

Assessing the Effectiveness of Training and Temporary Employment Schemes: Some Results from the Youth Labour Market

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Abstract: This paper suggests some ways in which labour market programmes of training and temporary employment should be evaluated. Essentially such programmes should be evaluated relative to the counterfactual: what would have happened to participants had they not participated? The paper discusses the problems of modelling this counterfactual and applies the resulting methods to data on a cohort of school leavers followed over a five and a half year period during the late 1980s.

I INTRODUCTION

In common with most other western European countries, Ireland responded to the rise in unemployment during the 1970s and early 1980s by expanding the provision of training and temporary employment programmes for the unemployed. This paper attempts to estimate the degree to which some of these programmes were effective during the mid-1980s in improving the chances of unemployed young people finding work.

II BACKGROUND

During the 1960s and early 1970s the role of manpower policy in Ireland was seen to lie in training the labour force (and retraining those sections of it leaving the declining traditional industries) and generally facilitating the efficient matching of the supply of, and demand for, labour. These aims were consistent with the then current ideas of an "active manpower policy" as advocated by the OECD in the mid-1960s. This policy in turn arose out of a European climate of full employment and economic growth.

In the mid-1970s the role of manpower policy was widened. Growth in

unemployment led, initially, to the introduction of a range of schemes whose main rationale was the provision of temporary jobs. So, in 1975, AnCO introduced the Community Youth Training Programme (CYTP). In the same year the Premium Employment Programme was introduced by the Department of Labour. This was a job-subsidy scheme to encourage recruitment. In the following year the Environmental Improvement Scheme run by the Department of the Environment was set up, and in 1977 the Department of Education's Temporary Grant Scheme for Youth Employment came into operation. These latter two – the Environmental Improvements Scheme and the Temporary Grant Scheme for Youth Employment – were both temporary employment programmes for young people. One of the most important of temporary employment schemes – the Work Experience Programme – was established in 1978.

The focus on temporary employment schemes arose because, during the mid-1970s, the employment crisis was held to be a temporary phenomenon. This view can be found in official reports and plans of the time. The Coalition Government's White Paper *Economic and Social Development 1976-1980*, for example, coupled a call for the maintenance of cost competitiveness via wage restraint with expressions of optimism that the economy's difficulties would be only temporary. At the end of the 1970s and into the 1980s, when the invalidity of this view became apparent, there was a discernible shift towards an expansion of training.

Initially, AnCO had been given responsibility for three areas of training – apprenticeship; in-firm training; and non-apprenticeship adult training. From the mid-1970s and subsequently this third strand came to take on an increasing importance: it became, in effect, training for the unemployed. Initially, non-apprenticeship adult training had been a relatively minor part of AnCO's activities, and all such training was undertaken directly by AnCO itself. However, as provision in this area increased, AnCO began to use the services of outsiders who were contracted to provide specific adult training courses. Thus there developed "external" training, alongside direct training by AnCO. The growth of AnCO adult non-apprenticeship training is charted in Table 1. It is clear from this table that there exists a strong correlation between the size of the unemployment problem and the provision of training.

While training provision for the unemployed continued to grow during the 1980s, increasing unemployment, and the need for governments to be seen to be "doing something" about the problem, led to a resurgence of temporary employment schemes, notably in the form of the introduction of the Social Employment Scheme in 1985.

Table 1: *Numbers Training Annually on AnCO Non-Apprenticeship Training Programmes*

<i>Year</i>	<i>Training Centres</i>	<i>External Training</i>	<i>CYTP</i>	<i>Other Workshops</i>	<i>Total</i>
1970/71	1,113	0	0	0	1,113
1975	5,302	1,415	0	0	6,717
1980	7,546	3,430	2,071	914	13,961
1984	16,717	9,771	5,567	3,279	35,335
1987	16,893	7,207	4,021	3,646	31,767

Source: AnCO Annual Reports, various.

III THE OBJECTIVES OF TRAINING AND TEMPORARY TRAINING SCHEMES

This paper focuses on training and temporary employment schemes. Since virtually all of these are provided for the unemployed, it is reasonable to assume that helping them to get a job is a major objective of such schemes.¹ Of course, it may not be the only objective. The immediate aim of many FAS courses is to provide individuals with specific skills. However, if these individuals cannot then obtain jobs one might legitimately question the effectiveness or suitability of the programmes. In addition, most of those, in our sample, who entered training programmes or temporary employment schemes did so from unemployment. One can readily conclude, therefore, that such young people are motivated to enter in the expectation that participation will improve their chances of escaping from unemployment.

