

Chapter 4

Labour Market Programmes and Activation Strategies: Evaluating the Impacts

Do active labour market programmes really improve labour market performance? Activation programmes reduce the number of people on benefits directly through the impact of their services on the programme participants, but also indirectly because some benefit recipients prefer to leave unemployment instead of complying with programme requirements. Intensive employment services and training programmes may have relatively favourable impacts on labour force participation and promote earnings progression, although these impacts often appear two or more years after individuals have participated in the programmes. Programmes can have displacement effects when participants find jobs to the detriment of non-participants thus reducing net job gains, but programmes can also have positive labour demand and multiplier effects.

Introduction

The OECD has long advocated active labour market policies (ALMPs) and regularly reaffirms this recommendation, as for example in the 1994 OECD Jobs Study. Welfare reform legislation in the United States in 1996 and the Luxembourg Employment Guidelines adopted by the EU in 1997 were key events defining the vision of active labour market policy based on *activation* principles, when benefit recipients are expected to look actively for work or participate in a programme to promote their job prospects – the so-called “mutual obligations” approach. In the United States, a large fall in caseloads occurred following welfare reform embodying these principles. Within the EU, the actual implementation of activation principles has been variable and so have been the outcomes: unemployment has fallen in some countries but has persisted at high levels in others.

The purpose of this chapter is to examine the impact of activation strategies and other ALMPs on employment outcomes, primarily based on the findings of evaluation studies of a wide range of programmes.¹ Section 1 outlines general considerations about the impact of activation and other programmes. Section 2 provides two initial examples of how microeconomic policies affect labour market outcomes. Section 3 summarizes evidence about the size of the impact of active programmes. Section 4 looks at the nature or quality of impact, in particular examining the possibility that certain policies may achieve long-term increases in earnings in addition to cuts in benefit caseloads.

Main findings

- **The *de facto* cut in benefit entitlements that is implicit in the “stick” element of activation programmes should be set at a moderate level.** Activation programmes sharply increase the total amount of employment services that are delivered to job-seekers. Some individuals respond by dropping their benefit claim rather than comply with participation requirements. But to allow this *sorting* effect to arise, employment services need to ensure that requirements are *moderate*, *i.e.* they should not be equivalent to blanket denial of benefit entitlement. In general, in the absence of effective activation programmes, benefit schemes for the long-term unemployed become unsustainable or excessively costly in the long term.
- **Effective activation strategies can have a significant impact on aggregate unemployment.** Welfare caseloads in the United States and unemployment benefit recipiency rates in Denmark, Ireland, the Netherlands and the United Kingdom have fallen by more than half from earlier peaks. Australia and New Zealand have recently experienced 25% to 30% falls over a shorter period. These improvements seem to be closely related to the introduction of activation programmes. Importantly, better job prospects for clients who receive activation services do not seem to come at the expense of other job-seekers, *i.e.* there is little evidence of net “substitution” or “displacement” effects.
- **Initial successes in reducing unemployment can start a “virtuous circle”.** Falling numbers of benefit recipients will create room for intensified support for those who

remain unemployed and for further policy reforms which intensify the activation strategy.

- **Microeconomic evidence provides information on what works.** *Job-search assistance* or “*work-first*” strategies often have a large impact and their cost is relatively low. *Long-term labour market programmes*, such as training and job creation measures, often have little or negative short-term effect on outcomes. However, *compulsory participation* in long-term programmes may have a “motivation” effect, encouraging people to find work before programme participation starts. *Intensive employment services, individual case management* and *mixed strategies with selective referrals to long-term labour market programmes* tend to have the largest impacts.
- **The time profile and the outcomes variables that are influenced differ between programmes.** “Work-first” programmes have a large employment impact in the short term which fades in later years. By contrast, favourable impacts for participants in SSP Plus in Canada, the Restart programme in the United Kingdom, and some training programmes have appeared only after about two years. “Work-first” programmes in some cases cause a reduction in entry wage rates, and in the longer term cause a long-term reduction in benefit reciprocity with a relatively smaller positive impact on employment rates. “Mixed” strategies and intensive employment services have impacts on employment or total earnings that approximately parallel and sometimes exceed what would be expected, given their impact on benefit caseloads.
- **Impact can vary sharply between apparently similar programmes.** The context and detailed content of programmes can be important determinants of their impact. Moreover, increased spending on certain functions of public employment services (PES) may face declining returns, especially if other (complementary) types of input are not also increased.

1. General ideas about different programmes and their impact

A. The nature of impact from activation programmes

“Activation” programmes differ from free public employment services in that participation is *obligatory* for relevant target groups. Key examples of activation programmes are requirements on unemployed people to attend intensive interviews with employment counsellors, to apply for job vacancies as directed by employment counsellors, to independently search for job vacancies and apply for jobs, to accept offers of suitable work, to participate in the formulation of an individual action plan and to participate in training or job-creation programmes. The main target groups for activation programmes are recipients (or claimants) of income-replacement benefits which are conditional on availability for work. This includes most recipients of unemployment benefits.² Comparable availability-for-work conditions often apply to lone-parent and social assistance benefits. Participation in employment services can also be made obligatory for disability beneficiaries, but the services involved are relatively specific.³

A practical rationale for activation programmes is that they can have a large impact on employment and unemployment outcomes in environments where benefit entitlements are of long or indefinite duration. Two more theoretical considerations are relevant when considering how the impacts achieved in this way can increase social welfare.

Activation programmes promote job search...

First, activation requirements increase levels of participation in employment services, thus making participants' job search more effective and/or enhancing their skills.⁴ As compared to arrangements which only *motivate* job search (such as cutting benefit levels), a *direct* job-search obligation allows the same outcomes to be achieved with a higher level of social protection.⁵ This argument applies across the whole range of measures – interviews, participation in training, etc. – that the unemployed person perceives as having costs, but which also contribute effectively to bringing him or her closer to employment. It is also arguable that some unemployed people have limited prior experience with employment services such as placement, counselling and training and in the absence of regulations may fail to take up services from which they would benefit.

... and ensure that benefits are only provided to the most needy

Second, given the “disutility” involved in complying with activation requirements, some potential claimants do not initiate a benefit claim, or people on benefits enter work or drop their claim earlier than they would otherwise have done. If the government is unable to devise programmes that are directly productive – in the sense that participation increases participants' job-finding chances or employability – activation programmes may be thought of as “workfare”, a job-creation or work-experience programme that pays unemployment benefits or an equivalent wage level but without delivering further services.⁶

“Workfare” can improve social welfare when the need for income varies across individuals in ways that the government is unable to measure directly. But in order to increase social welfare (more than can be achieved simply by varying benefit entitlement parameters), workfare requirements must be only moderately strict – they must not be so strict that they deter all benefit claims (Box 4.1). From an operational point of view, the strictness of workfare requirements (*e.g.* hours of work required in order to qualify for benefit, or an equivalent hourly wage) needs to be specified in the legal entitlement and eligibility conditions for benefits (or equivalent rules need to be implemented administratively).

Evaluation studies that assess whether, or to what extent, workfare requirements result in hardship or destitution can help in setting the strictness of workfare requirements appropriately. However, such evaluation studies will almost by definition be occasional, because if hardship or destitution outcomes could be measured accurately and at low cost for all individuals, the government would use such information to target benefits directly where they are most needed, instead of imposing workfare requirements.

B. Administrative capacity and benefit levels influence the need for activation

If all unemployed people are offered a place on a job-creation programme which must be accepted or benefit is lost, the benefit system becomes equivalent to a programme of public works, probably one which pays somewhat below-market wage rates in order to ensure that the demand for places can be satisfied.⁷

In less-developed countries, where the administration lacks meaningful records of family needs and incomes, public works programmes are indeed frequently the best instrument for delivering poverty relief to needy families while at the same time avoiding payments to individuals or families who have alternative sources of income. By contrast, countries with good administrative capacity have records that can help distinguish

Box 4.1. Activation strategies and workfare

There can be a case for workfare if there is heterogeneity in the benefit caseload. Individual situations probably vary along a continuum, but the general argument applies when there are just two groups:

A. The unemployed who have a relatively high marginal utility of income (probably because they have little alternative source of income) but are unable to find work, i.e. those who are involuntarily unemployed.

B. The unemployed who have a relatively low marginal utility of income (they may have income from assets or other family members, or be engaged in legal domestic production or illegal undeclared work, etc.) and are “voluntarily” unemployed, i.e. they could find work, but for them the difference between the net wage and benefit levels is not large enough to cover the disutility of work.

Workfare requirements *de facto* eliminate the benefit option for group B which is voluntarily unemployed: its members will not enter workfare, since this has the same disutility as market work, but pays less. At the same time, workfare requirements maintain a minimum level of social protection for those who most need it, the individuals in group A. Workfare can increase social welfare through better targeting of benefits (targeting benefits where the marginal utility of income is highest) and increased output in the economy (output by group B members who enter work). These gains need to be balanced against the welfare costs, which are the disutility of workfare participation for group A members and the costs of administering the workfare programme. These costs are both proportional to the size of group A, so a workfare programme will tend to have net benefits if group A is small relative to group B.

A conventional cost-benefit calculation, which considers whether benefit savings and (tax receipts on) the earnings gains generated by a programme exceed its cost, will also evaluate the “workfare” programme favourably if it has a relatively large *motivation effect* (exits from benefit by members of group B) and relatively low *actual participation rate* (by members of group A). So cost-benefit calculations that incorporate programme impacts through motivation effects are useful as a guide to programme selection, even when some programmes have a “workfare” character imposing disutility costs (whose size is not exactly known) on their participants.

However, a programme with harsh workfare requirements might deter all benefit claims, in many cases leading to entry to employment, while at the same time costing nothing to implement because it has no participants. A cost-benefit calculation would evaluate this programme positively because it generates benefit savings and employment gains at no cost to the public purse. But assuming that the original benefit entitlement increased social welfare as compared to absence of benefit, this cost-benefit calculation must be misleading. It does not take into account the fact that very harsh workfare requirements, like the absence of benefit entitlement, leave some people destitute (with near-zero income and a very low level of utility). To ensure that a workfare programme which passes a conventional cost-benefit test will also probably be social-welfare-enhancing, there need to be limits on its harshness so that it achieves sorting between groups A and B, rather than deterrence of all claims.

Most OECD countries’ activation strategies in principle aim not to use workfare – all programmes are intended to have “employment service” functions. However, referrals to activation programmes do empirically speaking have a deterrent effect, causing some individuals to drop their benefit claim, so it is appropriate to consider at a theoretical level that activation strategies deliver a mix of employment services and workfare.

between individuals who risk destitution in the absence of benefit and those who have alternative sources of income (from their own work and assets, or other family members, for example). Their employment services are able (through, for example, intensive contact between claimants and employment counsellors) to directly detect voluntary unemployment. This administrative capacity makes it possible to deliver benefits where they are needed at less cost or more effectively than can be done by only providing places on a public works programme. Countries with good administrative capacities, such as Denmark and Sweden, often have been able to afford relatively high benefit replacement rates. However, with high benefit replacement rates administrative capacity tends to nevertheless become overstretched, so that some voluntary unemployment still arises. Then workfare programmes can again have a role in labour market policy.

C. Activation policies, effective labour supply and job creation: a virtuous circle?

In the long term, labour demand responds to increases in effective labour supply.⁸ Experiencing higher effective labour supply, employers may reduce the wages they offer or they may pay the same wages but enjoy increased productivity – either way the profitability of new hires is increased and this motivates employers to create more vacancies. In the shorter term, these mechanisms may not be fully effective so that programme participants will displace non-participants,⁹ but, if programmes achieve a sustained increase in effective labour supply, their displacement effects can be expected to fade over time.

