The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Experimental Evidence from the United Kingdom

Peter Dolton, *University of Newcastle*

Donal O'Neill, *National University of Ireland, Maynooth*

This article examines the long-run effects of the Restart unemployment program in the United Kingdom. The program, aimed at the long-term unemployed, involved a combination of tighter monitoring of benefit eligibility rules and increased job search assistance. We compare the employment behavior of a treatment group who participated in the scheme with that of a randomly chosen control group for whom participation was delayed. While there is little evidence of a long-term benefit for women, the unemployment rate among males in the treatment group was six percentage points lower than that of the control group 5 years after the initial experiment.

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I. Introduction

In recent years many countries have introduced unemployment insurance reforms in an attempt to curb rising levels of unemployment. These reforms often combine stricter "work-search" tests, stipulating minimum levels of job search, and greater provision of job search assistance for the unemployed. However, very little is known about the long-term employment effects of these reforms. Given the absence of suitable long-run data, evaluations to date have tended to focus on the impact of these reforms on the unemployment spell in progress at the time of the study.[1] However, the short-run success of a program may provide a misleading indication of its cost-effectiveness if those who exit return to unemployment soon afterward.

In this article we use administrative data on individuals' monthly labor market status, matched with data from a controlled experiment, to evaluate the long-term effects of the U.K. Restart unemployment program. The Restart program consists of a compulsory interview for the long-term unemployed with an official of the Employment Office. The aim of the program is to reduce welfare dependency. The Restart process combines counseling and encouragement with tighter enforcement of the conditions necessary to qualify for unemployment benefits (UB). Administrative data is available for individuals for 5 years after they began participating in the scheme. The availability of these data allow us to examine whether the positive Restart effect identified in earlier work represents only a short-run solution to the problem of unemployment or whether the program improves the long-term prospects of the individuals involved.[2]

We examine the impact of Restart by comparing outcomes for a treatment group with those of a randomly chosen control group for whom participation in the process was postponed for 6 months. For females, providing control group members with the interview 6 months later eliminates most of the initial gains received by the treatment group. In contrast, the unemployment rate for males who participated in the Restart process at the appointed time was six percentage points lower than for males in the control group 5 years after the initial implementation of the experiment. Our data allow us to distinguish between treatments who were subjected only to the threat component of the program and those who received job search assistance in addition. Our analysis suggests that the threat component associated with being called for an interview may account for the short-run effects of the program, but the services provided

[1] See Woodbury and Spiegelman (1987), Decker (1994), and Gorter and Kalb (1996).

[2] For an analysis of the short-run effects of Restart, see Dolton and O'Neill (1996).

at the interview itself, such as job search assistance, may be important determinants of the long-run effects.

II. The Unemployment Benefit System and the Restart Program in 1989

To understand how Restart worked at this time, it is necessary to describe the U.K. welfare system.[3] The Employment Service is the government agency responsible for the administration and payment of unemployment assistance. The main contributory benefit is unemployment benefits. The individual's right to this payment depends on his or her participation in the national insurance scheme. The main noncontributory benefits are Income Support, Family Credit (similar to the earned income tax credit [EITC]), and Housing Benefit. To receive any of these benefits, individuals must be available for and actively seeking work. The Unemployment Benefit Offices (UBO) conduct individual assessments of benefit eligibility, although prior to Restart these checks were not rigorously enforced. The Employment Service is also responsible for the counseling and placement of the unemployed. These services are provided through a network of "high-street" Jobcentres.

In response to increasing long-term unemployment the Restart program was introduced nationally in April 1987.[4] The process began with the Restart office sending a letter to each individual approaching an unbroken period of 6 months claiming assistance.[5] This letter requested that the individual attend an interview at a stated date and time. 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 Restart interviews took place in Employment Service Jobcentres and lasted approximately 15–25 minutes. If the individual remained unemployed, additional interviews were held every 6 months. During the interview the counselor assessed the claimant's unemployment history, offered advice on benefits, job search behavior, training courses, and in some cases initiated direct contact with employers. For some individuals the Restart interview acted as a

[3] While the goals of the Restart program in operation today are the same as when it was introduced in 1987, the program has developed since its introduction. Further details are available in a supplementary appendix available from the authors upon request.

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

[5] A copy of the Restart office letter is available on-line at http:\\www.may.ie\academic\economics\staff_page09a.html.

stepping stone to other services such as Restart courses, Jobclubs, or Employment Training.[6]

A further feature of the Restart program was the threat to reduce or suspend a claimant's welfare payments. Attendance at the Restart interview is mandatory, and 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. They were then required to attend a Restart interview and to return with evidence of having done so before they were allowed to receive UB again.[7] In 1988–89 over 2.3 million Restart interviews were conducted at a cost of approximately £38 million.

III. The Experimental Design

The use of controlled experiments has been advocated as a means of overcoming sample selection problems inherent in evaluations of labor market programs (see, e.g., LaLonde 1986). In 1989 the Employment Service conducted an experiment to evaluate the impact of Restart. This study identified a sample of individuals approaching their sixth month of unemployment in the period March–July 1989 who were therefore eligible for the first in a series of Restart interviews. 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. Thus, the sample is one of the inflow to Restart and not the outflow from it.