The degree to which such schemes improve the labour market position of participants can only be measured relative to what participants' positions would have been if they had not participated. So, conventional measures of "effectiveness", such as the percentage of participants who get jobs after finishing a programme, are not proper measures of effectiveness, because they neglect the issue of how many of these participants would have got jobs even if they had never participated. To give an example: in some work on the now defunct Work Experience Programme (Breen, 1988) we estimated that, although over two-thirds of participants got jobs after participating, roughly this proportion of them would have got jobs even if they had not participated. Thus the effectiveness of the scheme was much less than its placement rate might have suggested.

1. Even if former participants acquire jobs after training we should like to know the degree to which they made use of their newly acquired skills. This, and related issues such as the impact of training on earnings, is a question we cannot pursue here.

IV DATA

In this paper we try to measure effectiveness as outlined above. The data on which this research is based come from repeated interviews of a sample of young people who left full-time post-primary education in the school year 1981-82. Initially they were interviewed in May and June 1983 as the 1983 School Leaver Survey. They were re-interviewed in November 1984 and again in December 1987/January 1988 when data were collected on 1,644 respondents. This was weighted to make the sample representative of the 1981-82 school leaver cohort. For each sample member we have a complete labour force history which contains information on each job, spell of unemployment, training programme, and so on, between leaving school and the 1987/88 survey. This information includes, of course, the date of entering and leaving each job, spell of unemployment, etc. More details of the sample are available in Breen (1991), Chapter 2.

In this paper we confine our analysis only to those young people who did not go directly into third level education after leaving school. Thus we are, for the most part, confining ourselves to young people who entered the labour market on leaving school. As a result of the exclusion of third level entrants, this reduces the size of the sample on which the results of this paper are based to 1,116. Table 2 shows the educational status of this subsample at the time of the later survey.

Table 2: *Educational Qualifications of Sample at 1987/88 Survey*

	<i>Percentage</i>
No Qualifications	9.9
Group or Intermediate Certificate	39.3
Leaving Certificate	48.1
Third Level Incomplete	0.6
Third Level	2.0

Note: Those sample members who entered third level institutions did so following a spell in the labour market.

V AIMS OF ANALYSIS

Our aim is to assess the effectiveness of training and temporary employment programmes in improving participants' chances of getting a job, using the data above. By training programmes we mean all non-apprenticeship courses of not more than six months' duration: in other words, this includes all courses that

fall under the heading of FAS (formerly AnCO) adult training. Temporary employment schemes refer chiefly to the Work Experience Programme and Teamwork.

We use two measures of the effectiveness of such programmes in helping young people to acquire jobs. The first of these is a measure of short-term effect, based on the probability of acquiring a job immediately following participation on a programme. The second is a longer-term measure, based on the probability of a former participant being in a job just under one year after participation ended. We do not confine ourselves to participants who completed such programmes: those who left without completing are included in the analyses. Since we shall be making comparisons between participants and the unemployed we exclude from our analyses all those who entered a programme directly from a job (roughly 10 per cent of our participant samples).

As outlined earlier, the effectiveness of a programme in securing some objective (such as improving the chances of participants getting a job), should be measured as an incremental effect. In other words, how much does the programme increase the likelihood of participants' getting a job *above what it would have been had they not participated*. So to assess effectiveness in this way we require more than merely placement rates: we also need some estimate of what would have happened under the counterfactual assumption. There are various ways in which we can try to make this latter estimate, but in this paper we do it by comparing what happened to a sample of people on training and temporary employment programmes with what happened to a sample of unemployed people who did not participate in any programme. The issue on which the correctness of this type of evaluation depends is whether or not this yields an accurate estimate of the outcome under the counterfactual scenario. This is something we discuss later in the paper. Before that, however, we outline the mechanics of our analysis.

(a) *Training*

We began by drawing seven samples of participants in training, defined as follows. The first sample comprised all those who were on a training programme in December 1983. The second comprised all those on a training programme in June 1984. The third through to seventh groups were defined similarly in respect of June and December of each year to December 1986. We refer to these, for convenience, as our "sampling dates". The choice of six-monthly intervals ensured that an individual would not be included in two samples in respect of the same period of training.² We terminated the

2. Though an individual could, of course, be included in more than one sample in respect of different spells of training. There are no cases of this in our data.

sampling in December 1986 because the level of participation in training after this date by members of the cohort was negligible. As well as these participant groups we also drew a corresponding set of seven samples of what we term comparison groups: these were all individuals who were unemployed or seeking a first job at each of these dates. Essentially our analysis comprises a comparison of the fortunes of the members of the seven participant and seven comparison groups.

(b) *Temporary Employment*

The set-up was essentially the same for temporary employment schemes, except that here we drew four annual samples (rather than seven semi-annual) of all participants at December 1983, 1984, 1985 and 1986, and corresponding comparison groups of the unemployed. This was necessitated by the longer duration of temporary employment programmes.

While there are a number of ways in which we could analyse these data, we adopted what is probably the simplest method to measure the long- and short-term effectiveness of training.

