The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Some Experimental Evidence from the U.K [1]

Peter Dolton[a]
Donal O'Neill[b]

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[a] Department of Economics, University of Newcastle-upon-Tyne, Newcastle, NE1 7RU, U.K. e-mail : peter.dolton@ncl.ac.uk

[b] Department of Economics, National University of Ireland, Maynooth, Co. Kildare, Ireland. e-mail: doneill@may.ie

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Abstract

In this paper we examine the long-term effects of the Restart unemployment program introduced in the U.K in 1987. The program was aimed at the long-term unemployed and involved a combination of tighter monitoring of benefit eligibility rules and increased job search assistance. We compare employment behaviour over a five year period for members of a treatment group who participated in the scheme with those of a randomly chosen control group for whom participation was delayed. We find that those who participated in Restart had significantly shorter unemployment durations than those excluded from the program. However, our results also show that the long-run effects of postponing participation in the scheme differs by gender. While there is little evidence of a long-term benefit for women in our sample, the unemployment rate among males in the treatment group was six percentage points lower than that for males in the control group five years after the initial experiment.

1. Introduction

Recent papers examining the effectiveness of unemployment programs have tended to focus on the duration of the unemployment/welfare spell in progress at the time of the study (Woodbury and Spiegelman (1987), Decker (1994), Gorter and Kalb (1996)). However recent work by Belzil (1995) suggests that the duration of unemployment preceding an employment spell may also have a direct impact on the duration of the subsequent employment spell. This 'scarring' or 'stigma' effect (Heckman and Borjas (1980)) suggests that evaluations of the long-term effects of unemployment programs should study the joint behaviour of unemployment and re-employment spells.

In this study we follow such an approach in evaluating the U.K Restart unemployment program. The Restart program consists of a compulsory interview for each unemployed person in the U.K after they have been registered as unemployed for 6 months. The interview with an official of the Employment Office is designed to help the long-term unemployed find a job and reduce their dependency on unemployment benefits (UB). In part it achieves this: by placing workers in contact with employers and training agencies; by altering the individual's approach to job search and by improving information on programs aimed at helping people make the transition back to work. Hence an important part of the Restart process is the positive help and encouragement given to the unemployed job seekers by way of advice, counselling and direct contact with employers. However, a feature of Restart is that it also has a negative threat component, in that the UB claimant is faced with the possibility of having their benefits reduced or suspended if they do not attend the Restart interview or are not deemed to be making genuine attempts to find work.

Previous work (Dolton and O'Neill (1996)) found that unemployment durations were significantly lower among individuals who took part in the Restart process. This was achieved both by inducing individuals (presumably those who were not eligible for benefits) to sign off receiving unemployment benefits and by helping individuals move off the unemployment register into

employment.[2] However this earlier work focused only on the impact of Restart on the initial unemployment spell. In this paper we extend this analysis by examining the long-run effects of the program. We look at not only the impact of Restart on unemployment durations but also on the duration in the subsequent 'out of unemployment' state, which we will subsequently refer to as the reemployment duration.[3] Such a question poses a real problem for work-search policies analyzed in the previous literature[4] because it has been suggested that people, when faced with an assessment of their eligibility for unemployment benefits either obtained marginal jobs offering no long-term prospects or sign off receiving UB to satisfy the Benefit Officer but return to claiming benefits after a relatively short period of time. This circular flow of individuals around the unemployment benefit system has been labelled the 'carousel' or 'whirligig' effect by Robinson (1995) and the '(un)merry-go-round' by Disney et al (1992) who suggested :

> "..it is possible that many of those leaving the register as a result of Restart simply join it again quite quickly." (Disney et al 1992)

To identify the Restart effect we use data from a controlled experiment consisting of a randomly chosen control group for whom participation in the process was postponed for six months.[5]

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[2] Since both the threat and counselling components of Restart were administered simultaneously it is difficult to identify the relative importance of the two components. We return to this later in the paper.

[3] The administrative data used indicate whether an individual was registered as unemployed or not. Among those not registered we are unable to distinguish between employed workers, individuals on training schemes or those who have left the labour force. Extrapolating from survey questionnaire data it would seem that a sizeable majority of those signing off unemployment in our sample are in fact exiting to employment. Hence, we use the term 're-employment duration' rather than 'out of unemployment' duration, though one must be careful in interpreting the findings.

[4] For a survey of this literature see Meyer (1995).

[5] Controlled experiments have been advocated as a means of overcoming self-selection bias traditionally associated with the evaluation of training programs (LaLonde (1985)). For a critical view on the role of experimental data in policy evaluation see Heckman and Smith (1995). The design of the Restart experiment overcomes some of the problems raised by Heckman and Smith. There was no scope for local employment offices to opt out of the randomisation. This is in contrast to the 90%

Our analysis shows that members of this group had unemployment durations which were significantly longer than individuals who received the interview at the appointed time. We find no evidence that the positive effects of Restart on initial unemployment durations are counterbalanced by a more rapid return to unemployment. The long-term effects of the program however, differs for males and females. For females providing control group members with the interview six months later eliminates any of the initial gains obtained by the treatment group. In contrast for males, providing control group members with the interview at a later date does not compensate for the earlier losses. The unemployment rate for males who participated in the Restart process at the appointed time was six percentage points lower than among those in the control group a full five years after their initial implementation of the experiment. This gender difference may reflect differences in the way the program operated for males and females.

2. The Restart program in 1989

In this section we describe how the Restart system worked at the time our data were collected.[6] Before doing so however, it will be useful to describe the circumstances surrounding the

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refusal rate they document for the JTPA experiments. Furthermore the eligibility condition for participation, that the individual be approaching their sixth month of unemployment is easily checked and thus reduces the ability of local administrators to bias the randomisation process. However, after the randomisation and interviews had taken place, individuals in the experiment were notified and given the option to 'opt-out'. 6% of the sample did so. Unfortunately we have no information on these individuals and thus cannot determine directly the selectivity of the 'opt-out' decision. An analysis of the observable characteristics of those remaining in the sample suggest that on this basis the randomisation between the control and treatment groups was not seriously effected by this opt-out.

[6] While the goals of the Restart program in operation today are the same as when it was introduced in 1987, the program has developed substantially since its introduction. In particular today's system is a seamless process of continued appraisal of the unemployed person's job search, suitability for training and eligibility for welfare. Further details are available in a supplementary appendix available from the authors upon request.

introduction of Restart. The Employment Service is the government agency responsible for counselling and placement of the unemployed in the U.K. It is also responsible for administration of payments of unemployment assistance. It operates through a network of 'high street' Jobcentres in which vacancies are advertised. Individual assessments of benefit eligibility on the other hand take place in Unemployment Benefit Offices (UBO). During the 1980's decisions were made which physically separated Jobcentres from the UBO's. It was hoped that this separation would reduce the stigma associated with use of the Jobcentres and help promote them as a placement service for everyone, not just the unemployed. As part of this process it was decided after 1982 unemployed individuals would no longer need to register with the Jobcentre in order to qualify for assistance. The net effect of these moves however was to reduce the capacity for effective search by the unemployed. In response to increasing long-term unemployment[7] the Restart program was introduced nationally in April 1987, in an attempt to restore contact between the Jobcentre and the UB claimant.

