Active Labour Market Programmes and Poverty Dynamics in Ireland

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Abstract

Active Labour Market Policies (ALMPs), which provide training and subsidised employment to the unemployed, are an important part of Ireland's welfare state. While a good deal of existing research is concerned with the effect of these policies on employment chances and on wage rates, none addresses the connection between poverty and ALMPs. Do these policies have an effect on poverty? That is, first, to what extent do these policies serve the low-income population, as a consequence of and in addition to their focus on those in precarious labour market situations? Second, to what extent do these policies function to lift people out of poverty in the medium term?

To address these issues we use longitudinal data from the Living in Ireland Survey (1994–2001) and examine how the respondents' situation in one year predicts participation in employment and training schemes in the next year, and then how participation in these schemes affects poverty status in the following year. Participants on both sorts of schemes are much poorer than the population average, and those on employment schemes (but not training schemes) are even poorer than one would expect given their observed characteristics.

Employment schemes and training schemes serve different purposes and different populations. A conventional logistic regression analysis seems to suggest that employment schemes (but not training schemes) positively increase the risk of poverty in the following year. This finding is not considered reliable, but rather it reflects the selection processes whereby those on employment schemes are in particularly vulnerable situations, in respects that are not picked up in the data set. A more rigorous analysis, using propensity score matching, reveals that employment schemes are neutral on poverty risk. Training schemes have a weak but insignificant protective effect.

Considering the risk of poverty approximately one year after participation begins, employment schemes (and to a lesser extent, training schemes) do not provide a mechanism for immediately exiting poverty. We add the caveat that it may be desirable to consider outcomes two or more years into the future, were data available, and that other outcome measures of quality of life should also be taken into account. Ultimately, with regard to both labour market and poverty outcomes, we find no evidence that participants of training schemes or employment schemes have either raised their employment chances or reduced their risk of poverty in the year following their participation.

Keywords: active labour market programmes; propensity score matching; employment policy

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Table of Contents

List of Tables

List of Figures

Abbreviations

ALMP	Active Labour Market Policy
ESRI	The Economic and Social Research Institute
EU	European Union
FÁS	Foras Áiseanna Saothair
GDP	Gross Domestic Product
ISEI	International Socio-Economic Index
LFS	Labour Force Surveys
LIS	Living in Ireland Survey
LTU	Long-term unemployment
NN	Nearest Neighbours
OECD	Organisation for Economic Cooperation and Development
SST	Specific skills training

1 Introduction

Ireland is usually characterised as a *liberal* welfare state in the Esping-Andersen sense, aligned with the United Kingdom and (at the end of the continuum) the United States, providing a minimalist and means-tested safety net (Esping-Andersen, 1990, 1999). In many respects this is fair, but in certain respects Ireland deviates significantly from the liberal model. One respect is the relative importance of Active Labour Market Policies (ALMPs), that is, policies that focus on improving the situation of the unemployed and lower-skilled workers by training and subsidy. In levels of both expenditure and institutional commitment to ALMPs, Irish practice has tended towards the higher end of the scale, average to above average in Organisation for Economic Cooperation and Development (OECD) and European Union (EU) contexts, and substantially higher than the UK.

Active labour market policies are intended to have effects at a number of levels, including macro-economic, redistributional and individual. Existing research has focused largely on the effects of programmes on medium-term employment status, a justifiable concern with the individual labour market outcome. If beneficial labour market effects cannot be observed at the individual level, it is hard to argue that programmes could have macro-economic or redistributional effects. In this research we retain the focus on individual outcomes, but rather than assess direct labour market effects, we take a redistributional perspective. We ask two distinct but closely related questions:

- To what extent do ALMPs target ‘poor’ households?
- To what extent do ALMPs have positive consequences for household poverty status in the medium term?

While the direct effects of ALMPs should lie in improving the productivity and employability of workers, it is also the case that their social consequences should be to ameliorate the situation of some of the more vulnerable members of society. An important question to ask, therefore, is whether those to whom ALMP opportunities are made available are indeed among the more deprived. Can we consider ALMPs to be *de facto* anti-poverty policies, or perhaps those who benefit are not those most in need, but rather those most in a position to benefit?

The second question is closely related but looks at consequences rather than selection: given participation in an ALMP scheme, is there any evidence of protection from poverty in the medium term? This is closely related to the issue of beneficial labour market effects, but imposes an additional condition that such changes also have the effect of reducing the risk of poverty below what it would otherwise be.

Poverty, defined in terms of equivalised household income, is affected by more factors than a single adult's employment situation. We can realisitically expect that the measured effect of participation in ALMP schemes on subsequent household poverty will be open to more imprecision than the more direct effect on employment outcomes or on individual income. Variability in post-treatment household income additional to that due to participation will arise from simultaneous changes in the labour market situation of other household members and from change in the household structure. The explicit motivation of this research is, however, to address the relationship between active labour market policy and poverty – answering the twin questions of whether ALMP schemes serve those in poverty, and whether they equip them sufficiently to escape it. Notwithstanding this, we also report employment status outcomes as they are instrumental to understanding changes in poverty status.

In what follows we first outline the background to active labour market policy and the history of its development in Ireland (section 2), and then present a summary of aspects of the literature on ALMPs in Ireland (section 3). We go on to conduct a series of empirical analyses using the Living in Ireland household panel survey, to address our twin research questions (sections 4 to 6). The empirical analysis combines more conventional descriptive methods with a 'pseudo-experimental' propensity score matching technique which is intended to give a less biased estimate of the effect of participation in ALMP schemes on the subsequent risk of poverty.

2 Background

Active labour market policy is a heterogeneous mix of supply and demand-side policy. On the supply side ALMP schemes are responsible for training and retraining the unemployed, and may assist in matching candidates to vacancies through the public employment service. On the demand side, ALMPs may involve employment subsidies to firms or even direct employment creation.

The fundamental goal of all ALMP is to reduce the number of people in open/passive unemployment. If training authorities respond quickly to changes in the composition of the labour market and tailor programmes to meet employer needs, specific skills training may address structural unemployment. Similarly, general training may bring into the labour force those who, for a variety of reasons, maintained only a tangential connection to the formal education system. Without the intervention of adult education, even in the form of general skills training, these people may be at risk of entering long-term unemployment.

The OECD (2000, p.176) have set out seven distinct objectives of an active labour market policy:

1. Job creation, either to reduce the number of registered unemployed in the short-run or to generate jobs persisting beyond the period of intervention, such as jobs in the social economy
2. Job redistribution, to re-order for equity reasons the job-seekers' ranks and to give the long-term unemployed a chance to enter into jobs which would otherwise be offered to others, and thereby maintain an attachment to the labour market for groups at risk
3. Skill and human capital acquisition, which may not lead to a job immediately but enhances the employability and productivity of the unemployed, whose skills are otherwise eroded by long spells of inactivity

4. Attitudinal changes, combating the discouragement and alienation of job seekers, enhancing their motivation and willingness to work; but also encouraging employers to recruit and overcome prejudices and stigmatisation
5. Increase of earnings, either in the long- or short-run; combating poverty and unemployment traps, particularly in low-wage and low-skill segments of the labour market
6. Macro-economic objectives, such as increasing the potential labour supply, and reducing structural unemployment without increasing wage push inflation
7. Addressing wider social objectives, such as promoting health, combating criminality and enhancing the social cohesiveness of communities.

2.1 Origin of active labour market policy

The notion of an ‘active’ labour market policy came to the fore in the 1970s as high inflation beset many of the world’s industrial economies, including Ireland. Two Swedish economists, Gösta Rehn and Rudolf Meidner, had successfully brought inflation under control in Sweden using a double-edged approach of labour retraining and solidaristic wage bargaining. The former ensured a ready supply of skilled labour, while the latter forced inefficient and under-performing firms out of business. Rehn and Meidner’s work influenced OECD thinking and was formalised as ‘manpower planning’ by the late 1970s. While Esping-Anderson (1985) classifies these measures as macro-economic tools to counter rising inflation (and not as a response to widespread unemployment), both inflation and unemployment are known to be heavily interdependent.

In essence, manpower planning ensured that firms had an adequate pool of skilled labour to draw from, while also retraining the unemployed and returning them to work. It followed that retraining would facilitate structural change and ultimately achieve full employment.

Outside of Sweden, ALMP was embraced as a possible solution to high levels of unemployment and coincided with the recognition of long-term unemployment as a distinct focus for concern. The shift from passively supporting the unemployed (i.e. through social welfare) to activation reflected an extension of the strongly interventionist Keynesian paradigm of the time.

2.2 Emergence of active labour market policy in Ireland

O'Connell and McGinnity (1997b) characterise Irish labour market policies during the 1960s and 1970s as mainly confined to the organisation of apprenticeship training and to matching supply and demand for labour. These policies were in-line with the OECD's 'manpower policy' aimed at achieving full employment and strong growth. However, with the onset of high unemployment and low growth in the 1970s, governments embraced the Swedish example by adopting a variety of active labour market policies. These included employment subsidies, training schemes and temporary public job creation schemes. From 1975, the Youth Training Programme, the Premium Employment Programme and the Employment Incentive Scheme were all introduced. In 1976 the Environment Improvement Scheme, the Temporary Grant Scheme for Youth Employment (Teamwork) and Community Workshops all targeted the phenomenon of youth unemployment. From 1987 onwards, the bulk of ALMP schemes were organised under the auspices of Foras Áiseanna Saothair (FÁS), the public employment service agency.

According to O'Connell and McGinnity (1997b), by the 1980s ALMPs had taken centre-stage in the government's response to mass unemployment. However, it was a commonly held view that unemployment was a transitory phenomenon. Emphasis was placed on demand-side measures to generate new employment places, while retraining schemes frequently focused on finding employment for the (relatively) most employable candidates – to the neglect of the most disadvantaged.

2.3 The emergence of FÁS

FÁS, Ireland's statutory training and employment authority, arose from the amalgamation of three agencies, of AnCo, the National Manpower Service and the Youth Employment Authority, under the Labour Services Act of 1987. The Act was silent on the rationale and organisational structure of FÁS, failing from its inception to echo the 'pre-emptive social democratic paradigm' of its preceding White paper. The bringing together of all three agencies under the FÁS umbrella sought to address inter-agency conflict and tacitly accepted the failure of each organisation to address record levels of unemployment.

A major reorganisation of activities within FÁS occurred in response to the 1992 Culliton Report. Culliton, though supportive of the role of FÁS in labour market activation, questioned if the agency's multiple goals could be better achieved by creating separate divisions for training the unemployed, vocational training, and industry-based training. What followed from these reforms were organisational slimming and beefed-up regional directors. Boyle (2005, p.38) identified the following post-reform characteristics of FÁS:

1. Multi-functional and multi-tasked
2. Largely autonomous of department officials, but connected to key politicians
3. Heavily regionalised with a small, low-cost centre
4. Representative, non-executive board with a powerful director-general position
5. Loosely coordinated at the centre, but with substantial autonomy from assistant-director down
6. Programmatic, not client-centred; focused on spending large programmatic budgets
7. Mix of in-house and out-sourced service provision for various functions
8. Fiscally opportunistic as it avoided long-term staff commitments and shared financial, legal and political costs of implementing policy

9. Cheap and retractable with low organisational fixed costs.

FÁS fended off repeated calls for a severing of the organisation along functional lines: including from the Task Force on Long-Term Unemployment (1994–1995) and again from the 1997 White Paper on Human Resource Development. Drawing much of its programmatic funding directly from European Structural Funds afforded FÁS a large measure of independence from the Department of Finance.