(i) Short-term effectiveness

Here our dependent variable was, of necessity, defined slightly differently for members of the participant and comparison groups. For members of the participant group it measured whether or not they entered a job immediately after leaving training or a temporary employment scheme. For members of the comparison group it measured whether or not they were in a job t months after the sampling date, where t is the average time remaining, at the sampling date, on the programme among the participant group. For example, consider the sample in training in December 1985 and the comparison group of individuals unemployed in December 1985. The former score 1 on the dependent variable if the state they entered after training was a job, 0 otherwise. At December 1985 the participants would have been on schemes of varying length, and would each have already participated for some period before December 1985. For this group we calculated the average period which they spent on the programmes after December 1985. This latter is our measure t . The members of the comparison group then scored 1 on the dependent variable if they were in a job t months after December 1985, 0 otherwise. In other words, the comparison is between the probability of getting a job after training and the probability of an unemployed person getting a job within the same time period.

(ii) Long-term effectiveness

Here the dependent variable was constructed in the same way for participant and comparison groups: 1 if they were in a job one year after the initial observation date (i.e., December 1986 for those on a programme or unemployed in December 1985), 0 otherwise. We excluded from this analysis any members of the comparison groups who themselves entered and left a programme during this one year interval.

VI AGGREGATION

The data on which these analyses were carried out were not collected specifically for the purpose of evaluating training and temporary schemes. As a result they are not ideal for this exercise. In particular, the numbers participating in training and temporary employment schemes are quite small, as Table 3 shows. This meant that we were unable to analyse specific programmes individually. In addition the small number of participants also meant that, at each sampling date, the numbers in the participant group were small. To overcome this we have aggregated the seven training participant samples taken at each date into one sample, and likewise for the four temporary employment scheme samples and the corresponding comparison groups. Thus the participant and comparison groups are not samples of individuals but samples of spells spent in training, temporary employment or unemployment, and each individual can, in theory at least, contribute more than one spell to each sample as well as appearing in more than one sample.

Table 3: *Sample Numbers Participating in Training and Temporary Employment Programmes*

Training	122
Temporary Employment	272
Comparison Group	1,562

VII METHODOLOGICAL ISSUES

As we have set up the analysis, we measure effectiveness not by the percentage of the participant group in jobs either immediately or one year after participation, but by the difference in the percentage of participants in jobs and the percentage of the comparison groups in jobs. Essentially, then, the comparison group is being used to tell us what would have happened to participants if they had not participated.

This identification of what happens to the comparison group with what would have happened to participants had they not participated is (relatively) unproblematic if individuals can be randomly assigned to either the participant or comparison group – in which case we should refer to the latter as a control group. This is the usual strategy adopted in controlled experiments – to evaluate the effectiveness of a new drug, for example. This approach has been followed in evaluating some manpower measures in the United States (e.g., Lalonde, 1986) though, as one might imagine, it is politically unpopular and difficult to do. It is more usually the case that participants and comparison group members are not randomly assigned. This leads to two difficulties. First, the two groups may, on average, differ in respect of characteristics which have a bearing on the outcome measure. In our case, there may be differences between the two groups according to, say, their educational qualifications, which will have a bearing on the probability of their acquiring a job. This means that it is not sufficient simply to compare the percentages getting a job among the two groups: we need information on how the two groups differ in respect of characteristics which also have a bearing on the likelihood of getting a job, and we need to take this into account in our analysis. The second problem is that we may not have measured all such relevant characteristics. For example, among the unemployed it may be those who are, in some broad sense, “better motivated” who participate in programmes. But such motivation may also be an asset in finding a job. If we have not measured this in some way then we will overestimate the effectiveness of the programme.

Overcoming the first of the two difficulties is straightforward; overcoming the second is not, and has led to a great deal of debate and empirical work, particularly in the United States. This began in the 1970s with the work of Heckman (1979) and others, who devised estimators of programme effectiveness which purported to take account of the effect of possible omitted variables such as motivation. A range of methods now exists for this purpose (see, for example, Heckman, Hotz and Dabos, 1987; Heckman and Hotz, 1989) and we shall be making use of some of these.

In the remainder of this paper we present our estimates of effectiveness, controlling for observed differences between members of the participant and comparison groups, and we test whether or not omitted variables are likely to have biased our results.

VIII PARTICIPATION IN PROGRAMMES

Tables 4a and 4b show the parameter estimates for a logit regression of the probability of being in a training programme at any of the seven sampling dates. Note that this is the probability of being on a programme, rather than

unemployed, at any particular point in time, and is thus not the same thing as the probability of entering a programme. We express this probability as a function of a set of independent variables defined as follows:

- GIC: a dummy variable; 1 if individual left school after sitting for the Group or Intermediate Certificate; 0 otherwise;
- LC: a dummy variable; 1 if individual left school after sitting for the Leaving Certificate; 0 otherwise;
- PREDUR: in the case of the comparison group this is measured as the duration of unemployment (in months) prior to the sampling date. For the participant group it is measured as the duration of unemployment (in months) prior to entry to the programme plus (for those who were unemployed prior to entry to the programme) the time spent on the programme;
- MILF: months in the labour force prior to the sampling date;
- NSW: number of jobs (spells of work) held since leaving school;
- NSU: number of spells of unemployment since leaving school;
- NST: number of previous spells on training programmes;
- NTS: number of previous spells on temporary employment schemes.

