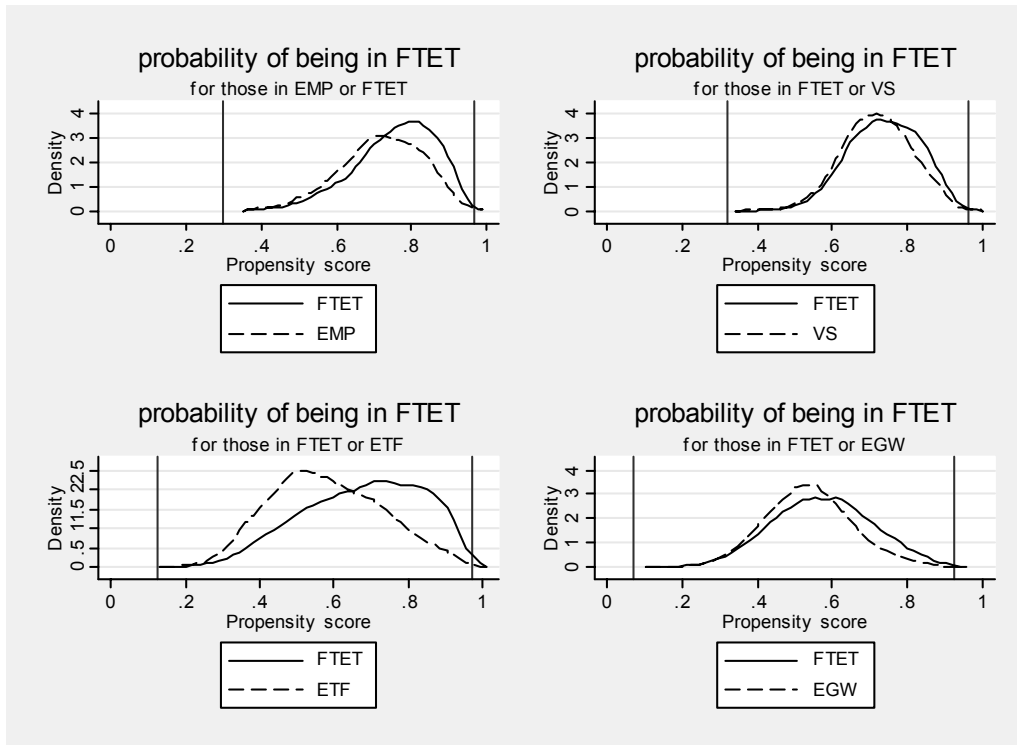
**Figure 5.2**: The probability of participating in the education option – comparing the distribution of estimated propensity scores for the other options.
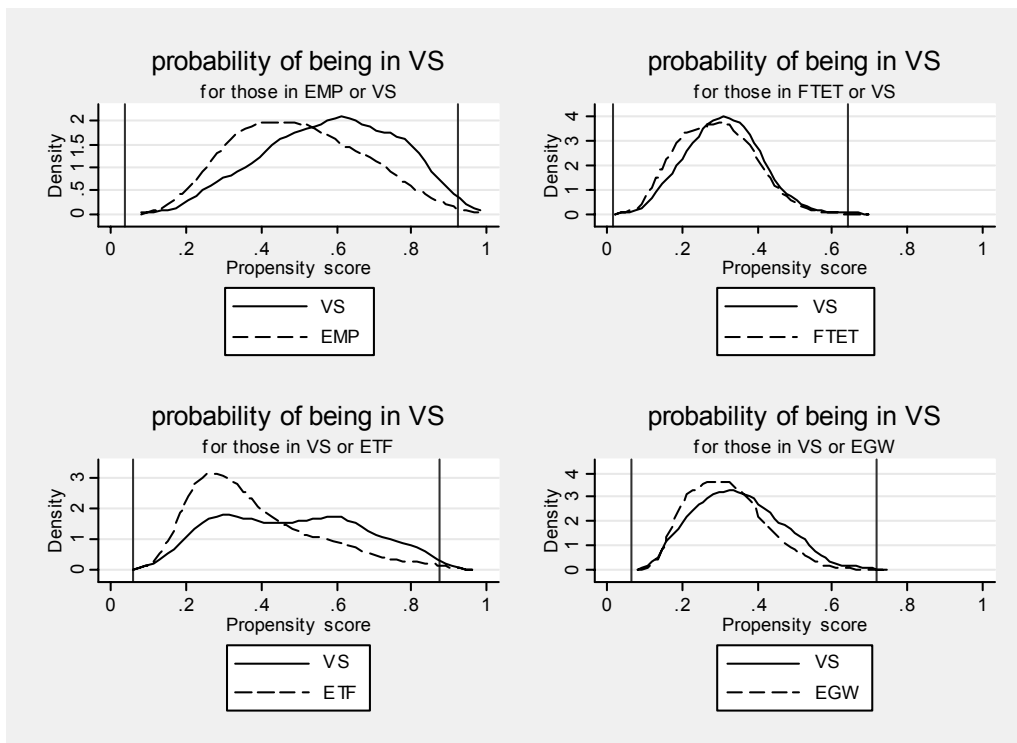