Individuals were selected for the sample on the basis of their National Insurance (NI) numbers. From this sample a control group of 582 people was randomly chosen. Members of the control group, although eligible for an interview, were not asked to attend. If they were still unemployed 6 months later, they were then brought into the Restart process. It must be stressed that the nature of our experiment is a partial one, in that it does not permanently exclude control groups from receiving the treatment but merely delays its receipt for 6 months. This is in direct contrast to most experimental settings, which involve the control group never receiving the treatment. Hence our experimental design permits an evaluation of the impact of postponing search assistance and work search tests by 6 months. This design also permits a test of whether unemployment leads to more unemployment, that is, a test of state dependence versus

[6] For more information on these and the other facilities provided by the Employment Service, see Disney et al. (1992).

[7] The threat component of Restart tended to be played down in official publicity. However, data supplied by the Department of Social Security show that between July and September 1989, approximately 272,000 adjudication officer decisions were made. In 57% of these cases the individual's payment was stopped.

heterogeneity. By design the only difference between the experiments and the controls is that members of the control group must have experienced an additional 6 months of unemployment before receiving the treatment. Any long-run difference between these two groups, therefore, reflects the impact of this extra unemployment on future unemployment.

The structure of the sample was such that it could also be linked to the Joint Unemployment and Vacancies Operating System (JUVOS) data collected by the Employment Service. The JUVOS data provide monthly administrative records of a claimant's employment status, which are free from recall and nonresponse bias. The data we use result in unemployment histories dating from January 1982 up until May 1994, 5 years after participation in the Restart experiment. Such long-term data are rare in an experimental setting and facilitate a detailed examination of the long-run effects of Restart.[8]

While the administrative data permit a long-run analysis of Restart, they do not identify the destination on leaving the UB register—it may be exit to employment, a training program, or simply signing off claiming assistance. To supplement the administrative data we use survey data, which are available for a shorter time period for approximately 60% of the original sample. These data contain self-reported work histories for each individual that distinguish between alternative exit states. The time line shown in figure A1 illustrates the timing of each stage of the Restart experiment.

Summary statistics for members of both the control and treatment groups with valid unemployment data are given in table A1. Statistics are provided for both the full sample and the restricted sample of individuals who replied to the survey questionnaire. We find no significant differences in the characteristics of the full and restricted samples. The average monthly unemployment rates for each group for the 7 years preceding the implementation of the experiment are also given in table A1. If the random assignment was successful, there should be no difference between the treatment and control groups during this period. A comparison of the unemployment rates in table A1 suggests that this was so for both the full and the restricted samples.[9]

[8] Studies by Couch (1992) and Friedlander and Hamilton (1996) also provide a long-run analysis of labor market programs in an experimental setting.

[9] It has been argued that flaws in the design of experiments can lead to misleading results (see, e.g., Heckman and Smith 1995). Several features of the design of the Restart experiment help reduce the likelihood of biased results. Every Employment Service office throughout Britain was contacted while constructing the sample, and there was no scope for local employment offices to opt out of the Restart experiment. This is in contrast to the voluntary nature of the Job Training Partnership Act experiments cited by Heckman and Smith. Having discussed the Restart scheme with members of the Employment Service, it is also our belief that at this time the

IV. The Long-Run Effects of Restart

Using data from the controlled experiment, one can estimate the average effect of Restart by comparing the average unemployment rates for both groups. Figure 1*a* plots the monthly unemployment rates for both the treatment group and the control group over the 12-year period for which we have data.[10] Figure 1*b* provides the same information in a different format, by tracing the difference in unemployment rates between control and treatment groups. The first vertical line at April 1989 corresponds to the date at which initial Restart letters were sent to those in the treatment group, while the second vertical line, 6 months later, represents the date at which control group members were given their first Restart interview.

These data show that unemployment rates for both groups were very similar in the months prior to the start of the experiment, rising to a high of 40% in September 1987. The fall in the unemployment rate in 1987 reflects the general improvement in the U.K economy at this time. In the months preceding the receipt of the Restart letter, the unemployment rate for both groups rose to 100% because, in order to qualify for the Restart program, individuals must have been unemployed for the previous 6 months.

Comparing the unemployment rates after April 1989 provides an estimate of the Restart effect. In the 6 months following the initial Restart interview, the unemployment rate for the treatment group was 10 percentage points lower than that of the control group, reflecting the control group's exclusion from the Restart process over this period. This gap closed when control group members entered the process, falling to only one percentage point about 6 months after the control group had received their interviews. However, over the next year, a six-percentage-point gap reemerged and was maintained for the remaining 3 years of the sample.[11]

As well as illustrating the long-run effects of the Restart treatment, these findings also provide evidence of state dependence. Offering the same services to two groups of individuals who, by design, are identical, except that members of the control group may have been unemployed for 6 months longer when they received the assistance, results in substantially different outcomes for both groups

Figure 2*a* and *b* repeats the above analysis separately for males and

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Restart program was not sufficiently well known for UB claimants to anticipate receipt of the interview. For a detailed discussion of the use of social experiments in evaluating labor market programs, see Heckman, LaLonde, and Smith (1999).

[10] Throughout this article we calculate the unemployment rate in a given month as the proportion of the sample claiming assistance in that month.