2.4 Flavours of active labour market policies

The constituency of ALMP is diverse. Specific skills training and apprenticeships may help those who received a formal school education and who now wish to specialise; whereas general training provides basic workplace skills to those who have been out of the labour force for some time, or to those who did not benefit from formal schooling to certificate level. Finally, direct employment schemes and employment subsidies may redress skills and human capital depreciation incurred by the long-term unemployed during their absence from the labour market. A summary table of training and employment schemes is included in Appendix One.

ALMPs are conventionally classified into five broad categories:

1. Public employment services: including information, placement and counselling services for the unemployed
2. Labour market training: including measures to enhance the skills of both employed and unemployed
3. Youth measures: including training, work experience and apprenticeships
4. Subsidised employment: including direct job creation measures as well as subsidies towards private sector recruitment and/or self-employment
5. Training and employment measures targeted specifically at the disabled.

Table 2.1: Typology of ALMP

	Market orientation	
Labour market leverage	*Weak*	*Strong*
Supply	General training	Specific skills training
Demand	Direct employment schemes	Employment subsidies

O'Connell and McGinnity (1997b) derived the following typology of ALMPs based on the conventional classifications (see Table 2.1):

General training Programmes in this category provide basic/foundational skills and are designed for those with poor educational qualifications. They include programmes for second-chance education, for women returning to work after child rearing, for long-term unemployed males, for young school-leavers, for people with disabilities, and may also offer training to develop community resources. Unlike vocational training which enhances employability by teaching specific skills, general training teaches general subjects often covered during second-level schooling.

Specific skills training The courses are designed to meet specific skills needs in the economy and are usually targeted at specific industries and occupations. An example is specific skills training as operated by FÁS, where the level of training is typically more advanced than that of general training.

Direct employment schemes These programmes consist of subsidised temporary employment in the public or voluntary sectors – which O'Connell and McGinnity (1997b, p.20) term a 'variant of conventional public works programmes'. While direct employment schemes may indeed lead to the provision of public goods/services, their over-riding purpose is that of employment generation. In Ireland, Community Employment is the largest direct employment scheme. Community Employment, which replaced the Social Employment Scheme in 1994, is targeted at the long-term unemployed.

Employment subsidies These are subsidies to the recruitment or self-employment of unemployed workers in the private sector. They may be paid to either employer or employee; and are designed to 'offset the relative unattractiveness' of a long-term unemployed candidate (O'Connell and McGinnity, 1997b, p.21). The subsidy may be seen as compensation for the greater costs of recruiting and training the long-term unemployed. The subsidy consists of a lump-sum payable on recruitment and continuing payment and/or exemptions from social insurance contributions. The Back to Work Allowance, launched in 1993, is paid directly to employees. Among those paid to employers are the Employment Incentive Scheme (1977–1994) and the Employment Subsidy Scheme (1992–1993).

2.5 Irish labour market in context

Large-scale unemployment beset the Irish economy in the early 1980s. In the first half of that decade unemployment more than doubled, rising from 7 to 17 per cent in five years. Some 226,000 people were registered unemployed in 1985 and in receipt of social welfare payments. Subsequent analysis would place the blame for Ireland's unemployment crisis directly at the door of the government. In particular, high levels of personal taxation eroded the reward for work and reduced the incentive for potential job seekers.

High unemployment was far from a passing phenomenon, and levels of unemployment twice the OECD average persisted for another ten years after 1985. With an average of 15 per cent of the labour force out of work during this decade, the corresponding problem of long-term unemployment also worsened. Long-term unemployment (LTU) refers to a cohort of the unemployed who have been out of work for one year or more. Rates of long-term unemployment were already high in Ireland, with three-fifths of all those unemployed in 1985 having been out of work for more than a year. Combined with an increasing number of lay-offs as the macro-economic context deteriorated, levels of LTU peaked at

Table 2.2: Labour force trends

Year	Labour force	Employed	Unemployed	Long-term unemployed	Unemployment rate	Long-term unemployment
	'000s	'000s	'000s	'000s	%	%
1988	1,327.7	1,110.7	217.0	137.8	16.3	10.4
1989	1,307.8	1,111.0	196.8	128.0	15.0	9.8
1990	1,332.1	1,159.7	172.4	110.2	12.9	8.3
1991	1,354.4	1,155.9	198.5	119.7	14.7	8.8
1992	1,371.8	1,165.2	206.6	116.5	15.1	8.5
1993	1,403.2	1,183.1	220.1	125.4	15.7	8.9
1994	1,431.6	1,220.6	211.0	128.2	14.7	9.0
1995	1,459.2	1,281.7	177.4	103.3	12.2	7.1
1996	1,507.5	1,328.5	179.0	103.3	11.9	6.9
1997	1,539.0	1,379.9	159.0	86.3	10.3	5.6
1998	1,620.4	1,494.0	126.4	63.6	7.8	3.9
1999	1,685.9	1,589.1	96.9	41.5	5.7	2.5
2000	1,745.9	1,671.4	74.5	27.7	4.3	1.6
2001	1,787.0	1,721.9	65.1	20.8	3.6	1.2
2002	1,840.9	1,763.9	77.0	21.7	4.2	1.2
2003	1,875.5	1,793.4	82.1	27.2	4.4	1.5
2004	1,920.3	1,836.2	84.2	26.3	4.4	1.4
2005	2,014.8	1,929.2	85.6	27.6	4.2	1.3
2006	2,108.3	2,017.0	91.4	29.6	4.3	1.4

Sources: *Labour Force Survey* (Central Statistics Office, various years); *Quarterly National Household Survey* (Central Statistics Office, various years).

11 per cent of the labour force by the mid- to late-1980s (see Table 2.2).

By any measure, Ireland's problem was significant. Not only were levels of unemployment high, but outflows to new jobs (exits from unemployment) were low. Because of this, people's attachment to the labour force became weakened as they spent long periods out of work. Tackling LTU became a priority for government. While the government may have hoped to remedy unemployment through sound macro-economic management and economic growth, the profile of those long-term unemployed was complicated by inertia. It was widely recognised that the paralysis associated with long-term unemployment was

Table 2.3: Indicative table of ALMP participation, 1983–2002

Year	Training	%	Employment subsidies	%	Direct employment	%	Total
1983	29,958	65.2	11,000	23.9	5,000	10.9	45,958
1990	37,686	66.0	4,792	8.4	14,598	25.6	57,076
1992	30,600	58.8	3,831	7.4	17,642	33.9	52,073
1993	29,065	51.5	9,532	16.9	17,822	31.6	56,419
1994	33,682	38.2	17,420	19.8	37,038	42.0	88,140
1997	28,850	26.0	26,115	23.5	56,090	50.5	111,055
1998	14,238	14.9	41,859	43.8	39,520	41.3	95,617
1999	15,789	17.2	39,581	43.0	36,579	39.8	91,949
2000	15,510	18.1	36,686	42.8	33,549	39.1	85,745
2001	17,693	21.5	33,807	41.1	30,692	37.3	82,192
2002	17,533	22.4	32,862	42.1	27,718	35.5	78,113
Mean	20,471	22.6	32,619	36.6	37,312	40.8	90,402

Note: Direct employment = Community Employment (all years), Teamwork (pre-1997), Part-Time Job Opportunities Programme (pre-1997).
Sources: O'Connell and McGinnity (1997b), Indecon (2002), ILO Laborsta Database.

complex, potentially independent of improvements in the macro-economy and unlikely to be resolved by a tightening of the labour market alone. With this in mind, the expansion of ALMP schemes, and in particular subsidised employment, became a priority for public policy.

2.6 Participation and throughput

The pattern of expansion in ALMP participation (or throughput) is broadly responsive to changes in the unemployment rate over time (see Table 2.3). In 1983 overall participation stood at almost 46,000 – comprising 65 per cent training, 24 per cent employment subsidies and 11 per cent direct employment. Within seven years, overall numbers rose some 11,000, but more dramatic was the change in the composition of ALMP schemes. Participation in training remained at a constant 66 per cent, but employment subsidy schemes lost significant ground to direct employment. Following the birth of Community

Employment in 1994, the numbers engaged in direct employment schemes continued to grow until it peaked at 56,090 in 1997. Employment subsidy schemes, on the other hand, peaked at 41,859 (or 44 per cent of total participation) in 1999, and remained only slightly below this level thereafter. After 1994, as the demand for labour started ramping up, the number in training schemes began to fall, to a low of 14,250 in 1998.

Overall provision, which peaked at 111,055 in 1997, has fallen in response to a general tightening of the labour market. In a visible reorientation of public policy regarding ALMP schemes in 1998, participation in training was halved and direct employment fell by some 6,500 places. This trend in the composition of ALMP schemes was largely maintained up to 2002, with direct employment schemes such as Community Employment shrinking to pre-1994 levels. By 2002 training accounted for just one-fifth of all ALMP participation. The die has clearly been cast in favour of subsidised employment schemes, with a supporting role for training and an ever-decreasing allocation of places to direct-employment schemes.

2.7 Expenditure commitment and intensity

When measured as a portion of Gross Domestic Product (GDP) Ireland has always made a generous commitment to ALMP. In the period where data is available from the OECD's Social Expenditure Database (1985–2001), Ireland typically spent 1.4 per cent of GDP on labour market programmes – consistently above our European neighbours. The ratio of ALMP expenditure to GDP in these countries has been a remarkably constant 0.9 per cent. The turnaround in Ireland's economic fortunes from the mid-1990s explains the dramatic decline in the ratio of ALMP expenditure to GDP, as the latter began to soar with the contribution of multinational firms.