The main aim of the Restart process was to reduce the amount of time people spend unemployed and reduce claims of UB by those who were essentially not available for work or who were not making the appropriate effort to find employment. The process began with the Restart office sending a letter to each individual approaching an unbroken period of 6 months claiming UB. This letter requested that the individual attend an interview at a stated date and time.[8] Interviews took place in Employment Service Jobcentres and lasted approximately 15-25 minutes. In some instances individuals were excused attendance at the Restart interview mainly because they had already obtained a job or a place on a training program or had withdrawn their benefit claim. The service was targeted at the long-term unemployed with the first interview taking place after six months of unemployment and subsequent meetings following every six months provided the individual remained unemployed. During the interview the counsellor assessed the claimant's recent unemployment history and offered advice on benefits, search behaviour, training courses and in some cases initiated direct contact with employers. On completion of the interview the Restart counsellor recommended a course of action for the

[7] In 1987, when Restart was introduced, there was 1.3 million individuals who had been out of work for over a year. This corresponds to over 40% of those unemployed. The figure for 1979 was 25%.

individual designed to end their unemployment spell. For many individuals the Restart interview acted as a stepping stone to other services such as Restart courses, Job Clubs or Employment Training, many of which were available prior to Restart.[9]

While the Restart process may have affected unemployed claimants through these channels, a direct consequence of the process was the threat to reduce or suspend a claimant's welfare receipts. The Child Poverty Handbook (1993) describes the process thus:

> "If you decline all offers of assistance, your case will be referred to an adjudication officer who may decide either that you are not really available for work or, if you have refused an offer of employment or training, that you should be disqualified from benefit for a specific period" (page 17).

Attendance at the Restart interview was mandatory, in that it is a condition of receiving benefits that claimants attend an employment interview when asked to do so. Those who failed to attend the initial appointment were sent two more letters requesting them to do so. If they still had not attended an interview by the time of the third letter their names were flagged at the UBO and they were then required to attend a Restart interview and to return with evidence of having done so, before they were allowed to sign on to receive UB again.

3. Data

[8] A copy of this letter is provided in Appendix 1.

[9] Restart courses attempt to re-motivate discouraged claimants and also improve job search and interview skills. The courses usually last for five days and help focus individuals on the problem at hand. Job Clubs cater for more 'job-ready' claimants and provide practical facilities such as postage stamps and stationary free of charge as well as further help on telephone and interview techniques Claimants are expected to commit themselves to attend four half days in every week. Employment training offers a range of training at various skill levels and was presented as a program to 'train the workers without jobs for the jobs without workers'. For more information on these and the other facilities provided by the Employment Service see Disney et al (1992).

In 1989 the Policy Studies Institute was commissioned by the Employment Service to evaluate the impact of Restart. This study identified a sample of individuals approaching their 6th month of unemployment in the period March-July 1989 who were eligible for a Restart interview. A random sample of 8,925 of these individuals was chosen to take part in the study. Individuals were retained in the sample even if they subsequently did not attend a scheduled interview, as such, the sample is one of the inflow to Restart and not the outflow from it. Every Employment Service office throughout Britain was contacted while constructing the sample in order to eliminate regional biases. Individuals were selected for the sample from the inflow lists on the basis of their National Insurance (NI) numbers. The NI digit sequence used corresponds to that used by the Joint Unemployment and Vacancies Operating System (JUVOS) cohort data base and is known to result in a random 5 percent sample. Of this set a control group of 582 people was randomly chosen, again by means of previously specified NI digit sequences. Members of the control group, although eligible for an interview, were not asked to attend the initial Restart interview. If they were still unemployed 6 months later, members of the control group were then brought into the Restart process. What we evaluate in this paper therefore is the impact of postponing the Restart process by six months.[10]

The structure of the sample was such that it could also be linked to the JUVOS data collected by the Employment Service. These records provide monthly information on the claimant's unemployment history dating back to January 1982 which is free from recall and non-response bias. Another advantage of having access to the JUVOS data is that it is an ongoing operation which when matched to our experimental data provides us with individual claimant histories up until May 1994, more than five years after the receipt of the Restart interview. Such long-term data are rare in an

[10] In order for our experiment to yield accurate estimates of the impact of Restart it is important that the behaviour of the control group members provides an accurate description of behaviour in the absence of the program. This would not be the case for instance if members of the control group anticipated being called for an interview as they approached their sixth month of unemployment and acted on this belief. In this case we would underestimate the impact of Restart. Having discussed the Restart scheme with members of the Employment Service it is our belief that at the time our data were collected the Restart program was not sufficiently well known for UB claimants to anticipate receipt of an interview. There is some evidence of this in our data in that less than one percent of the control group are recorded as having requested an interview.

experimental setting and facilitate a detailed examination of the long-run effects of Restart. [11]

The administrative data also contain information on an individual's travel to work area[12] which was linked to the National Online Manpower Information System (NOMIS) to obtain monthly data on local labour market conditions dating back to August 1985. We use this information to control for local labour market conditions at the commencement of both the unemployment spell and the reemployment spell. A description of the variables used in this study are presented in Appendix 2.

While the administrative data have many advantages they do not identify the destination state on leaving unemployment. It may be exit to: employment, a training programme or simply signing off claiming UB. The impact of Restart on exits to these alternative states was studied in earlier work using survey data which are available for a shorter time period for a subset of the original data (Dolton and O'Neill (1996)).

4. Univariate Hazard Functions

To examine the relationship between initial unemployment duration, re-employment durations and the Restart program we first estimated hazard functions for the initial unemployment and reemployment spells separately. The hazard function, h(t), gives the probability of exit from a state in a short interval after t, conditional on the state being occupied at t.[13] For the individual hazards estimated in this section we adopt the proportional hazard specification:

$$h_i^j(t) = h_o^j(t) \exp(X^{j'} \beta^j) \qquad \mathbf{0}$$

[11] Studies by Couch (1992) and Friedlander and Hamilton (1996) also provide a long-run analysis of labour market programs in an experimental setting. The former examined training schemes for employed workers and analyzed subsequent wage gains, while the latter focused on families eligible for AFDC, which for the most part are headed by single mothers. The focus of our paper are the long-term unemployed.

[12] This is a detailed local regional indicator consisting of 380 groupings corresponding to the surrounding geographical area in which one could reasonably commute daily to work.