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

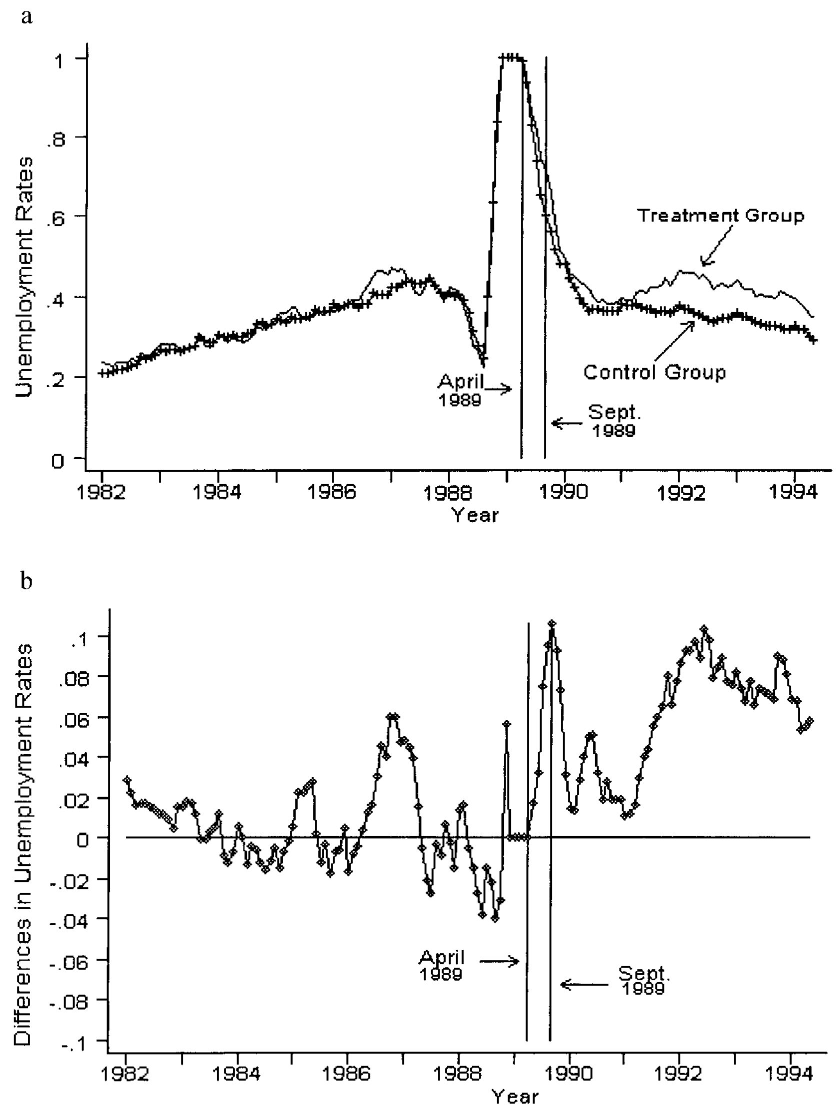

FIG. 1.—*a*, Unemployment rates in treatment and control groups. *b*, Difference in unemployment rates between the control and treatment groups.

females. In both cases a gap emerges during the period in which the control group was excluded from the process. This gap declines when control group members receive their first interview. However, there is a striking difference in what happens after this point. For males, the gap reemerges so that the unemployment rate in the treatment group is six percentage points lower than in the control group 5 years after the experiment. In

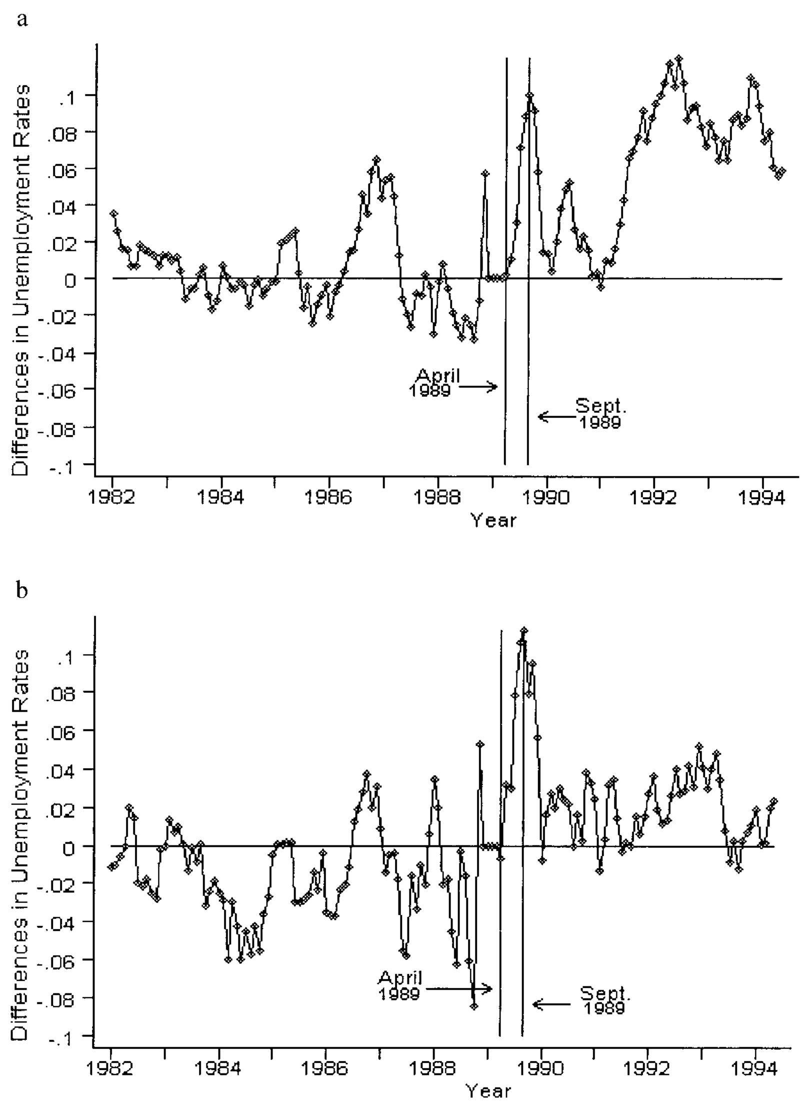

FIG. 2.—*a*, Difference in unemployment rates for males in the control and treatment groups. *b*, Difference in unemployment rates for females in the control and treatment groups.

contrast, for females there is no evidence that Restart has a substantial long-run effect. The gap becomes statistically insignificant when control group members receive their interview and remains so throughout the remainder of the sample. We now examine some possible explanations of these results.