Although some factors such as displacement are expected to offset the programme impacts that are achieved at the microeconomic level, other factors work in the opposite direction. For instance, there may be a multiplier mechanism: initial successes in reducing the number of benefit recipients create room for intensified support for those who remain unemployed, contributing to a virtuous circle of declining unemployment.¹⁰ There is also some evidence to suggest that “social interaction” effects are important influences on unemployment rates, so that reductions in unemployment among programme participants are likely to be influencing unemployment rates among non-participants in the same direction.¹¹ Overall, it should not be assumed that programme impacts at the macroeconomic level will be smaller than impacts at the local and microeconomic level: instead, this issue should be assessed in the light of detailed microeconomic evaluation evidence and macroeconomic experience.

2. Two examples of the impact of policies on labour market outcomes

The impact of active labour market programmes (ALMPs) on labour market outcomes can be documented in various ways. In some cases there is a clear correlation between the introduction of new activation strategies and changes in aggregate labour market outcomes. Impacts can also be evaluated by comparing labour market outcomes between participants (i.e. individuals who participated in active programmes) and non-participants. And evaluations can be based on comparisons between labour market developments in areas where new measures are implemented (on a “pilot” basis) and developments in other areas. This section sets the scene with two country examples of the impact of policies on labour market outcomes.

A. Activation programmes in 2003 in New Zealand

Countries that have been sometimes cited as labour market success stories – or perhaps even “job miracles” – of the 1990s include Austria, Denmark, Ireland, the

Netherlands, the United Kingdom and (for welfare reform) the United States. The story-line in these cases is that outcomes improved sharply due to a certain number of labour market policy reforms (although different analysts may emphasise different reforms).¹² More recently, Australia and New Zealand have been added to the list of possible success stories.

In 1998, New Zealand integrated benefit administration and employment services into a single agency (Work and Income) and introduced a system of internal performance monitoring, and authorities stepped up research into the impact of ALMPs (see Chapter 5 for further details). Then in 2003, New Zealand implemented a range of activation programmes:

- **WRK4U** (Work for You) seminars for potential new claimants of benefits, which reinforce the message that work is available and should be considered ahead of benefit payments. These seminars were implemented early in 2003 in selected areas of the country. By late 2003, benefit applications had fallen by 10 to 20% in these areas relative to others and the programme was extended to the rest of the country.¹³
- An **increase in staffing**, announced in May 2003, was expected to reduce caseloads from around 220 to 160 unemployed per case manager (OECD, 2004b).
- The **Jobs Jolt** initiative, announced in August 2003, included under the general heading of “clear and strong expectations” a threat to cut benefits for jobless people who move to remote areas with little prospects for paid work (a list of such areas has been established); a requirement on those who lose potential jobs through a positive drugs test to undergo drug and alcohol education; and streamlining and automation of operational systems used to contact and potentially sanction clients who breach work-test obligations. Among other programmes relating to unemployed beneficiaries were contracting of specialists to work on a one-on-one basis with people who have been without work for eight or more years; a programme to give the long-term jobless training linked to industries with labour and skill shortages; employment coaching for skilled and work-ready jobless people; and a requirement on unemployed people aged 55-59 to be active and available for work (*The Jobs Letter*, August 2003; www.jobslatter.org.nz).

Since 2000, the total number of benefit recipients in New Zealand had been falling slowly (by up to 10% a year) but the fall accelerated through 2003 and 2004: by August 2004, the total had fallen more than 30% compared with one year earlier (Chart 4.1). Given the coincidence of timing, it seems reasonable to conclude that much of the latter large fall can be attributed to the activation programmes.

B. Motivation effects of benefits in France

Motivation effects arise when benefit recipients step up job-search efforts (or drop their benefit claims) as the time approaches when benefit levels fall or participation in a programme becomes compulsory. Such effects are well-documented in studies of limited-duration unemployment insurance benefits. Chart 4.2 shows this for individuals in France in years around 1990 who were entitled to 14 months of benefit at a wage-related rate, after which benefits fell to a low (non-wage-related) level. The income reduction after 14 months of unemployment was larger for individuals with high prior earnings. As the chart shows, the pattern of re-employment rates was strongly shaped by benefit entitlements: re-employment rates increased significantly during the three months before benefit exhaustion, especially for benefit recipients with high prior earnings.¹⁴

Insofar as activation requirements are partly equivalent to the elimination of benefit entitlements for individuals who are able to find market work, they will have motivation

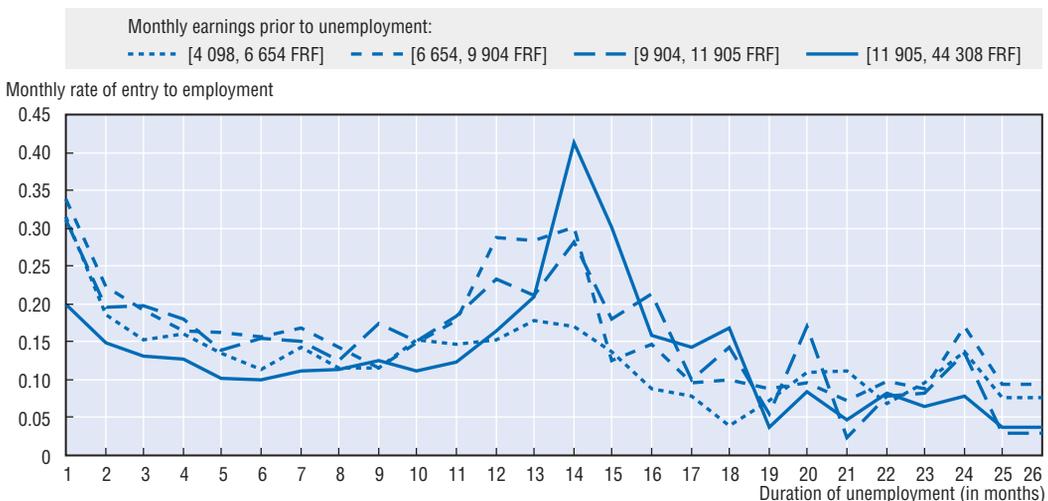
Chart 4.1. **Annual percentage change in the number of jobseekers on unemployment benefit, New Zealand, 1997-2004**



WRK4U: Work for You
WINZ: Work and Income New Zealand

Source: Unemployment Benefit Caseload data supplied by the Department of Labour, Strategy Group, New Zealand.
Statlink: <http://dx.doi.org/10.1787/045162142031>

Chart 4.2. **Monthly rate of entry to employment by duration of unemployment and for four levels of former earnings for people entitled to 14 months of unemployment benefits, France, 1986-1992**



a) The line labelled [4 098, 6 654 FRF] reports statistics for individuals who earned between 4 098 and 6 654 FRF per month prior to entry to unemployment, and similarly for other earnings ranges shown.

Source: Dormont, B., D. Fougère and A. Prieto (2001), "L'effet de l'allocation unique dégressive sur la reprise d'emploi", *Économie et Statistique*, No. 343, pp. 3-28 (www.insee.fr/fr/ppp/collections.htm).

Statlink: <http://dx.doi.org/10.1787/506045587316>

effects similar to those shown in Chart 4.2. However, the motivation effects of programme participation requirements will typically be more diffuse (i.e. not concentrated at a particular duration of unemployment so much as the motivation effects of UI exhaustion) because the timing of programme participation requirements is itself diffuse.¹⁵ This means that the motivation effects are more difficult to identify statistically in this case, but they

probably are still present, and need to be taken into account in order to understand evidence about programme impacts.

3. The size of programme impacts

This section examines evidence about the size of the impact from different types of ALMPs. Impacts on benefit caseloads are often mentioned because these can be compared with movements in caseloads at the macroeconomic level.¹⁶ Findings often appear to be valid across a reasonably wide range of jobseeker characteristics. In particular, impacts on outcomes (such as the increase in employment rates caused by a programme) are often equally favourable for more disadvantaged groups (Box 4.2).

A. Job-search assistance and case management

Job-search assistance and case management can facilitate and promote transitions from welfare to work. They can take several forms.

Initial registration for placement and job-search monitoring are often effective...

As mentioned above, New Zealand's WRK4U information seminars for potential new claimants reduced inflows to benefits in pilot areas by 10% to 20%. Another intervention used in certain states of the United States is a requirement for proof of job search prior to the benefit claim.¹⁷

Box 4.2. How does the impact of ALMPs vary across groups of jobseekers?

It could be argued that ALMPs will be most effective for groups that are easy to place in the labour market. However, impacts are not necessarily greatest for the latter because many of them will rapidly re-enter employment even without assistance. Michalopoulos and Schwartz (2001) conclude that JOBS programmes for welfare recipients in the United States have helped reduce caseloads among disadvantaged groups at least as much as for less-disadvantaged groups. The impacts of these programmes on re-employment earnings have been somewhat less consistent, but job-search assistance has been especially helpful in terms of re-employment earnings for disadvantaged groups. Bloom *et al.* (2003) similarly conclude, from their pooled regression analysis of experimental findings (see Box 4.3 below), that impacts "are not consistently larger or smaller for clients that are likely to be easier or harder to employ". Maré (2002) reports for New Zealand that there is "surprisingly little variation in estimated impacts for different subgroups... Broadly speaking, interventions that are relatively effective for one group of jobseekers are also relatively effective for other jobseekers".

Findings that participation in some training courses reduces employment and earnings in the short run but increases employment and earnings after two or three years have been reported (see below) for German unemployed workers, US welfare recipients and US displaced workers – three groups with quite different labour market characteristics and histories, and different benefit entitlements.

In some cases, however, patterns of impact do differ across labour market groups. Estimated impacts quite often differ between men and women: US surveys of training programmes found the most consistently positive results for adult women (Martin and Grubb, 2001), while the Restart experiment in the United Kingdom (Dolton and O'Niell, 2002) found a large long-term impact of the Restart interview conducted after six months of unemployment for men but not for women (see note 21).

Once on benefits, the frequency of contacts with employers may be important. In the United States, the Maryland Work Unemployment Insurance Work Search Demonstration in 1994 examined the impact of i) dropping the usual requirement for regular reporting of job-search contacts, ii) telling benefit claimants that their reported contacts would be verified with the employer, and iii) increasing the number of required employer contacts from two to four per week. The last two experiments reduced the average duration of benefit spells by about one week, or some 10%, compared with outcomes of the first experiment (Benus *et al.*, 1997, as summarized in OECD, 2000). Australia's Job Seeker Diary, in which job applications are listed, has had a similar impact.¹⁸

In addition to monitoring job search, some countries require participation, at a certain point in a spell on benefits, in job-search training courses of up to several weeks' duration. In Finland, job-search assistance courses lasting at least five days increased employment probabilities by 4 percentage points on average, with relatively greater impact for individuals who have less education (Tuomala, 2000).¹⁹ In Austria, a job-search training programme (training for eight days spread over six weeks) was estimated to reduce the remaining duration of the unemployment spell by about one third (Weber and Hofer, 2004). According to multiple further studies, much of the impact from job-search training courses arises from motivation effects, i.e. some of the individuals who have been referred to a course drop their benefit claim rather than attend the training.²⁰

... as are intensive interviews and individual action plans

Dolton and O'Niell (2002) report findings for compulsory Restart interviews conducted in the United Kingdom in 1989. These interviews, which lasted about 20 minutes, were conducted after six months of unemployment. They reduced the male unemployment rate five years later by 6 percentage points (a 15% to 20% reduction in the actual numbers unemployed), as compared to a control group for whom participation in the first six-monthly interview took place six months later.²¹

Interviews are also central to the process of setting up "individual action plans". A prominent example is Ireland's "Employment Action Plan" process. Around 2000, participants had on average five contacts with their case officer (whereas other EU countries have often used just one or two interviews to set up written agreements with jobseekers), and around a quarter of those who attended interviews were referred to training or education programmes (see: www.fas.ie/FAS_Review/SF.html; OECD, 2003a, Box 4.8). High proportions of participants exited from benefit. In two areas where all those who were already unemployed for six months or more were referred (in other areas, only those who crossed the six-month duration threshold over a six-month period were engaged, but not the stock of long-term unemployed), total unemployment fell, over the next 20 months, by a quarter or more relative to unemployment in surrounding areas (Corcoran, 2002; see also O'Connell, 2002).