[13] For a more detailed discussion of hazard functions see Lancaster (1990).

where j is used to index a spell type i.e j= unemployment, reemployment and $h_o^j(t)$ is the baseline hazard for duration type j at time t. X^j is a vector of explanatory variables for each individual i, including an indicator for receipt of the Restart interview and β^j is a vector of unknown parameters.

In estimating equation (1) we follow Meyer (1990) and adopt a semi-parametric approach which estimates jointly the baseline hazard and the coefficients on the covariates. This approach avoids biases resulting from misspecification of the baseline hazard. For interval data of the type analyzed here Meyer derives the likelihood function for a sample of N individuals to be:

$$L(h^{j_o}, \beta^j) = \Pi_{i=1}^{N} \left[1 - \exp\left(-\exp(X_i^j \beta^j)\gamma(t_i+1)\right)\right]^{c_i} . \exp\left(-\exp(X_i^j \beta^j) \sum_{s=1}^{t_i} \gamma(s)\right)$$

where c_i is a censoring indicator with c_i=1 for a completed spell and c_i=0 if the duration is right censored. $\gamma(s)$ is defined to be the log of the integral of the baseline hazard over the interval s to s+1. Maximisation of the log-likelihood, ln(L) with respect to h_o (or equivalently the $\gamma(s)$ terms) and β, under the constraint that the hazard pieces are non-negative provides consistent estimates of the baseline hazard pieces and the parameter vector β.

The results of estimating the hazard functions for the unemployment and reemployment spells are given in columns one and two of Table 1 and Figures 1 and 2, respectively. The estimates on the unemployment equations summarise the findings of earlier research. The estimated coefficients on the controlling regressors in Table 1 indicate the instantaneous probability of leaving unemployment is lower for: men; those aged over 35[14] and those who live in an inner city area. The variable UCHANGE measures the change in local area unemployment rate in the two months preceding the start of the unemployment spell and JCHANGE measures the change in the two months preceding the start of the reemployment spell. Thus a positive coefficient on these variables indicate that unemployment was

[14] We have performed the same estimation using age as a continuous variable. The results do not change.

increasing in the local area prior to the commencement of the spell. Since all unemployment spells started at approximately the same time UCHANGE captures only regional variation in unemployment rates. However since the timing of reemployment spells across individuals in the sample may differ JCHANGE captures both temporal and regional variation in local labour market conditions. The coefficients on these variables indicate that those who become unemployed in times of high unemployment stay unemployed for longer but those who find work under poor local conditions are likely to be employed longer. This latter finding may be capturing some unobserved attribute of these individuals such as motivation.

In terms of our evaluation of Restart the most important findings are the estimated coefficients on the control variables. In the unemployment equation we see that the coefficient on control group in the hazard is significant and negative showing that individuals who did not receive a Restart interview at 6 months of unemployment have a significantly lower probability of exiting unemployment than those who received the interview. Our data show that the median duration of unemployment for control group members was 13 months compared to approximately 11 months for members of the treatment group. The nature of these differences is further highlighted in Figure 1 which shows the hazard function for both the control and treatment groups.[15] From this we see the striking difference in the hazard functions in the 5-6 months following the initial Restart interview. Over the period in which the control group were excluded from the process, members of this group were only about 70-80 percent as likely to exit unemployment as members of the treatment group. We also notice a significant spike in the hazard functions approximately 6 months after the initial interview, which is consistent with attendance at the Restart interview at one year's worth of unemployment. The fact that this spike is more pronounced for the control group is consistent with this being their first meeting with the Restart counsellor.

The hazard functions for the reemployment equations are given in Figure 2. They reveal a clear spike at 11 months. Many of those who move off the register seem to stay off for just under a year before signing on again. At first sight this seems a curious feature of the data so we investigated the

[15] In what follows we use the zero (reference) duration point as the 6th month of unemployment since all our sample have been unemployed for at least 6 months.

possibility of administrative reasons inducing either employers to cease contracts after one year or individuals to seek a way back into UB status after a minimum prescribed time out. We could find no evidence that either of these were true. Hence to check the robustness of this finding we used event history data from the National Child Development Survey (NCDS) to examine the duration of 'out of unemployment' spells. The NCDS is a longitudinal data set following the lives of all those born in a week in March 1958. The data contain complete monthly records on the individual's labour market history between the ages of 16 and 33. We use these work histories to calculate the durations of the first spell out of unemployment after leaving full-time education. We distinguish between four states: full-time work, part-time work, out of the labour force and training. The hazard functions associated with each of these states are presented in Figure 3-6.[16] We see that for both full-time jobs and training spells there is are spikes in the hazard at both 6 months and 12 months. Of these two exit types the spike is largest for those on training schemes which accords with our general view that many training schemes last for 12 months. We are not clear as to why there should be a spike in the full-time employment hazard at one year. Among the explanations which we explored were the possibility that employees' rights increase with tenure thus making short-term jobs advantageous from an employer's perspective. However this does not seem to fit in with a one year spike. In order for employees to acquire the right to a statutory redundancy they must satisfy a qualifying length of service with the employer which is generally two years.

To further identify the source of this spike we used the self-reported work histories available for a subset of the Restart sample who responded to survey questionnaires administered by the SCPR. These data were not used in the long-run analysis for this period because they end in May of 1990, a little over a year after the initial interview. As a result many of the individuals may not have finished their reemployment spell by this time and some may not have begun such a spell. The advantage of the self-reported data however, is that for those individuals who had exited unemployment we can identify the state into which they exited. Of those individuals who reported a reemployment duration of 11

[16] Only the first three years of the hazards are shown on these figures. Small sample sizes beyond this produced estimates with large standard errors which tend to distract from the early part of the spell. Since we are only interested in the early part of the spell in this paper we omitted the remainder

months we can identify exit states for approximately half of them. 75% of the these reported exiting to a government training scheme while 17% reported exiting to a job. Thus it appears that the spike at 11 months which dominates the reemployment hazard is driven by individuals taking training courses which last for just under a year.[17]

Comparing the control and treatment groups we see very little difference in their reemployment spells. Both show the spike at 11 months and there is no difference in average duration. The median reemployment duration for both groups is approximately 15 months. Thus there is no evidence that members of the treatment group return to unemployment quicker than those of the control group.

5. The Bivariate Duration Model

Estimating the equations separately for both duration types ignores the possibility of correlation between unemployment duration and subsequent employment duration. Belzil (1995) discusses several possible reasons for such a relationship. Included among these are the deterioration of skills when unemployed and the stigma associated with long-term unemployment both of which result in a negative correlation and the possibility that job search while unemployed may improve the job match which leads to a positive correlation. To model the relationship between unemployment and subsequent employment we specify a joint distribution for both random variables U (unemployment) and E (reemployment). We denote this distribution by $f_{U,E}(u,e)$. To derive the likelihood function for our sample we must distinguish between three types of observations:

(i) people for whom we observe *completed* spells for both unemployment and reemployment spells

(ii) people whose unemployment spell is censored and therefore we do not observe a

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of the hazard from the graph.