Even more remarkable has been the ratio of ALMP expenditure to public social

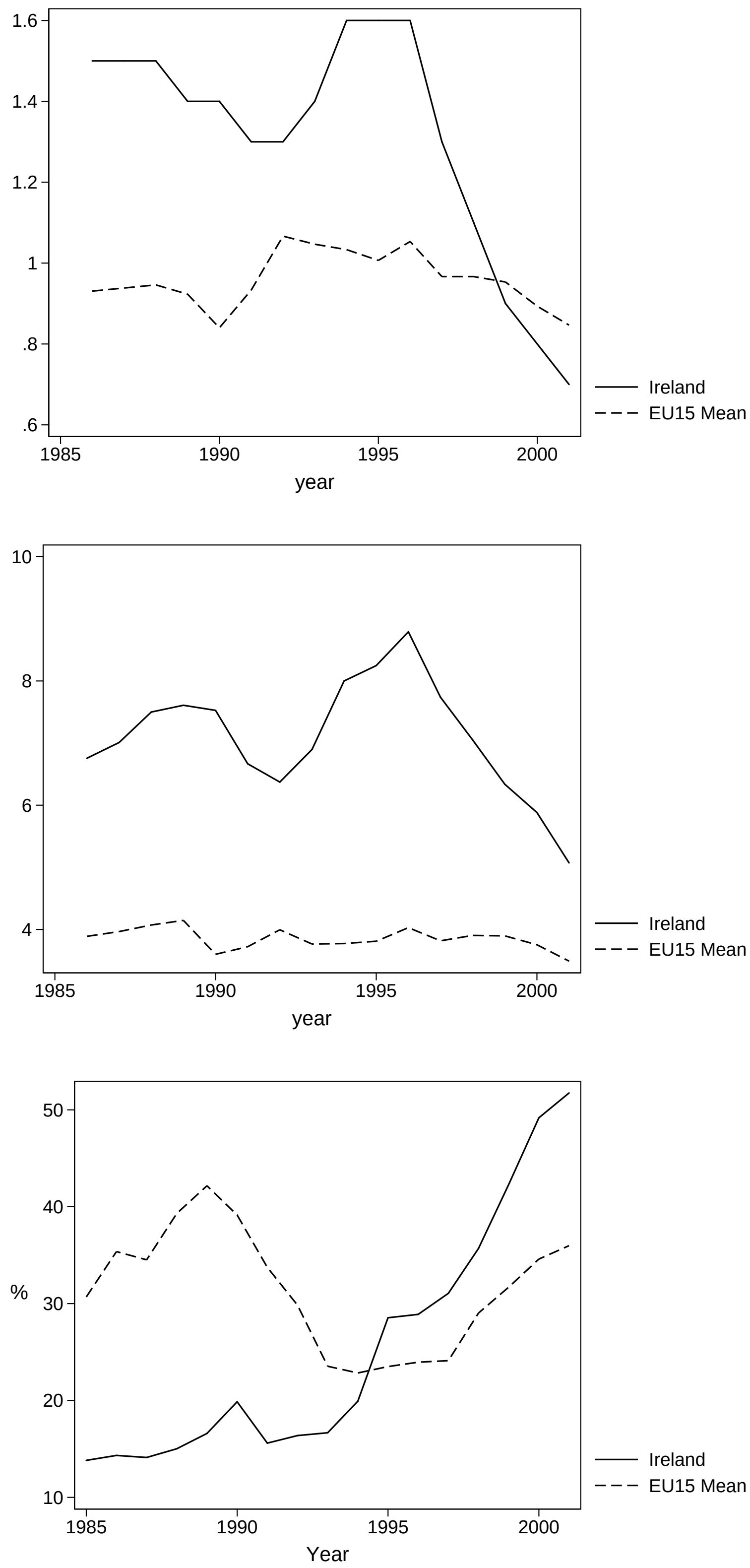

Figure 2.1: (a) ALMP expenditure relative to GDP; (b) ALMP expenditure relative to total public social expenditure; (c) ALMP expenditure per registered unemployed as a percentage of average production wage. Source: OECD (1994), OECD (various), ILO (Laborsta).

Table 2.4: Scale of ALMP participation, 1983–2002

Year	ALMP as a percentage	
	of labour force	of unemployment
1983	3.5	25.1
1990	4.3	33.1
1992	3.8	25.2
1993	4.0	25.6
1994	6.2	41.8
1997	7.2	69.8
1998	5.9	75.6
1999	5.5	94.9
2000	4.9	115.1
2001	4.6	126.3
2002	4.2	101.4
Mean	5.5	89.3

Sources: O'Connell and McGinnity (1997b), Indecon (2002), ILO Laborsta Database.

expenditure (health, education etc), which has consistently been twice the European average. The ratio peaked at 8 per cent of total public social expenditure in 1995 (reflecting the expansion of places in subsidised employment schemes such as Community Employment) but fell sharply thereafter. Again, this reflects the reorientation of labour market policy and the steady reduction in subsidised employment places.

The intensity of ALMP expenditure relative to European norms is highlighted by Figure 2.1(c). Here we show gross government expenditure on ALMP schemes per head of registered unemployed.[1] We then express expenditure per head as a ratio of the average production wage, which approximates the earnings of a single male production worker.[2] During the dark days of double-digit unemployment (mid-1980s to mid-1990s), government spending on ALMP

[1] A composite series of the total number unemployed, generated from Labour Force Surveys (LFSs) and augmented with local Employment Office Register data where LFS is missing (France: 1985–90, Official Estimates; Germany: no LFS data pre-1991; Luxembourg: Register Data Only; Netherlands: 1996 LFS missing; Portugal: 1998 LFS missing; UK: 1985/86 LFS taken from World Development Indicators).

[2] Denominated in 'national euro': an OECD term for the retrospective conversion of historical time-series to euro at the fixed euro-conversion factor.

schemes *per capita unemployed* was just half of the European average. Expenditure intensity did not meet (or exceed) the EU15 average until the reversal of fortune in the Irish labour market and the ensuing fall in unemployment post 1998. The purpose of this illustration is to demonstrate that while Ireland is often cited has having devoted significant resources to ALMP schemes (relative to both GDP and public social expenditure), in reality the intensity of this expenditure when expressed per capita unemployed, and relative to the average production wage, falls far short of the European average for the same period.

3 Existing research on active labour market policies

While the social sciences offer a menu of ex-post theories justifying intervention in the labour market, Webster (1997, p.4) dutifully reminds us that, ‘as a remedy for idleness, labour market programmes have a longer tradition than formal economic theory’. She dates the emergence of ‘modern’ unemployment to the transition from agrarian feudalism to industrial capitalism, and points out that sixteenth-century workhouses sought to achieve many of the same basic aims as modern labour market policies (training, employment and a ‘moderated’ wage) – albeit with more coercion.

Two main schools of thought exist regarding labour market programmes. The first school believe a *laissez-faire*/unchecked market is characterised by myopia, uncertainty and imperfect information, and is incapable of producing efficient/equitable outcomes in the absence of intervention. A cycle of poverty may arise if individuals or groups are allowed to fall too far below socially acceptable standards. In the words of Webster (1997, p.9), ‘failure breeds failure.’

A second school – while acknowledging the desirability of a *laissez-faire* self-clearing approach to labour market management – also accepts that, for reasons of equity or social justice, the achievement of a purely *laissez-faire* solution may not be possible. Instead, interventions are justified where they reduce the disincentives to work or train. In this liberal perspective, high taxes on labour, high replacement rates and a high minimum wage are distortionary and act to disincentivise work.

The theoretical pedigree of ALMPs has often been questioned, as many of its advocates focus on policy objectives before ever advancing a textbook case for labour market failure. In fact ALMP has rarely made headway into mainstream macro-economic debate. Demand-side macro-economics tends to view the unemployed as the tail-end of a homogenous labour queue, while labour market programmes may rearrange this queue, they will not resolve the basic causes of unemployment (Webster, 1997). Supply-side macro-economists argue that government intervention itself contributes to unemployment and should be avoided. Notwithstanding this, ALMP has received rigorous attention in the work of Layard et al. (1991) and Calmfors and Lang (1995), and is summarised in Hill and Halpin (forthcoming).

Evaluations of the micro-economic (individual level) impact of labour market policy typically examine supply- and demand-orientated policies separately. Supply programmes aim to enhance the human capital (skills, employment chances and potentially the earnings of participants), whereas demand-side programmes act to reduce the price of labour by offering subsidies to employers, in theory making it more attractive to hire eligible job-seekers. Demand-side measures effectively ‘create’ new demand for labour through direct public employment schemes.

3.1 Previous evaluations of micro effects

Denny et al. (2000) evaluated the employment and earnings outcomes of ALMP participants against a group of non-participants over the period 1994–1996. A treatment group of 1,473 respondents from a stratified sample of the 1996 FÁS Follow-up Survey was identified.[1] A corresponding control group was drawn from the first two waves (1994/5) of the Living in Ireland Survey (LIS). Only persons who were unemployed during the first wave of the LIS, and at risk of participating in ALMP schemes, were admitted to the final control group of 558. Denny et al.’s methodology involved a comparison of ALMP participants from the time they left their programmes with a control group who remained in open-unemployment and chose not to participate in ALMP schemes. This first step in establishing a

[1] Persons completing any one of 14 courses/schemes in the period April–July 1994.

control group is based on employment status alone and does not take into account other factors that may influence selection into labour market programmes. Specifically, the control group in this study were (on average) older, more likely to hold no qualification and more likely to have been unemployed for two years or more.

When outcomes in 1996 are compared, ALMP participants where twice as likely as the control group to have obtained employment. The same is true of employment type, where ALMP ‘graduates’ are more than twice as likely to have secured fulltime employment. Their report cautions of significant deviation in employment outcomes between the varieties of ALMP scheme: recipients of specific skills training were the most likely to secure employment (75 per cent), followed closely by those whose employment was subsidised (70 per cent). The progression to employment from general training was less significant (47 per cent), while only 36 per cent of those engaged in direct employment progressed to mainstream employment.

To obtain a more robust estimate of the differences in the effect of participation accruing to both groups, it is necessary to take account of the difference in their observable characteristics, such as age, sex, education and employment history. Denny et al. (2000) fitted a logistic model of employment probabilities using the LIS control group as a reference category. After controlling for observed differences, they reported that the employment probabilities for participants in three flavours of ALMP scheme (employment subsidy, specific skills training, general training) are significant and positive (compared to the reference category, or openly unemployed comparison group). Only those who completed direct employment schemes showed no significant increase in employment probabilities compared to the reference category. The effects of having a secondary or tertiary education are positive and significant, while the effects of being out of work for more than one or more than two years are negative and significant.

Denny et al. conducted a final robustness test on their model to ensure that unobserved latent characteristics (e.g. motivation, social networks) did not influence the selection process. For example, if those engaging in ALMP schemes had stronger social networks or better personal motivation at the outset, this would lead us to seriously overestimate the positive contribution of ALMP schemes to employment chances. Implementing a two-stage procedure to model selection and outcomes separately, they reported no significant correlation in the residuals of either model. O'Connell (2002) summarises: 'specific skills training are shown to have substantially greater employment chances than the comparison group, and indeed, than participants in the less market-orientated general training programme.'

Applying the same methodology to estimate the earnings effect of participation, Denny et al. repeated their finding of a significant positive effect of specific skills training (SST) on earnings – though the effect on wages is overall weaker than that reported for the probability of re-employment. Furthermore, by interacting SST with sex, age and unemployment duration, they find that SST is most effective at raising the earnings of women and of those over 25 years of age (though neither is strongly significant). The authors found that the principal impact of ALMP participation is to raise the probability of employment (particularly for ALMP schemes with strong labour market linkages, such as specific skills training), rather than to enhance the earnings of participants. These findings were later confirmed in the published analysis of O'Connell (2002).

3.2 Quasi-experimental approaches

Conniffe et al. (2000) sought to benchmark propensity score estimates of the employment effect of training schemes against those of O'Connell and McGinnity (1997b) who employed the standard multiple regression/selection bias testing approach. Both papers used FÁS Follow-Up Survey respondents as their treatment group and the Annual Survey of School Leavers for their control.

Using standard methods, O'Connell and McGinnity adjusted for selection bias using covariates such as education and previous unemployment histories. In their study, Conniffe et al. set out their case for not matching on all possible covariates – since previous studies have shown it sufficient to match merely on propensity scores. They began by estimating a naïve binary assignment model, using the maximum number of covariates available. Noting that many socioeconomic variables are highly collinear, and that a variable's utility should be judged not by its statistical significance but by its contribution to achieving 'balance' between treatment and controls, Conniffe et al. showed how the requirement of balancing among covariates is met using a parsimonious assignment model – omitting superfluous (and likely collinear) information on social status and previous unemployment.

Both treatment and control groups were matched within certain bands of their propensity scores. Conniffe et al. overlaid probability distributions of both groups and identified a lack of common support for the highest propensity scores (as probability approaches 1). They justifiably attributed this to a selection effect – namely the controls are better educated and less likely to move into training schemes. Since fewer propensity-score matches may be found as the probability of participating approaches certainty, the authors merged their fifth and sixth sextiles. In a final act, they weighted each band by the proportion of observations it contains – so those bands containing more matching will contribute more to the overall treatment effect.