France's Personalised Action Plan for a New Start toward Employment (*Service personnalisé pour un nouveau départ vers l'emploi*, SPNDE), introduced in 1999, involved interviews with youth after six months of unemployment and with adults after 12 months of unemployment. About 20% of those interviewed were referred to training or social support, 25% were referred to personalised job-search assistance and the remainder to general job-search assistance programmes (such as a job-search training course), with a second interview to take place two months later. Estimates suggest that this programme had only a modest impact on longer-term exits from unemployment and social assistance (RMI) benefits.^{22, 23}

But job-search support requires adequate staff resources in employment services

The frequency of intensive interviews is constrained by staff caseloads – the ratio of clients to employment counselling staff. Many researchers and practitioners view staff caseloads as a critical constraint on PES performance. Some studies have confirmed this (Box 4.3), although findings have not always been consistent.²⁴

A pooled regression analysis of experimental findings for US welfare recipients (Box 4.3), found that work-first approaches had the largest impact on outcomes as measured over two years. A measure of “job-search efficacy” also played a major role in White’s study (2004) on the New Deal for Young People in the United Kingdom. Nevertheless, overall evaluations of job-search-oriented policies are not always positive.²⁵ And as discussed in the next subsection, in longer-term follow-up studies, the performance of training programmes has tended to catch up with that of work-first approaches.

B. Vocational training and subsidised employment programmes

Simplistic evaluations of training and job-creation programmes may be misleading...

Job-search support may not be enough to escape unemployment when jobseekers do not have the skills needed to find jobs or when their potential productivity is low. And for more-employable workers, training may improve the quality of the jobs obtained.

Participants in training and job-creation programmes have less free time for job search than jobseekers who do not participate. During programme participation, job-entry rates tend to fall – the so-called “lock-in” effect. This is illustrated in Chart 4.3. The employment rates of participants in programmes in Switzerland declined, relative to those of matched non-participants, for the first 80 days after entry to the programme. After another 240 days, employment rates for former participants in vocational and “other” training programmes had just caught up with employment rates of matched non-participants. But even after 400 days, employment rates for former participants in job-creation programmes (EP-PU and EP-PR) had not caught up with those of matched non-participants.²⁶ This pattern of outcomes is commonly found, and it explains why statistical evaluations have often concluded that long-term programmes have little or no positive impact.

The patterns of programme impacts shown in Chart 4.3 for Switzerland are characteristic of evaluation findings from other countries. Impacts differ by type of programme:

- Evaluations of *training* programmes often find a negative or only a small positive impact on participant outcomes for the first one or two years. However, over the past decade a number of long-term follow-up studies have been conducted and it seems that impacts followed over a sufficiently long time period after the training can in some cases become quite strongly positive.²⁷
- Evaluations of *hiring subsidies* (i.e. private sector subsidised employment programmes) frequently find a positive impact of participation on employment even when “employment” is defined to arise only after a transition to unsubsidised employment. In Chart 4.3, this is illustrated by results for the “temporary job” programme.²⁸
- Most evaluations of *public-sector job-creation* programmes find a small or even a negative impact at all times.

In the best of cases, i.e. successful training programmes, participation in long-term ALMPs seems to achieve impacts comparable to those arising from successful strategies that focus on job search, placement and individual attention, but only after some years and

Box 4.3. Pooled regression findings about “what works”

The table below summarizes findings from random-assignment evaluations of different service strategies implemented by 59 employment offices for welfare recipients throughout the United States. Treatment groups on average had two-year total earnings 18% higher than control groups. But treatment groups at offices where the service strategy was one standard deviation above average for “emphasis on quick job entry” and “emphasis on personalized service” had two-year total earnings 42% higher than control groups.

A local unemployment rate one standard deviation above average, or high staff caseloads, could reduce the impact of employment services, but their coefficients were smaller than those of the main service strategy variables. Differences in client characteristics also had relatively little influence on the size of impacts – they were estimated to explain about 16% of the variation in programme impacts across offices, but when implementation-related factors were added, the variance explained jumped to 80%.

How the impact of employment services on total earnings per participant over a two-year period was influenced by programme implementation, rates of participation in programme activities, and the economic environment in the local area^a

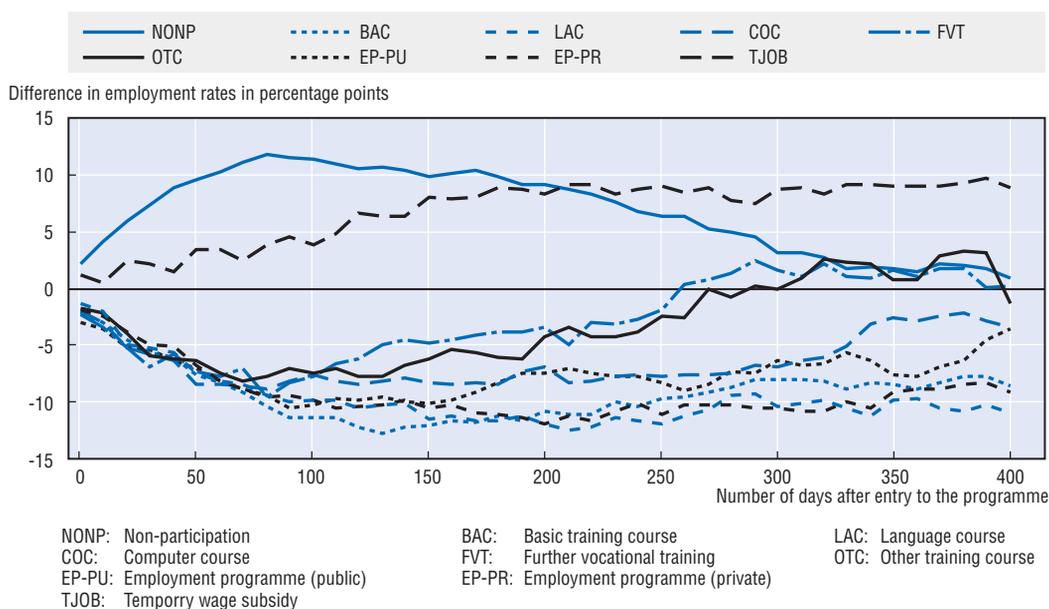
Programme characteristic	Regression coefficient ^b (USD)	Partially standardized regression coefficient ^c (USD)	Standard error (USD)
<i>Programme implementation</i>			
Emphasis on quick job entry	720***	720***	134
Emphasis on personalized service	428***	428***	107
Closeness of monitoring	-197	-197	121
Staff caseload size	-4***	-268***	1
Staff disagreement	124	124	83
Staff/supervisor disagreement	-159*	-159*	96
<i>Programme activities</i>			
Basic education	-16**	-208**	6
Job search assistance	1	12	9
Vocational training	7	71	11
<i>Economic environment</i>			
Unemployment rate	-94***	-291***	30

***, **, * statistically significant at 1%, 5% and 10% level respectively.

- Regression based on 69 399 individuals who were in either a treatment group or a control group created by random assignment at one of 59 offices. “Programme implementation” and “Programme activities” variables are measured at the office level. “Programme implementation” variables are based on a questionnaire addressed to staff at each office. “Programme activities” variables refer to the difference between the treatment group and control group percentage rate of participation in the activity. The content of activities was not standardized across offices. The coefficients shown were estimated simultaneously with about 20 further coefficients which controlled for individual characteristics (see source for details).
- Regression coefficients are reported in 1996 dollars per unit change in each independent variable. The grand mean impact (i.e. the estimated impact averaging across all individuals, irrespective of individual characteristics or site, for individuals who were in one of the treatment groups), was USD 879, or 18 per cent of the counterfactual.
- Partially standardized regression coefficients are reported in 1996 dollars per standard deviation change in each independent variable.

Source: Bloom, H., C. Hill and J. Riccio (2003), “Linking Program Implementation and Effectiveness: Lessons from a Pooled Sample of Welfare-to-Work Experiments”, *Journal of Policy Analysis and Management*, Vol. 22, No. 4, pp. 551-575 (www.mdrc.org/announcement_hp_40.html).

Chart 4.3. **Composite effects of participation by unemployed people in ALMPs on their relative employment rates, by number of days after entry to the programme, Switzerland, 1998 and 1999^a**



a) Results based on matching participants in each programme with comparable participants in other programmes and non-participants (who are attributed a hypothetical programme starting date from the sample distribution of actual programme starting dates). The impacts relate to programme starts and outcomes in 1998 and 1999, but the data set also includes 10 year individual labour market histories which were used for matching.

Source: Gerfin, M. and M. Lechner (2002), "A Microeconomic Evaluation of the Active Labour Market Policy in Switzerland", *Economic Journal*, Vol. 112, No. 482, pp. 854-893, and Internet Appendix (www.siaaw.unisg.ch/lechner/gl_ej).

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at greater cost. But as Section 4 discusses, when earnings as well as employment and unemployment outcomes are considered, the case for training and education programmes might be strengthened further.

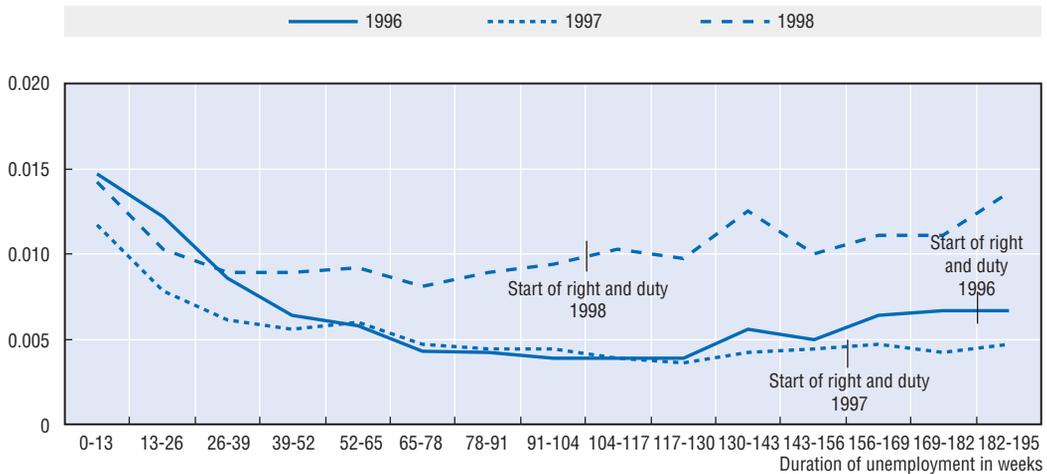
... and motivation effects (when jobseekers find jobs before getting into programmes) can be strong

In some countries and in some circumstances, referrals to long-term training and job-creation programmes can become compulsory. In contrast to the usual situation for job-search training courses, a final referral is not usually a "surprise" to the individual since in employment counselling the possibility of referral to a longer-term programme would often be discussed in advance, and general rules prescribing participation at a particular duration threshold are widely known.

In Denmark, the obligation to participate in labour market programmes applied to all unemployed people after four years of unemployment starting in 1994, after three years of unemployment starting July 1996 and after two years of unemployment starting in January 1998. As Chart 4.4 shows, monthly rates of entry to employment tend to stop falling and then begin to rise about six months before participation in programmes is scheduled to become compulsory.²⁹ Geerdsen (2003) similarly shows that the impact of compulsory referrals to programmes arises well before the time when the participation obligation becomes finally effective.³⁰

Chart 4.4. Changes in patterns of transition from benefit to employment as rules concerning referrals to labour market programmes were changed in Denmark, 1996 to 1998

Weekly rates of transition to employment or ordinary education by duration of unemployment, with statistical adjustment for unobserved heterogeneity^a



a) With correction only for observed heterogeneity, transition rates for a standard person (30 to 49 years old) at long benefit durations were about half those shown here (i.e. about 0.005 per week in 1998).