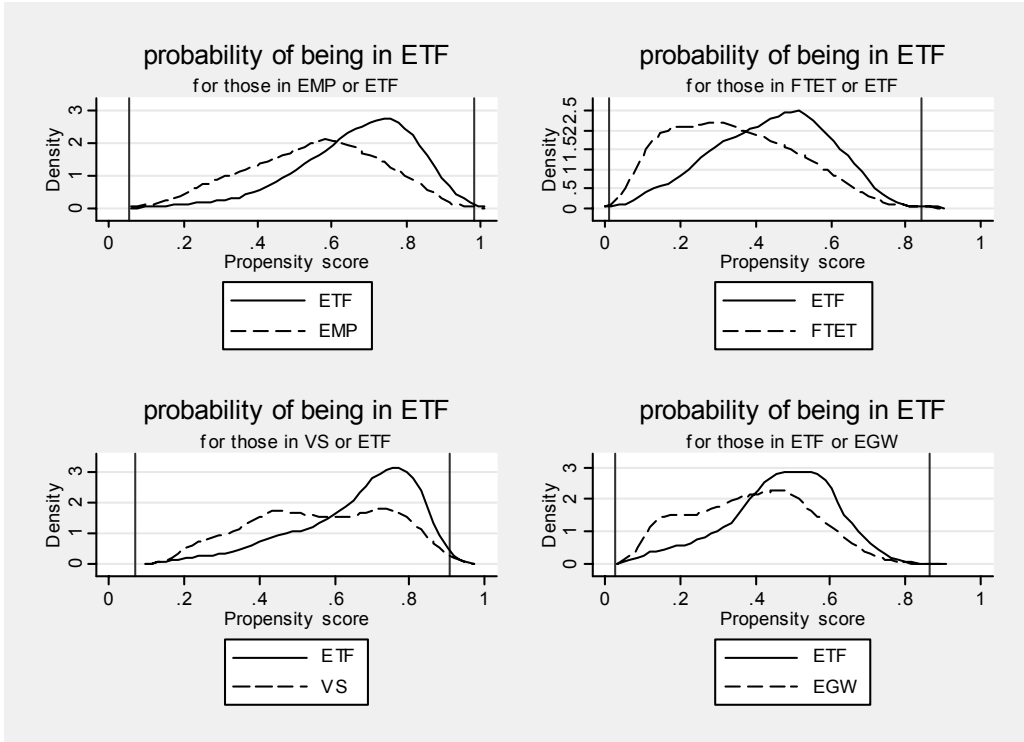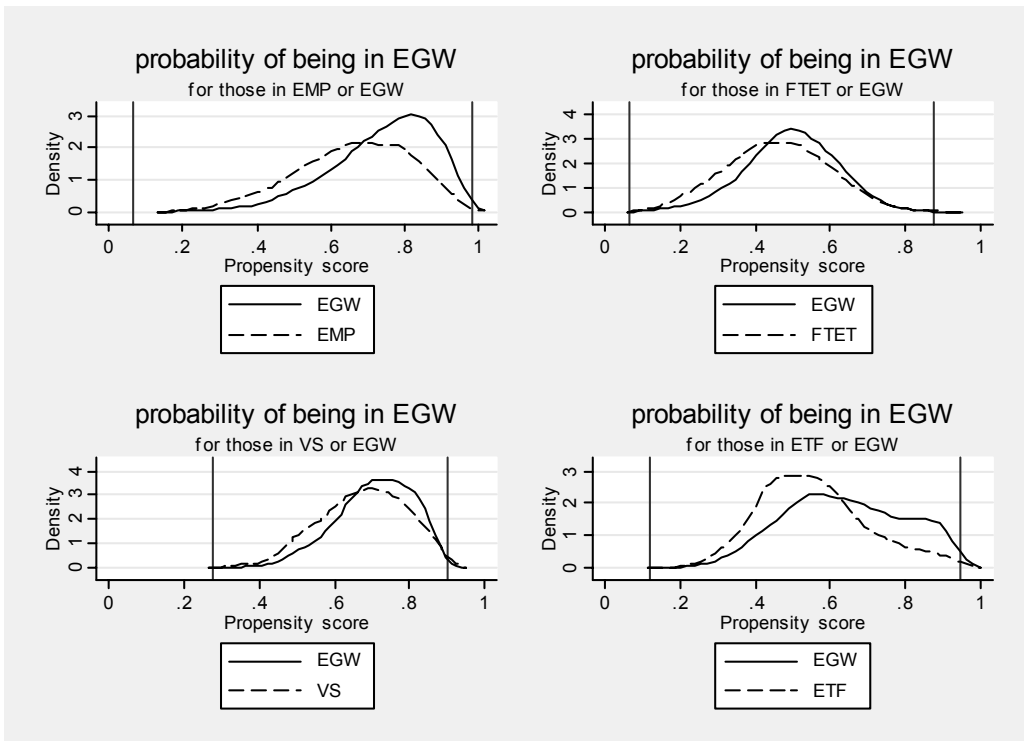


**Figure 5.3**: The probability of participating in the voluntary sector option – comparing the distribution of estimated propensity scores for the other options.