Conniffe et al. (2000, p. 305) concluded that although classical and propensity score approaches led to similar findings, 'far fewer assumptions were made in the propensity score approach and we think it has probed deeper into the data structure'.

3.3 Evaluations of supply-side measures

Breen (1991) analysed the effectiveness of training and employment schemes using a five-year follow-up survey of 1981/2 school-leavers. The cohort included ALMP participants and non-participants. Breen found that labour market training schemes increased the short-term employment probabilities of young people, but it is unclear if reported long-term effects are due to selection basis.

3.4 Evaluations of demand-side measures

Breen and Halpin (1989) surveyed 400 firms to evaluate the impact of a wage subsidy scheme. Importantly, they found that 68 per cent of hirings were *deadweight* (i.e. hirings what would have been made even in the absence of the subsidy), while 21 per cent represented a *substitution effect* to avail of the subsidy. Displacement was found to be low, with only 8 per cent of subsidised hirings removing an existing employee.

4 Methodology

The empirical exercise this paper presents uses longitudinal data to address the following two research questions: Who participates in schemes? What happens to their poverty risk afterwards? Longitudinal data enable us to look at the antecedents of participation a year before, and the outcome a year after. We use a relatively conventional approach, based on descriptive statistics and logistic regression models, but also a pseudo-experimental method intended to give a better estimate of the causal effect of participation (see section 4.4).

4.1 Living in Ireland Survey

The Living in Ireland Survey (LIS) is a particularly important resource for understanding the dynamics of Irish society through the second half of the 1990s. It was a household panel survey and ran from 1994 to 2001, following a panel of respondents and interviewing all adult members of their households. It has good information on income and therefore necessarily on the dynamics of household income and of poverty. It has extensive labour market information, which allows us to track, with some restrictions, individuals' participation in training and employment schemes.

4.1.1 Longitudinal perspective

The longitudinal perspective that panel data provide has a number of advantages. First, we can track individuals over time and assess their *exposure* to factors such as poverty or unemployment, rather than simply observing their *state* at one timepoint. Second, we can observe 'dynamics': movements in and out of states. Third, the temporal order gives us a greater ability to discriminate between correlation and cause, giving us more power to distinguish between selection effects (where the individual's prior characteristics have effects both on the likelihood of experiencing the 'treatment' and on the nature of the outcome) and causal effects (where the 'treatment' has effects on the outcome that are separate from those arising from the individual's characteristics). In practical terms, the longitudinal structure allows us to observe individuals as they enter, participate in and leave ALMP-type schemes, and to observe their medium-term outcomes. This before-and-after perspective gives us much greater power to assess the true effects of participation in ALMP schemes, compared with looking at cross-sectional outcomes.

4.1.2 Poverty dynamics from the Living in Ireland Survey

The LIS has been used extensively to assess poverty dynamics (Callan, 1996; Nolan, 2002; Layte, 2001; Layte et al., 2001; Whelan et al., 2003; Callan et al., 2004, *inter alia*) over the 1990s, and has been superseded more recently by the EU-SILC. In this research we use household poverty as our primary outcome variable, and define it in a manner as close as possible to that used by the Economic and Social Research Institute (ESRI). Because we are interested in the effect on standard of living, rather than the more direct effect on labour market outcomes, the focus is appropriately on equivalised household income, rather than individual labour income. We take household net income as reported by the LIS, and equivalise it using the ESRI 'A' equivalence scale which treats the first adult in the household as one unit, subsequent adults as 0.66 units and children as 0.33 units. We take 60 per cent of median equivalised income as the default poverty line throughout this paper, where the median is calculated within years across households, weighted according to the LIS household weight. The use of wave-specific medians removes the need for deflating with a price index.

Table 4.1 summarises mean and median equivalised household income, and the per cent poor at the 60 per cent-median rate, across the eight years of the survey. The strong income growth over the period is reflected in the rising mean

Table 4.1: Equivalised household income and poverty by wave, 1994–2001 (weighted LIS data)

Wave	Equivalised net income (IE£/month)		Percentage poor
	Mean	Median	
1994	138.01	115.23	14.7
1995	148.58	125.00	17.4
1996	155.76	128.19	17.5
1997	166.11	142.81	17.5
1998	186.72	159.47	18.0
1999	200.70	181.32	20.3
2000	227.43	200.89	18.8
2001	255.85	234.03	20.2

Note: Poverty is defined as below 60 per cent of the median household income

and median income, with the median more than doubling over the period. However, the proportion falling below the poverty line also rises over the period, from under 15 to over 20 per cent. It is of course probable that some of these people are becoming ‘poor’ while experiencing rising incomes, or at least without experiencing income decline, since the poverty line is rising so sharply through the period.

These poverty rates are slightly higher than those reported by the ESRI, but the trend across the eight years is in accord with the ESRI figures.

We can get a fuller picture of the evolution of relative poverty, and the sensitivity to the precise relative poverty line by reference to Figure 4.1. While the proportion below 75 per cent of median household income shows no particular trend, the 60 per cent and 50 per cent lines are consistent with each other in showing a rise that is close to monotonic. The rate of extreme relative poverty, falling below 30 per cent of median household income, is very low, rarely rising above 1 per cent of the sample.

Moving in and out of poverty The advantage of the longitudinality of the LIS is the view on poverty dynamics *per se*. How stable is poverty status? As

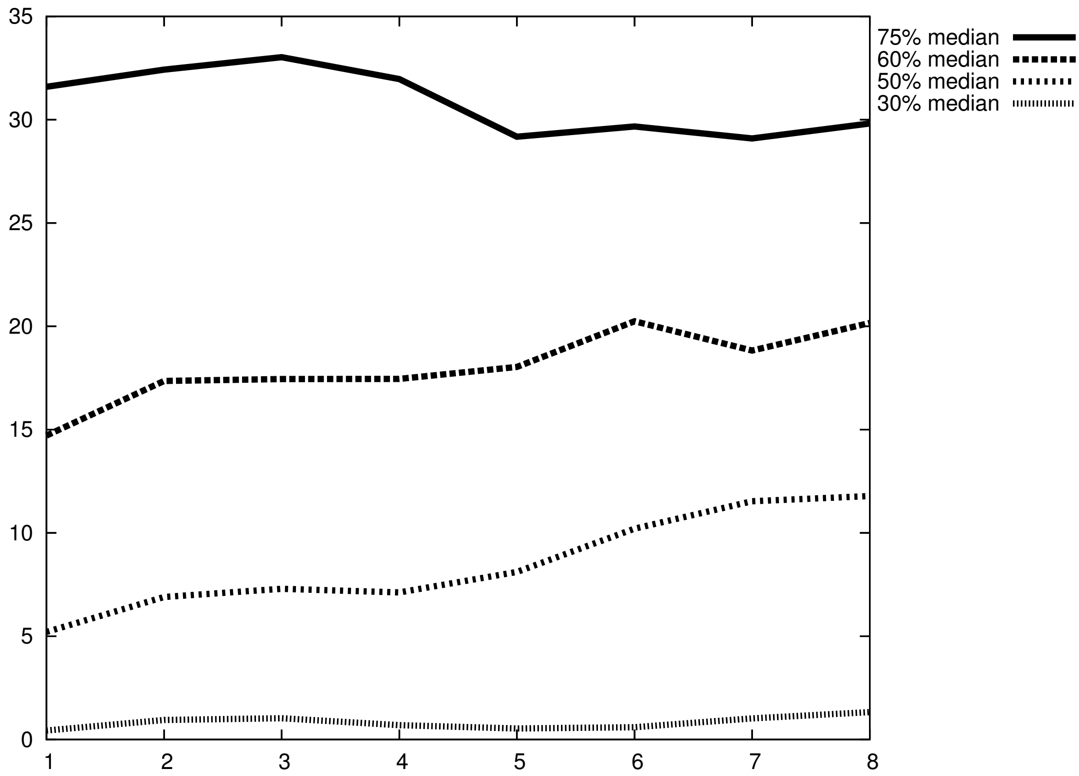

Figure 4.1: Percentage under a variety of percentages of median income

Table 4.2 shows, approximately 45 per cent of respondents experience at least one spell of poverty over the eight waves, but less than 4 per cent are poor the whole time – there is a great deal of movement. Table 4.3 summarises this mobility in terms of the year-to-year turnover between income bands. Respondents with less than 30 per cent of median income in one year have a one-in-five chance of staying there the next year, compared to a one-in-three chance of moving above median income. Less severely poor low-income individuals seem to have a somewhat lower chance of improving their situation: those between 30 and 50 per cent of median income have a less than 50:50 chance of being above 50 per cent of the median the following year, and those between 50 and 60 per cent of the median have only a 35 per cent chance of relative improvement. This is a picture of some movement, down as well as up: chances of getting out of severe poverty are good, but there is a good deal of persistence from one year to the next, particularly for less severe poverty.

Table 4.2: Exposure to poverty over eight waves: number of times below 60 per cent of median

Number of spells in poverty	Sex: Male	Sex: Female	Total
0	57.5	51.8	54.6
1	13.7	14.2	13.9
2	7.9	7.1	7.5
3	5.5	6.1	5.8
4	6.2	6.2	6.2
5	2.0	3.1	2.6
6	2.2	3.5	2.8
7	2.3	3.4	2.9
8	2.7	4.6	3.7
Total	100.0	100.0	100.0

Note: weighted data, restricted to respondents present at all waves

Table 4.3: Year-on-year relative income transitions

Previous year	Percentage of median household income, current year					Person years
	$<$ 30%	30–50%	50–60%	60–100%	Above	
Under 30% median	19.4	13.1	11.7	22.5	33.3	299
30–50% median	2.6	53.2	17.8	19.7	6.7	3,167
50–60% median	1.3	20.6	43.8	29.3	5.1	4,157
60–100% median	0.5	5.4	12.0	59.5	22.6	12,994
Above median	0.4	0.9	1.0	11.7	86.0	22,689
Total	0.8	8.0	9.7	28.4	53.0	43,307

4.1.3 Observing exposure to active labour market programmes

The LIS contains extensive information on labour market participation, and it is this that we use to observe participation in ALMP schemes. The labour market information is recorded in two principal ways, with monthly status calendars as well as more detailed information about the status at interview. While the more-or-less continuous monthly information is attractive, and would in theory permit a broader range of analyses, the level of detail is less than for the status at the time of interview and as a result is less satisfactory for identifying spells in ALMP schemes. In particular, it distinguishes inadequately between education and ALMP-related training, whereas the variables relating to the status at interview are better at identifying participation in either state-sponsored training or subsidised employment.

Unfortunately, even then the amount of detail is substantially less that that available to O'Connell and McGinnity (1997b), with the result that we cannot replicate their detailed classification of ALMP schemes, and can do no more than distinguish between employment schemes and training schemes. The analysis that follows, therefore, uses data from the time of interview only (and thus up to eight consecutive observations per individual), and can distinguish between participation in training and employment schemes.