In all the analyses reported in the paper we tested whether or not the coefficients for men and women were statistically significantly different. In most cases they were not. However, in modelling the probability of being on a training programme the male and female coefficients are quite different, as Tables 4a and 4b show.

Among both men and women, labour force history, as measured by the variables NSW, NSU, NST and NTS, has the effects one might have anticipated: those who have had more jobs are less likely to participate (though not significantly so among men); those who have had more spells of unemployment are more likely, while previous spells on training or temporary employment schemes reduce the likelihood. The likelihood of participation reduces the longer an individual has been in the labour force, reflecting the fact that the inflow to these programmes occurs relatively early in young people's careers. The positive coefficient for the effect of previous duration of unemployment reflects the fact that, by and large, young people enter training because they are finding it difficult to get a job. The major difference between men and women lies in the effect of educational qualifications. Among men there is no relationship between education and participation, controlling for other variables in the model. Among women there is a positive relationship: the higher the level of qualification the more likely is participation.

A similar picture emerges from Tables 5a and 5b which relate to the probability of being on a temporary employment scheme. What is also noteworthy

Table 4a: *Log-Odds of Participating in Training at Sampling Date: Men*

	Log-Likelihood	-130.87	
	Restricted (Slopes=0) Log-L.	-188.10	
	Chi-Squared	114.46	
	Degrees of Freedom	8	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-12.9790	142.4	-0.091 (0.92739)
GIC	12.3667	142.4	0.087 (0.93080)
LC	11.7614	142.4	0.083 (0.93418)
PREDUR	0.554597E-01	0.5003E-01	1.109 (0.26764)
MILF	-0.212872	0.4273E-01	-4.981 (0.00000)
NSW	-0.902745E-01	0.3519	-0.257 (0.79754)
NSU	2.28847	0.3686	6.208 (0.00000)
NST	-0.821294E-01	0.3945	-0.208 (0.83507)
NTS	-2.32020	0.9002	-2.577 (0.00996)

Table 4b: *Log-Odds of Participating in Training at Sampling Date: Women*

	Log-Likelihood	-166.68	
	Restricted (Slopes=0) Log-L.	-230.50	
	Chi-Squared	127.62	
	Degrees of Freedom	8	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-3.20454	0.7793	-4.112 (0.00004)
GIC	1.30308	0.7921	1.645 (0.09993)
LC	2.33735	0.7520	3.108 (0.00188)
PREDUR	0.677762E-01	0.3226E-01	2.101 (0.03565)
MILF	-0.132504	0.2872E-01	-4.613 (0.00000)
NSW	-1.30543	0.3379	-3.864 (0.00011)
NSU	1.83272	0.2903	6.313 (0.00000)
NST	-1.36266	0.5300	-2.571 (0.01014)
NTS	0.761263	0.4153	1.833 (0.06680)

Table 5a: *Log-Odds of Participating in Temporary Employment Schemes at Sampling Date: Men*

	Log-Likelihood	-170.00	
	Restricted (Slopes=0) Log-L.	-211.84	
	Chi-Squared	83.666	
	Degrees of Freedom	8	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-1.65998	0.4234	-3.921 (0.00009)
GIC	0.139050	0.4088	0.340 (0.73375)
LC	0.615892	0.4216	1.461 (0.14402)
PREDUR	0.326971E-01	0.2658E-01	1.230 (0.21857)
MILF	-0.781066E-01	0.2194E-01	-3.560 (0.00037)
NSW	-1.31301	0.3945	-3.329 (0.00087)
NSU	1.88524	0.2722	6.926 (0.00000)
NST	-0.683569	0.3743	-1.826 (0.06783)
NTS	-0.846969	0.3530	-2.399 (0.01643)

Table 5b: *Log-Odds of Participating in Temporary Employment Schemes at Sampling Date: Women*