Source: AM (Danish Ministry of Labour) (2000), *Effects of Danish Employability Enhancement Programmes*, Copenhagen (www.bm.dk/english/ – documents – order publications).

Statlink: <http://dx.doi.org/10.1787/026017646200>

Given that there are more unemployed people at short durations of unemployment than at long durations, an obligation to participate in a programme late in the unemployment spell can have an impact that is out of proportion to its actual participant numbers. Activation strategies in Australia and the United Kingdom have made participation in employment programmes (Work for the Dole in Australia; employment options in New Deal for 25 Plus in the United Kingdom) compulsory in principle for the long-term unemployed who engage in no other activities, but actual participation rates in the programmes remain quite low. This means that these programmes are quite cheap, yet may still be having a large impact. Activation strategies in Nordic countries involve higher rates of participation in long-term labour market programmes, making them much more expensive and perhaps also increasing “lock-in” effects.

It is important to keep motivation effects in mind, even when it is difficult to estimate their size precisely. Equation specifications which do not model motivation effects correctly when they are present are mis-specified, and are liable to give biased estimates of the impact of programmes on their participants (Box 4.4).

More generally, there is some evidence that benefit caseloads respond to “news” about a change in the labour market policy regime, even before the individuals concerned have directly experienced any change. For example, in Ireland in 1996 the beneficiary total started reacting to extensive media coverage of a statistical survey that had suggested a high incidence of fraudulent benefit claims, apparently before concrete anti-fraud measures were implemented (OECD, 1998, p. 147); Carling *et al.* (2001) emphasise that benefit cuts in Sweden in 1996 affected behaviour several months before they were actually implemented. As described by Mead (2004), a significant part of the impact from US welfare

Box 4.4. **Biases in estimating programme impact when motivation effects are ignored**

In contexts where motivation effects operate (i.e. where hazard rates to employment are affected by *future* obligations to participate in a programme), conventional methods for estimating the impact of programmes on their *actual* participants may give biased results.

Conventional methods compare the hazard rate of participants – during programme participation and in the months or years following programme participation – with the hazard rate of (otherwise comparable) non-participants, who constitute a “control” group. But in a policy environment like Denmark’s, hazard rates for this control group are increased by the ongoing “threat” of referral to a programme: hazard rates for participants are less affected because they are in any case low (the “lock-in” effect). After programme participation has ended, the “threat” effect may still be greater for control groups than it is for (ex) participants, because unemployed people who have not previously participated in a programme will tend to be prioritised for future participation in a programme. In other words, ex-programme participants can *de facto* receive benefits on a relatively passive basis in the first few months after their participation has ended and this might lead to an overly-negative impression about the impact of the programme in which they have recently participated.

Random-assignment experiments could give biased estimates for similar reasons, especially if they are not implemented keeping the “control” group carefully separate from the services delivered to the “treatment” group. Owing to limited knowledge or limited rationality, control group members may expect to be referred to the same type of services as the treatment group: indeed, programmes which are successful at the experimental stage quite often are in fact applied to control group members, in the longer term. Through motivation effects, the “treatment” in a random-assignment experiment may (partly) affect behaviour also for the control group. As Bloom and Michalopoulos (2001) note, “a random assignment study may underestimate the impact of a reform that generates effects by changing community-wide views about welfare because it is impossible to insulate the control group from such changes”.

In most places, this chapter refers in the conventional way to differences between outcomes for experimental treatment groups and control groups as “impacts”. One justification for this is that (because motivation effects usually influence the control group in the same direction as the treatment group), impacts reported this way are probably moderately well correlated with the (unknown) true impacts. Nevertheless, the likely biases should always be kept in mind.

reform on caseloads arose just from intensive national or state-level debate and press reports that delivered a new message, even before the new policies were enacted.

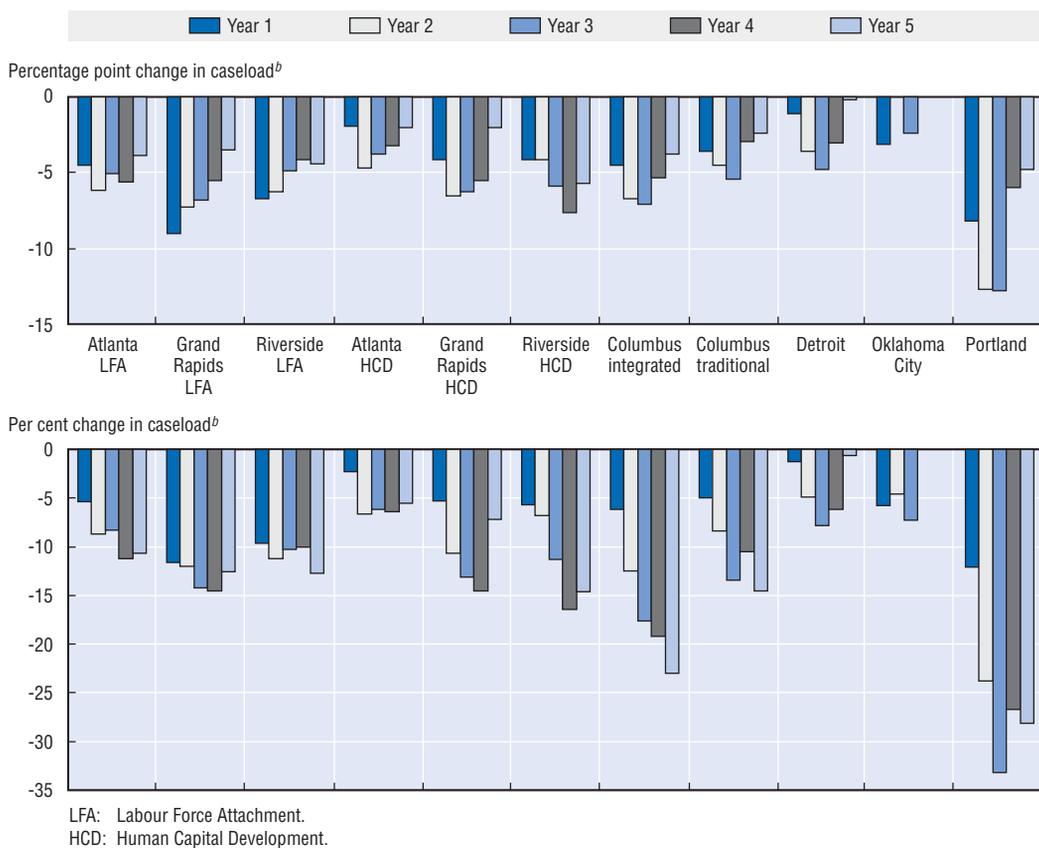
C. Strategies combining job search and programme participation as a package hold promise

In practice, employment services often offer a combination of job-search support and participation in training or other long-term ALMPs. Such “mixed” strategies allow case managers to present training or similar options to clients along with possible opportunities for regular employment, and to select the most effective instruments for each client. These strategies may be particularly effective. Strategies as a whole are not often evaluated, perhaps because most countries only operate one main strategy and there is no counterfactual. But when “mixed” strategies are evaluated, their reported impacts will

incorporate the motivation effects from the long-term ALMP component, insofar as these effects arise within the period of experimental participation in the strategy.

In the US National Evaluation of Welfare to Work Strategies (NEWWS) in the mid-1990s, some of the “strategies” were in fact limited to using just one main instrument (training), but the most successful strategy was the “mixed” strategy in Portland (Chart 4.5). A key to the success of this programme was the close collaboration between welfare officers and staff from employment services and an emphasis on employment as the goal, although more disadvantaged clients could be enrolled in education or training activities. Staff “emphasised ways to solve problems related to nonparticipation” but “did, however, ultimately sanction noncompliant individuals”. Also “Portland also employed full-time job developers

Chart 4.5. **Absolute and relative impacts on welfare caseloads by year after random assignment to employment services, United States, mid-1990s^a**



- a) Titles refer to the location of different programmes within the National Evaluation of Welfare to Work Strategies (NEWWS). “Labor Force Attachment” programmes emphasised quick entry to employment and “Human Capital Development” programmes required participation in education or training. Other than the last three, programmes were rated as using high or very high sanctioning. (See the source for more details of programme characteristics.) Random assignment to a treatment group or a control group took place from 1991 to 1994.
- b) Percentage point change in caseload is the percentage of the treatment group still receiving welfare, less the percentage of the control group still receiving welfare. Per cent change in caseload is treatment group caseload still receiving welfare as a percentage of the control group caseload still receiving welfare (each relative to sample size, and less 100%). Data refer to the last quarter of each year.

Source: Hamilton, G., S. Freedman, L. Gennetian, C. Michalopoulos, J. Walter, D. Adams-Ciardullo, A. Gassman-Pines, S. McGroder, M. Zaslow, S. Ahluwalia, and J. Brooks, with E. Small and B. Ricchetti (2001), *How Effective Are Different Welfare-to-Work Approaches? Five-Year Adult and Child Impacts for Eleven Programs*, Manpower Demonstration Research Corporation (<http://aspe.hhs.gov/hsp/NEWWS/>).

Statlink: <http://dx.doi.org/10.1787/000054804122>

to work with participants once they began actively looking for a job though, unlike other developers in work-focused programs in this evaluation, they encouraged participants to seek “good’ jobs, that is, higher-paying jobs with benefits” (Hamilton et al., 2001).

These findings support the view that benefit administration and placement services should be closely co-ordinated (though not necessarily integrated) and that case management should be relatively intensive and use mixed strategies.

D. Impacts on flows into unemployment

Impact estimates such as those from the NEWWS only include motivation effects that reduce the average *duration* of spells for the existing caseload. However, in a full-scale implementation such strategies will plausibly also have motivation effects on initial *entries* to the caseload – motivating some people to retain an existing job or find a new one without applying for benefit, or just not apply for benefit. Grogger et al. (2003) track monthly rates of entry to welfare and exit from welfare through to 2001 to show that in California declining entries were more important, as a proximate cause of falls in the welfare caseload, than rising exits. Acs et al. (2003) conclude that “changes in welfare policy, expansions of the EITC, and attendant shifts in attitudes toward work and welfare likely play an important role in these trends”. This US experience suggests that the total impact of activation measures on caseloads could have been about twice the impact measured in programme evaluations – given that the latter at most measure programme impacts on the duration of spells that have already started, not impacts on entries.

“Work-first” strategies sometimes cause people to take jobs of lower quality (see below), but there is little evidence that they reduce re-employment spell durations. This (negative) finding is reported by Black et al. (2003) in relation to job-search training in Kansas and by UK studies of at least four different programmes – the Restart programme, Jobseeker’s Allowance, and New Deals for Young People and for the Long-term Unemployed.³¹ Employment-focused programmes within the NEWWS in most cases *increased* the proportion of all job entries for which the first employment spell lasted four or more quarters.³² This finding is hard to explain except in terms of motivation effects.³³ From a sociological point of view: “... the activation test and associated workfare programs are not only disciplining welfare recipients, they also serve the purpose of deterring dissatisfied workers from leaving their jobs” (Marston et al., 2004, citing Bedder, 2000). So although evidence from other countries is quite limited, it is possible that motivation effects which reduce unemployment durations are matched by similar-sized motivation effects that reduce entries to unemployment, as in the United States.