[17] While exits to training dominate the hazard one should not conclude from this that most people are exiting to training schemes. In fact this is not the case. From the survey data we find that 50% of those exiting went to a job, 21% exited out of the labour force and only 16% exited to a government training scheme. It is the fact that the vast majority of these schemes last for 1 year which tends to exaggerate their impact via the hazard.

subsequent reemployment spell.[18]

(iii) people whose unemployment spell is not censored but who have a censored employment spell.

To capture the contribution of these individuals to the likelihood we define: c_1=1 if neither spell is censored and 0 otherwise, and c_2=1 if the unemployment spell is not censored, 0 otherwise.

The contribution of the first type of person to the likelihood is:

$$f_{U,E}(U = u, E = e)$$

For the second type of person the likelihood contribution is:

$$Pr(U > u, -\infty < E < +\infty) = 1 - F_U(u)$$

where F_U is the cumulative marginal distribution function for U.

For the third type of person the contribution to the likelihood function is

$$P(U = u, E > e) = P(E > e\_U = u)\, P(U = u) = (1 - F_{E\_U}(e))\ f_U(u)$$

where $F_{E|U}$ is the conditional cumulative distribution function.

The likelihood function can then be written as

[18] As pointed out by Ham and LaLonde (1996) these type of individuals can introduce dynamic sample selection bias into policy evaluation even in the context of experimental data. Even if the group are randomly assigned at the implementation of the experiments the samples who subsequently obtain employment are unlikely to maintain this characteristic. This was a particular problem in their analysis of the National Supported Work Demonstration where 17% of the treatment group never obtain employment and one is faced with making an important choice between continuing and fresh unemployment spells. However this is less of a problem in our analysis where the treatment administered is of very short duration and where over 94% of both samples were subsequently reemployed.

which is maximised using standard nonlinear optimisation techniques. To do so we must specify a distribution function $f_{U,E}$. We follow Belzil (1995) in assuming a bivariate normal distribution.[19] In particular we assume that the relationship between unemployment and reemployment durations can be written as follows:

$$\ln t_u = \alpha' X_u + \varepsilon_u$$

where we assume that the error terms are jointly normal distributed:

$$\varepsilon = \begin{pmatrix} \varepsilon_u \\ \varepsilon_e \end{pmatrix} \sim N\left[\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma = \begin{pmatrix} \sigma_u^2 & \sigma_{ue} \\ \sigma_{eu} & \sigma_e^2 \end{pmatrix} \right]$$

The system is identified by the inclusion of UCHANGE in the unemployment equation and by the inclusion of JCHANGE in the reemployment equation. Both of these are exogenous variables computed using the linked NOMIS data on the average local unemployment rates.

The results from estimating this system are given in Table 2. Looking at these results we see that the treatment effect on unemployment duration is not affected by modelling the reemployment duration endogenously, with control group members having significantly longer spells of unemployment. The coefficient on control group in the reemployment equation is insignificant. This is not what one would expect if the Restart interview merely initiated a circular flow of individuals through the system. The remaining coefficients have signs which are consistent with the univariate modelling of section 3. Finally we note that the coefficient on unemployment duration in the employment equation is small and insignificant. The insignificant estimate of ρ suggests that this variable can be treated as exogenous for the purpose of estimating the reemployment equation.

A possible explanation for these results may stem from the fact that our data relate to the long-term unemployed. Specifically since all the sample are long-term unemployed it is possible that no further 'stigma' effects of unemployment duration accrues after one has reached the 6 month threshold

[19] Similar specifications have also been used in other contexts (Lillard (1993)).

of unemployment which conditions inclusion into the data. Hence the disadvantages of being unemployed accrue at six months or less of unemployment and the marginal effect may be small beyond this threshold level. Alternatively the result may in part be driven by the inclusion of spells out of the labour force and training spells in our reemployment durations. For instance training spells tend to be of fixed duration and may vary little with past unemployment duration. We do, however, find some evidence of a scarring or stigma effect when we look at the coefficient on unemployment histories preceding the interview date. Individuals with poor employment records preceding the Restart experiment have significantly shorter reemployment spells. The extent to which this reflects unobserved heterogeneity or true state dependence cannot be determined from our results.

6. The Long-Run Effects of Restart

In this section we present evidence on how the previous findings translate into differences in long-term employment prospects for both groups. Figure 7(a) plots the unemployment rates for both the treatment group and the control group over the 12 year period for which we have data. Figure 7(b) provides the same information in a different format, namely the difference in unemployment rates between control and treatment groups. The first vertical line at March 1989 corresponds to the date at which initial Restart letters were sent to those in the control group[20], while the second vertical line six months later represents the date at which the control group were provided with their first Restart interview. The period in between therefore represents the interval during which the control group was excluded from the Restart process. The advantage of presenting Figure 7(b) along with 7(a) is that differences between the control and treatment group are difficult to discern from Figure 7(a) during times when the unemployment rates of both groups are falling sharply. This is particularly true of the period immediately after the start of the experiment.

Looking at these figures we see that before the experiment began the unemployment rates for both groups were very similar rising to high of 40% in September 1987. This reinforces the view that a random assignment of individuals was achieved in the experiment. The fall in the unemployment rates in 1987 reflects the general economic improvement in the U.K economy at this time. We also notice that in the months preceding the receipt of the Restart letter the unemployment rate for both groups rose to 100%. This is because in order to qualify for the Restart program individuals must have been unemployed for the previous six months.

Comparing the unemployment rates after the experiment date reinforces our earlier conclusion that programs such as Restart may have long lasting effects on unemployment rates. In the 6 months following the initial Restart interview the unemployment rate for the treatment group had become 10 percentage points lower than that of the control group, reflecting the control groups exclusion from the

[20] The precise month varies slightly across individuals with the vast majority receiving the letter in March.

Restart process over this period. This gap closed when control group members entered the process falling to only 1 percentage point about 6 months after the control group had received their interview. However the important finding from these data is that this convergence in unemployment rates was only temporary. Over the next year a 6 percentage point gap between the control group and the treatment group reemerged and this gap was maintained for the remaining three years of the sample.[21] These differences in unemployment rates translate into an average difference of 5.5 months reduction in unemployment for the treatment group relative to the control group over the post-interview period.