A. Benefit Threat or Job Search Assistance?

Since both the "carrot" and "stick" components of Restart were administered simultaneously, we cannot directly identify their individual effects. This drawback is not unique to Restart. As noted by Meyer in his survey of U.S. labor market experiments, "the [U.S.] experiments make it difficult to determine which treatments are likely to be most successful, as most were a combination of services and enforcement" (1995, p. 128). There are a small number of experiments carried out in the United States that attempt to examine the relative importance of enhanced placement services and work search requirements in reducing unemployment durations (see, e.g., Corson and Nicholson 1985; Corson et al. 1989; and Johnson and Klepinger 1994).

In the Charleston Demonstration (Corson and Nicholson 1985) and the Washington Experiment (Johnson and Klepinger 1994) different combinations of eligibility checks and job search assistance were assigned to different treatment groups to examine their relative importance. However, in some cases it appears that the letter inviting claimants to the job search seminar was perceived as an eligibility check rather than an offer of job search assistance, thus making it difficult to distinguish between the two types of treatment. In their analyses Johnson and Klepinger conclude that it was the receipt of the letter rather than actual participation at the seminar that resulted in people signing off. Thus, it seems that the perceived threat effects may have been important even for individuals scheduled only for job search assistance.

An interesting feature of our survey data that enables us to get some insight into the relative importance of the two components is that almost 20% of the treatment group did not attend a Restart interview. These individuals could not have benefited from the improved placement services and were therefore only subjected to the threat component of Restart. To examine whether these individuals are driving our findings, we repeated the analysis, excluding them from the treatment group. The results are presented in figure 3. In contrast to the previous results, we find no significant difference between the unemployment rates of the treatment and control groups during the period in which the control group was excluded from the process. This suggests that most of the short-run impact of Restart may result from the threat effect associated with the tighter eligibility checks outlined in the initial call to the Restart interview.[12] However, there is still a significant long-run Restart effect in these data. This suggests that the services received by these individuals at the inter-

[12] These results are likely to overstate the importance of the threat effect since presumably some of the treatment group members who did not attend the interview found work naturally during this period for reasons unrelated to the Restart process. Unfortunately, there is nothing we can do to identify these individuals in our data.

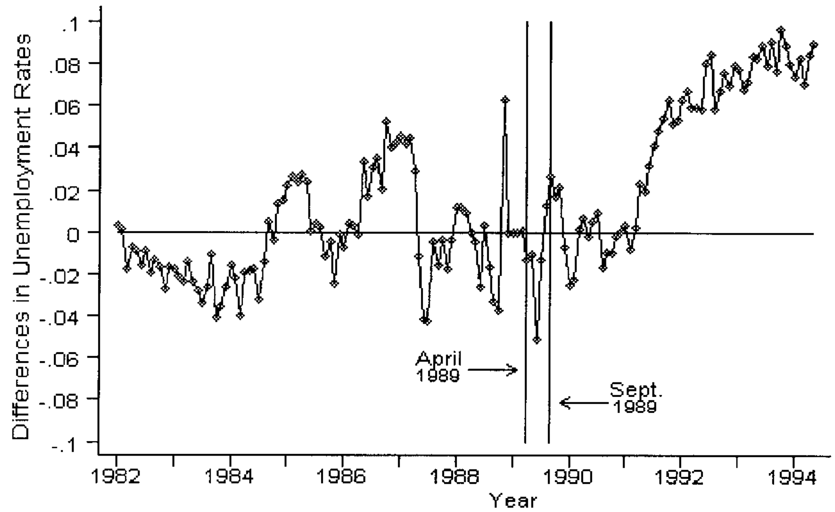

FIG. 3.—Difference in unemployment rates between all control group members and treatment group members who received an interview.

view, such as improved job search assistance, enhanced placement services, or improved access to other government services, may be important determinants of the long-run effects we identified earlier.[13]

Differences in the experimental designs almost certainly explain part of the differences between the results we obtain and the conclusions reached by researchers in the Washington Experiment. However, it is also likely that part of the explanation is due to differences in the nature of unemployment in the United States and United Kingdom. While unemployment rates were systematically lower in the United States (7.1%) than the United Kingdom (10.7%) over this period, the inflow rate into unemployment was actually lower in the United Kingdom (1.4% per month) than in the United States (2.3% per month). The difference in unemployment rates is accounted for by differences in the outflow rate from unemployment. In 1988 the average completed duration of unemployment spells in the United Kingdom was 8 months compared to less than 3

[13] When we examine restricted samples from within the treatment group, we risk losing the randomness provided by the initial experiment. However, we do not believe that this is driving the above results. We have looked at the characteristics of the restricted treatment group and found that they are quite similar to both the full treatment group and the control group. For example, the difference in unemployment rates for the restricted group and the control group averaged over the 6 years prior to the Restart experiment was less than one half of one percentage point. This is negligible when compared with the six-percentage-point differential observed in the final 4 years of our data.