**Figure 5.4**: The probability of participating in the environmental task force option – comparing the distribution of estimated propensity scores for the other options.



**Figure 5.5**: The probability of remaining on the Gateway – comparing the distribution of estimated propensity scores for the other options.
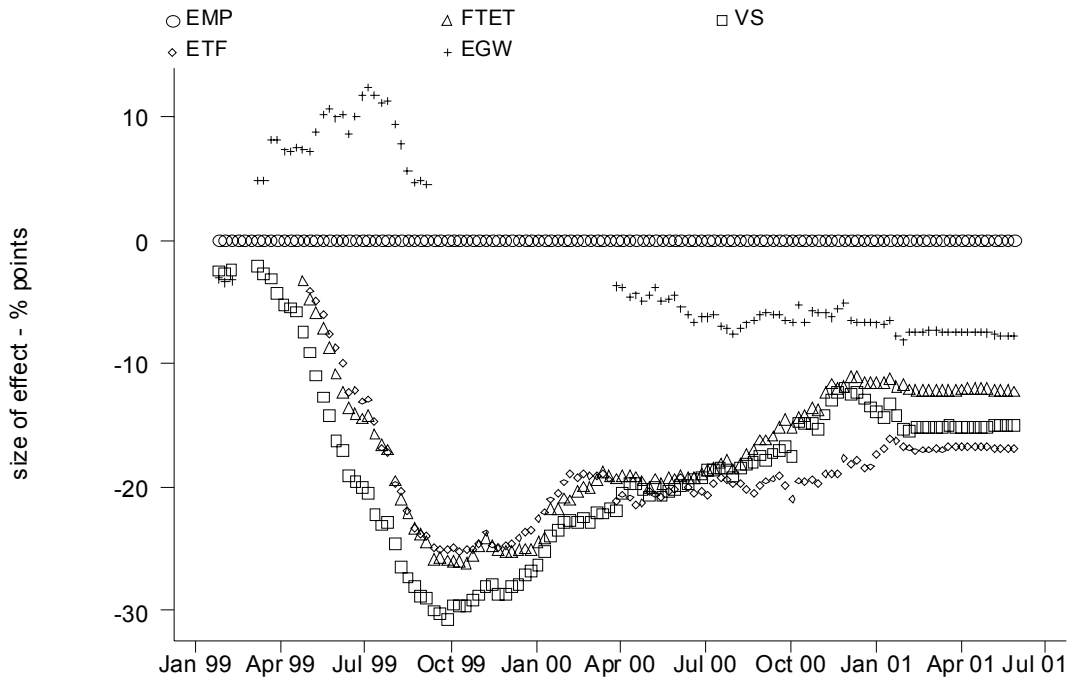
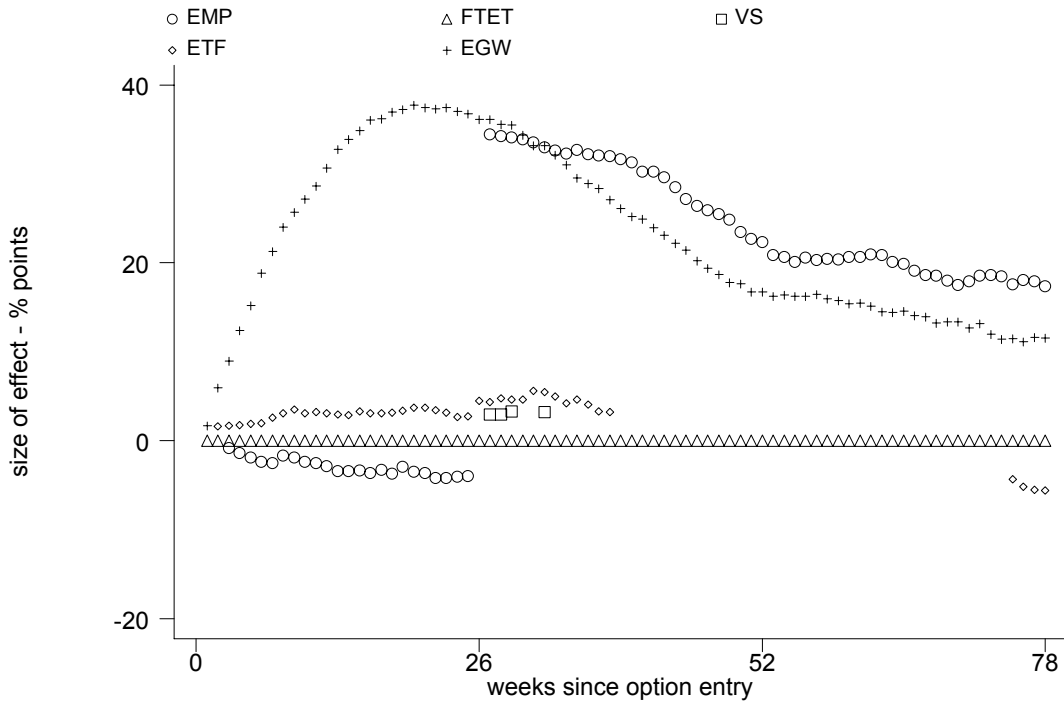**Figure 5.6**: The effect on unemployment of participating in alternative options for those in the employment option – trends over time.