One possible explanation for the reemergence of the gap later in the sample period may be differences in the exit states of the control and treatment group. To examine this we use the self-reported work histories discussed earlier. We distinguish between exits to a job, government training scheme, fulltime education or out of the labour force. We look at the exit states for control group members exiting during the interval in which the treatment-control gap was reduced; that is the period of their first Restart interview. Of the control group members exiting at this time only 60% exited to a job, training scheme or full-time education, while the remaining 40% exited out of the labour force. This contrasts with the exit states for the treatment group, 76% of which exited to a job, training scheme or fulltime education and only 24% of which exited to out of the labour force.[22] It is reasonable to believe that exits to any of the first three of these states; a job, training scheme or full-time education provide long-run benefits over and above the initial effect of taking individuals off the register. These may include increasing the human capital of the individual either directly on government schemes or through work experience, increasing individual employees confidence or simply improving the signal being sent to future perspective employers. Exits out of the labour force on the other hand, while reducing the current stock of unemployed, are less likely to have long-run effects, and indeed may lead to a greater detachment from the labour force. If this is so then it not surprising that the reduction in the unemployment gap which occurs when the control group receive their interview could not be

[21] This gap is statistically significant at the 5% level. Confidence intervals are omitted from the diagram for clarity.

[22] The difference between the control and treatment group in both cases is statistically significant at the 5% level.

sustained in the long-run. [23]

The importance of distinguishing between exit states is also evident when we compare unemployment rates between the treatment and the control group separately by sex. Figures 8(a) and 8(b) show the difference in unemployment rates for the treatment group and the control group for males and females respectively. In both cases we see the emergence of a gap during the period in which the control group was excluded from the process and the elimination of the gap when members of the control group were offered their first interview. However there is a striking difference in what happens after this point. For males, as for the population as a whole, the gap reemerges so that the unemployment rate for males in the treatment group is six percentage points lower than for males in the control group 5 years after the initial experiment. In contrast, for females there is no evidence that Restart has a significant long-run effect. The gap becomes insignificant when control group members receive their interview and remains insignificant throughout the remainder of the sample

Again the evidence suggest that a possible explanation for these differences may lie in destination states. For males 18% of the treatment group and 23% of the control group exit out of the labour force. For females however the numbers are 42% and 40% respectively. Thus many of the women in our sample exited out of the labour force and from a Restart perspective it does not matter if the interview takes place at 6 or 12 months : individuals who are ineligible for benefits will be forced to sign off no matter when they are interviewed.

The greater propensity of females to exit out of the labour force may reflect the division of labour which still exists in many households today. Although over the last 10 years earnings of females have been accounting for a larger proportion of family income for most households it is still the case that the wives income is a second income. Furthermore the responsibility for childcare within the family still falls disproportionately on the female partner. Under these circumstances it is possible that a significant proportion of women who were claiming UB would not be available for or willing to take

[23] The importance of timing was also emphasised in earlier work (Dolton and O'Neill 1996). In that study the Restart effect was allowed to be time varying. The results showed that for exits out of the labour force, postponing the interview for 6 months had little effect; what mattered was the receipt of the interview. However for exits to employment the receipt of the Restart interview 6 months later did not compensate the control group for having being excluded earlier.

up employment if a job was found.[24] This may explain why when confronted by the Restart officer many females chose to sign off and exit and the labour force. In these circumstances it is not surprising that the initial benefits of Restart would last only so long as the control group members were excluded from the process.

7. The Policy implications of Restart

For males included in the experiment it thus appears that participation in the Restart process substantially reduced unemployment rates in the long-run. Furthermore it appears that differences in exit states may be an important explanation of this long-run effect. In reaching policy relevant conclusions however it is important to discuss the extent to which our estimates of the impact of the Restart program would generalise to the situation whereby the scheme was operated on a national basis.[25] In evaluating labour market schemes such as Restart, issues of deadweight loss (many of those helped by the scheme would have exited unemployment even without Restart) and substitution (many of those who exit Restart may do so at the expense of individuals currently employed) naturally arise. While these costs are difficult to measure, by targeting the program at long-term unemployed it is hoped that the deadweight burden of such a scheme would be reduced substantially. Furthermore the experience of the control group suggests that many of the treatment group would not have exited without the Restart process.

The substitution hypothesis is often presented in terms of a fixed number of jobs : thus if individual A gets a job it must be at the expense of individual B. The evidence on vacancies suggest that demand side constraints such as these are not the only factor explaining the rise in unemployment and that supply side factors may have a role to play. To the extent that this is the case the above

[24] An example of this was evident in one of the Restart interviews on which we sat in. When asked if she was available for work a female UB claimant answered yes. However when asked about the hours during which she would be available for work she ruled out much of the morning after ten o'clock and a large portion of the afternoon between two and four.

[25] For a discussion on the use of micro experiments in making macro inferences see Garfinkel et al (1993).

substitution hypothesis is no longer as valid. However, it may still be the case that a type of substitution occurs, in that individuals participating in the program may be hired sooner than those not in the program (in our case the short-term unemployed). While we acknowledge that part of the impact of Restart may reflect a reorganisation of the stock of unemployed it is important to realise that altering the stock of unemployed in this fashion may be important in reducing equilibrium unemployment.[26]

It has been suggested that our experimental results may also reflect a type of substitution, namely treatment group members being pushed ahead in the job queue at the expense of control group members. Ideally we would have liked the Restart counsellor not to have known the identity of those taking part in the experiment. If this check had been included in the experimental design then there would have been no scope for the type of substitution described above. We believe that this was probably not the case with the experiment we are describing and so there was the potential for the Restart counsellor to put job openings aside for members of the treatment group at the expense of control group members. While this would show up as a significant treatment group effect in our results the net effect of such a scheme operated on a national basis would be zero. The problem arises because the circumstances faced by control group members is no longer an adequate reflection of what they would face in the absence of the experiment. However, the data suggest that this is not how Restart operated. In the survey data which we have, treatment group members were asked if they were offered a job suggested to them by the counsellor. Only one percent of the treatment group answered yes to this question. Similar results are obtained from individual responses to a question asking where they had heard about the first job they received after the Restart interview. Less than one percent of the treatment group reported hearing about the job at the Restart interview. Excluding these individuals from the analysis does not alter any of the results in our paper. Direct placement of treatment groups members by counsellors does not seem to be an important part of the Restart process. This substantially limits the ability of the Restart counsellor to distort the Employment Service in favour of the treatment group and increases the likelihood that the results we obtain could also be present on a national level.[27]

[26] See Layard, Nickell and Jackman (1991).

[27] It is still possible that the Restart counsellor by improving the job search skills of the treatment

8. How does Restart work and what does it cost ?

At first sight, it may seem that the Restart effect we estimate is too large to be attributable to a 15 minute interview. However, as mentioned earlier the interview was only the beginning of the Restart process. We now describe in more detail some of the channels through which Restart works. In the letter requesting attendance at the interview the claimant is told that "The adviser you see will be able to give you access to up to date information on jobs and how best to look for work, training and other opportunities available, setting up your own business, and unemployment benefits, income support and other benefits, including those which you may be able to get when you start work." The letter goes on to say that "we will also tell you about your responsibilities while signing on and what you must do to remain eligible for payments of benefits.." To try and capture some of these effects Table 3 groups the Restart channels into 5 main categories: direct placement, search behaviour, improvements in information, benefit suspension and a stepping stone to other services. These categories are discussed in more detail in Appendix 3.