E. Assessing overall impacts of activation strategies

Impacts at the microeconomic level can account for large changes in aggregate caseloads

Cyclical factors influence unemployment rates independently of the impact of activation strategies. Therefore, when looking at aggregate unemployment outcomes, the focus should be on how benefit reciprocity rates have changed on a cyclically-adjusted basis, for example comparing years around 2000 with years around 1990 (two peaks of the cycle), or years around 2003 with years around 1993 (two troughs of the cycle).³⁴ In the United States, some observers in the late 1990s feared that the decline in welfare rolls would be reversed in the next recession: several econometric studies using aggregate data had estimated that much of the caseload decline was due to the strong economy.³⁵

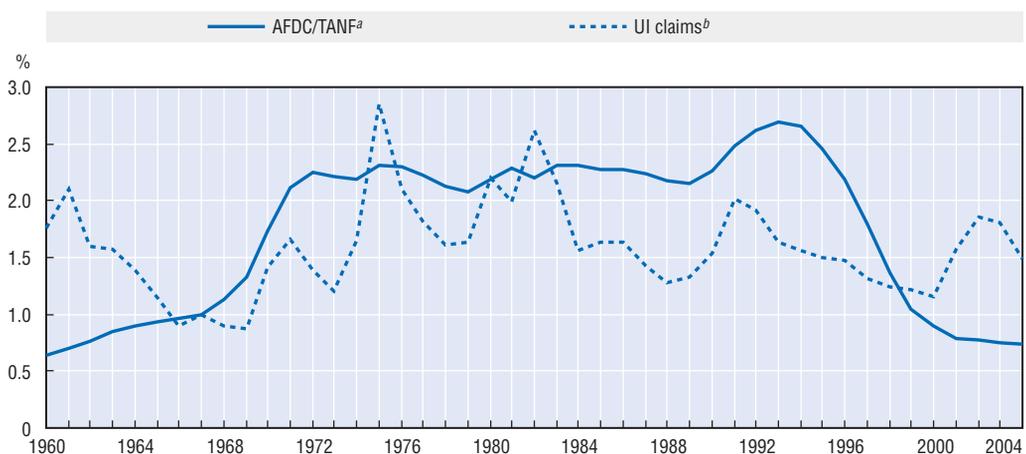
However, at the depth of the most recent recession the adult caseload on welfare remained about three times lower than it was in 1994 (Chart 4.6). Cyclical factors may have had a temporary impact accelerating the declines in the late 1990s and slowing the declines in the early 2000s, but structural factors have played a key role.³⁶ These include welfare reform, “making work pay” programmes (particularly the Earned Income Tax Credit – see Chapter 3 of this publication), and restrictions on benefit entitlements (time limits, and in some cases “diversion” strategies which might be interpreted as *de facto* restrictions on entitlement³⁷). Of these, welfare reform may have been the most important factor: for example, workfare-type programmes (in Wisconsin) were able to reduce caseloads by 90% but there are probably no examples of financial incentives having such a large impact.³⁸ In this interpretation, the 1993-2003 fall in caseloads can be thought of as the product of three falls of 30% ($0.7 \times 0.7 \times 0.7 = 0.34$): the microeconomic impact of activation programmes on caseload durations;³⁹ the microeconomic impact of the same programmes on entries which was approximately equal in size; and the microeconomic impact (no doubt affecting both durations and entries) of financial and entitlement variables.

In the United Kingdom, unemployment benefit caseloads by the early 2000s were down by about two-thirds from peak levels. Microeconomic evaluations suggested a long-term impact on male benefit recipiency rates of 15% from the first Restart interview (conducted after six months of unemployment, from 1986 onwards: see above) and probably at least as much from Jobseeker’s Allowance (JSA) legislation (1996).⁴⁰ Several other initiatives plausibly had similar effects.⁴¹

In Australia, DEWR (2003) reported that commencement in Intensive Assistance (the standard programme for disadvantaged or long-term unemployed) had a net impact on the

Chart 4.6. Adults on welfare and persons on unemployment insurance, United States, 1960-2004

Percentage of population aged 15-64



a) Calendar-year number of average adult AFDC/TANF recipients. Data include recipients in Separate State Programs (SSPs) from 2000 onwards.

b) 52-week average of weekly continued UI claims.

Source: For AFDC/TANF: 1960 and 1961 extrapolated using AFDC caseload data in *Social Security Statistics*; 1962-1969 estimated from <http://aspe.hhs.gov/hsp/indicators04/appa-tanf.htm>, Table 1 (data for financial years); 1970-2002, *ibid.*, Table 2 (data for calendar years); 2003 and 2004, extrapolated based on www.ncsl.org/statefed/welfare/caseloadwatch.htm. For UI weekly claims: before 1967, estimated from Bassi and McMurrer (1997), figures for IU/TU: from 1967 onwards: www.workforcesecurity.doleta.gov/unemploy/claims.asp.

Statlink: <http://dx.doi.org/10.1787/827177141747>

employment rates of participants (12 or 16 months later) of only one percentage point in the case of those who commenced in April 2000. However, this increased to about 6 percentage points for those who commenced in mid-2001 (with 32% of participants entering employment by November 2002, compared with 26% in the comparison group). The impact of the other main Job Network service, Job Search Training for shorter-term unemployed, also increased. The total number of three-month employment outcomes achieved by Job Network clients nearly doubled between 2002 and 2004,⁴² so it seems likely that net impact of Job Network services continued to increase as the performance management framework took hold (see Chapter 5). Total benefit recipient numbers in Australia have fallen by nearly a quarter over the three years to end 2004. Again, it seems that the increasing impact of labour market programmes at the microeconomic level may be able to account for much of the improvement in aggregate outcomes.

At a given point in time, both cyclical and labour market factors affect outcomes. However, structural factors appear to be most important in determining the volume of long-term unemployment or dependency on social assistance benefits.⁴³ Cyclical factors (or the collapse of an asset bubble, for example) have a stronger influence on fluctuations in short-term unemployment.

But there are cases where activation strategies fail

Some researchers have concluded that programmes generally have little impact. In Australia, Douglas (2002) states: “Be realistic – even the most effective interventions do not have a big impact” and Kinnear *et al.* (2003) state: “Evaluations have found that after several years both program and control groups have similar numbers in employment, similar incomes, and similar numbers on welfare, suggesting that job-search programs may not have (measurable) long-term advantages.” Even some relatively detailed literature reviews (for example de Koning, 2001) have arrived at similarly negative conclusions.

Such negative findings are not necessarily justified. NEWWS evaluations found that percentage impacts on caseloads (the variable that determines a programme’s impact on the aggregate caseloads, assuming that rates of entry to benefit are unaffected) do not fade after several years (Chart 4.5). Historically, US evaluations of large-scale social programmes in the 1960s found disappointingly small impacts, but in the 1980s and the 1990s larger impacts were measured experimentally, and then the impact of actual implementation tended to exceed what had been measured. One explanation for such apparent contrasts is that large impacts arise because refined versions of the most successful programmes are implemented, producing an impact greater than the average impact of programmes that were implemented historically. Nevertheless, in some cases evaluations find that activation programmes have almost no impact. This subsection considers – somewhat speculatively – why apparently-similar programmes might be quite successful in some cases, and yet fail in others.

Failure may reflect unbalanced development of public employment services...

According to one model of PES offices, PES output can be modelled as a “production function” with multiple inputs. This implies declining returns as any single input is increased.⁴⁴ In some cases (where inputs are complements), an increase in the level of a second input can revive the marginal productivity of a first input. For example, the enforcement of availability for work is complementary with placement services. Without enforcement, the offer of services after a while satisfies demand, and further increases in

the service offer have little additional impact. Conversely, attempts at enforcement which lack a basis in placement services will have limited effectiveness. In line with this analysis, relatively large impacts from early activation measures have been reported where there was previously no effective enforcement of benefit conditionality (e.g. the United Kingdom before 1986 and Ireland before 1996).

The impact of a new programme may increase at first as the administration gains experience and optimises details of implementation, but impact may later decline:

- The first jobseekers referred may have the impression that they have been specially picked out, creating motivation effect (perhaps a “threat” effect or “encouragement” effect) which fades as the programme becomes regular.
- Participants may increasingly be repeat or serial attenders, i.e. individuals for whom this type of intervention has already been unsuccessful.
- When a programme has been implemented for a while, it may need to become more loosely targeted in order to maintain its participant numbers.
- Other partly similar programmes (substitutes) may be introduced.

One solution when declining returns are encountered is to move onto more intensive programmes. In the 1990s, the United Kingdom dropped some light and voluntary programmes (e.g. Job Clubs) which had previously been successful. This might be related to the increased job-search content of the regular regime (such as fortnightly “active signing” interviews, introduced in 1996) which was by then implemented for all jobseekers. By the end of the decade, more intensive programmes (New Deals) had become the new cutting edge of the labour market policy strategy.

If the production function model of PES output is relevant, no recommendation in favour of increasing one particular input can be stable. If a broad consensus emerges in favour of a particular input x , input x will be expanded, its effectiveness will decline, and experience will start to show that another policy y has higher marginal productivity. Since it is hardly possible to empirically model the full PES production function in detail once and for all (its characteristics will vary with locality and client group, for example), PES governance through impact evaluation findings will face problems of instability and limited transferability of the lessons learned. This suggests that, although attempts at modelling the production function more completely should continue, they need to be complemented with continuous and localised evaluation. One solution is a quasi-market (see below) where jobseeker outcomes are systematically used to evaluate providers and poor-performing providers are systematically driven out of the market.⁴⁵

... PES governance problems,...

A labour market programme may have little impact because it is not effectively implemented. This might be the case for intensive case management if there is no obligation to attend, or for an individual action plan if the supply of training or job creation places is too low or not earmarked for action plan participants.

... weakness of motivation effects, or...

In recent years microeconomic studies of the influence of unemployment benefit levels and UI durations have become available from an increasing number of countries. Some recent estimates suggest an elasticity of benefit durations with respect to the level of benefits even above the top of the 0.2 to 0.9 range identified from the literature by

Layard *et al.* (1991).⁴⁶ This may be because the recent studies use actual policy changes or “natural experiments” as the source of exogenous variation in benefit entitlement conditions,⁴⁷ and because some of them are from countries with a relatively high replacement rate (an increase in replacement rate from 0.8 to 1.0 might increase unemployment proportionately more than an increase from 0.08 to 0.10).

High benefit elasticities are evidence that, despite administrative monitoring of availability and job search, jobseekers still have room for manoeuvre in deciding how actively and effectively they seek work. This may mean that, where replacement rates are high, activation programmes need to be relatively intensive before they have a major impact.

... *barriers to job creation*

Countries with overly strict employment protection tend to have lower employment rates, but there is no clear direct link with unemployment rates (OECD, 2004a, Chart 2.5). Comparing outcomes through time, Denmark, Ireland and the United Kingdom, three countries with low or moderately-strict employment protection legislation, have low unemployment rates now, but they had high unemployment rates in the 1980s: “flexicurity” is only a successful formula when its social protection is associated with an effective active labour market policy. Conversely, some countries with relatively strict employment protection have been able to keep unemployment low for long periods with active programmes.⁴⁸

Once unemployment is high, certain labour market regulations probably slow down the job-entry and job-creation processes, slowing the impact of activation programmes and reducing the apparent reward/effort ratio. Although employment will eventually adjust to an increase in effective labour supply, some labour market rigidities can make it more difficult for labour supply to become “effective”. For example, if labour market institutions discriminate against part-time work, people seeking part-time work may not be part of the effective labour supply.

4. Long-term impacts on employment and earnings

So far, this chapter has focused on the impact of activation strategies on benefit dependency and employment. However, these policies can also help improve the match between jobseekers and available jobs, while also possibly improving career prospects – and thus the productivity and earnings of placed jobseekers. This would promote job stability, reducing the risk that, once in work, “activated” individuals go back to unemployment benefits or leave the labour market.

Strategies should try to ensure that “activated” individuals do not quickly lose their jobs or drop out of the labour market...

Measures that achieve transitions from benefits to employment do not necessarily indicate a positive longer-term impact on employment rates. Van Ours and Vodopivec (2004) estimate (for a reference case, *i.e.* 30-year-old males in Slovenia) that a shortening of the maximum duration of unemployment benefit from 12 months to six months increased the employment rate at six months by 9 percentage points (from 49% to 58%), as well as increasing the not-in-the-labour-force rate by 9 percentage points. However, as little as six months later the impact on the employment rate had become much smaller (3 percentage points) and for female beneficiaries it was negative (–4 percentage points).