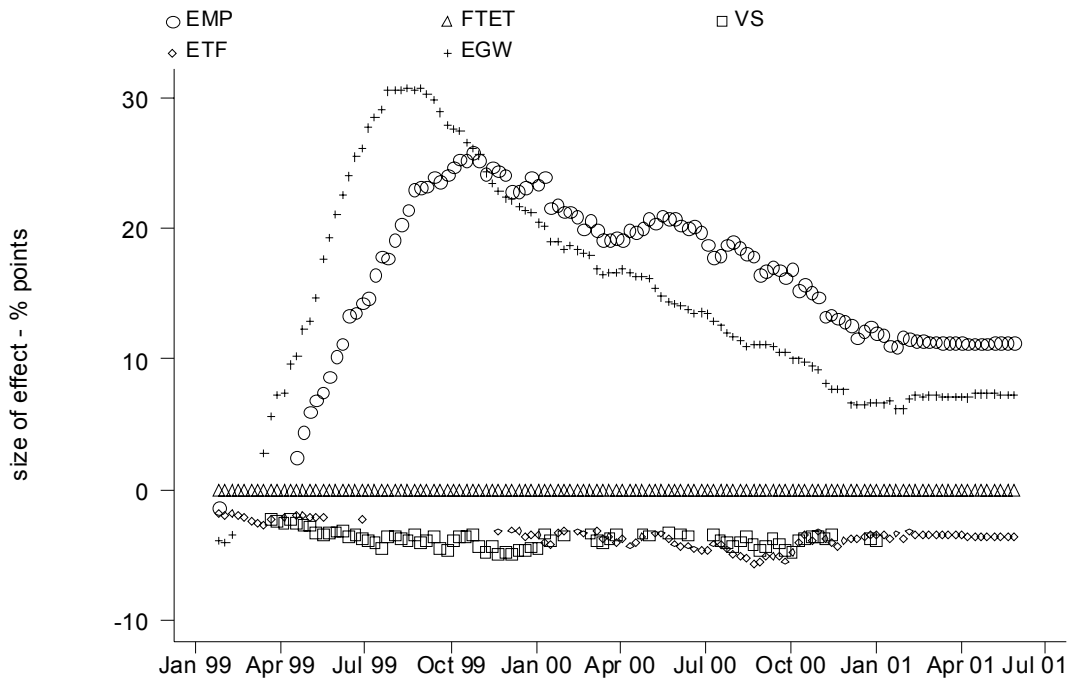


**Figure 5.7**: The effect on unemployment of participating in alternative options for those in the education option – trends over time.