Table 4.4 summarises the pattern of participation in ALMP schemes between 1995 and 2000, the period of coverage where we have at least one prior year (to provide information on status prior to participation) and one subsequent year (for information on medium-term outcome). Training schemes show a steady pattern of decline from about 0.5 per cent to less than 0.25 per cent of adults in the sample (weighted). Participation in subsidised employment schemes, however, rises to a peak of over 2 per cent in 1998, falling back to 1.3 per cent in 2000. The second panel gives unweighted numbers, which are key to the suitability of this data set for evaluation of the effects of ALMP schemes – it shows that we have about 650 person–wave observations of participation. Allowing for some

Table 4.4: Observed participation in ALMP schemes at time of interview, 1995–2000

Year	Employed	Unemployed	ALMP		Non-employed	Total
			employment	training		
	Weighted percentages					
1995	46.46	7.52	1.31	0.51	44.19	
1996	46.32	7.73	1.45	0.48	44.02	
1997	48.22	6.60	1.61	0.48	43.09	
1998	49.84	4.88	2.17	0.37	42.74	
1999	53.88	3.10	1.79	0.24	40.98	
2000	54.67	2.56	1.31	0.23	41.23	
Total	49.93	5.38	1.61	0.39	42.69	
	Unweighted numbers					
1995	3,263	446	103	33	3,108	6,953
1996	2,973	391	91	26	2,881	6,362
1997	2,912	292	84	22	2,566	5,876
1998	2,598	220	86	23	2,272	5,199
1999	2,181	149	66	16	1,814	4,226
2000	3,206	177	81	21	2,633	6,118
Total	17,133	1,675	511	141	15,274	34,734

losses due to missing data, this represents an adequate number of cases to support the statistical analysis reported below, though the numbers on training schemes suggest that we may have less power to detect effects relating to them.

4.2 Medium-term outcome focus

Our focus for both research questions is on the medium term. This is true of the first question to the extent that we are interested in the effect of prior poverty on the chance of participation in an ALMP scheme the following year, and it is even more true of the second question, where we are concerned with the consequences of participation for exposure to poverty in the following years.

What we attempt to determine is to what extent participation in schemes has consequences ‘down the line’, that is, after the scheme has completed. The main policy justification for interventions of this nature is that they should have beneficial consequences which persist after their completion, both at the individual and at the societal or market level. We therefore look at the individual's household poverty status in the year after participation as our main outcome variable. Without data limitation, we might like to look at longer periods, say two to three years after participation, and to insist that the outcome was measured only if the individual was no longer a scheme participant.[1] However, with only eight waves of data, we do not have sufficient observations of persons entering and completing schemes to look at outcomes further in the future than one year after participation.

4.3 Two analytical passes – descriptive and 'pseudo-experimental'

The analysis reported below can be divided into two main sections: a primarily descriptive exercise which addresses both questions, outlining the characteristics of those who participate in schemes and their experiences after participation, using direct summaries and logistic regression; and a more formal attempt to judge the true effect of participation, using propensity score analysis (see section 4.4).

The aim of the descriptive analysis is multiple. First, and most generally, what are the observed characteristics of those participating in schemes? These should correspond to the formal recruitment requirements, but will also reflect the broader social context. Second, to address our first research question, we investigate what role poverty might have in predicting participation, once we account for characteristics such as unemployment history, household structure and so on.

Schemes will naturally have a target clientèle which is poorer than the average

[1] In theory, persons are not permitted to participate in successive schemes, but this is not always observed to be the case in the data.

by virtue of their weaker labour market positions, but there are a number of potential mechanisms by which the population actually served may be more or less poor than one would expect on the basis of their characteristics. On the one hand, clients could be poorer than their observed characteristics would suggest, if recruitment takes account of unmeasured characteristics, for instance by selecting those most in need of assistance. On the other hand, participants may have unobserved characteristics that make them more likely to seek out advantageous opportunities, through for example being ‘better’ at interacting with the welfare system, or having other characteristics that make them more likely to benefit from participation. We use logistic models of participation to address these questions.

The descriptive analysis also makes a first pass at answering the second question: once we control for their observed characteristics (which will make ex-participants much poorer than the population average), do we see an effect on later poverty exposure? We again use logistic regression to assess the effect of participation on the odds of poverty, controlling for observed characteristics. Insofar as the observed characteristics adequately capture the difference between participants and non-participants, the parameter estimate for participation can be regarded as an estimate of its net causal effect.

However, if participants are systematically different from non-participants in ways that are not captured by the variables in the model, this estimate will be seriously biased. If participants carry some negative characteristics that we do not observe, such as poorer labour market chances that might make recruitment to schemes more likely on the grounds of greater need, they are also likely to perform less well in poverty terms afterwards than non-participants with the same observed characteristics. This will lead to an estimate of the effect of participation that is biased downwards. If, on the other hand, they carry unobserved positive characteristics, like a greater ability to work the system, better social networks, and so on, they are likely to have better poverty outcomes than non-participants. In this case the estimate of the effect of participation will be unduly positive. Because of this problem, the remainder of the analysis uses the pseudo-experimental approach of propensity score matching.

4.4 Propensity score analysis

Propensity score analysis is at heart a 'pseudo-experimental' method. That is, it uses observational data – data collected from the world 'as it is', rather than generated through manipulating the world in an experiment – to draw conclusions about causal relationships in a manner as close as possible to the experimental method. True experiments will typically take two groups, effectively identical through matching or randomisation, and expose one to a 'treatment'. Insofar as the groups are identical in all relevant respects, and as their experience differs only in respect of exposure to the treatment, there is a very strong rationale for identifying any difference in outcome as caused by the treatment.

By contrast, conventional use of observational data to assess the causal effects of 'treatments' such as ALMP schemes poses a problem: how to we compare the outcome of the 'treated' group with the outcome that an identical but untreated group would have had. This so-called 'counterfactual' comparison is what experiments achieve by matching groups prior to treating one of them. It is clear that a simple comparison of, say, scheme participants with the population at large is not an adequate way of assessing this, as participants have characteristics that will make them poorer than average even after a beneficial scheme, so the usual strategy is to measure the difference in risk of poverty between participants and non-participants, controlling for measured characteristics such as age, gender, education, labour market history and so on.

This is unsatisfactory in two respects. First, it involves assessing the effect of participation by comparing participants with the population at large, most of whom are not likely to benefit from or participate in the schemes. A more relevant comparison would be between participants and non-participants drawn from a population 'eligible' for participation. The second way in which this is unsatisfactory is that unobserved characteristics may still have an effect on the outcome, resulting in a biased estimate of the effect of participation. A particularly important mechanism by which this can take effect is selection, whereby unobserved characteristics of individuals which predispose them to participation, also have an effect on the outcome. Thus, for instance, welfare officers might be more likely to offer schemes to persons they feel are more likely to benefit from them, on the basis of characteristics (such as energy, initiative and so on) which are not captured in our data sets and which will also raise the individuals' labour market prospects in the medium term. Correspondingly, if schemes are a 'last resort' participation may tend to be more common among those with the poorest labour market prospects.

In the former case conventional analysis will over-estimate the benefit of participation, confusing the effect of the selection of slightly more able persons into schemes with the concrete effect of participation, while in the latter it will under-estimate it. An excellent overview of the evaluation problem is given in O'Neill (2000).

The propensity score approach attempts to solve both problems in a pseudo-experimental framework. It does this by matching participants in the sample with non-participants who are as like them as possible, thus matching a treatment group with a 'control' group. Differences between them in outcome can thus be considered an estimate of the causal effect of treatment, as long as the matching process is adequate. Matching can in principle be done across a range of variables – gender, age, labour market experience and so on – but the more variables available the more difficult it is to find a matching individual. What is novel about propensity score matching is not the pseudo-experimental comparison but the means of matching individuals. Rosenbaum and Rubin have demonstrated that it is in principle sufficient to match participants with individuals who have the same estimated probability of participation, but who did not in fact participate (Rosenbaum and Rubin, 1983, *inter alia*). This estimated probability is calculated on the basis of a probit or logistic regression model of participation. The use of this estimated probability or 'propensity' is what gives the method its name.

When we are concerned with estimating the effect of participation on participants ('of treatment on the treated'), matching requires the assumption that once we condition for a set of variables predicting participation, Z, the distribution of the outcome given non-participation, Y_0 (which, critically, is not observed for programme participants, and is thus the 'counter-factual'), is independent of whether participation occurs (D):

$$E(Y_0|Z, D = 1) = E(Y_0|Z, D = 0) = E(Y_0|Z)$$

(Smith and Todd, 2005, equation 8).

In other words, once we control adequately for variables predicting participation, the fact that individuals did or did not actually participate does not give us any more information about their probable outcome. Smith and Todd (2005, p.313) further caution that matching is only justified when performed over the *common support region*. Observations where the support of Z does not overlap fall *outside* this area. In other words, matching is valid only for those participants whose predicted probabilty of participation is overlapped by the distribution of predicted probability of non-participants.

When these assumptions hold, we can calculate the 'effect of treatment on the treated' as

$$TT = E(Y_1 - Y_0|D = 1) = E(Y_1|D = 1) - E_{Z|D=1}\{E(Y_0|D = 0, Z)\}$$

The first term can be calculated from the observed outcomes for participants, and the second from the matched non-participants (Smith and Todd, 2005).

Matching is concerned solely with selection on observables. Propensity score matching is an innovation of Rosenbaum and Rubin (1983), concerned with matching participants and non-participants on their estimated probability of participation, $P(X)$. They show that when matching on X produces consistent estimates, so too does matching on $P(X)$, and matching on $P(X)$ is much more efficient than matching on X if X contains many variables.

The conditional independence assumption requires that all variables affecting both participation and outcomes in the absence of participation be included in the matching. Smith (2000) notes that this requires careful thought as to what variables do and do not affect participation and outcomes. It has been shown that matching reduced the raw bias in earnings between participants and eligible non-participants – drawn from the same local labour market and with earnings information collected in the same way (Heckman et al., 1997, 1998). Remaining bias is further shown to be of the same magnitude as that of experimental techniques. Dehejia and Wahba (1999) employ propensity scores to match on pre-programme earnings. They concluded that matching eliminates the vast majority of bias. However, this finding is contested by Smith and Todd (2005) who claim it is sensitive to their choice of sample and X variables.

Smith (2000, p.12) identifies important differences between matching and the regression approach to evaluation:

1. Matching is non-parametric, thereby avoiding functional form restrictions implicit in linear regression.
2. Evidence suggests avoiding these functional form restrictions can be important in reducing bias (Dehejia and Wahba, 1999; Smith and Todd, 2005).
3. Importantly, matching highlights the 'support' problem.[2] Since it may not always be possible to match every value of $P(X)$ appearing in the participant group with $P(X)$ values from non-participants, the area of 'common support' of matched values of $P(X)$ may not include all cases from the participant group. (Heckman et al., 1997; Dehejia and Wahba, 1999). In contrast, impact estimates based on simple regressions on X often ignore this problem.

We are also mindful of some caveats relating to matching. Specifically:

1. Matching does not remove the problem of variable selection. Heckman et al. (1997) have shown estimates produced by matching to be sensitive to the choice of variables used to construct $P(X)$.
2. The 'balancing test' of Rosenbaum and Rubin (1983), as implemented by Dehejia and Wahba (1999) and Lechner (1999), will help to determine whether or not to include higher-order interaction terms for a given X. But it does not aid in selecting variables to include in X to start with (Smith and Todd, 2005).
3. The choice of matching method may make a difference in small samples. The available choices are discussed in Heckman et al. (1997). In this study we implement Nearest Neighbour (NN) matching to approximate a counterfactual for the treated. NN may be implemented with or without replacement – where a non-participant may be matched/used more than once.

[2] The support of a distribution is the set of values for which it has a positive density (or non-zero probability).

4. The estimation of propensity scores adds variation beyond the normal sampling variation. According to Smith and Todd (2005, p.13), NN matching with one matched comparison may result in understated standard errors.