In the United States, NEWWS evaluations found short-term impacts of various strategies on employment rates that average about 60% to 70% of impacts on welfare reciprocity rates. In leaver studies (interviews of mothers shortly after they have left welfare), about the same proportion of leavers report that they are in employment. In aggregate statistics, the increase between 1993 and 2001 in the number of single mothers employed was slightly over half the decrease in the number of single mothers receiving welfare. Overall, several types of evidence for the United States point to the conclusion that the proportion of welfare leavers who are employed is about 60% (OECD, 2003a).⁴⁹

Microeconomic impact estimates suggest that substantive employment services often have a large positive impact on employment as compared to their impact on benefit caseloads – i.e. they help keep individuals in the labour market – and increase earnings:

- US impact evaluations often do not report employment directly (in terms of the weekly or monthly average employment rate) or average earnings (per hour or per week worked).⁵⁰ However, all NEWWS sites were still showing some reduction (albeit often not statistically significant, taken individually) in welfare reciprocity rates among participants in the fifth year after entry to the programme, but only the most successful sites (Riverside, Portland and to a lesser extent Columbus) still had a clearly positive impact on “per cent employed in a year” and “total earnings in a year”.⁵¹
- UK evaluations show a similar pattern. Lissenburgh (2001), evaluating the impact of the New Deal for the Long-term Unemployed Pilots found “evidence of a large Gateway effect during the first quarter of the evaluation period, with Gateway only entrants [i.e. those who left benefits before entering any intensive activity] leaving JSA [i.e. unemployment benefit] to an extent that is 31.5 percentage points greater than their comparators. About half of this effect is due to exits for unsubsidised employment, but exits to economic inactivity and unknown destinations account for almost all of the other half”. By contrast, participation in intensive activities had a positive impact on exits to employment and no impact on exits to inactivity.
- In Canada, a critical difference emerged in the long term between regular SSP, which provided an income supplement for three years to welfare recipients who entered full-time work, and SSP Plus which provided the same bonus but also offered intensive support and individual counselling (including advice – e.g. about child-care and transportation – after participants found jobs). In the months shortly before and after the termination of the three-year income supplement offered by regular SSP its impact on the rate of full-time employment declined rapidly, becoming zero in the fifth year after random assignment. By contrast, the impact of SSP Plus on full-time employment stayed high at about 6 percentage points through to the last period of data collection (year 5, quarter 2). As compared with regular SSP, SSP Plus reduced welfare reciprocity rates in year 5 by 8.8 percentage points (42.9% vs. 51.7%), and increased employment rates (first two quarters) by 5.8 percentage points (52.3% vs. 46.5%), but increased earnings by 22% (CAD 7 037 vs. CAD 5 777) (Michalopoulos et al., 2002).⁵²

There is also some evidence that participation in training, after an initial “lock-in” period, can leave rates of benefit receipt unchanged yet have a positive impact on employment rates. In Germany, Lechner et al. (2004) report that in Germany a “very intensive full-time programme with a duration of typically two years, called retraining, which qualifies for a different profession than the one currently held” has a positive impact on employment rates of its participants (as compared with other training programmes as

well as compared with non-participation) starting about three years after programme commencement. In total, over the seven years after programme commencement, the cumulated duration of benefit receipt is increased by about 10 months but the cumulated duration of employment also rises slightly. Jacobson *et al.* (2004) report similar findings for displaced workers who participated in vocational training at community colleges in Washington State. They observe that positive impacts on quarterly total earnings, following participation in the equivalent of two or three quarters of full-time training, typically first appear two or three quarters after training has ended. Positive impacts were only found for training in quantitative and technical subjects, so the evidence suggests that certain kinds of training have a positive long-run impact, and probably not that all training has a positive impact.

... and to promote wage progression

The long-term unemployed and disadvantaged programme participants usually enter work at relatively low wages. Earlier entry to work and greater employment stability probably result in some increase in wages, but for those who stay in the same job, progress is usually slow (about 2% per year, for former US welfare recipients⁵³). Experimental programmes which offer in-work support to promote greater employment stability and faster wage progression are now underway in the United Kingdom and the United States (see www.mdrc.org – barriers to employment). However, the earlier Post-Employment Services Demonstration (PESD) in four US states found little impact⁵⁴ (see 1998 summaries at <http://peerta.acf.hhs.gov/taevents/chron.htm>). Poppe *et al.* (2003) provide a recent review with a “welfare-to-work” perspective on traditional programmes such as career planning and training:

“Workforce intermediaries often make two critical mistakes. The first is encouraging people to take “any job” quickly, rather than think strategically about how a particular job placement can help individuals move along a career path. The second common mistake is providing technical training that does not carry credit or articulate towards a degree or certificate... it is important that what [people] begin during preemployment services can be continued after placement and that it all build towards a recognized credential, such as a degree or certificate...”

Challenges facing plans to use training for wage progression are that many people find that participation in training is incompatible with full-time work, and that education and training tend to have less impact than “work-first” programmes for individuals who have an initially low level of educational attainment (a finding noted by, for example, Bloom and Michalopoulos, 2001). After a return to work it is hard to maintain high levels of participation in case-management services, since people are busy with other things and there is no direct legal obligation or benefit incentive for them to participate. So there is also a case for relatively traditional PES approaches which focus on delivering high-quality employment services during the spell of compensated unemployment, even if this sometimes prolongs the initial spell out of work. To allow a fair comparative assessment of different strategies, it is clearly essential to track participants’ employment and earnings outcomes for a number of years.

Conclusions

Individual labour market programmes quite frequently have evaluated impacts of about 15% on benefit caseloads. In one sense the record is held by Portland’s welfare-to-work programme in the United States, where the experimental treatment group had total

numbers remaining on benefit about 30% lower than the control group from the third year onwards. Large impacts only exceptionally arise directly from participation in long-term programmes (e.g. training or job-creation), but they can arise from packages that combine employment service interventions with potential compulsory referral to longer-term programmes for certain subgroups or for all who continue to be unemployed for long periods (e.g. the Danish “active period of benefits” and the UK New Deals).

In an historical perspective, the most effective labour market policy reforms have involved a succession of policy changes, perhaps several which have an evaluated caseload impact of 15% and one or two more whose impact is not known directly (e.g. change in legislation or sanctions regime, restructuring of PES operations) but are considered to be of similar importance. Taking the latter as well as the former into account, some well-known activation strategies might have been expected to reduce caseloads by a factor of two or three which is approximately what actually happened. This suggests that successive policy changes can have a cumulative effect and that there are not important “hidden” offsets (due to displacement effects, or the fact that ALMPs do not directly increase aggregate demand) to the “visible” microeconomic impact of programmes. Alternatively, factors such as displacement may partly offset the impacts that arise at microeconomic level while these factors themselves are counterbalanced by the multiplier effects mentioned in Section 1.C above. Either way, a general conclusion is that aggregate impacts can be large if programmes with a large microeconomic impact are devised, their good performance is identified and their implementation is generalized, which are the general principles of performance management analysed further in Chapter 5.

Although the motivation effects from activation strategies tend to be important for reducing aggregate caseloads, the microeconomic evidence suggests that this is not the only channel of impact from ALMPs. Examples such as New Zealand’s WRK4U and Canada’s SSP Plus programmes suggest that some voluntary programmes delivering counselling, assistance and job-search training can have a large impact. Some long-term vocational training programmes deliver net benefits when outcomes are followed up for two or more years. In the case of obligatory programmes, alongside the evidence of motivation effects arising from the obligation to participate, there is evidence of perhaps smaller but longer-term impacts attributable to enhancements of job-search skills or employability delivered by programme participation. A general conclusion is that the performance management framework for active labour market programmes needs to set up a kind of “level playing field” – one which values activation strategies and programmes that are not of an activation nature, and will examine benefit caseload reductions as well as increases in employment and earnings. Again, these principles are analysed further in Chapter 5.

Notes

1. This chapter continues a history of OECD reviews of evaluations of what works, for whom and why, in active labour market programmes (OECD, 1991; Fay, 1996; Martin and Grubb, 2001) and more general reviews of active labour market policies and the public employment service (OECD, 2001a and a series of country reviews, most recently OECD, 2001b).
2. Unemployment benefits are sometimes paid to older workers without an availability-for-work requirement, although several OECD countries have recently abolished or begun abolishing these exemptions – see OECD country reviews on older workers entitled *Ageing and Employment Policies* for further details. Exemptions may also apply for workers on temporary lay-off, and during short-term sickness.

3. OECD (2003a, Chapter 4) examined relationships between caseload trends for unemployment benefits and other working-age income-replacement benefits. In general terms, measures that reduce unemployment benefit caseloads tend to result in some individuals transferring to other (inactive) benefits, but also success in reducing unemployment makes it politically easier and operationally more useful to restrict access to early retirement benefits, disability benefits or other benefits, for individuals who still have significant work capacity.
4. PES counsellors need to judge accurately which kind of activity will be most effective for each client, which may involve offering the client a choice of activities while at the same time telling him or her that inactivity is not an option – a tension that illustrates the need for skilled employment counselling, and is characteristic of modern activation strategies.
5. For some unemployed, given the costs of job search, it is not worth searching for a job, even though it would be worth accepting a job offer that arrived without search effort. In this case, when an obligation to search is enforced, at the time a job offer arrives the search costs are sunk costs and the job will be accepted.
6. Job-creation programmes often pay their participants a supplement above the usual unemployment benefit level. Some economists advocate a “Job Guarantee” for the long-term unemployed, which may differ from “workfare” in that the income support level corresponds to the minimum wage rather than benefit, or perhaps that the job is permanent. Mitchell and Wray (2004) give references to a number of papers that advocate or that criticise the Job Guarantee. Evaluations often find that job-creation programmes have a positive impact on job-finding chances for some months after participation in them has ended, but this is not large enough to offset the negative impact (lock-in effect) that arises during participation.
7. For a public works programme to provide effective social protection against destitution, places on it must be kept open. A “workfare” programme must similarly keep places open for it to be effective in deterring voluntary unemployment.
8. “Since 1850 the British labour force has grown by 240% and the number of jobs has grown by, guess what, 240%” notes Layard (2001), who also presents a cross-country comparison of labour supply growth against employment growth.
9. However, even in the short term there is little direct empirical evidence for displacement effects from training or activation measures. Calmfors (1994) claims that studies indicate large substitution effects from counselling and job-search assistance measures, but does not give specific references. Dahlberg and Forslund (1999) estimate, using panel data for 260 Swedish municipalities, that training programmes have small (not statistically significant) displacement effects whereas relief work (i.e. job creation) and other subsidised work (workplace induction, trainee replacement, and work experience) schemes have long-run displacement effects of about 65%. In another study focusing explicitly on wider labour market effects, Hasluck et al. (2003) found that the Employment Zones (EZ) programme, which mainly provides intensified employment counselling, increased monthly outflow rates by about 1 percentage point for its target group, with no evidence of any reduction in monthly outflow rates among unemployed clients outside the EZ target group in the same local areas. De Boer (2003) finds that New Zealand’s WRK4U scheme, which increased the take-up of work by potential claimants before the start of a benefit spell, did not reduce outflow rates for existing UB clients. The displacement effects of other measures, such as hiring subsidies which do not increase effective labour supply, by contrast might be expected to persist indefinitely (e.g. shifting the structure of employment away from the industries that do not qualify, or reducing hazard rates out of short-term unemployment if only long-term unemployed qualify).
10. The intensification of the activation strategy in Denmark during the 1990s took place by progressively advancing the start of the “activation period” (during which unemployed people had to participate in an employment or training programme 75% of the time) as described in Section 3.B. In the United Kingdom, it was probably only possible to implement “fortnightly signing” (in 1996) and the succession of New Deal programmes (from 1998 onwards) because caseloads had already fallen substantially. In the United States, as welfare caseloads fell some states developed a broader range of employment services which helped to place the hard-to-place and keep people in work after placement.
11. To a considerable extent, people do what they see their neighbours, friends and family doing, so when a microeconomic programme influences the behaviour of its participants, unemployment outcomes for non-participants are influenced in the same direction. Examining the impact of a change in unemployment benefit entitlements which directly affected some groups but not others in Austria, Lalive (2003) concluded that “there are strong indirect effects on the entitled, strong positive spillovers on the non-entitled, and... social interactions are about as important as the

direct effects of the policy change.” A number of studies have documented this type of effect, described in terms of “neighbourhood effects”, “network effects”, “external habit formation” and “ethnic enclaves”.