As noted earlier, direct placement of individuals by the Restart counsellor does not seem to be an important part of the Restart process, with only one percent of the treatment group reporting receiving a job offer in this fashion. There is more evidence that Restart altered search techniques with almost 15 percent of the treatment group stating that Restart counsellor suggested alterations in their search behaviour. Further evidence of this is provided by the greater reliance by treatment group members on formal search channels for information on their first job, though this difference is not significant. Looking at the differences in awareness of labour market programs we see that in many cases individuals in the treatment group had a greater awareness of the various schemes than those in the control group however these difference are only significant for information on special aids to employment, a labour market program aimed at individuals with disabilities. The evidence concerning

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group member may cause the treatment group member to obtain a job which the control group member was not aware of. However, in this case the circumstances of the control group have not been directly altered and it is still valid to use their outcomes as proxies for the outcome which would occur

the threat of benefit reduction is mixed; there is no significant difference in the self reported likelihood of control and treatment group members having their eligibility assessed or in the minimum wage either would be willing to accept.[28] However 8% of the treatment group did report taking an action directly as a result of receiving the letter calling them to attend the Restart interview. This may be viewed as a response to the perceived threat contained in the interview. Finally the last rows of the table indicate the extent to which the Restart interview acted as a stepping stone to other services. Almost twice as many treatment group members as control group members reported participating in a formal government training scheme over this period, while a greater proportion of treatment group members report using a Job Club over this period. However, this latter difference is not statistically significant. Finally 4% of the treatment group reported attending a Restart course. While it is difficult to quantify the impact of these schemes on unemployment duration[29] the results do indicate that the impact of Restart was not restricted to a single course of action.

While the earlier results indicate that Restart significantly reduced long-term unemployment it is natural to ask at what cost was this achieved. The final section of this paper provides estimates of the cost effectiveness of Restart by measuring the costs and benefits from the perspective of the UB system. We include as benefits the estimated savings on UB payments over our sample period. We calculate this as the average reduction in weeks spent unemployed by the treatment group in each of the 5 years after the experiment times the average weekly UB payment which we estimate as £48 for men and £38 for women. We then calculate the present value of these savings using both a 5% and 10% discount rate. The results are given in the first seven columns of Table 4. The top row provides the result for men while the second row provides the results for women. The present value of savings for men using the higher discount rate is £580, with the annual savings significant in each year except

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in the absence of the program.

[28] See Dolton and O'Neill (1995) for a more detailed analysis of the impact of Restart on reservation wages.

[29] For instance attempts to capture the effects of alterations in search techniques on unemployment duration by including a dummy equal to one if change was suggested produced a significant negative effect of search on the probability of leaving unemployment. However this almost certainly reflects the fact that those told to alter their search are likely to be less able individuals most in need of help rather than a negative search effect per-se.

the second, during which the control group closed the gap on receipt of their interview. For women the estimated savings is £167 but none of the differences are significant in this case.

To measure the costs of Restart we begin with the administrative costs of the program. The 1988/89 government's public expenditure white papers reported that 2.3 million Restart interview were carried out in that year at a cost of £38m. This gives a cost per interview of £15. Clearly the UB gains over and above the administrative costs of the interview are very large. However, as pointed out above, part of the Restart effect we estimate may operate by channelling individuals into other services such as Restart courses, Job Clubs or training schemes. Our costs need to be adjusted to include these services. The Expenditure White paper estimates that expenditure per Restart course in 1988/89 was £100 per place and £120 per Job Club place. Since Restart courses and Job Clubs cater to a different clientele it is unlikely that the same individual would avail of both services. We assume an equal split between programs resulting in a per capita cost of £110. Making the conservative assumption that all members who went through the Restart process were placed as a result of attendance at one of these advisory programs[30] we obtain a statistically significant estimated net gain of £455 for men and an statistically insignificant gain of £42 for women. Since many of the women exited the labour force it is reasonable to assume that they would not have availed of the advisory centres. Possibly a better estimate of the net gain for women is the benefit net of interview costs, which is £152. However again this estimate is not statistically different from zero.[31]

These estimates are crude in that we take no account of the extra benefits in the form of higher wages or taxes once our claimants begin to work or the extra cost associated with the deadweight and displacement effects discussed earlier.[32] However similar methods have been used in the U.S literature to cost welfare reforms. This allows us to compare our results with the findings from the U.S. The

[30] This overestimates the costs of Restart since some Restart participants will be moved out of the labour force as a result of ineligibility without using any of these services.

[31] Our estimates of the net gains of Restart do not make adjustments for attendance at government provided training schemes. As mentioned earlier 12% of the treatment group report attending a government training scheme as opposed to 6% of the control group. Unfortunately we have no information on the training costs associated with Restart.

[32] For an example of a study which attempts to estimate deadweight and displacement effects in the context of training see Deakin (1996).

changes in the system involved in the Restart experiment were more radical than that in many of the U.S work-search experiments. For instance many U.S states already had some degree of monitoring of claimants and so what was measured was a tighter enforcement of the rules. In contrast in the U.K we are essentially measuring the impact of reestablishing a monitoring service. Thus in terms of a comparison of the benefits of the system we may expect larger gains in the Restart program.[33] Provided the changes are properly costed however, the cost-benefit analysis should still provide a valid basis for comparison. Meyer (1995) reports costs-benefit analysis for many of the U.S programs. The average gain from these programs was $95. However, two of these programs the Washington exception reporting scheme and the Nevada experiment seem to be outliers. When these are excluded the average gain is $27. In this context our estimated gain of £455 for males seems large, but it must be remembered that the U.S estimates only measure benefits in the first year after the program and thus only provide an estimate of the short-run gain. If we restrict our findings to the same time span we obtain a net loss of £29. If we relax the assumption that all males are placed through Job Clubs or Restart courses we obtain a short run gain of £81, which is larger than the U.S result but much smaller than the estimated long-run gain.[34] While short-run evaluations may be cheaper to conduct our results clearly show that ther are instances in which they can seriously underestimate the value of the monitoring and work search schemes.

7. Conclusion.

In this paper we examine the long-run impact of the Restart unemployment program. The administrative data used in this paper is well suited to this type of analysis in that it includes a randomly assigned control group whose members were excluded from the Restart process for a period of time.

[33] However Restart was targeted at the long-term unemployed who by definition have proved most difficult to move off the register. This was not the case in the U.S experiments.

[34] Since the long-run studies which are available for the U.S such as Friedlander and Hamilton (1996) suggest that the benefits of the welfare reforms they examine tend to disappear after three to four years, the bias resulting from focusing on the short-term may be less severe in the other U.S experiments than we have found for Restart.

The availability of a control group helps us identify the Restart effect, while the availability of administrative data allows us to examine the long-run effects of the program.