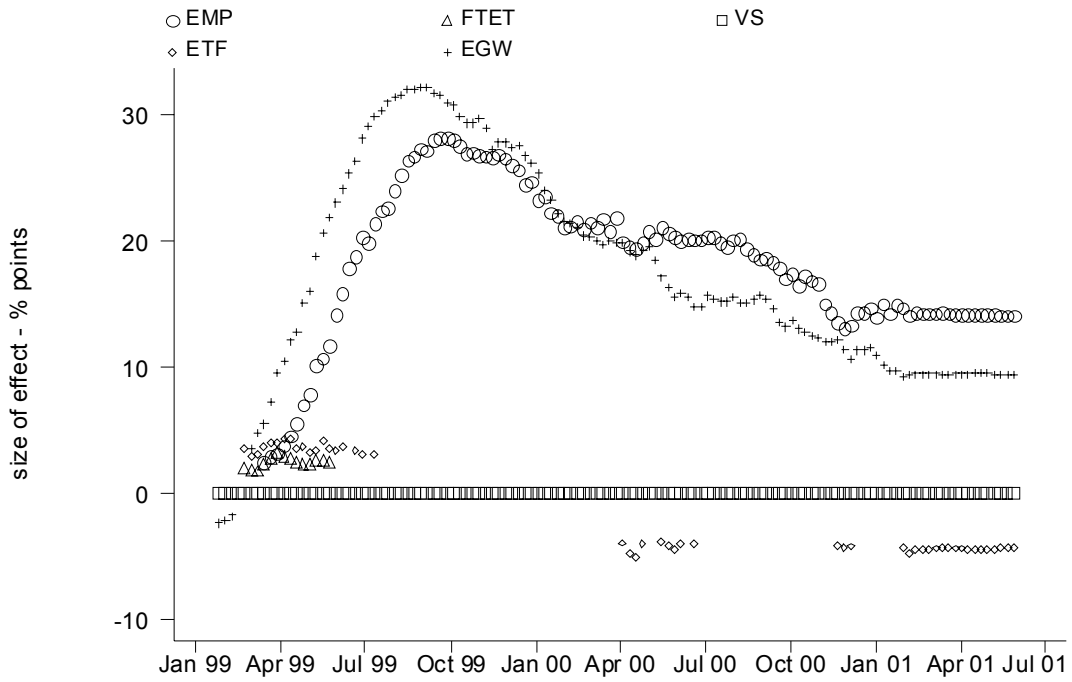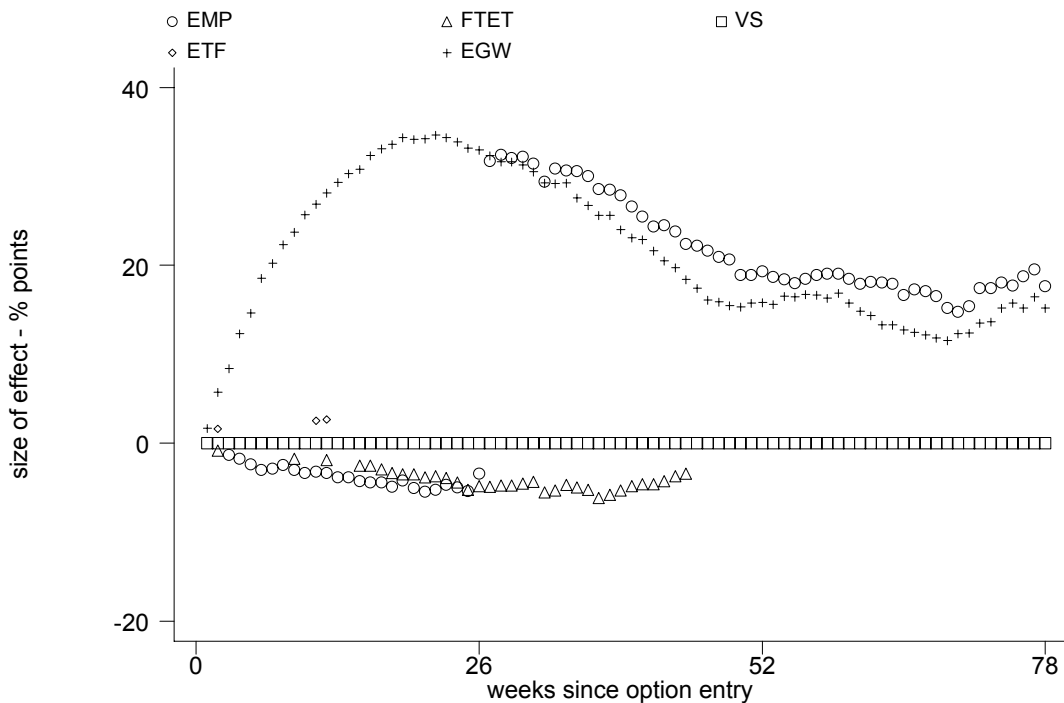
**Figure 5.8**: The effect on unemployment of participating in alternative options for those in the voluntary sector option – trends over time.
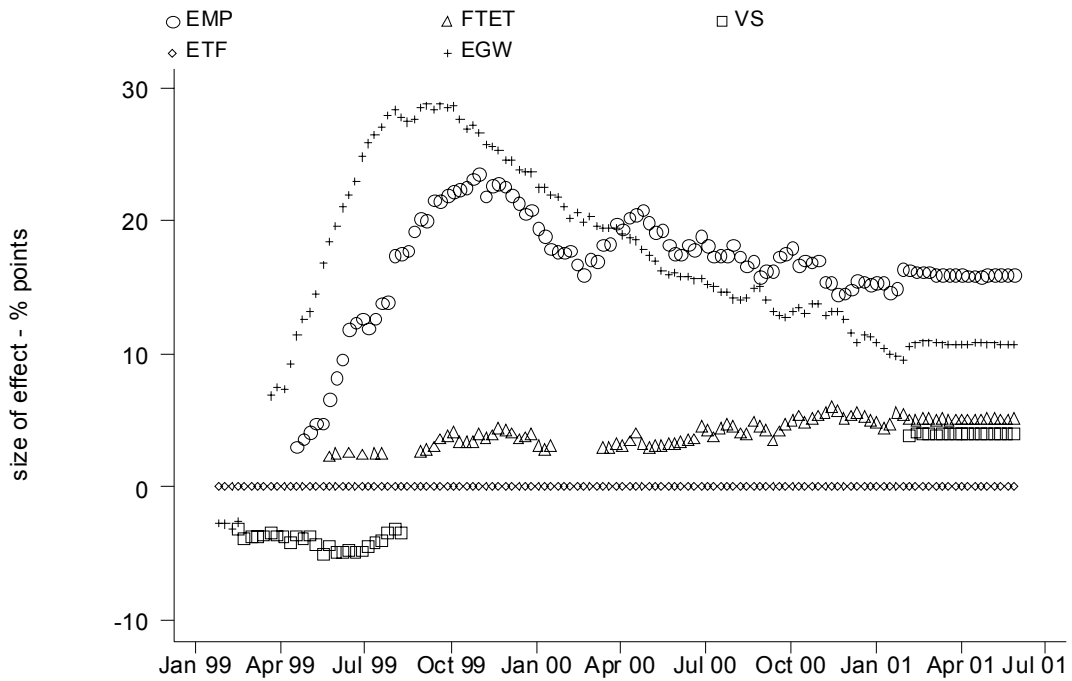


**Figure 5.9**: The effect on unemployment of participating in alternative options for those in the environmental task force option – trends over time.