12. Studies citing different selections of countries as examples of (successful) reforms include Barrell and Genre (1999) and Auer (2000); also many studies have analysed a particular (notably the Dutch or Danish) “jobs miracle”. OECD (2003a, Table 4.3) provides a single-page listing of what are thought to have been significant labour market policy reforms in Denmark, Ireland, the Netherlands and the United Kingdom.
13. Participation in WRK4U seminars is voluntary, so these seminars do not qualify to be called an activation programme strictly as defined in this chapter. However, these seminars emphasize that obligations will be associated with benefit receipt. As of March 2004, more than half of the 23 500 people who had attended a WRK4U seminar had decided that they did not require an Unemployment Benefit (report by kiwinews.co.nz, 3 March 2004). De Boer (2003) reports findings from the pilot implementation of WRK4U, which was called Jump Start.
14. The fact that increases in job-entry rates in Chart 4.2 do not occur only at 14 months, but also during several months before and after this, indicates that individuals are prepared to lose up to several months of benefit (when entering a job before 14 months) or live for several months at the lower rate of benefit (when entering a job after 14 months) in order to find a better job match; but relatively few find it worthwhile taking a job a year earlier or a year later. This suggests that gains from potential improvements in job-match quality are comparable to the costs of delaying entry to employment for a few months, but not usually for a year. Therefore, public policy should seek to nearly eliminate unemployment spells of such long duration. By contrast, short search-unemployment spells can be productive. Employment services which permit a better immediate job match, but also make job-search more productive and thus permit a better job match later, will not necessarily shorten the duration of these spells. The positive impact from such services may instead take the form of greater employment stability and higher earnings.
15. Occasionally, in situations where participation in labour market programmes becomes obligatory at a relatively well-defined point in the unemployment spell, researchers can observe impacts on hazard rates similar to those that arise from benefit exhaustion (see Lissenburgh, 2001, for the UK New Deal for the Long-term Unemployed; Geerdsen, 2002, for activation in Denmark).
16. Impacts on employment are analysed further in Section 4. Benefit reciprocity is not an informative outcome for individuals who have exhausted time-limited benefits, and in this case researchers usually focus on employment as the main outcome variable (*e.g.* Cockx and Ries, 2004). Microeconomic studies do not usually report impacts on unemployment as defined and measured in labour force surveys.
17. Ashenfelter *et al.* (2005) find that additional verification of reported job-search contacts by UI claimants prior to the first benefit payment reduced the rate of qualification for benefits by about 8% in one state but had little impact in three others. Also in the United States, “diversion” strategies – which seek to reduce the number of entrants to the ongoing benefit caseload – are often described as significant element in state welfare-to-work strategies. Nathan and Gais (1999, p.22) reported that about half the sites they examined required new applicants to conduct some sort of initial and often independent search for work. Two-thirds of the sites reviewed families for “diversion” assistance, which can be a lump-sum cash payment or loan in exchange for waiving eligibility for cash benefits for some time, such as six months.
18. In 1997, Australia’s requirement on jobseekers to list eight job applications per fortnight in a Job Seeker Diary (JSD) over the first three months of their spell reduced the average duration of benefit spells by 0.9 fortnights (about 7%), an average impact similar to that reported in Maryland. However the impact varied from 1.5 fortnights in the quartile of regions with the lowest unemployment rates to 0.5 fortnights in the quartile with the highest unemployment rates (Borland and Tseng, 2004). When unemployment is high, many applications are made only to meet formal job-search requirements, so their real effectiveness may decline.
19. A small experimental study of job clubs (with 1 015 participants and controls) also found that job-club participants were more often employed and less often unemployed than the controls (Malmberg-Heimonen and Vuori, 2000, summary in English by Raisanaen, 2003).
20. Experiments in Charleston, New Jersey, Washington and Wisconsin in the 1980s, summarized by Meyer (1995), involved in certain variants both work-search assistance (delivered through one or two intensive interviews or more frequent contacts with the Employment Service) and attendance at job-search training courses (varying in duration from one three-hour session to five half-day sessions). In these variants, impacts on average weeks of benefits received ranged from 0.5 to

0.8 weeks (roughly 5% reductions, as compared to the control group average of 15 weeks). The detailed report from the Washington experiment, which examined the timing of the impacts on hazard rates, suggested that “the shorter durations of UI receipt are due to the costs of appearing at the UI office”. In other cases there was no clear evidence concerning the relative impact from individual assistance *versus* job-search workshops, or concerning motivation effects *versus* a direct impact from services. In the Maryland UI Work Search Demonstration, one treatment “where claimants were required (usually in the third to fifth week of their claim) to participate in a four day job-search training workshop reduced the average duration of UI payments by 0.6 week (about 5%). The overall impact came largely through a 28% increase in the hazard rate (i.e. the proportion of people whose status changes over a given time period) of out of UI for the two weeks immediately preceding the date of the scheduled workshop: the hazard rate during the workshop period itself fell, and evidence concerning the period after the workshop was mixed” (summary in OECD, 2000). Black *et al.* (2003) report outcomes from an experiment in Kentucky where some claimants were on a random-assignment basis sent a letter notifying them of an obligation to attend re-employment services (job-search training and preparation courses, in three-quarters of the cases) usually in the third or fourth week of their unemployment spell. This treatment reduced average weeks of UI receipt by about 2.2 weeks (probably about 15%) and “much (but not all) of the effect results from a sharp increase in early exits from UI in the experimental treatment group compared to the experimental control group”, i.e. from exits before attending the re-employment services. In Australia, DEWRSB (2001) estimated, using statistically-constructed controls, that the “compliance” effect accounted for most of the total impact on off-benefit outcomes of Job Search Training (JST), a programme to which unemployed people could be referred at unemployment durations of three months or more: the relatively large size of the “compliance” effect was related to the fact that many more people were referred to the training than actually attended it.

21. One explanation for the large size of impact from Restart interviews is that, for some participants, the interviews acted as a stepping stone to further services, such as Jobclubs and Employment Training. Another factor might have been the relative novelty of Restart, since prior to 1996 there were few obligatory interventions in the unemployment spell in the United Kingdom. The lesser long-term impact for women appeared to arise because “motivation” effects led to labour force withdrawal more often than it did for men who mainly entered employment, and because the offer of further services was less helpful for potential part-time workers.
22. A statistical model, with controls for a number of individual characteristics, estimated quite large impacts on the probability of exit from jobseeker status over the next four months, +6 to +9 percentage points for adult long-term UI and social assistance (RMI) beneficiaries (but not significant for long-term unemployed youths), but many of those who exited from jobseeker status soon returned. Nevertheless, for adults referral to SPNDE reduced the probability that individuals would still be jobseekers four months later by 3 percentage points (about 6%)(Micheau *et al.*, 2001).
23. After 2001, France replaced the SPNDE by the PAPND which involves drawing up a Personalised Action Plan (PAP) for each unemployed person early in the unemployment spell, followed by at least one interview every six months. Owing to the universal coverage of the new scheme and the fact that benefit entitlements were reformed at the same time (the replacement rate is now constant rather than declining with unemployment duration), it has been difficult to evaluate the impact of this new arrangement by either microeconomic or macroeconomic methods (Peer Review Programme, 2004a).
24. In the Netherlands, a treatment which consisted of giving more counsellor time to jobseekers (which was used in a variety of ways, notably to more thoroughly check reported job search, to provide additional assistance *e.g.* with writing job application letters, and to acquire vacancies from local sources and advertise these in the waiting room) increased the job application rate for workers whose previous job had been permanent by 30% but reduced the success rate per job application, so that the impact on the job-finding rate was smaller (11%, and not statistically significant). And for workers whose previous job had been temporary, the treatment reduced the job-finding rate by 50%: this result arose because “people in the treatment group who last had a temporary job are assisted and stimulated in their attempts to find a permanent job... temporary jobs are usually found more quickly than permanent positions” (Gorter and Kalb, 1996). An experiment in California where some welfare case managers were given half the caseloads of others found that those with smaller caseloads did not achieve greater impact (Freedman *et al.*, 1994).
25. In Finland, a 1998 policy reform introduced interviews at fixed intervals, individual action plans and job-search courses/job clubs. Aho *et al.* (2000), on the basis of follow-up surveys, found that the individual action plans and more clearly the job-search courses facilitated job-search activity, and that the reforms seemed to be quite successful if evaluated in terms of the satisfaction of the jobseekers. However, the new services had not led to higher rates of employment. Instead,

- participants more often entered long-term ALMPs. The researchers concluded that unemployment was largely structural, so increasing activation programmes did not much help in solving the problem. Räsänen (2003) presents some interpretation of why “after the recession, in the boom years of the late 1990s, effectiveness [of labour market programmes] did not improve significantly”, while also citing some positive evaluation findings (see note 19) and recent improvements in labour market policy.
26. Another evaluation using Swiss data (Lalive *et al.*, 2000) estimated that for women the lock-in effects of employment and training programmes (except language training) were compensated by fairly strong positive employment gains after the end of programme participation.
 27. Friedlander and Burtless (1995) analysed some of the first US five-year follow-up studies, concluding that only higher-cost educational programmes enabled welfare recipients to stay off welfare. Hotz *et al.* (2000), using data which follow up outcomes for participants in Californian random-assignment experiments over nine years, report that “the early superiority of the Riverside program with its stress on job-search assistance rather than basic skills training is lessened over time. In later years the programs in countries such as Alameda and Los Angeles [which engaged about two-thirds of participants in basic education or vocational training] are doing as well as, or even slightly better, than Riverside”. Dyke *et al.* (2005) report similar findings for participation in intensive training within welfare-to-work programmes in Missouri and North Carolina. In some other studies, even in the longer term training does not reduce benefit receipt and yet positive impacts on employment and earnings emerge (see Section 4).
 28. In Chart 4.3, the most favourable outcomes shown (for participants in a temporary wage subsidy) and the least favourable (for participants in language courses) might reflect selection biases that can arise in non-experimental data (see Chapter 5, Box 5.2 for further discussion).
 29. AM (2000, p. 115) stresses that “... a significant increase in the transition rate for people who have been in the unemployment benefit system for two to three years is observed in 1998... It is precisely this group which was covered by the advance which took place in [the] course of 1998”.
 30. Rosholm and Svarer (2004) use another method to estimate “motivation” effects. First, using a rich empirical data set, they estimate for each individual the probability of entry to an ALMP conditional on personal characteristics and duration of the unemployment spell. Then they use this probability as an explanatory variable (the so-called “threat effect”) in a second equation for the hazard rate out of unemployment. They estimate that the “threat effect” arising from the Danish suite of programmes over 1998-2002 reduced average unemployment durations for men by an average of three weeks (a reduction of 8%). Actual participation in programmes affected duration by smaller amounts (positive or negative, depending on the programme). Movements in the Rosholm-Svarer “threat” variable will be quite trended and thus it may be difficult to accurately separate its impact from that of other trended variables such as duration dependence (the phenomenon of declines in hazard rates with duration of the unemployment spell). Changes through time (change in patterns of hazard rates compared with changes in the activation regime) arguably identify the impact more convincingly. But the Rosholm-Svarer approach is particularly interesting at the conceptual level, as a way of modelling the motivation effects from programme participation obligations (see Box 4.4).
 31. Dolton and O’Niell (2002) found that re-employment durations were not significantly different for the Restart treatment group. For the New Deal for Young People (NDYP), Blundell *et al.* (2001) estimated that impacts on exits to “sustained” jobs were comparable to impact on exits to all jobs with little evidence of any net “spill-over” (substitution) effects. McVicar and Podivinsky (2003) report that “Relative re-entry rates [to unemployment] for those of the target and comparison age groups having previously experienced a six months or more spell of unemployment have changed little since the introduction of NDYP”. Lissenburgh (2001) concludes, based on a matched comparison group evaluation of pilots of the New Deal for Long-term Unemployed, that “Pilot entrants had lower levels of job satisfaction than their comparators” and yet “once the programme had enabled participants to leave unemployment, they tended not to return”.
 32. Riverside LFA was a slight exception (it increased entries into jobs lasting four or more quarters by 4 percentage points but also increased entries into jobs that lasted less than three quarters by 7 percentage points, so the share of the latter in all job entries increased), but this was the most “work-first” of the programmes, urging participants to take any job, no matter how casual or low paid (Freedman, 2000).
 33. Hamilton *et al.* (2001) appeal to motivation effects to explain how one NEWWS programme (Grand Rapids LFA) had a negative impact on both benefit receipt and employment five years later. They suggest that this programme encouraged certain individuals to enter employment but then stay

off assistance after they lost the job, even though they remained eligible, “a pattern that may reflect the national climate in the aftermath of the federal welfare legislation of 1996”.