Table 1
Self-Reported Destination States of Sample Members for the Final Quarter of 1989 (%)

	Destination States			
	Full-Time Work	Part-Time Work	Out of Labor Force	Claiming Assistance
Male treatment group	22	10	10	46
Male control group	20	7	16	48
Female treatment group	15	25	25	26
Female control group	15	22	26	31

months in the United States. The average duration of uncompleted spells in both countries was 24 months and 3 months, respectively.[14] There is also evidence that the search behavior of unemployed workers in both countries differed substantially during this period. Newly unemployed people in Britain made only three job applications per month, on average, in 1987 compared to eight for their counterparts in the United States.[15] These comparisons suggest that programs aimed at enhancing placement services, improving job search assistance, and maintaining contact with the labor market may potentially be more effective in the United Kingdom than in the United States.

B. Timing and Gender Effects

As mentioned earlier, the gap between the unemployment rates of the males in the control group and all males in the treatment group reemerged in 1991 after having being eliminated when the control group workers entered the process. For women there is no evidence of a long-run effect. While it is difficult, given our data, to identify the precise reasons for these effects, some potential explanations emerge when we use the survey data to distinguish between exit states for these groups. Table 1 provides a detailed breakdown of the destinations of sample members for the last quarter of 1989. The top portion refers to males, while the bottom portion shows the results for females.

Looking at the results for males, we see that while the proportion claiming assistance over this period is similar for both groups, important differences emerge when we look at other destination states. If we measure inactivity as either "claiming assistance" or being "out of the labor force," we find that 64% of the members of the control group were inactive in the months following the control group interview compared to only 56% for the treatment group. It is clear from this that treatment group members are less likely to experience a loss of human capital through disuse and

[14] Layard, Nickell, and Jackman (1991), p. 223.
[15] Layard et al. (1991), p. 276.

are less likely to be stigmatized than members of the control group, many more of whom have left the labor force. The effects of these differences are likely to emerge later if labor market conditions change. This seems to be the case in 1991 when the U.K. economy entered a recession. Our long-run results indicate that individuals who received the treatment were more able to cope with the adverse labor market conditions that prevailed at the beginning of the 1990s.

The results presented for women in the bottom panel suggest that distinguishing between exit states may also provide part of the explanation of the gender differences. For most households it is still the case that the responsibility for child care falls disproportionately on the female partner. Under these circumstances it seems likely that a significant proportion of women who were initially claiming assistance may not have been available for or willing to take up full-time employment. When confronted by the Restart officer, many females may choose to sign off and leave the labor force rather than enter full-time employment. This seems to be the case in our data, irrespective of when the interview was received. Only 26% of women in the treatment group were still signing on in this period compared to 46% of men in the treatment group. However, in contrast to males, approximately one-quarter of females left the labor force altogether.[16] We have examined this issue formally in earlier work (Dolton and O'Neill 1996). In that paper we estimated a competing risks model of unemployment duration, distinguishing between exits out of the labor force and exits into a job. The results of that analysis also show that people who exited the labor force were much more likely to be women, while men were significantly more likely to exit unemployment into a job.

The other striking feature that emerges from this analysis is the number of women who exited into part-time jobs. The proportion of women in part-time jobs is almost three times larger than the corresponding figure for men. Given the number of women who exit the labor force or enter part-time jobs, it would appear that the employment expectations and potential for skill development among women in the treatment group may have been lower than for similar males. This reduces the benefits of early treatment for women attending Restart and may explain the lower long-run effects of the program for women. It is important to note that this explanation relies on unobserved compositional differences between males and females in the sample. It says nothing about the effectiveness

[16] We have further evidence of this from the Restart interviews that we sat in on. In some of those interviews female workers declared themselves available for work. However, when pressed further by the Restart officer, some of these women admitted that they would only be able to work very restrictive hours.

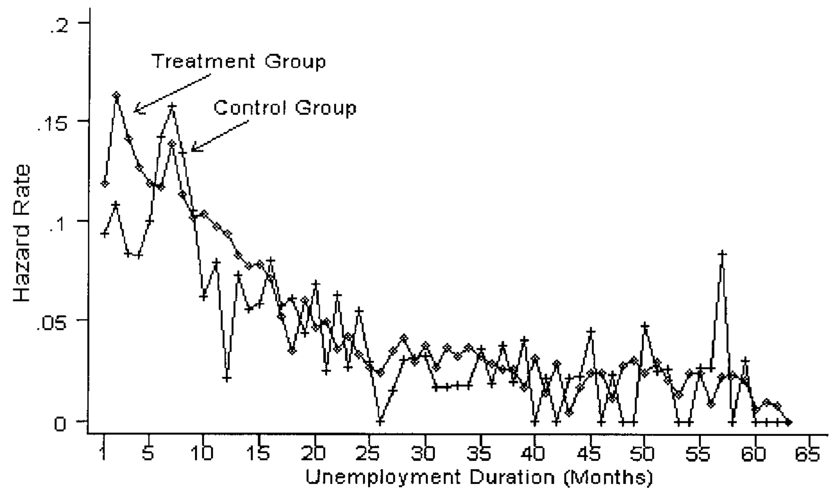

FIG. 4.—Unemployment hazards for the control and treatment groups

of the program for two individuals identical in every respect other than gender.