The results of our paper show that the Restart program resulted in a reduction in time spent unemployed in the short run and that this effect was not offset by subsequent shorter spells out of unemployment. In fact the results for our sample suggest that while previous unemployment history matters for reemployment tenure, the duration of the unemployment spell immediately prior to signing off has little impact on subsequent duration 'out of unemployment'. We suspect this finding may in part be due to the fact that all our sample had experienced at least six months unemployment. The exclusion of individuals with shorter durations may prevent us from identifying the scarring or stigma effect of the earlier unemployment spell. Furthermore the inclusion of training spells in our sample, many of which last for a specified period may also hinder identification of a scarring effect.

Extending the analysis to the long-run we find that the combination of benefit checks and counselling present in the Restart course can provide a cost effective way of reducing unemployment. Our findings however show important differences between men and women. The initial gain experienced by females in the treatment group is eliminated once the control group are brought into the process. We feel that this reflects the fact that for many women Restart operated by moving them out of the labour force. For men however extending the program to the control group six months after the treatment group received it fails to compensate them for their earlier losses. Male members of the treatment group who were subjected to the tighter monitoring of eligibility status and who were provided with an improved counselling service had unemployment rates which were six percentage points lower than the control group five years after the initial interview. This differs from the female result in part because of the greater tendency for Restart to place males into employment, education or training rather than out of the labour force.

To examine the Restart process in more detail we documented some of the channels through which the Restart effect is believed to operate. We found no evidence that direct placement by counsellors was important. In contrast it seems as though the Restart process alters search behaviour, helps identify ineligible claimants and acts as a stepping stone to further programs provided by the Employment Service. Unfortunately the design of our experiment does not allow us to identify which

of these routes is most important. Thus while we can document in detail the impact of the program an important policy question remains unanswered. This shortfall highlights a common drawback with many of the experiments presently used to evaluate labour market programs. In order to provide answers which can be better tailored to particular policy questions it would be useful if experiments were available in which only one component of the program differed between the control and treatment group.

Finally we provided a cost-benefit analysis of the Restart program. The main impact of Restart for women was to move them off the register and out of the labour force. While this may reduce official unemployment it does little to develop the skills of these individuals within a working environment. Our estimates of the cost effectiveness of the program for males on the other hand suggests that the monitoring and counselling services provided by Restart is an effective way of reducing long-term unemployment among men. Furthermore our results highlight the potential for large errors in evaluations which focus only on the short-run impact of such programs.

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Table 1
Single Equation Estimates of Unemployment and Reemployment Duration Hazards
(Standard Errors in parenetheses).

Variables	Unemployment Equation	Reemployment Equation
Control	-.19*	.09
	(.05)	(.06)
Uchange	-3.86*	
	(.52)	
Male	-.24*	.76*
	(.026)	(.04)
Age >35	-.09*	-.51*
	(.03)	(.03)
Inner City	-.21*	.03
	(.03)	(.04)
Past Unemp	-.42*	.48*
	(.04)	(.05)
Jchange		-1.26*
		(.38)
Log Likelihood	-24082.01	-20060.22
N	7934	7569

* denotes statistical significance at the 5% level

Table 2
Bivariate Unemployment-Reemployment Estimates
(standard errors in parentheses)

Variables	Unemployment Equation	Reemployment Equation
Constant	1.59*	2.08*
	(.05)	(.22)
Control	.25*	.03
	(.07)	(.08)
Uchange	5.15*	
	(.82)	
Male	.20*	-.18*
	(.04)	(.05)
Age >35	.09*	.11*
	(.04)	(.04)
Inner City	.25*	-.04
	(.04)	(.06)
Past Unemp	.42*	-.20*
	(.06)	(.09)
Jchange	.	.89*
		(.44)
Log T_u		-.06
		(.16)
Parameters		
σ_u	1.20*	
	(.01)	
σ_e	1.10*	
	(.02)	

ρ	-.06 (.17)	

* denotes statistical significance at the 5% level

Table 3.
Channels though which Restart operates
(standard errors in parentheses)

Action	Control Group	Treatment Group
Direct Placement by Restart counsellor	N.A	1.02%
Search		
Restart Officer advised change of search techniques	N.A	14.73%
Found first job via friends	44% (8.2)	39% (2.1)
Had Information on:		
Family income Supplement	68.3% (2.8)	73.4% (.57)
Job Start Allowance	39.8% (2.9)	41.5% (.8)
Jobs in Remploy factories and Sheltered Workshops	28% (5.65)	35.8% (1.75)
Special aids to Employment	1.5 % (1.5)	14.9% (1.3)
Benefit Threat		
Took action as a result of receiving Restart letter	N.A	8%
Visited UBO to discuss eligibility	14.9% (2.0)	17.55% (.6)
Minimum Weekly wage needed to work	£95	£93
Stepping Stone to other Service		
Government Training	6.8% (1.4)	12.1% (.49)
Job Clubs	5.2% (1.2)	7.6% (.4)
Restart Courses	N.A	3.6%

Table 4
Cost-Benefit Analysis of the Restart program (£'s)

Sex	Ben. Year1	Ben. Year2	Ben. Year3	Ben. Year4	Ben. Year5 .	P.V.B 5%	P.V.B 10%	Interview Costs	Job Club Costs

Male	-96*	-58	-153*	-230*	-192*	-647	-580	15	110
Female	-82	-27	-14	-54	-14	-178	-167	15	110

Install Equation Editor and double-click here to view equation.

Figure 1.
Unemployment Hazards.

Install Equation Editor and double-click here to view equation.

Figure 2
Reemployment Hazards

Figure 3
Full-Time Hazard: NCDS

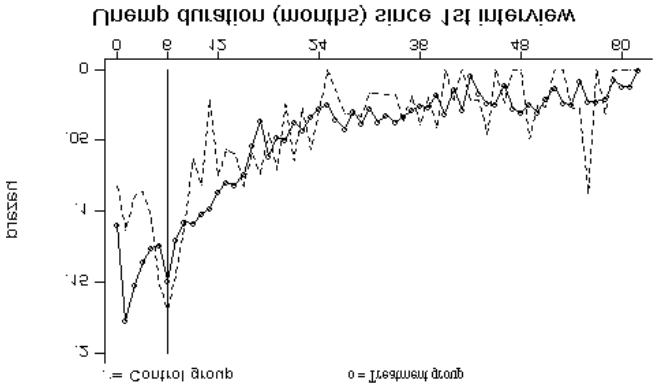

Figure 4
Training Hazard: NCDS

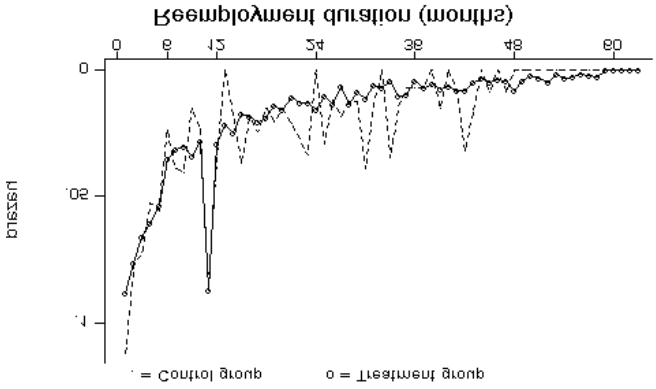

Figure 5
Part time Hazard : NCDS

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click here to view equation.