5 Descriptive analysis

5.1 Poverty and participation rates

Our first research question concerns the nature of the population that ALMP schemes target. The programmes are designed to target the long-term unemployed, those with poor skills, and those with poor labour market histories. This will clearly involve a *de facto* focus on groups characterised by low income and other observed features. However, it remains an empirical question the extent to which participation is linked to unobserved characteristics, say, unobserved advantage (e.g. a better understanding of how the welfare system works) or disadvantage (e.g. unobserved labour market difficulties that make recruitment more likely).

Table 5.1 reports the percentage participation in schemes, broken down by a number of factors (measured the year before). The data are weighted cross-sectionally and all available observations are used, so the numbers represent person–years rather than individuals. Almost 2 per cent of observations are in schemes, with more than four-fifths being in employment as distinct from training schemes. The bivariate relationships may well be misleading but it is interesting to examine the effect on participation of gender, age, household structure and employment status, as well as poverty.

Gender has a small but significant effect, males being more likely to participate overall but females more likely to take training courses. Marital status has interesting effects, with the separated and divorced having quite high participation in employment schemes (though, particularly for divorce, the number of observations is quite small). The widowed have low participation overall and the never-married have high rates of training – both effects most likely explained by age. Having a preschool age child in the house, and having children in general, raise the rate of participation in employment schemes and

Table 5.1: Predictors of participation in employment schemes, training schemes and all schemes, weighted data, pooled across years

	Per Cent participating in scheme			Person–years
	Employment	Training	All	
Sex of respondent				
Male	1.68	.28	1.96	20750
Female	1.53	.38	1.91	21861
Marital status				
Married	1.51	.17	1.67	23675
Separate	2.91	.34	3.25	1006
Divorced	8.06	0	8.06	208
Widowed	.04	.02	.06	3052
Never married	1.9	.67	2.57	14669
Preschool child in house				
None present	1.49	.35	1.84	37349
Preschooler present	2.43	.19	2.61	5262
Number of children				
None	1.02	.45	1.47	21029
1	2.35	.15	2.49	6970
2	2.1	.21	2.31	7097
3	1.99	.24	2.23	4469
4	2.13	.46	2.59	1786
5	2.79	.18	2.97	652
6	1.11	.32	1.43	377
7	.62	.85	1.47	183
8+	9.44	0	9.44	48
Age group				
Under 20	.31	1.44	1.75	3221
20–29	1.76	.56	2.32	8338
30–39	2.28	.21	2.5	8657
40–49	2.42	.27	2.68	7687
50–59	2.03	.14	2.17	5869
60–69	.54	.04	.58	4455
70 plus	0	0	0	4384
Educational achievement				
Minimal	1.77	.17	1.95	9550
Incomplete secondary	2.24	.45	2.69	14458
Complete secondary	1.48	.43	1.9	12008
Diploma/Degree	.18	.15	.33	6596
Employment status $(t-1)$				
Employed	.25	.09	.33	21001
Unemployed	5.9	1.39	7.29	2466
Employment scheme	54.85	.47	55.32	645
Training scheme	8.74	9.06	17.8	185
Non-employed	.63	.38	1.01	18313
Poor at $t-1$				
Not poor	1.32	.28	1.6	34931
Poor	2.88	.57	3.45	7680
Total	1.6	.33	1.94	42611

generally speaking depress participation in training, though the effects by number of children are somewhat unstable. Age shows interesting patterns: the youngest and oldest are least likely to participate in employment schemes, while training schemes are strongly skewed to the young, being dominated by the under-30s. This is unsurprising since the majority of training schemes are targeted towards young labour market entrants, specifically apprenticeships, vocational training, Youth Reach and the Vocational Training Opportunity Scheme. When we look at education we find that the only group to be above average in participation in both types of scheme are those with incomplete secondary education. Those with the poorest qualifications have surprisingly lower participation, especially in training schemes – this is most likely an age effect again, as this group will be predominantly older. Interestingly, those with complete secondary education have a disproportionately high take-up of training schemes, while predictably those with some third level education have low take-up overall.

Employment status is a problematic predictor. While employment and training schemes are mostly of relatively short duration, and are not intended to be 'chained', we find in practice that being observed in a scheme the previous year is very highly correlated with being in one during the current year (some Community Employment schemes do have durations in excess of twelve months). To some extent this may be due to the survey mechanics, where the gap between successive interviews is a year on average, but some of the time can be much less (a late interview one year followed by an early one in the next year's fieldwork). However, it is also due in large part to persons remaining in the system for periods well in excess of 12 months. Thus more than half of those observed on employment schemes a year earlier were still on employment schemes, and those on training schemes have about an 18 per cent chance of being in a scheme the following year, approximately half-and-half training and employment. Being employed or being out of the labour market are relatively stable states, with low rates of flow into schemes, but the unemployed have high rates of entry to both types.

Finally, we look at poverty: how does one's poverty status the previous year affect participation on a scheme during the current year? Not taking account of any other variables, those with less than 60 per cent of median income the previous year are approximately twice as likely to be on a scheme as those above the line. On its own, poverty is a predictor of participation, but it is not clear whether this can be 'explained away' in terms of gender, employment status, age, household structure and so on. To address this issue we now move on to a multivariate analysis.

5.2 Do schemes 'target' the poor?

As we have seen, a range of factors, some of which are also correlates of poverty, are associated with participation in ALMP schemes. To address the question of whether poverty has an additional effect on top of the effects of these covariates, we fit a series of logistic regressions. If the net effect of poverty on participation is zero, we can say that we have accounted for the apparent effect of poverty on participation. If it is positive, that would suggest that schemes target or are more attractive to persons in poverty, over and above the non-poor with the similar measured characteristics. If it is negative, it would suggest that schemes preferentially target or are attractive to non-poor persons.

As is apparent from Table 5.1, training schemes and employment schemes recruit different types of individual. We therefore model the two destinations separately. Indeed, we would like to disaggregate further, ideally following the fourfold classification of O'Connell and McGinnity (1997a,b) outlined in Table 2.1, but are constrained to distinguishing between training and subsidised employment by the numbers and the level of detail available in the data set. In what follows we present paired logistic models, predicting participation at wave t using characteristics measured at wave $t-1$ (we use robust standard errors that take account of the presence of repeated observations per individual). The variables we consider are largely the same as in Table 5.1: gender, marital status, age (in 10-year bands; for this analysis the age range is restricted to 16–59 years of age), whether there is a preschool child present, the number of children in the household, highest educational qualification, employment status and poverty status. We additionally include an index of occupational quality, the International Socio-Economic Index (ISEI) score, set to zero for those without an occupation (Ganzeboom et al., 1992; Ganzeboom and Treiman, 1996), and an index of recent labour market history.[1]

What is first evident from the table is that there are substantial differences between employment schemes and training as destinations, and that the model pooling them is in some respects an uninformative average. This is particularly evident in the age estimates, where we see two distinctly different and significant profiles for the specific destinations collapsing a much weaker pattern for the combined model.

Working through the table from the top, we see first that while females are estimated to be more likely to participate, this effect is very far from significant. Compared with those currently married, the never-married have enhanced rates of participation in both types of schemes, and the divorced of participating in employment schemes. The age profiles already referred to are particularly distinct: as seen in Table 5.1 recruitment to employment schemes is strongest in the middle years, while for training there is a close to monotonic decline with age. Having a preschooler present has no effect for men or women on entry to employment schemes, but has a negative effect for training (and since no males with preschoolers in the sample actually enter training, this can be read as an effect for females). The latter, but not the former, is consistent with Table 5.1. Large families seem to push males but not females into schemes – this is significant at 1 per cent for the pooled destinations, at 5 per cent for employment

[1] This is calculated as the proportion of the last 24 months spent unemployed. In order not to lose cases where the available information did not cover 24 months, it is calculated on however much history is available.

Table 5.2: Logistic regressions predicting participation in schemes in year t, using covariates measured in year $t-1$.

Model:	All schemes		Employment schemes		Training schemes	
	$\hat{\beta}$	s.e.	$\hat{\beta}$	s.e.	$\hat{\beta}$	s.e.
Female	0.153	0.120	0.122	0.145	0.267	0.190
Marital status (ref=married)						
Separated	0.443	0.255	0.302	0.291	0.956	0.555
Divorced	0.926*	0.415	0.992*	0.433	.[(a)]	
Widowed	-0.869	0.591	-1.247	0.749	0.385	0.967
Never married	0.733**	0.163	0.706**	0.183	0.654*	0.309
Age group (ref=<20)						
20–29	-0.128	0.167	0.819**	0.270	-0.590**	0.224
30–39	0.372*	0.180	1.635**	0.276	-1.501**	0.373
40–49	0.549**	0.197	1.761**	0.290	-0.909**	0.346
50–59	0.307	0.210	1.667**	0.297	-1.844**	0.474
Pre-schooler (main effect)	-0.084	0.195	0.111	0.202	-0.862*	0.398
by female	0.358	0.246	0.391	0.265	.[(a)]	
Number of children (main)	0.151**	0.046	0.124*	0.050	0.207	0.126
by female	-0.141**	0.051	-0.111	0.058	-0.080	0.120
Education (ref=none/primary)						
Incomplete 2ndry	-0.002	0.132	-0.023	0.142	0.266	0.365
LC and similar	0.072	0.148	0.003	0.168	0.423	0.362
Dip/Degree	-0.542*	0.268	-0.733*	0.343	0.269	0.484
Employment status (ref=employed)						
Unemployed	2.259**	0.298	2.355**	0.363	1.771**	0.444
Emp. scheme	5.203**	0.173	5.474**	0.196	1.799**	0.565
Training scheme	3.386**	0.320	2.578**	0.468	3.798**	0.439
Non-employed	1.000**	0.274	0.665	0.351	1.449**	0.414
Occupational score	-0.010*	0.005	-0.014*	0.006	-0.004	0.008
Unemployment history						
linear	3.349**	0.577	3.352**	0.635	3.394**	1.219
squared	-3.643**	0.586	-3.768**	0.634	-3.317**	1.283
Poor	0.334**	0.105	0.341**	0.122	0.297	0.205
Intercept	-5.924**	0.352	-7.204**	0.454	-6.654**	0.662
Number of observations:		31580		31580		31495[(a)]
Initial log likelihood:		-3456.592		-2857.167		-978.313
Log likelihood:		-2302.931		-1715.355		-817.004
Model degrees of freedom:		24		24		22
Pseudo R-squared:		0.334		0.400		0.165

Note: (a) No males with preschool children, or divorcees entered training schemes, involving a loss of 85 person–years and the dropping of the relevant parameters in the training scheme model

*: p $<$ .05; **: p $<$ .01

Robust standard errors

schemes and has the right sign but is insignificant for training schemes (relatively few entrants to training will be old enough to have large families).

Education no longer shows the U-shaped effect seen in Table 5.1, but the low recruitment of those with third level qualifications persists for employment schemes though not for training.

Employment status the previous year is strongly significant. Reflecting the story from the bivariate analysis, persistence in the state (i.e. staying in a scheme from year to year) is a very strong pattern. There are very large significant parameter estimates for remaining in each sort of scheme, along with large significant estimates for moving from one type to the other. Unsurprisingly, unemployment at the $t-1$ interview date raises the probability of entering a scheme. The interesting difference between the scheme types is in the effect of non-employment. Being outside the labour market is not significantly different from being employed in the effect on the odds of entering employment schemes (i.e. it is very low), but it significantly raises the chances of entering training.