	Log-Likelihood	-160.60	
	Restricted (Slopes=0) Log-L.	-231.88	
	Chi-Squared	142.57	
	Degrees of Freedom	8	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-2.10238	0.7847	-2.679 (0.00738)
GIC	1.62964	0.7806	2.088 (0.03682)
LC	2.50084	0.7600	3.291 (0.00100)
PREDUR	-0.873574E-01	0.4495E-01	-1.944 (0.05195)
MILF	-0.699554E-01	0.2321E-01	-3.014 (0.00258)
NSW	-1.58345	0.3606	-4.391 (0.00001)
NSU	1.55940	0.2832	5.507 (0.00000)
NST	-1.11198	0.4972	-2.236 (0.02532)
NTS	-1.09045	0.5833	-1.870 (0.06155)

here is that the likelihood of participating declines (rather than increases) the longer a woman has been unemployed (though the coefficient falls marginally short of statistical significance) whereas it does not among men. The kinds of schemes entered by men and women are probably rather different: women most probably entered the Work Experience Programme, while men are more likely to have been found in either WEP or Teamwork or its forbears. It was generally accepted that entry to WEP took place relatively soon after leaving school: indeed it was to prevent this that a rule refusing entry to WEP for anyone who had been out of school for less than six months was instituted during the mid-1980s (see Breen, 1988).

IX EFFECTIVENESS OF PROGRAMMES

In analysing the effectiveness of programmes we use five explanatory variables in addition to those used in the analysis of participation. These are:

- SEX: a dummy variable; 1 for women; 0 for men;
 PEDTR: a dummy variable; 1 if the state prior to training was full time education; 0 otherwise;
 PNIL: a dummy variable; 1 if the state prior to training was not in the labour force; 0 otherwise;
 TRAINING: a dummy variable; 1 if the individual was participating in a training programme at the sampling date; 0 otherwise (i.e., if the individual is in the comparison group);
 SCHEME: a dummy variable; 1 if the individual was participating in a temporary employment scheme at the sampling date; 0 otherwise (i.e., if the individual is in the comparison group).

X SHORT-TERM EFFECTS

Table 6 shows the observed percentages in jobs among the training and temporary employment groups and their comparison groups. This relates to the short-term effectiveness of the programmes, hence, as explained above, the measure relates to the percentage of participants in jobs immediately after participation and the percentage of the comparison group in jobs within the same time period.

Clearly, participants were more likely to get jobs immediately after participating than were non-participants in the same time period. However, these figures take no account of the observed differences between participants and the comparison group members. Table 7 contains the results of a logit regression analysis of the probability of being in a job after training or within the

Table 6: *Short-Term Effects of Training and Temporary Employment: Percentages in Jobs*

Training		Temporary Employment	
Participant	Comparison	Participant	Comparison
28	11	37	10.5

same length of time among the comparison group, as a function of the set of independent variables discussed earlier, plus the dummy variable TRAINING which distinguishes the participant from the comparison group.³ The fact that this variable has a significant positive coefficient shows that participation does confer advantages in finding a job, at least in the short term, even controlling for observed relevant differences between programme participants and unemployed non-participants.

Table 7: *Log-Odds of Being in a Job Immediately Following Participation in Training Programme or Within Same Time Period*

Log-Likelihood		-565.00	
Restricted (Slopes=0) Log-L.		-617.70	
Chi-Squared		105.40	
Degrees of Freedom		8	
Significance Level		0.32173E-13	
Variable	Coefficient	Std. Error	T-ratio (Sig.Lvl)
INTERCEPT	-1.77899	0.3679	-4.835 (0.00000)
GIC	0.311290	0.2714	1.147 (0.25143)
LC	0.520999	0.2698	1.931 (0.05348)
SEX	0.454476	0.1627	2.794 (0.00520)
PREDUR	-0.521052E-01	0.1522E-01	-3.424 (0.00062)
MILF	-0.123177E-01	0.9915E-02	-1.242 (0.21412)
NSW	0.146435	0.1331	1.100 (0.27132)
NSU	-0.138021	0.1224	-1.128 (0.25948)
NST	0.225128E-01	0.1887	0.119 (0.90501)
NTS	0.240040	0.1897	1.265 (0.20573)
PEDTR	-0.395750	0.2389	-1.657 (0.09758)
PNIL	-0.598047	0.2704	-2.212 (0.02699)
TRAINING	1.21902	0.2482	4.912 (0.00000)

3. This is equivalent to assuming that the effect of training or temporary employment on the log-odds of having a job are constant across all levels of the other explanatory variables. We carried out analyses comparable to those shown in Tables 7 and 9 in which we allowed the effects of training and temporary employment to vary according to the level of the other variables – e.g., according to educational qualifications, duration of unemployment, and so on. However, we found no evidence that the effects varied significantly in this manner.

Table 8 shows the same thing in respect of temporary employment schemes: here the variable SCHEME distinguishes participants from the comparison group. Again, this variable has a positive and significant effect, suggesting that participation in temporary employment schemes also confers short-term advantages in finding a job.