C. Employment or Reemployment Effects?

The results so far clearly indicate that Restart had a significant effect on unemployment rates. In steady state we can write the unemployment rate as the ratio of the "inflow rate" (the rate at which people leave employment for unemployment) and the "outflow rate" (the rate at which people leave unemployment for employment). Hence, Restart may reduce unemployment rates either because it helps people find jobs quicker or because it helps employed participants keep their new jobs longer. We can examine the impact of Restart on unemployment duration (beyond the initial 6 months) by plotting the exit hazard for the treatment and the control groups.[17] The results shown in figure 4 reveal negative duration dependence for both groups. Furthermore, we note the striking difference in the hazard functions for the control and treatment group in the 5–6 months following the initial Restart interview. In particular, we see that over the period in which the control group was excluded from the process, members of this group were only 70%–80% as likely to exit unemploy-

[17] In this analysis the reference point (duration equals one) corresponds to the seventh month of unemployment, since by construction all our samples have been unemployed for at least 6 months.

ment as members of the treatment group.[18] We also notice a significant spike in the hazard functions approximately 6 months after the start of the experiment, which is consistent with attendance at the Restart interview at 12 months of unemployment. The fact that the spike is more pronounced for the control group is consistent with this being the first meeting with the Restart counselor.

To examine the impact of Restart on reemployment duration, we must allow for the possibility that the duration of unemployment affects future reemployment duration.[19] To allow for this, we need to control for initial unemployment durations when examining the reemployment durations. However, we cannot simply condition on initial unemployment duration in the reemployment hazard. The fact that Restart affects the duration of unemployment may remove the comparability between the control and treatment groups in any equation that holds unemployment duration constant. If Restart affects the probability of employment, then other things being equal, we are more likely to observe an employment spell for treatment group members than for control group members. If there is heterogeneity within groups, then it is likely that the employed treatments will have inferior characteristics than employed controls, contaminating the comparability of the groups. We allow for the possibility of dynamic sample selection bias by estimating a bivariate duration model, including the duration of unemployment as an explanatory variable in the reemployment duration equation, while also controlling for possible correlation in the error structure. In particular, we assume that the relationship between unemployment and reemployment durations can be written as follows:

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

$$\ln t_e = \beta' Z_e + \gamma \ln t_u + \varepsilon_e,$$

where t_u is the duration of the initial unemployment spell, t_e is the duration of the subsequent reemployment spell, X and Z are vectors of explanatory variables that are described in detail in appendix A, and ϵ_u and ϵ_e are error terms that we assume are jointly normal distributed.[20]

The estimation results presented in table 2 indicate that the correlation coefficient, ρ, is statistically insignificant. This implies that the duration of the most recent unemployment spell can be treated as exogenous for the purpose of estimating the reemployment equation. This suggests that dy-

[18] We have estimated confidence intervals for these hazard functions that support the differences discussed in the text. However, for the sake of clarity we have not included them in the figure.

[19] Some examples of this are provided in Heckman and Borjas (1980) or Belzil (1995).

[20] The model is described in more detail in app. B.

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)
Male	.20*	−.18*
	(.04)	(.05)
Age > 35	.09*	.11*
	(.04)	(.04)
Inner City	.25*	−.04
	(.04)	(.06)
Past Unemp.	.42*	−.20*
	(.06)	(.09)
$\dot{U}_1$	5.15*	
	(.82)	
$\dot{U}_2$		.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.

namic sample selection bias is not a problem in this Restart experiment.[21] Furthermore, our earlier conclusions are not affected by the more detailed modeling of the process. Members of the control group have significantly longer spells of unemployment, while reemployment durations are not significantly different between the two groups.[22] From this it would appear

[21] The Restart interview itself is if of short duration and as such does not really interrupt the unemployment spell. In this respect Restart is similar to the JTPA-CT program analyzed by Eberwein, Ham, and LaLonde (1997), in which dynamic sample selection bias did not appear to be a serious issue. Furthermore, since we have 5 years of follow-up data, a large proportion of both samples (94%) experience an employment spell during this period. This would further reduce the likely impact of selection bias. We are grateful to Robert LaLonde for helpful comments on these issues.

[22] In estimating the model, we also control for local unemployment conditions, sex, age, living in an inner city, and previous unemployment histories. These estimated coefficients tend to have the expected signs. We find some evidence of a scarring or stigma effect when we look at the total proportion of time spent unemployed preceding the interview. Individuals with poor employment records prior to Restart have significantly shorter reemployment spells. The extent to which this reflects heterogeneity or true state dependence cannot be determined from our results.

Table 3
Cost-Benefit Analysis of the Restart Program (£; Standard Errors in Parentheses)

	Benefit Year					Interview	Jobclubs
Sex	1	2	3	4	5	Costs	Costs
Male	−101*	−61	−165*	−247*	−206*	15	110
	(44)	(69)	(72)	(72)	(74)		
Female	−88	−29	−15	−59	−15	15	110
	(53)	(70)	(68)	(67)	(63)		

* Denotes statistical significance at the 5% level.

that the program is most effective in reducing the spells of unemployment duration rather than extending reemployment durations.

V. Cost-Benefit Analysis of Restart

While Restart may have significantly reduced unemployment rates among the long-term unemployed, it is natural to ask what the cost of these reductions was. In this section of the article we provide estimates of the cost-effectiveness of Restart from the perspective of the UB system.[23] 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, multiplied by the average weekly UB payment, which we estimate as £48 for men and £34 for women. We then calculate the present value of these savings using a 10% discount rate. The yearly benefits are given in the first five columns of table 3. The present value of savings for men is £619, with the annual savings significant in each year except the second. For women the estimated saving is £181, but none of the female differences are statistically significant.