Alongside the employment status at the previous wave's interview, we also include the index of the labour market history, the proportion of the past two years spent in unemployment (i.e. ranging from zero to one). This is included as a linear and a squared effect, in order to allow it have a non-linear effect. All three models have very similar effects: as the number of months an individual has spent unemployed rise from zero to about 12, the chance of recruitment rises, but then it falls back again, more or less completely by 24 months. This may reflect eligibility criteria, such that as one's unemployment experience rises so does one's eligibility, with approximately twelve months giving empirically 'full' eligibility. For persons with greater unemployment experience who have not been recruited, it may be that they have unobserved reasons making them ineligible or unwilling to participate in ALMP schemes: if they have not taken the opportunity by about 12 months, they are less and less likely to do so.

Finally, we look at the additional effect of poverty on recruitment – controlling for this set of variables which are also highly predictive of poverty. Poverty at wave $t-1$ has a substantial, significant positive effect on participation in employment schemes. However, while the magnitude of the effect for training schemes is similar, it is completely insignificant: those entering training schemes may be poor but they are not more poor than their observed characteristics would suggest.

5.2.1 Targeting the poor? A summary

What emerges from this analysis is a story that endorses the view of O'Connell and McGinnity (1997b) that different categories of schemes have very different clientèles. Employment schemes seem to recruit from those in their middle years, without high levels of education, and with problematic labour market status. Moreover, among this population they seem to favour those who are poorer. On the other hand, training schemes focus predominantly on the young, do not seem to exclude those with higher levels of educational qualifications, and do not exclude those outside the labour market. Given their bias towards the young, recruitment from outside the labour market can be presumed to mean recruitment from the educational system. And finally, poverty over and above that consistent with their other observed characteristics does not increase the odds of entering training schemes.

In sum, we have a picture of two very different types of programme, with one attempting to help people with longer-term labour market and poverty difficulties, which can often mean people with relatively intractable problems, and the other attempting to boost the skills and prospects of those early in their career. The latter group may typically be experiencing difficulties of insertion, of not having marketable skills, which can be presumed to be fairly tractable problems in the context of a training scheme.

To answer our research question, it is clear that both sorts of schemes have client bases that are disproportionately poor, but only employment schemes seem to target those who are poorer than one would expect given their observed characteristics.

5.3 Do schemes have an effect on medium-term risk of poverty?

We now move on to our second question, and look at the impact of participation in ALMP schemes on poverty outcomes in the medium term. For now we use a similar framework to the preceding section, and fit logistic regressions. The first model we use considers the chance of poverty at $t+1$, using the same control variables as above, with participation on a scheme at t, and poverty status at t as the key explanatory variables. This model can be considered to deal just with the odds of being poor at $t+1$, and to say nothing about change from t to $t+1$. We therefore supplement it with two more models, looking at the chances of becoming poor, and the chances of exiting poverty. That is, for those who are not poor at t we model the chances of being poor at $t+1$, and *vice versa*. Naturally, model 1 has many more cases than models 2 and 3.

The first model, looking at the odds of poverty at $t+1$ for all cases, poor or not at t, shows some interesting patterns. Gender has no significant effect, but marital status does: married couples are least likely to be poor, the separated and divorced most likely. The age profile suggests that the middle years in our range (30–49 years old) are most likely to be in poverty. Having a preschool child in the house makes poverty more likely, but the number of children has a contrary effect for men and women, raising the risk of poverty for men but not for women. Education has a clear protective effect, as does occupational prestige. The unemployment history variable is not significant. Poverty at t has a very strong predictive effect for poverty at $t+1$, as is to be expected, but the magnitude is very big, increasing the odds by a factor of more than 12 ($e^{2.513} = 12.34$). This

Table 5.3: Logistic regressions predicting poverty outcome in year $t+1$.

Model:	Odds of poverty at $t+1$		Odds of entering poverty		Odds of exiting poverty	
	$\hat{\beta}$	s.e.	$\hat{\beta}$	s.e.	$\hat{\beta}$	s.e.
Female	-0.041	0.061	0.003	0.080	0.133	0.106
Marital status (ref=married)						
Separated	0.504**	0.114	0.785**	0.154	-0.054	0.185
Divorced	0.462*	0.220	1.008**	0.353	0.305	0.367
Widowed	0.353*	0.163	0.428*	0.204	-0.181	0.305
Never married	0.206**	0.074	0.298**	0.102	-0.043	0.128
Age group (ref=<20)						
20–29	0.132	0.079	-0.010	0.106	-0.289*	0.137
30–39	0.408**	0.090	0.172	0.124	-0.743**	0.155
40–49	0.356**	0.096	0.146	0.131	-0.609**	0.157
50–59	0.219*	0.100	-0.118	0.139	-0.642**	0.162
Pre-schooler (main effect)	0.236*	0.092	0.238*	0.121	-0.321*	0.144
by female	0.056	0.113	0.070	0.147	0.039	0.184
Number of children (main)	0.074**	0.021	0.160**	0.029	0.040	0.035
by female	-0.092**	0.025	-0.097**	0.034	0.087*	0.041
Education (ref=none/primary)						
Incomplete 2ndry	-0.176**	0.057	-0.345**	0.081	-0.081	0.092
LC and similar	-0.651**	0.069	-0.739**	0.094	0.524**	0.114
Dip/Degree	-1.098**	0.119	-1.307**	0.140	0.697**	0.236
Employment status (ref=employed)						
Unemployed	-0.206	0.148	0.033	0.176	0.190	0.214
Emp. scheme	0.940**	0.134	0.775**	0.168	-1.271**	0.266
Training scheme	0.471	0.247	0.436	0.313	-0.611	0.391
Non-employed	0.506**	0.100	0.413**	0.126	-0.634**	0.168
Occupational score	-0.014**	0.002	-0.013**	0.003	0.016**	0.004
Unemployment history						
linear	0.687	0.366	2.127**	0.445	1.427*	0.569
squared	0.448	0.376	-1.124*	0.472	-2.421**	0.552
Poor	2.513**	0.050	(a)		(a)	
Intercept	-2.608**	0.150	-2.512**	0.195	0.180	0.255
Number of observations:		40956		34122		6834
Initial log likelihood:		-18864.191		-9101.750		-4373.796
Log likelihood:		-12566.010		-8497.142		-3963.153
Model degrees of freedom:		26		25		25
Pseudo R-squared:		0.334		0.066		0.094

Note: (a) poverty status at time t is a constant for models 2 and 3
*: $p < .05$; **: $p < .01$
Robust standard errors

gives us our motivation for the second and third models: since poverty is such a strong predictor by virtue of a tendency to remain in the same state, it will give a different insight to look at the determinants of *change* in poverty status (see below).

However, remaining with the first model we can now move our focus to the estimate of the effect of participation in employment or training schemes. Controlling for this broad range of predictors of poverty, we find that training schemes have no significant effect on poverty at $t + 1$; however the employment schemes appear not to have a protective effect, but to occasion a significant rise in the risk of poverty: participation in employment schemes raises the odds of poverty by a factor of about 2.6.

Let us consider the two ‘change’ models. The direction of these models is different, as the estimates of the effect of education show: as one’s level of education rises the chance of entering poverty falls, and the chance of exiting poverty rises. Again training schemes have no significant effect, but employment schemes substantially raise the risk of entering poverty and even more strongly lower the chance of getting out of poverty.

The picture that emerges, particularly in respect of employment schemes, is certainly contrary to the goals of ALMP – on the face of it, it is saying that employment schemes make people poorer in income terms, not richer. That this estimate is made while controlling for a range of relevant variables makes it appear all the more damning. However, it is likely that the explanation for this estimate is not a direct causal effect of schemes in raising the risk of poverty, but a selection effect. That is, it is likely that participants on employment schemes have characteristics, other than those captured by the control variables, which make them more prone to poverty. Insofar as this is true, the estimate is biased and does not reflect the true effect of participation.

We could attempt to remedy the problem within the context of this approach by searching for more variables to predict poverty, in the hope of controlling for the hitherto unmeasured differences of employment scheme participants, but it is likely that the data set does not contain all the relevant variables, and indeed that many of the relevant variables are effectively unobservable in a survey context. We therefore move on to our second analytical strategy, to attempt to cope with this selection problem.

6 Propensity score matching analysis

6.1 Carrying out the matching

We fully exploit the three-timepoint nature of the data we have extracted from the LIS in this analysis. We extract all observations of three consecutive years, and use covariates measured in year 1 ($t-1$), to predict participation in year 2 (t) and then use the propensity scores derived from the prediction to assess differences in outcome in year 3 ($t+1$). Our core outcome variable is of course household poverty status, but we also consider two other outcomes to give a broader perspective. The first of these is employment status: the most direct mechanism by which a scheme could be expected to alleviate poverty is by improving employment chances. The second of these is self-reported health status. This is to throw a different light on the process. It may be, particularly for employment schemes, that their effect on employment chances, and therefore indirectly on poverty avoidance, is relatively small but that there is a positive effect on subjective quality of life. If this is the case, it is likely to show up in self-assessed health.

The first stage of the propensity score analysis is to develop the selection model. This predicts the probability of participation in a scheme at t, using information from $t-1$. We have a little more freedom in carrying out this modelling than when modelling for directly analytical purposes, because the goal is a strongly predictive model, rather than one that can be interpreted. This means we have a greater tolerance for multicollinearity, insignificant parameters and imparsimony. We take advantage of this by including not only all the variables used in the logistic regressions of sections 5.2 and 5.3, but also a range of other contextual variables. First we allow age to have a cubic form instead of using 10-year bands; we include income information in the form of the log of the proportion of the median income rather than just as a poor–non-poor dichotomy; we include self-reported health status at $t-1$; whether the respondent is a medical card

Table 6.1: Predicted probabilities by participation, propensity score match models

Participate		Obs	Mean	Std. Dev.	Min	Max
All schemes	no	39654	0.014	0.049	1.34e-11	0.836
	yes	751	0.267	0.266	.0009466	0.875
Employment	no	39810	0.010	0.045	4.29e-10	0.845
	yes	595	0.318	0.282	.000671	0.867
Training	no	37301	0.004	0.011	2.41e-13	0.461
	yes	156	0.042	0.074	.0004539	0.421

holder; housing tenure (owner, private tenant, local authority tenant, rent-free); social class (using the Goldthorpe scheme); regional (NUTS3) unemployment rate time-series; and finally NUTS3 region and wave (as sets of dummy variables).

We use Leuven and Sianesi's psmatch2 module for Stata to estimate the models (Leuven and Sianesi, 2003), and use 'nearest neighbour' matching.

We do not present the direct results of the estimation of the model, because its purpose is predictive. We do present some summary statistics for the predicted probabilities in Table 6.1. As can be seen, the mean predicted probability for participants is far higher than for non-participants, but the range for participants and non-participants is roughly similar. Given the far larger number of non-participants, it is therefore relatively easy to match participants with non-participants who have similar probabilities of participation.