Table 8: *Log-Odds of Being in a Job Immediately Following Participation in Temporary Employment Programme or Within Same Time Period*

	Log-Likelihood	-350.39	
	Restricted (Slopes=0) Log-L.	-437.12	
	Chi-Squared	173.46	
	Degrees of Freedom	12	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-1.63903	0.4669	-3.510 (0.00045)
GIC	0.496743	0.3544	1.402 (0.16101)
LC	0.702099	0.3537	1.985 (0.04716)
SEX	0.582747	0.2031	2.870 (0.00411)
PREDUR	-0.548159E-01	0.2275E-01	-2.409 (0.01598)
MILF	-0.356013E-01	0.1289E-01	-2.762 (0.00574)
NSW	-0.721980E-01	0.1969	-0.367 (0.71388)
NSU	0.193619	0.1679	1.153 (0.24895)
NST	-0.257014	0.2890	-0.889 (0.37385)
NTS	-0.708463E-01	0.2711	-0.261 (0.79381)
PEDTR	-0.508802	0.2978	-1.709 (0.08753)
PNIL	-0.758435	0.3166	-2.395 (0.01660)
SCHEME	1.66267	0.2419	6.873 (0.00000)

In passing we note that the effects of the other variables are as we might have expected. Those most likely to get a job following either participation or unemployment are those who have the highest levels of educational qualifications and who have been unemployed for the shortest time. Young people appear to have a better chance of getting a job early in their career rather than later. Once we take account of these effects, then the variables measuring the number of previous jobs, spells of unemployment, training and temporary employment programmes, have no effect on the likelihood of getting a job. It is interesting to note that those who enter programmes from outside the labour market (from full-time education or elsewhere) tend to do relatively poorly in finding a job, and that girls appear more likely to get jobs than boys.

Given that participation has positive effects, how large is this effect? Since the coefficients in Tables 7 and 8 relate to a logit analysis, the partial effect of a variable on the probability of getting a job is not linear: it depends on the levels of all the other variables in the model. However, if we take the average member of our joint training participation and comparison group sample, we estimate that participating in training increases his or her chances of finding a job in the short term by 16.5 percentage points. Carrying out the same calculation for temporary employment schemes we arrive at a figure of 22.8 percentage points.

XI LONG-TERM EFFECTS

Table 9 shows the observed percentages in jobs among the training and temporary employment groups and their comparison groups one year after the sampling date. This relates to the long-term effectiveness of the programmes.

Again, there are clear differences between the participant and comparison groups, though these are less than those shown in Table 6.

Table 9: *Long-Term Effects of Training and Temporary Employment: Percentages in Jobs*

<i>Training</i>		<i>Temporary Employment</i>	
<i>Participant</i>	<i>Comparison</i>	<i>Participant</i>	<i>Comparison</i>
48	31	45	31

In Table 10 we analyse the probability of having a job after one year using the logit regression framework. In this case we can include participants in both types of programme in the one analysis because the dependent variable is defined in the same way for the comparison group in both cases.⁴ Here we note that, controlling for all relevant differences between participant and comparison groups, the variable SCHEME is still strongly significant, whereas the variable TRAINING, while positive, is not significant. The coefficients of the other variables are as we might have anticipated: jobs are more likely to be found by those with better qualifications and shorter prior spells of unemployment. For the average member of our combined sample, participating in a temporary employment scheme improves his or her probability of having a job one year later by 26 percentage points.

4. Whereas in the analysis of short-term effects the definition of the dependent variable for each comparison group depended upon the mean number of months of remaining participation among the relevant participation group.

Table 10: *Log-Odds of Being in a Job One Year After Participation in Training or Temporary Employment Programme or Within Same Time Period*

	Log-Likelihood	-1005.2	
	Restricted (Slopes=0) Log-L.	-1087.8	
	Chi-Squared	165.24	
	Degrees of Freedom	13	
	Significance Level	0.32173E-13	
<i>Variable</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>T-ratio (Sig.Lvl)</i>
INTERCEPT	-1.07345	0.2579	-4.163 (0.00003)
GIC	0.655708	0.1810	3.622 (0.00029)
LC	0.942292	0.1840	5.121 (0.00000)
SEX	0.183555	0.1109	1.655 (0.09791)
PREDUR	-0.238621E-01	0.1053E-01	-2.267 (0.02340)
MILF	-0.173311E-01	0.8398E-02	-2.064 (0.03905)
NSW	0.124074	0.1064	1.166 (0.24348)
NSU	-0.547486E-01	0.9574E-01	-0.572 (0.56744)
NST	0.292096E-01	0.1389	0.210 (0.83340)
NTS	0.133654	0.1442	0.927 (0.35413)
PEDTR	-0.276460E-01	0.1693	-0.163 (0.87027)
PNIL	0.954407E-01	0.1799	0.531 (0.59566)
TRAINING	0.288366	0.2211	1.304 (0.19215)
SCHEME	1.09093	0.1746	6.249 (0.00000)

At this point, then, our provisional conclusions are that both training and temporary employment programmes confer a positive short-term benefit in improving the chances of getting a job, but, while this relative improvement persists for at least a year among those who were in temporary employment, it disappears within this period among those who were in training.