Figure 6
Our of Labour Force Hazard : NCDS

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click here to view equation.

Figure 7(a)
Unemployment Rates for Treatment and Control Group

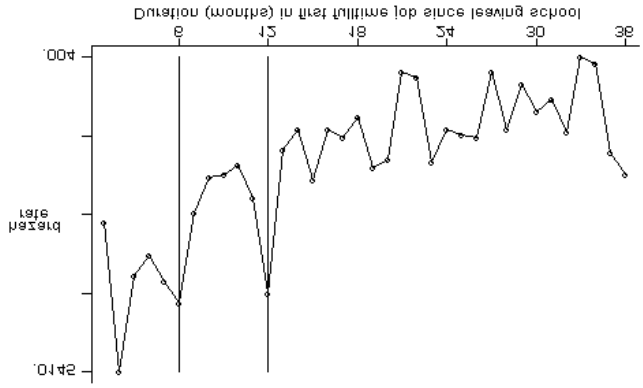


Figure 7(b)

Difference in Unemployment Rates between the Control Group and the Treatment Group.

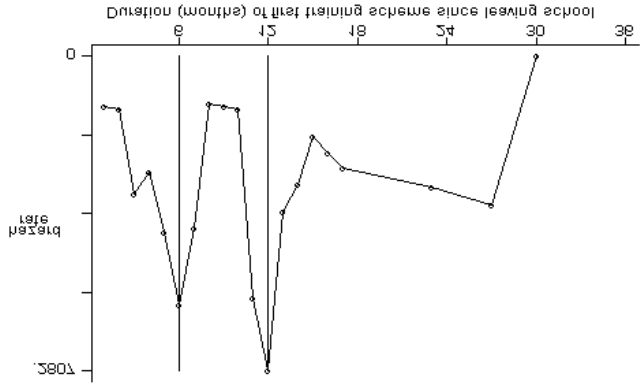
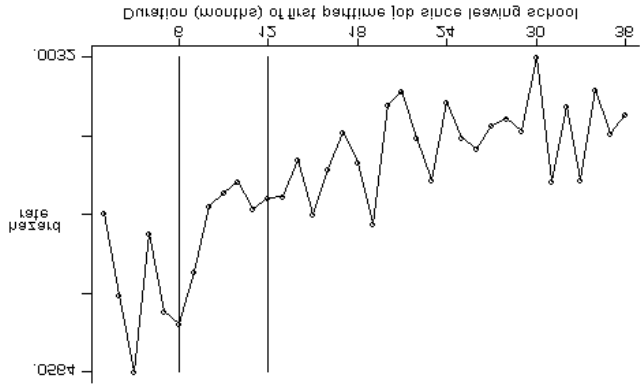


Figure 8(a)
Difference in Unemployment Rates for Males in the Treatment and Control Group

Install Equation Editor and double-click here to view equation.

Figure 8(b)
Difference in Unemployment Rates for females in the Control and Treatment Groups.



Appendix 1: Restart Letter

Please read these notes before completing the attached form

1. The information you provide on this form will be used to help us give you advice and information to get you back to work and to confirm that you satisfy the conditions for receipt of benefit.

2. The adviser you see will be able to give you access to up to date information on

***** jobs and how best to look for work;

***** training and other opportunities available;

***** setting up your own business; and

***** Unemployment Benefit, Income Support and other benefits, including those which you may be able to get when you start work.

3. At the interview we will also tell you about your responsibilities while signing on and what you must do to remain eligible for payments of benefits and credits of National Insurance contributions. To get Unemployment Benefit, NI credits and Income Support as an unemployed person, you must be available for, capable of and actively seeking work.

4. Being available for work means you must :

a be available to work with an employer, for every day you claim benefit (you cannot just look for self employed work);

b be able to start work with an employer immediately (or at 24 hours notice if you have someone to look after; or 48 hours notice if you are doing voluntary work); and

c not reduce your chances of finding work because of;

***** the kind of work you are willing to do; or

***** the rate of pay you will accept; or

***** where you are willing to work; or

***** the hours you are willing to work.

5. If we think you have restricted your chances of getting a job because you have put limits on the work you will accept, your benefit may be affected. If this is the case, your claim will be sent to an independent adjudication officer for a decision. If this happens we will tell you.

6. You must **actively seek work**. This means that you must take some steps, each week, to find work. It will help if you bring to your interview a note of what you have done to seek work, the jobs you have applied for and any replies you have received from employers. we will tell you more about this condition at your interview.

7. If you do not appear to be doing enough to seek work, your benefit will be suspended straightaway for up to 2 weeks, and your claim will be sent to an independent adjudication officer for a decision. If

this happens we will tell you.

Appendix 2: Data Description

Control = 1 if the person was not scheduled to receive an initial Restart interview at 6 months of unemployment and 0 otherwise.

Uchange = percentage change in the person's local unemployment level in the two months preceding the start of the unemployment spell, calculated from the NOMIS data.

Male = 1 if individual was a male, 0 otherwise

Age>35 = 1 if individual was aged over 35, 0 otherwise.

Inner City = 1 if individual lived in an inner city, 0 otherwise.

Past Unemp: Proportion of the individual working life between 1982 and the Restart Interview which was spent in unemployment, calculated from the JUVOS data.

Jchange = percentage change in the person's local unemployment level in the two months preceding the start of the reemployment spell, calculated from the NOMIS data.

Log T_u = log of unemployment duration.

Appendix 3: Restart channels

a). direct placement by counsellor. To measure this we use a variable which indicates if the individual received a job offer as a result of a placement suggested by the Restart counsellor.

b). search behaviour. This includes alterations to search techniques suggested by the Restart counsellor and differences in search techniques employed by groups. For the latter we use the proportion of both groups who heard about their first post Restart job from friends and family. We view this as a measure of informal job search as opposed to the more formal methods likely to be suggested at the Restart interview.

c). improvement in information. We look at whether the individual was aware of schemes such as Family Credit which is an in work benefit similar to the Earned Income Tax Credit in the U.S and various courses for those with disabilities.

d). Benefit suspension: We measure this by visits to the unemployment benefit officer to discuss benefit eligibility. We also look at differences in the minimum wage which would be acceptable to the individual, since the threat effect may induce individuals to reduce their reservation wage as well as actions taken as a result of receipt of the Restart letter.

e). Stepping stone to other services: To measure this we look at differential use of government training schemes and Job Clubs between the treatment and control group as well as use of Restart courses by treatment group.