6.2 Results

We now consider the difference in outcome between participants and matched non-participants. As above, we first present results for all schemes, and then for employment and training schemes separately. For poverty as an outcome, we see (Table 6.2) that nearly 28 per cent of scheme participants are below the poverty line one year later, compared with 'unmatched controls' (effectively, the

Table 6.2: Results of the PS matching analysis

	Treated	Controls		Difference	S.E.	t-stat
		Unmatched	Matched			
Percentage poor at $t+1$						
All schemes	27.7	17.6	26.3	1.4	3.2	0.44
Employment schemes	30.0	17.6	23.9	6.1	3.6	1.69
Training schemes	18.8	16.1	23.9	-5.1	5.4	-0.95
Percentage employed at $t+1$						
All schemes	24.7	51.4	44.9	-20.2	3.3	-6.06
Employment schemes	20.1	51.4	42.9	-22.8	3.8	-6.04
Training schemes	42.7	52.5	45.3	-2.6	6.6	-0.39
Mean health score at $t+1$						
All schemes	1.73	1.78	1.71	.016	5.6	0.28
Employment schemes	1.78	1.78	1.83	-.048	.06	-0.79
Training schemes	1.52	1.72	1.63	-.111	.10	-1.10

general population) at 18 per cent – as we know, participants have characteristics that give them a much higher risk of poverty. When we select those controls with the closest estimated propensity to participate, this difference evaporates: members of the sample who have characteristics that make them as likely to participate as participants are (according to our propensity score model), but who did not, have a 26 per cent chance of poverty. The remaining difference is well below significance, which allows us to conclude that once we properly control for the characteristics of participants, the estimated effect of participation in schemes is not significantly different from zero. On average, participation in ALMP schemes has no measurable effect on poverty one year later.

When we distinguish between employment and training schemes, we see that the differences noticed above are replicated. Those on employment schemes have a 30 per cent risk of poverty at $t+1$, but matched controls have only a 24 per cent risk. This looks like a big difference but the t-statistic (which does not take into account that the propensity score is based on a model, and is therefore likely to be inflated) is only 1.69, so the difference is at best marginally significant. Nonetheless, the best we can say about employment schemes is that there is insufficient evidence to suggest that they are damaging; there is certainly no evidence here to suggest that they have any positive effect whatsoever on poverty risk. This is in distinction to the logistic regression results in Table 5.3 where the inference is that employment schemes have a clearly damaging effect: once we take proper account here of the characteristics of participants, we see this damaging effect reduced to a neutral effect.

Training schemes are very different. Those on training schemes have a 19 per cent poverty risk at $t+1$, compared with 16 per cent for unmatched controls. That is, they are really not very different from the population at large in this respect. When we match them with non-participants with similar predicted probabilities of participation, we find that the matched controls have a higher risk of poverty, at 24 per cent, though again the difference is not significant. This corroborates our analyses above: poverty is not a significant predictor of participation, and participation is not a significant predictor of poverty risk at $t+1$. Moreover, training schemes and employment schemes clearly serve quite different clientèles.

However, apart from re-emphasising the training/employment scheme difference, our analysis of poverty outcomes has a largely negative finding: there is no evidence of benefit, defined in terms of poverty risk in the year after participation.

Now, household income is a fairly indirect consequence of participation. We can expect that most cases of improvement in household income are due to improvements in employment status, and we can attempt to unpick any change (or lack of change) in income in terms of change (or not) in employment status. We therefore also present the effect of participation on employment status (a working/not-working dichotomy) as an outcome. Here we see a stark effect: 25 per cent of scheme participants are employed a year later, compared with 45 per cent of matched controls, a very significant difference. This is almost entirely due to the effect of employment schemes, where only 20 per cent of participants are employed at $t+1$. Those on training schemes have an employment rate that is not significantly lower than that of matched controls.

If the propensity score matching has fully accounted for differences between participants and matched non-participants, this suggests that employment schemes are positively damaging in terms of employment chances, but it remains possible that there are further unobserved differences. If that is the case, it suggests that employment schemes are being used for people with fairly intractable labour market difficulties over and above those captured by the variables used in the model, and that these difficulties persist after their participation in a scheme. One other fact that must be considered is the relatively high chance of being found in an employment scheme again at $t+1$, as evident in the analysis in Table 5.2. Empirically, being on a scheme at t is strongly associated with being on a scheme at $t+1$, implying that many of those who are not working at $t+1$ will be in employment schemes. If this were as an alternative to *work*, then our focus on employment as an outcome is perhaps an underestimate of the benefit of the scheme. Notwithstanding this, it is likely that for most people the scheme is an alternative to unemployment.

It is also worth noting that the observed employment penalty is noticeably stronger than any income-related poverty penalty. We can infer from this that the employment of matched non-participants is not particularly well paid.

The effect of schemes on employment status may be relatively direct, and on income deprivation relatively important, but income poverty is not the only potential benefit of scheme participation. In particular, community employment schemes are often designed to combat isolation and social exclusion, and may deliver lasting benefits to quality of life that are not reflected in employment or income changes. This is obviously harder to measure, and in the available data the closest proxy is self-reported health status.[1] Clearly, this will be affected by

[1] In the LIS this is measures as a five-point scale from very good (1) to very bad (5), with higher scores

biological factors and medical events that are independent of participation, but it will also be affected by participation through the benefits of regular activity, boosting self-esteem and combating anxiety and depression.

We find there is very little systematic difference in self-reported health status between participants and matched controls – both for aggregate-ALMP and disaggregated scheme types. Perhaps surprisingly, employment scheme participants are not significantly less subjectively healthy than the population at large, and are even slightly more healthy than the matched controls (though not significantly). Those on training schemes are more healthy than the population but not more than the matched controls, most probably due to their age profile. Data limitations may mean that it is not an exceptionally powerful test for improvement in quality of life, but the formal conclusion is that ALMP schemes have no measurable effect on subjective health in the following year.

representing worse health.

7 Conclusion

We have asked a number of questions about Active Labour Market training and employment schemes, by exploiting the power of longitudinal data to compare the post-treatment outcomes of scheme participants with non-participants who were closely matched using rich pre-treatment information.

One consistent pattern that emerges from the analysis is the substantial difference between participants in employment schemes and those in training schemes. In respect of our first research question, it is clear that employment schemes focus quite sharply on those with particularly low income. We see that poverty predicts participation even when controlling for a wide range of individual and household characteristics. This is not true of those entering training schemes; while they are much poorer than the population average, their poverty status has no additional explanatory power when modelling entry.

Those on training schemes are predominantly younger and appear to be either experiencing difficulties in integrating into the labour market or taking advantage of opportunities to improve their skills. Conversely, those on employment schemes have an age-profile biased in the other direction and have quite a high probability of persisting in schemes rather than finding employment. We find that while training schemes appear to serve those with problems of labour market insertion, employment schemes focus on those with more intractable problems of low employability.

Our second question focuses on outcomes, on whether participation has consequences for the risk of poverty one year after participation. Here our first pass at quantifying the contribution of schemes to poverty in the subsequent year shows a strong damaging effect of employment schemes. Relative to people with matched characteristics, participants have a substantial increase in the odds of poverty. This, however, cannot be interpreted as a causal effect of the schemes. Rather, those selected for schemes have unobserved characteristics which raise their poverty risk. It is likely, for instance, that places on schemes are preferentially given to those with the poorest labour market prospects. Insofar as the models presented in Table 5.3 do not take account of these 'selection' characteristics, this naïve parameter estimate for the effect of participation will be biased.

When we account for selection bias by estimating the treatment effect relative to matched controls only, we find no evidence that either form of scheme has an effect on the risk of poverty a year after participation. That is, the difference in poverty outcomes between scheme participants and their matched controls lies approximately within our reported standard error. Moreover, the same analysis shows that employment schemes seem to reduce the chances of employment. These are quite negative findings, insofar as the purpose of the policies is to improve the lot of those most vulnerable in the labour market.

There are limitations to our perspective. First, outcomes one year after participation may be too soon. It may be that benefits of employment schemes only emerge after participation of a longer duration, or that placement rates in employment are stabilising during the first twelve months after exiting a programme. This was suggested by O'Connell and McGinnity (1997a) who opted for a two-year post-programme evaluation. It may also be the case that returns to training schemes are not fully realised in the first post-treatment observation. Second, our quality-of-life measure is poor: self-reported health is an important but imperfect proxy for general wellbeing. It is possible that other, sharper measures would show greater improvements in quality of life, if not poverty or employment. Third, for some categories of employment schemes and of clients, participation may be an immediate, if not lasting benefit – persons with very low employability may be psychologically and socially (if not financially) better off than on unemployment benefit, and may be enabled to make real contributions to their communities. Such people may well manage to participate in a sequence of schemes, despite this not being intended by the policy makers. This sort of benefit may be outside the range of outcomes envisaged and it is not addressed by the present research.

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Appendix One: Classification of schemes

Table A.1: Matrix of Training and Employment Schemes

Type	*Name*	*Aim*	*Eligibility Criteria*	*Benefits*	*Duration*
Subsidised Employment	Community Employment: Part-time Integration	To help the long-term unemployed re-enter the active workforce by breaking their experience of unemployment, by returning them to a work routine and enhancing their personal/technical skills.	>25 yrs if >12 mths in receipt of: UB, UA, OPFP, Widow(er)'s Pensions, Deserted Wife's, Farm Assist. >18 if disabled. Adults dependents of above. >18 if Traveller, UB/UA any duration. >18 Refugees; ex-offenders; drugs task force; resident on offshore islands. Time served on recognised training/employment counts.	Work for average 39 hrs p/fortnight. Secondary benefits retained. Additional off-scheme work allowed.	One year. Extendable to two depending on individual needs.
Subsidised Employment	Community Employment: Part-time Job	As above; but gives extended access to employment to older people people who may have been unable to secure regular employment for some time.	>35 yrs if >3 yrs in receipt of: UB, UA, OPFP, Widow(er)'s Pensions, Deserted Wife's, Farm Assist, Carer's Allowance. >35 yrs if disabled. Adults dependents of above. >18: Traveller (on UB/UA for >1 yr); refugees; resident on offshore islands. >35 yrs ex-offender if >3 yrs in receipt of UB/UA.	As above.	Three years.

continued...

Table A.1: (continued)

Type	*Name*	*Aim*	*Eligibility Criteria*	*Benefits*	*Duration*
Subsidised Employment	Job Initiative	To assist long-term unemployed persons to prepare for work opportunities in the open labour market; by providing participants with work experience, training and development opportunities.	$>$35 yrs if $>$5 yrs in receipt of: UB, UA, OPFP. Time served on other training/employment schemes counts.	Going-rate for work done. Secondary benefits retained	Three years
Subsidised Employment	Back to Work Enterprise Allowance	To encourage people receiving certain social welfare payments to become self-employed.	In receipt of UA/UB for $>$3yrs. Or $>$12 mths on OPFP, disability, farm assist, pre-retirement, carer's, widow(er), deserted wife's, prisoner's wife's. Or $>$3yrs on Disability Benefit. Relative of above. Ex-offender.	Retain a percentage of their social welfare payment for four years. Secondary benefits retained.	Four years
Training	FÁS Apprenticeship	To learn the necessary skills, knowledge and attitudes to become a qualified craftsperson.	$>$16 years with Junior Cert; or $>$25 years with $>$3 yrs work experience.	Apprentice wage + training allowance.	Four years.
Training	FÁS Training (Vocational)		Unemployed. Some require Leaving Cert.		Varies. 11-47 wks.

continued...

Table A.1: (continued)

Type	*Name*	*Aim*	*Eligibility Criteria*	*Benefits*	*Duration*
Training	Youth Reach	Provides opportunities for basic education, personal development, vocational training and work experience.	15-20, early school leaver, no formal qualifications.	Weekly allowance, travel allowance, childcare. Additional payment if $>$12mths on UB/UA or CE.	1-2 yrs.
Training	Vocational Training Opportunities Scheme (VTOS)	To give unemployed people education and training opportunities which will develop their employability.	$>$21 years. $>$6mths UE receipts. Or lone parent, disability allowance or dependent spouse.	Benefits retained. Travel allowance. Childcare support.	30 hours p/wk, for 2 yrs.