XII OMITTED VARIABLES AND SELECTION BIAS

We turn now to the second problem we identified in studies of this kind: are there omitted variables which, had we been able to include them, would have altered our conclusions concerning effectiveness? We applied a number of tests for such omitted variables: all gave the same result. Here we report the results of the simplest such test.

Consider the general case where we have two equations: the first of these is a selection equation:

$$y_1 = \sum_i a_i z_i + u_1 \quad (1)$$

and the second is termed the substantive equation:

$$y_2 = \sum_j b_j x_j + cy_1 + u_2. \quad (2).$$

Note that the LHS variable from Equation (1) is on the RHS of Equation (2). In Equation (2) the x_j variables are assumed to include all the z_i variables. Equation (1) might model access to training, for example, with Equation (2) modelling some effect of training, with the variable y_1 distinguishing between those who received training ($y_1=1$) and those who did not ($y_1=0$). The coefficient c will only be an unbiased estimate of the effect of participation on y_2 if there do not exist variables w_k omitted from both (1) and (2) which influence both y_1 (the likelihood of participation) and y_2 (the outcome). How do we test whether or not such variables exist? The simplest way to do this is to estimate Equations (1) and (2) and examine the correlation between the residuals. Doing this we find that, for both the training and temporary employment programmes, there is indeed a non-zero correlation between the residuals from the selection and substantive equations. In such cases the most straightforward approach to overcoming this problem of correlated residuals is to use instrumental variables on the substantive equation (Lee, 1979). In this case the instrument we use is the fitted values from the selection equation. This is because it is highly correlated with the variable y_1 but is uncorrelated with the residuals from Equation (1). We then estimate Equation (2) using two stage least squares.⁵ Recent research on overcoming sample selection bias was, as noted earlier, developed by Heckman (1979) and another possibility is to employ his approach directly.⁶ The idea behind this is that, because of omitted variables correlated with both y_1 and y_2 , the error in Equation (2) will not have a zero expectation. The method corrects for this by adding, to Equation (2), a quantity termed the inverse Mills' ratio. This is the ratio of the density to the distribution function of a normal variate evaluated, for each individual, at the estimated value of Equation (1) (that is the value $\sum_i a_i z_i$). Less technically, this quantity can be thought of as the "instantaneous risk"

5. Ideally we should want to estimate our instrumental variables correction to Equation (2) via a logit or probit equation. This would necessitate estimating the model using non-linear two stage least squares. Here we report the results of a simpler strategy.

6. For a previous application of the method to Irish data and an extended discussion see Breen (1986), especially pp. 101-108.

of being in the group which receives training. By the inclusion of this quantity the coefficients for the other variables in Equation (2), including c , should be purged of bias arising from the omitted variables.

Table 11 shows the results of estimating Equation (2) applied to our data by ordinary least squares – i.e., a linear probability model that does not correct for the correlated errors; by two stage least squares (instrumental variables); and by the Heckman method. The results are shown in respect of both the short-term and long-term effects of training and temporary employment schemes. The first point to note is that the OLS (linear probability) model returns a slightly higher measure of the effectiveness of programmes than does the logit evaluated at the means. Our concern here, however, is whether the two stage least squares and the Heckman estimates are significantly different from the OLS estimates.

Table 11: *Effects of Possible Omitted Variables on Coefficient Estimates (standard errors in parentheses)*

	<i>Method</i>		
	<i>Uncorrected</i>	<i>Corrected</i>	
	<i>OLS</i>	<i>2SLS</i>	<i>Heckman</i>
Coefficient for effect of training			
short-term effect	0.198 (0.034)	0.234 (0.113)	0.251 (0.095)
long-term effect	0.067 (0.050)	0.062 (0.167)	0.035 (0.138)
Coefficient for effect of temporary employment schemes			
short-term effect	0.301 (0.032)	0.864 (0.138)	0.777 (0.098)
long-term effect	0.250 (0.050)	0.102 (0.195)	0.181 (0.143)

In the case of the effects (short- and long-term) of training the OLS, two stage least squares and Heckman estimates are virtually identical. In the analysis of the short-term effects of temporary employment schemes both the instrumental variables and the Heckman technique substantially increase the estimated effect of temporary employment programmes. We can conclude, then, that omitted variables do not seem to be biasing our estimates of employment programme short-term effectiveness downward. However, the high degree of

sensitivity of the parameter is a little worrying. Most puzzling is the fact that, if we correct for omitted variables in this case, we arrive at an estimate of the short-term effectiveness of temporary employment schemes which is implausibly large – larger, indeed, than the raw placement rate for schemes of this type. In other words, even if participants' prior probability of getting a job were zero, an effect of participation of this magnitude would far exceed the observed placement rate.