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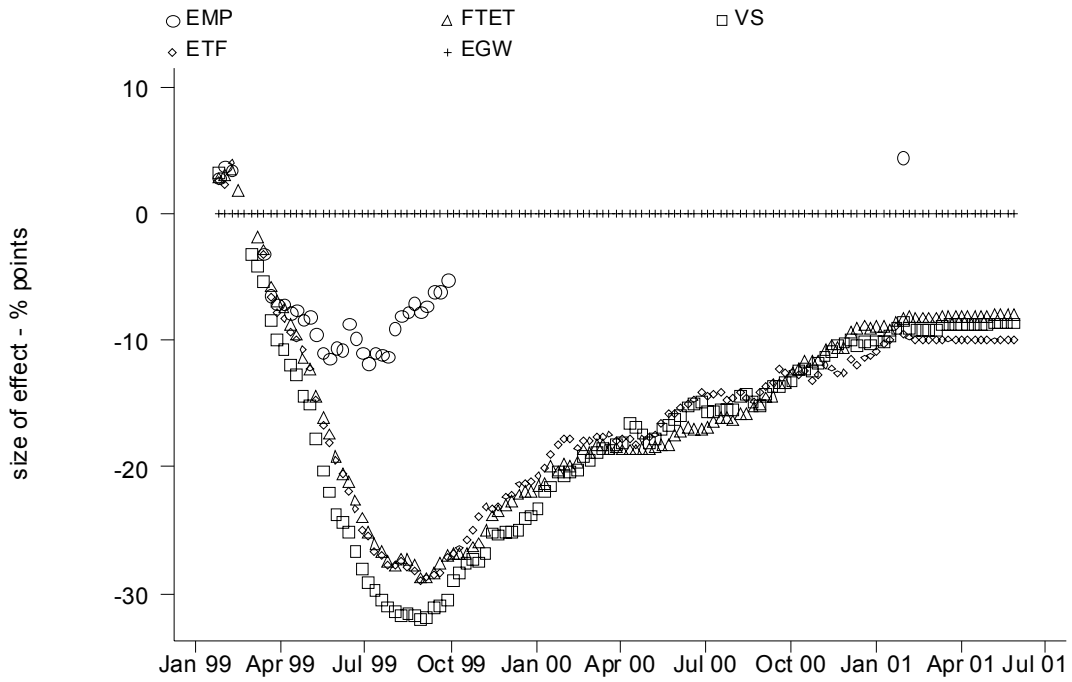
**Figure 5.10**: The effect on unemployment of participating in alternative options for those remaining on the Gateway – trends over time.