We measure the cost of Restart by looking at the administrative costs of the program. The 1988–89 government's public Expenditure White Paper reported that 2.3 million Restart interviews were carried out in that year at a cost of £38 milllion. This gives a cost per interview of £15. The UB gains over and above the administrative costs of the interview are clearly very large. However, as noted earlier, part of the Restart effect may operate by channeling individuals into other services such as Restart courses, Jobclubs, or training schemes. Our costs need to be adjusted to include these services. The Expenditure White Paper estimates that expenditures per Restart course in 1988–89 were £100 per place and £120 per Jobclubs place. Since Restart courses and Jobclubs cater to a different

[23] Throughout our study we restrict our attention to the direct benefits associated with lower unemployment rates. There may be indirect benefits such as reductions in crime and improvements in health. Unfortunately, we have no way of quantifying these effects in our study.

clientele, it is unlikely that the same individual would use 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,[24] we obtain an estimated net gain of £494 for men and £56 for women. Since many of the women exited the labor force, it is reasonable to assume that they would not have used the advisory centers. Possibly a better estimate of the net gain for women is the benefit net of the interview cost, which is £166. However, as we mentioned earlier, none of the yearly benefits for women are statistically significantly different from zero.[25]

Meyer (1995) reports cost-benefit analyses for many of the U.S programs. The average gain from these programs was $95. However, two of these programs, the Washington exception reporting treatment and the Nevada experiment, seem to be outliers. When these are excluded, the average gain is $27. In this context our estimated gain of £494 for males seems large. This is even more so when we recall that our restart experiment is only pure for the first 6 months of the experiment. After this, our treatment effects are measured relative to a group that received a delayed treatment. The results reported in this section, therefore, are likely to underestimate the program effects relative to a situation in which the control never received the treatment. Against this, however, is the fact that the U.S. estimates only measure benefits in the first year after the program. If we restrict our findings to the same time span, we obtain a net loss of £24. If we relax the assumption that all males are placed through Jobclubs or Restart courses, we obtain a short run gain of £86, which is larger than the U.S. result but smaller than the estimated long-run gain.

Our cost-benefit analysis takes 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. If members of the treatment group gained employment at the expense of workers not receiving the treatment (displacement), then our experimental results are likely to overstate the impact of Restart on the entire economy.[26] Such an outcome is more likely if the number of jobs available is fixed. In this case, Restart may simply move people to the top of the job queue,

[24] This overestimates the costs of Restart since some Restart participants will have left the unemployment register simply because they did not satisfy the eligibility checks.

[25] Our estimates of the net gains of Restart do not make adjustments for attendance at government-provided training schemes; 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.

[26] For a microanalysis of displacement in the context of experimental studies, see Davidson and Woodbury (1993).

either as a result of greater search intensity, tighter eligibility controls, or because preferential attention is given to treatment group members by Restart counselors. However, framing the argument in this way may be misleading. The evidence on vacancies suggests that demand side constraints cannot explain all the rise in unemployment in the United Kingdom in this period (see, e.g., Layard et al. 1991) and that supply side factors may have been important.

To avoid counselors' manipulating the services in favor of the treatment group ("creaming"), it would have been ideal if the Restart counselor had not known the identity of those taking part in the experiment. We do not believe that this was the case. Nevertheless, we are confident that there was little scope for the Restart counselor to provide special attention to members of the treatment group.[27] In the survey data, treatment group members were asked if they were offered a job that had been referred to them by the counselor. Only 1% of the treatment group answered yes to this question. Excluding these individuals from the analysis does not alter any of the results in our article.

Finally, if displacement effects played an important role in explaining our experimental findings, one would expect to see only small changes in the aggregate unemployment rate following the introduction of Restart. This does not seem to have been the case. Aggregate time-series studies that analyze the impact of Restart support our findings.[28]

VI. Conclusion

In this article we examine the long-run impact of the Restart unemployment program using experimental data in which members of a randomly assigned control group were excluded from the Restart process for 6 months. The presence of a control group helps identify the Restart effect, while the availability of administrative data allows us to examine the long-run effects of the program. We find that the combination of a threat effect and job search assistance provides a cost-effective way of reducing long-term unemployment in the United Kingdom. Individuals who participated in the program experienced a reduction in time spent

[27] White and Lakey (1992) note that "the PSI research team visited local offices to observe Restart in operation. Our view based on this observation was that biased treatment of those in the survey was unlikely. Those in the survey sample formed a small part of a much larger caseload at any local office (about 1 in 100 of those interviewed over a period). The pressures of the case-load in our view left little scope for the Restart counselors to give special treatment to those in the survey sample" (p. 16).

[28] Disney et al. (1992) examine the impact of Restart using aggregate data on unemployment flows and vacancies and conclude that Restart "accounts for a very substantial part of the fall in long-term unemployment (during the late 1980's)" (p. 238).

unemployed in the short run. The initial gain experienced by females in the treatment group is eliminated once the control group is brought into the process. For men, however, providing the treatment to control group members 6 months later failed to compensate them for their earlier losses. Male members of the treatment group had unemployment rates that were six percentage points lower than the control group 5 years after the initial interview. These results are all the more remarkable when we note that the experiment was a partial one that delayed participation of the controls for 6 months rather than withholding the treatment altogether.