34. Several factors make it difficult to estimate the impact of ALMPs on the level of unemployment in cross-country regressions: a) the dependent variable would have to be labour-force-survey unemployment rates (which are measured comparably, but are often weak proxies for the target group of active or passive labour market programmes) or unemployment beneficiary rates (which are also a function of country-specific entitlement rules for unemployment, social assistance and perhaps other benefits); b) the explanatory variables would need to include indicators for the effectiveness of several types of “active” policies (e.g. benefit eligibility criteria, regular PES interventions, longer-term ALMPs); and c) in a country where unemployment has stayed low for many years, the case for restraint in relation to benefit levels, wage inflation, etc., may lose political support until eventually unemployment rises despite a relatively effective system of labour market programmes.
35. According to Bell (2001), the research community had not convincingly demonstrated the impact of welfare reform and there was substantial agreement among scientific studies, nine of which he summarises, that falls in unemployment in the 1990s were the main cause of falls in welfare caseloads. Findings that policy variables had little impact appear to arise in time-series econometric estimates because the welfare caseload has been a long distributed lag function of reform measures. Reasons for the long lag are: a) reforms in the short run affect rates of flows off and onto benefit, and, since the welfare caseload has a long average spell duration, it only changes slowly; b) reforms were partly implemented before they were legislated, and behaviour starts responding to media information, word of mouth and rumours even before reforms are implemented; c) even after reforms are legislated, years may pass before the administration is implementing them effectively. In data terms, policy changes are discrete events: the archetypal data item for welfare reform would be a dummy for 1996 welfare reform (PRWORA) legislation that jumps from 0 to 1 in 1996. Any simple regression with no lag structure will therefore estimate a near-zero coefficient on welfare reform and attribute the caseload decline to other trended variables in the equation (in data up to 2000, an unemployment variable could play this role). For a regression to be correctly specified, the PRWORA dummy would need to have a lag ranging from about year -4 to year $+15$ (see OECD, 2003a, Chapter 4, for background evidence on this point). In practice, the choice is then between a regression that specifies a highly flexible lag structure, when coefficient estimates may be unbiased but are statistically insignificant, and a regression that imposes a simple lag structure, which gives a biased estimate of long-run coefficients. The underlying problem is that without a *a priori* knowledge of the lag structures, time-series data alone have little information content.
36. Since TANF is essentially an unemployment benefit (i.e. a benefit that is conditional on availability for work), the TANF caseload should be more cyclical than the AFDC caseload was. But in a counterfactual case of no welfare reform, it seems likely that AFDC caseloads would not have changed much.
37. Since there is now no federal entitlement to welfare, the concept of benefit entitlement can now only refer to state practices. Owing to administrative flexibility in the application of time limits (permitted by exemptions and other factors), few cases have arisen where families without other income lose all benefits. The threat of time limits may have had more impact on caseloads than their actual implementation. Some experts see a risk that diversion programmes result in poor families receiving too little or even false information about their potential eligibility for food stamps and Medicaid (Gais, 2000).
38. Grogger (2003) estimates using data for federal and state EITC rates that from 1993 to 1999 welfare reform (waivers and TANF) reduced caseloads by 8.5% and financial incentives (benefits and EITC) reduced caseloads by 7.9%, almost as much. But in this study, the independent variables together explain less than a third of the 1993-99 fall in welfare caseloads.
39. Many US states have not implemented employment strategies as sophisticated as Portland’s strategy, which reduced caseloads by 30% (on the basis of treatment compared to control group outcomes). However, the full impact of actual implementations was no doubt greater than the average impact reported in NEWWS experiments. Actual implementations were generally “work-first”, playing down the human capital development programmes which had lesser impact in the experiments. As argued elsewhere, estimated impacts probably understate full impacts, because in the experiments reforms also influenced control group behaviour to some extent. Another issue is that in experiments, impacts on the actual rate of participation in employment services were quite limited. On average across NEWWS and 23 other US evaluations conducted from 1983 to 1996, about 31% of control group members participated in a “programme activity” as compared with 54% of treatment group members, i.e. the treatment group actually received more

services than the control group in only about one quarter of cases (Greenberg *et al.*, 2003, Table 2). Full-scale implementations may have a larger impact on service delivery, since after a while participation in employment services becomes standard procedure. Mead (1998) sets out several such mechanisms for impacts on aggregate caseloads to exceed impacts reported in random-assignment experiments.

40. JSA legislation, among other things, made benefit conditional on fortnightly in-person attendance at a short interview. Smith *et al.* (2000) report that JSA legislation increased the hazard rate off benefit by 11%, after controls for the effects of the improving economy. However, they also report that the hazard rate back onto benefit after leaving it was 26% higher before JSA legislation. Although no estimate with controls for the improving economy is cited, JSA legislation was probably at least partially responsible. The total impact of JSA legislation on caseloads would reflect the sum of impacts through these two routes.
41. Among other UK programmes were the Restart interviews from 12 months onwards (whose impact is not covered by the random-assignment experiments reported by Dolton and O’Niell), the introduction of a range of low-cost labour market programmes (job-search training courses, Work Trials, One-to-One, etc. with evaluated impacts, but generally applying to only a small proportion of the unemployed), the “stricter benefit regime” with an increase in sanctions in the early 1990s, and the New Deals introduced after 1996.
42. In Australia the number of long-term job outcomes (employment for more than three months claimed by employment service providers in relation to JST and IA and their equivalents in the third Job Network contract period) has nearly doubled since 2002 (from 96 000 in 2002-03 to an expected 180 000 in 2004-05), suggesting that programme impacts have increased (although research publications that estimate outcomes for a plausible control group are not yet available). For benefit reciprocity data, see the *Monthly Profile of Labour Market and Related Payments* at www.workplace.gov.au.
43. Social assistance caseloads in six countries (OECD, 2003a, Chart 4.6) have responded very little to the economic cycle in some cases and fairly strongly in others, but even in the latter case structural factors seem to be important (a year’s caseload depends more on which decade it is in than on which year of the decade it is in).
44. Calmfors (1994) lists several reasons for expecting declining returns to scale for various types of labour market programme. Räisanen (2003) cites a research finding that the effectiveness of training programmes is higher in Southern Finland, where their volume is lower than in the North and participants have not so often already participated in previous programmes.
45. A well-managed quasi-market will measure the relative impact of different providers on jobseeker outcomes. The impact of providers, as compared to a counterfactual of no service provision, is irrelevant for good governance (unless zero service provision is a serious policy option). The quasi-market approach leaves still much scope for research as the government needs to document the service package used by different providers so as to assess externalities, measure impacts on outcomes for which providers are not rewarded, etc.
46. Recent studies of the impact of benefit entitlements include Carling *et al.* (2001) and Benmarker *et al.* (2004) for Sweden, Roed and Zhang (2003) for Norway, Van Ours and Vodopivec (2004) for Slovenia, Cockx and Ries (2004) for Belgium and Lalive and Zweimuller (2004) for Austria. Roed and Zhang use a very large sample of data where quirks of an administrative nature generate variation in entitlements, and Cockx and Ries examine hazard rates to employment around the time of benefit exhaustion (evidence conceptually similar to that shown in Chart 4.2 here); the four other studies estimate benefit impacts based on comparing outcomes before and after benefit entitlement rules were changed. The Norwegian study estimates an elasticity of the (off-benefit) hazard rate with respect to benefit level of -0.95 for men and -0.35 for women, the first Swedish study estimates an elasticity of -1.6 , the Slovenian study implies an elasticity of -1 or larger: and the second Swedish study implies an elasticity of unemployment duration with respect to the replacement rate of 0.6 . A longer duration of benefit entitlement was estimated to increase actual unemployment durations by a third in Slovenia and by a half in Austria (here UI entitlement increased from 30 to 209 weeks: given that Austria has indefinite UA benefits the change examined may have resembled a permanent increase in benefit level). The Belgian study found that the employment rates, in a sample of family dependants with an ongoing entitlement to benefit, averaged about 4% while entitlement continued, and rose to 16% soon after impending future exhaustion of benefit entitlement (in the Belgian system, this was usually “news” to the individual concerned) was notified and to 25% by 14 months after exhaustion.
47. Individual variation in the level of benefits actually received is not a good source of variation for estimating behavioural impact: for example, when comparing two otherwise-identical individuals

- of whom the first is receiving benefit and the second is not, it may be that the first is not sufficiently available for work and has lower benefits – but also lower job-finding chances – for this reason.
48. In Sweden over 1965 to 1975 huge wage compression occurred and benefit replacement rates were high for low-paid workers, but active policies were in place and unemployment was kept low until the end of the 1980s.
 49. In recession (from 2000 to 2003) lone parents in the United States lost in absolute terms about one-quarter of the employment rate gains achieved in the 1990s (Sherman *et al.*, 2004). But employment rates of other groups in the labour market also fell, so lone parent employment rates fell only slightly in relative to other labour market groups.
 50. US evaluations usually use data from quarterly social insurance contribution records, which only report whether an individual has been employed during each quarter and total earnings in the quarter.
 51. "... most programs continued to significantly reduce welfare receipt at the end of year 5. This result is somewhat surprising, given that few programs increased employment and earnings above control group levels in year 5. This pattern is especially striking for Grand Rapids LFA, which decreased receipt below the control group by 3 percentage points at the end of year 5, but led to a similar *reduction* in percentage employed during that year" (Hamilton *et al.*, 2001). According to Michalopoulos (2004, p. 20), "the employment-focused mixed-activity programs stand out. They generated the largest effects on earnings among the most disadvantaged, but reduced welfare payments by less than they increased earnings. In contrast, the job-search-first programs reduced welfare payments by more than they increased earnings."
 52. The impact of additional employment services in SSP Plus arose only from the third year onwards, but in the longer term these services apparently increased total earnings more than the in-work benefits of regular SSP. Card and Hyslop (2005) conclude that "Despite the extra work effort engendered by the program's incentives, SSP had no long-run impact on wages and little or no long-run effect on welfare participation". Foley (2004) suggests that SSP motivated mothers to enter full-time jobs within a year in order to qualify for the subsidy, but these gains were not sustained, so employment services are needed in order to improve job-match quality.
 53. More specifically, Poppe *et al.* (2003) cite findings that – for former welfare recipients and for individuals who had persistently low earnings initially – the coefficient on work experience for those who stay with the same job is 2% per year after controlling for other observed characteristics. Loeb and Corcoran (2001) however estimate that wages of young women grew on average by about 7% for each year of full-time work experience, with little evidence that returns – measured this way – were lower for former welfare recipients than for others. In their perspective, policies which achieve continuous employment in full-time jobs will thereby also achieve wage progression.
 54. Typical support services that were made available at the PESD sites included encouragement, counseling, referrals, help with car repairs, some rental assistance, promotion of EITC, and employer mediation (see the 1998 summaries at <http://peerta.acf.hhs.gov/taevents/chron.htm>).

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