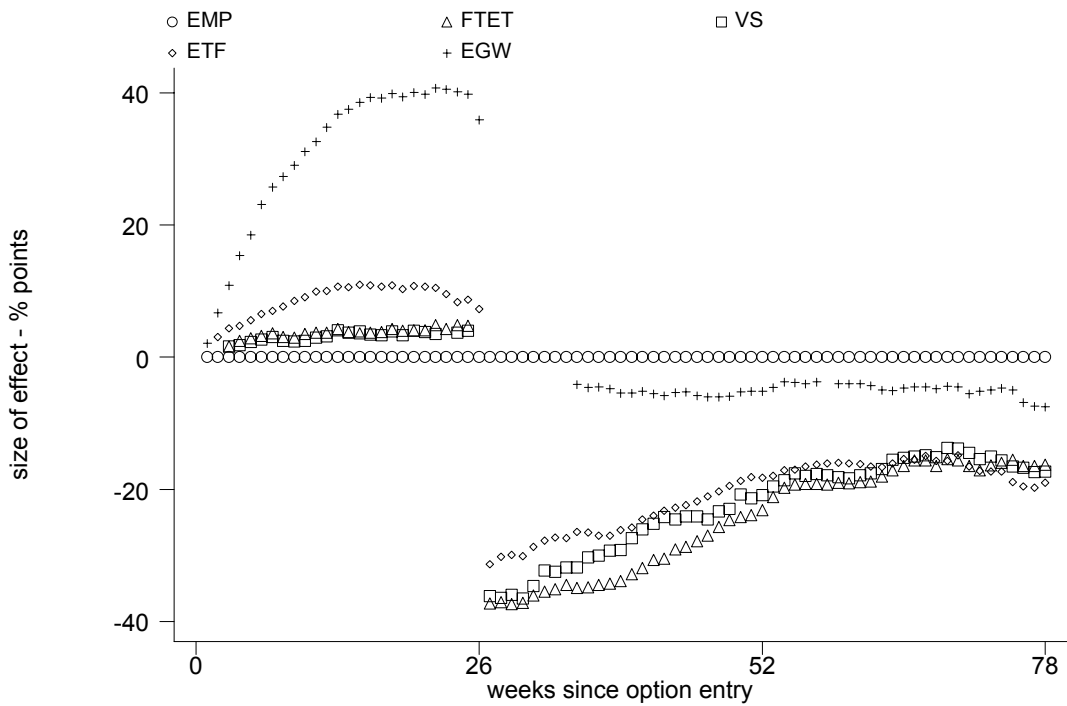


**Figure 5.11**: The effect on unemployment of participating in alternative options for those in the employment option – trends over time since option entry.

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**Figure 5.12**: The effect on unemployment of participating in alternative options for those in the education option – trends over time since option entry.



**Figure 5.13**: The effect on unemployment of participating in alternative options for those in the voluntary sector option – trends over time since option entry.
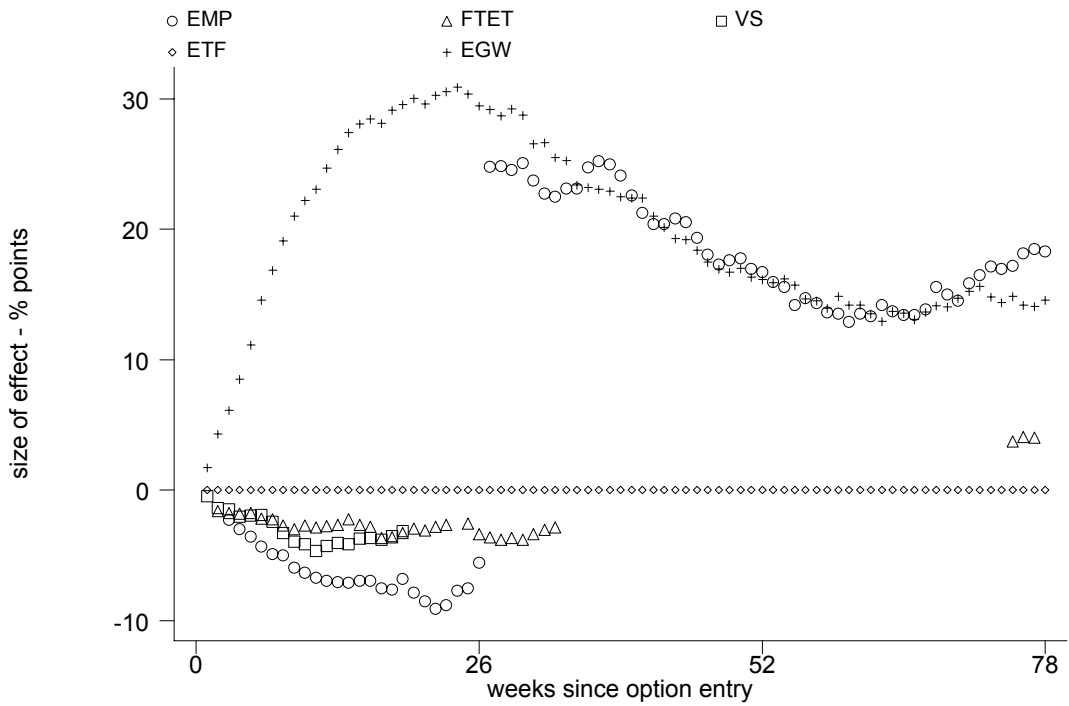
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**Figure 5.14**: The effect on unemployment of participating in alternative options for those in the environmental task force option – trends over time since option entry.