In examining the channels through which Restart operates, we found that the threat component of Restart may have been important in generating the initial benefits of the program. However, in addition, the services provided at the Restart interview played an important role in generating the long-run effects we estimate. This has implications for studies examining the importance of work-search tests relative to enhanced counseling. If the timing of benefits from alternative labor market schemes differs, then short-run studies risk not only ignoring potential gains but also doing so in a way that biases the evaluation.

Appendix A

Data Appendix: Variable Definitions

Control = one if the person was not invited to receive an initial Restart interview at 6 months of unemployment, and zero otherwise.

$\dot{U}_1$ = percentage change in the local unemployment level in the 2 months preceding the start of the unemployment spell. This was calculated using the National Online Information System data (NOMIS). The NOMIS data contain monthly indicators of local labor market conditions at the level of an individual's travel-to-work area. Travel-to-work areas approximate self-contained labor markets in that commuting to work tends to occur within the boundary of the area. There are 380 travel-to-work area defined in our data, and the local labor market conditions for each of these areas are available over the period of our study.

$\dot{U}_2$ = percentage change in the person's local unemployment level in the 2 months preceding the start of the reemployment spell, calculated from the NOMIS data.

Male = one if individual was a male, zero otherwise.

Age > 35 = one if individual was aged over 35, zero otherwise.

Inner City = one if individual lived in an inner city, zero otherwise.

Past Unemp. = proportion of the individual working life between 1982 and the Restart interview that was spent in unemployment, calculated from the JUVOS data.

Log t_u = log of unemployment duration.

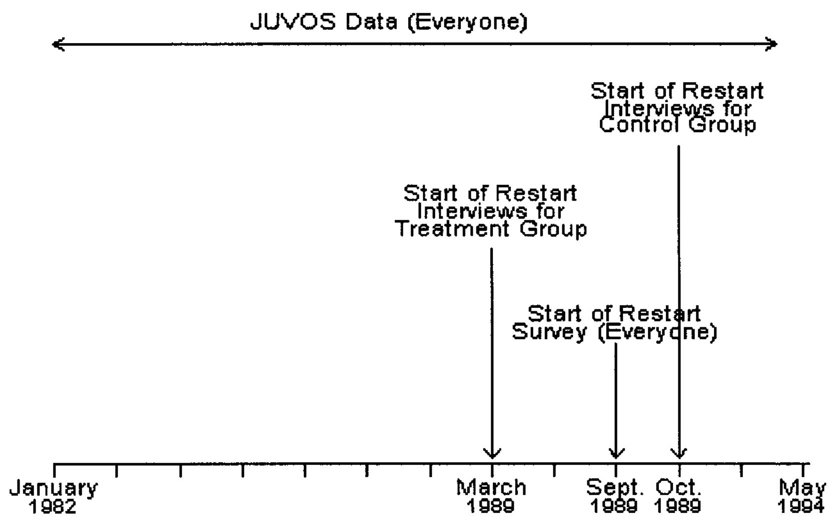

FIG. A1.—Timeline for restart experiment

Table A1
Summary Statistics (Standard Errors in Parentheses)

	Full Sample			Restricted Sample		
Variable Name	Treatment	Control	Difference	Treatment	Control	Difference
Male	.70	.73	−.03	.68	.70	−.02
	(.005)	(.019)	(.02)	(.007)	(.026)	(.03)
Age > 35	.35	.38	−.03	.39	.44	−.05
	(.005)	(.021)	(.02)	(.007)	(.028)	(.03)
Inner city	.20	.21	−.01	.18	.20	−.02
	(.004)	(.018)	(.02)	(.006)	(.022)	(.02)
Unemployment rate:						
1982	.22	.25	−.03	.22	.22	.00
1983	.28	.29	−.01	.27	.25	.02
1984	.31	.31	.00	.29	.29	.00
1985	.35	.35	.00	.33	.35	−.02
1986	.38	.39	−.01	.37	.40	−.03
1987	.43	.43	.00	.41	.42	−.01
Sample size	7,462	472		4,370	292	

Appendix B

The Bivariate Duration Model

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 distinguish between three types of observations: (i) people for whom we observe completed spells of unemployment and reemployment spells, (ii) people whose unemployment spell is censored and thus have no reemployment spell, and (iii) people whose unemployment spell is ended but who have a censored employment spell. To capture the contribution of these individuals to the likelihood, we define: c_1 = one if neither spell is censored, and zero otherwise, and c_2 = one if the unemployment spell is not censored, zero otherwise.

The likelihood function can then be written as

$$L = \sum_i \Big(c_{1i} \ln\left[f_{U,E}(u,e)\right] + (1 - c_{2i}) \ln\left[1 - F_U(u)\right] + (1 - c_{1i})\, c_{2i} \ln\left\{\left[1 - F_{E|U}(e)\right] f_U(u)\right\}\Big),$$

which is maximized using standard nonlinear optimization techniques. To do so we must specify a distribution function $f_{U,E}$. We follow Belzil (1995) in assuming a bivariate normal distribution. In particular, we assume that the relationship between unemployment and reemployment fen as follows:

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

$$\ln t_e = \beta' Z_e + \gamma \ln t_u + \varepsilon_e,$$

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

The system is identified by the inclusion of U_1 in the unemployment equation and by the inclusion of U_2 in the reemployment equation. Both of these are exogenous variables computed using the linked NOMIS data on the average local unemployment rates. Since all unemployment spells started at approximately the same time, U_1 captures only regional variation in unemployment rates. However, since the timing of reemployment spells across individuals in the sample may differ, U_2 will capture both temporal and regional variation in local labor market conditions.

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