Some of these difficulties may arise because the set of programmes characterised as temporary employment schemes is rather diverse. It is made up primarily of two types of temporary employment: one where the young person is placed in a firm or business (as was the case with the Work Experience Programme); the other where the young person is placed in a voluntary or community organisation (as with Teamwork). Clearly, the chances of getting a job are greater for the first type of placement than the second. Data from WEP (see Breen, 1988) clearly show that the overwhelming majority of those who obtained jobs on completion of the programme were in fact "retained" by the employer with whom they had been placed on WEP. As a result of these important differences in what constitutes a temporary employment programme it seems likely that any more reliable measure of their effectiveness would have to be undertaken using data which allowed us to identify more accurately the particular type of programme.

Turning to the coefficients for the long-term effect of temporary employment programmes, the two stage least squares estimate is less than half that of the OLS coefficient and is not statistically significant. The Heckman estimate lies between the OLS and two stage least squares coefficients, but it is not statistically significantly different from zero. These findings suggest that unmeasured variables are influencing both the likelihood of entering such a scheme and the probability of having a job one year after the sampling date. Thus, the positive effect of temporary employment programmes is not robust to tests for omitted variable bias. There has always been a suspicion of high levels of deadweight in WEP in the sense that employers may well have been using the scheme to fill a vacancy which they were, in any case, intending to fill and were recruiting, for this purpose, young people who would, even without WEP, have been very likely to get a job. As a result, while a scheme like WEP may have helped young people to get jobs more quickly than they otherwise would (and hence would show a positive short-term effect) we should not expect it to have a longer-term effect. That we find no such longer-term effect supports the results of previous research into the programme (Breen, 1988).

XIII CONCLUSION

Participation in labour market programmes is not unequivocally advantageous. Clearly one would expect that a training programme would give participants new skills (broadly defined) which should be of help to them in securing a job. Similarly, participating in a temporary employment scheme gives some young people access to potential employers and gives experience of work. Furthermore, since the chances of getting a job decline the longer a young person has been unemployed (for evidence relating to the youth labour market see Breen, 1991) the fact that participation removes the bulk of participants from unemployment may itself be advantageous. However, the degree to which these potential benefits are realised depends, among other things, on employers' perceptions of the usefulness and nature of such programmes. For example, if they view temporary employment programmes as simply "make work" schemes disguising unemployment then young people may not gain much labour market advantage from participating. In addition, of course, when young people enter such programmes, their own job seeking efforts may well be curtailed. Thus the overall effectiveness of labour market programmes will be the outcome of such a mix of factors.

This paper has dealt with some of the issues involved in estimating the overall effectiveness of training and other labour market programmes. The evaluation issues raised, however, extend to any non-experimental evaluation (i.e., which does not use random assignment): thus they are of potentially very wide applicability.

Finally, Table 12 compares four measures of the effectiveness of training and temporary employment programmes using our data. These measures range from the very simple – placement rates – through simple comparisons of participants and non-participants, to comparisons which take account of observed and unobserved differences between the two groups. Note that in the case of temporary employment schemes we are able only to suggest a range in which their short-term effectiveness lies. The lower figure is obtained from our logit estimates, as described in the text, while the higher figure is the upper bound to effectiveness set by the raw placement rate. The results reported in Table 12 demonstrate the importance of using a participant group for comparative purposes and the necessity to control for both observed and unobserved differences between this group and the programme's participants.

It will not always be the case, as it is here, that the measures proposed in this paper will yield effectiveness estimates lower than placement rates. For example, a policy of targetting interventions at those who have the poorest labour market position will almost inevitably depress placement rates. On the other hand, it ought not to reduce the measured effectiveness of the scheme

Table 12: *Percentage Measures of Effectiveness*

	<i>Short-term</i>		<i>Long-term</i>	
	<i>Training</i>	<i>TempEmp</i>	<i>Training</i>	<i>TempEmp</i>
1. Placement Rate	28	37	48	45
2. Difference between Participant and Comparison Groups	17	26.5	17	13.2
3. Difference between Participant and Comparison Groups controlling for observed differences	16.5	22.8 – 37	7*	25
4. Difference between Participant and Comparison Groups controlling for unobserved differences	16.5	22.8 – 37	7*	10*

*not significantly different from zero at 5% level.

if we measure effectiveness in the way described. If evaluations could be carried out of individual programmes, then we might well find that some of those with low placement rates – such as Travellers' Workshops – actually had a high effectiveness measure, because the individuals for whom they cater have such very low prior probabilities of finding a job. Viewed from a broader perspective, the need to evaluate manpower programmes in the way we have outlined is important in any cross-national comparisons. A comparison of Irish placement rates with those of similar programmes in, say, Germany, would almost certainly show higher placement rates in the latter, regardless of the actual worth of the programmes themselves. A measure which took account of the pre-participation differences in the chances of participants getting a job would obviously be a fairer basis on which to draw comparisons.

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