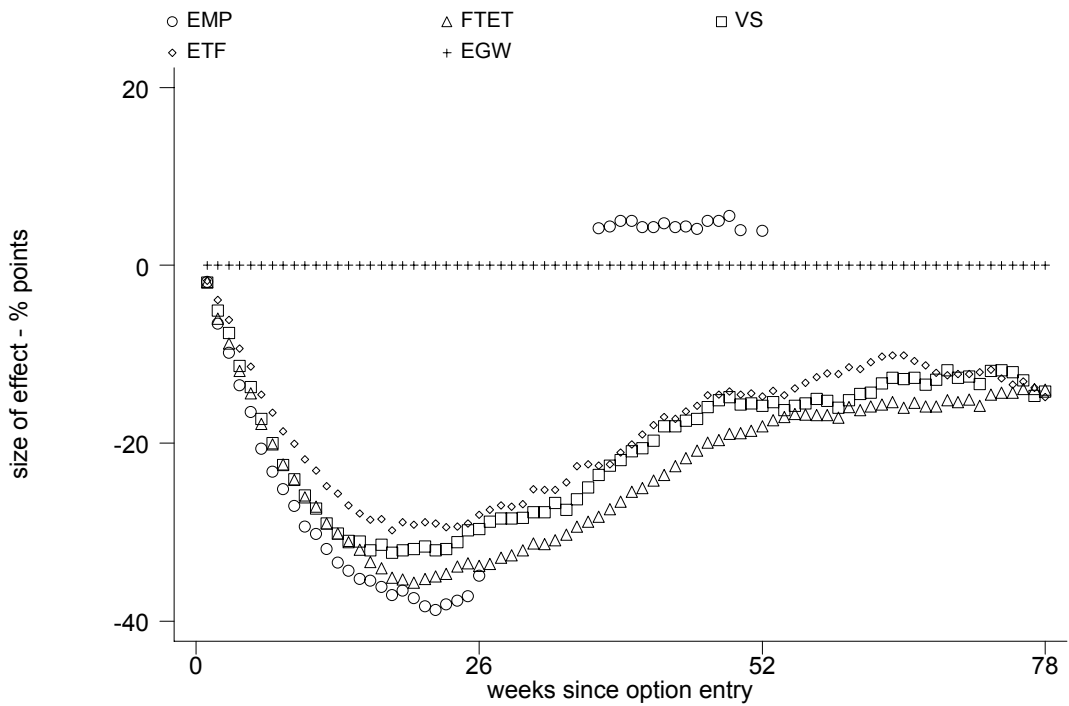


**Figure 5.15**: The effect on unemployment of participating in alternative options for those remaining on the Gateway – trends over time since option entry.
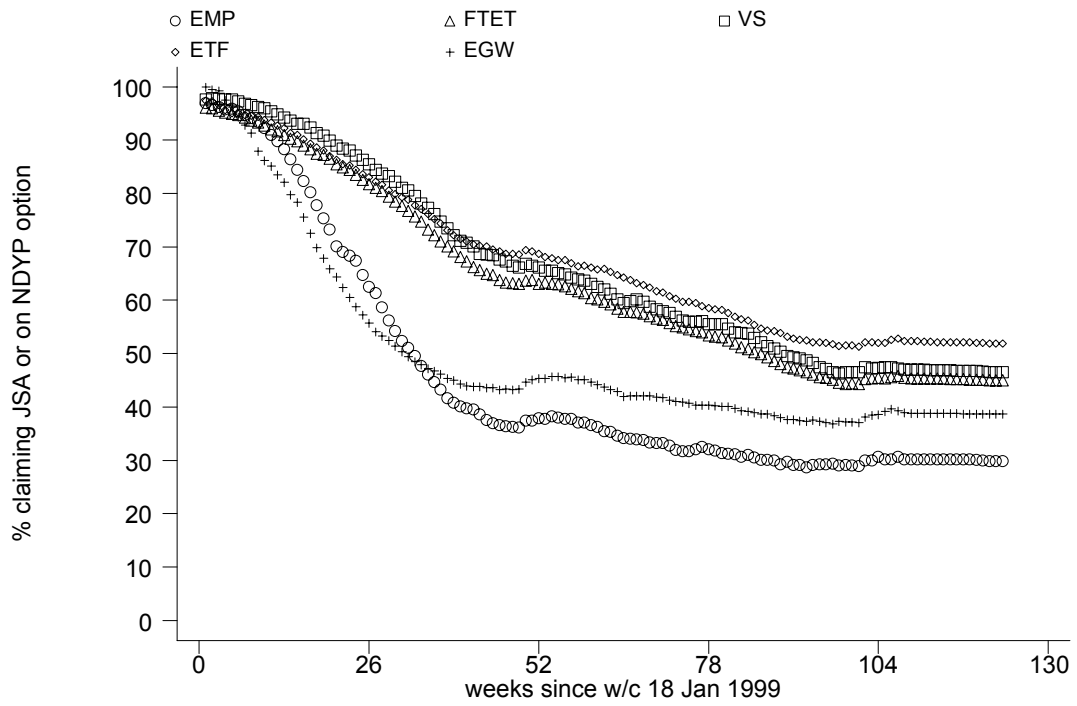
**Figure 6.1**: Levels of unemployment by option – trends over time