

# Does active labour market policy work?

## Lessons from the Swedish experiences<sup>S</sup>

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### Abstract

The Swedish experiences of the 1990s provide a unique example of how large-scale active labour market programmes (ALMPs) have been used as a means to fight high unemployment. This paper discusses the mechanisms through which ALMPs affect (un)employment and surveys the empirical studies of the effects of ALMPs in Sweden. The main conclusions are: (i) there is hardly any evidence for a positive effect on matching efficiency; (ii) there are some indications of positive effects on labour force participation; (iii) subsidised employment seems to cause displacement of regular employment, whereas this appears not to be the case for labour market training; (iv) it is unclear whether or not ALMPs raise aggregate wage pressure in the economy; (v) in the 1990s, training programmes seem not to have enhanced the employment probabilities of participants, whereas some forms of subsidised employment seem to have had such effects; and (vi) youth programmes seem to have caused substantial displacement effects at the same time as the gains for participants appear uncertain.

On the whole, ALMPs have probably reduced open unemployment, but also reduced regular employment. The overall policy conclusion is that ALMPs of the scale used in Sweden in the 1990s is not an efficient means of employment policy. To be effective, ALMPs should be used on a smaller scale. There should be a greater emphasis on holding down long-term unemployment in general and a smaller emphasis on youth programmes. ALMPs should not be used as a means to renew unemployment benefit eligibility.

## 1 Introduction

During the last decade there has been an increasing international interest in active labour market policies, i.e. measures to raise employment that are directly targeted at the unemployed. According to conventional definitions, these policies comprise: (i) job broking activities with the aim of improving the matching between vacancies and unemployed; (ii) labour market training; and (iii) job creation. Recommendations to expand the use of these policies have become standard from international bodies, such as the OECD and the EU Commission (e.g. OECD, 1994; European Commission, 2000). In the EU, the European Council agreed in 1997 on an employment strategy that includes active labour market policy as a key ingredient,<sup>1</sup> and many member states have followed these recommendations.<sup>2</sup>

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<sup>1</sup> See [http://europa.eu.int/comm/employment\\_social/elm/summit/en/papers/guide2.htm](http://europa.eu.int/comm/employment_social/elm/summit/en/papers/guide2.htm).

<sup>2</sup> This is evident from the national action plans on employment. The plans for 2001 are available at [http://europa.eu.int/comm/employment\\_social/news/2001/may/naps2001\\_en.html](http://europa.eu.int/comm/employment_social/news/2001/may/naps2001_en.html).

The recent interest in active labour market policies motivates a thorough evaluation of how successful the active labour market programmes (henceforth denoted ALMPs) in various countries have been. Sweden is then a case of particular interest, as this is the country where the focus on active labour market policy has been the greatest. Partly this reflects an old tradition; partly it was the response to a sudden and steep increase in unemployment in the early 1990s. At their peak in 1994, ALMPs in Sweden encompassed more than 5 per cent of the labour force and expenditures accounted for more than 3 per cent of GDP.

The Swedish case is interesting from the point of view of evaluation because a large number of studies of the effects of ALMPs have been made. Recent studies have been able to draw on an internationally unique data material: the National Labour Market Board (AMS) provides a longitudinal data set with the event history of all unemployed individuals registered at the public employment offices since 1991. This makes it possible to trace the effects of participation in ALMPs for a very large number of persons over long periods. The Swedish experiences are of great interest also because they illustrate clearly the interdependence between "passive" unemployment support and "active" measures, which has been the subject of much recent policy discussion (see e.g. European Commission, 2000).

This paper surveys the evidence on the employment effects of Swedish active labour market policy. The focus is on how ALMPs affect *regular employment*, i.e. employment excluding participation in programmes. The motivation for this focus is that employment generation is widely considered to be the primary aim of active labour market policy, even though there are also other goals, such as social-policy aims of mitigating the consequences of open unemployment and contributing to a more even income distribution, as well as additional macroeconomic aims of, for example, raising productivity growth. The results from studies of Sweden will be compared with the evidence from macroeconomic studies based on cross-country or panel data for the OECD countries. Such a comparison is highly relevant, because the latter studies, originating with Layard et al. (1991), have usually been interpreted to give strong empirical support for the effectiveness of active labour market policy as a means of raising employment.

The paper is structured as follows. *Section 2* gives a background picture of how ALMPs have been used in Sweden. *Section 3* identifies a number of theoretical mechanisms. *Section 4* surveys Swedish microeconomic studies of the effects on the individuals participating in ALMPs, and *Section 5* surveys Swedish macroeconomic studies of the general-equilibrium effects. *Section 6* reviews the studies based on cross-country or panel data for OECD countries. *Section 7* sums up the results and draws policy conclusions.

## **2 Active labour market policy in Sweden**

There is a long tradition of active labour market policy in Sweden. In the beginning of the 20<sup>th</sup> century, municipal employment offices were built up (Thoursie, 1990). In the depressions of the inter-war years, the government organised relief works and special youth jobs. In 1948, the foundations of modern labour market policy were laid when the National Labour Market Board was instituted.

### **2.1 The thinking behind labour market policy**

The thinking around Swedish labour market policy was, at least before the 1990s, guided mainly by the principles laid out by two trade union economists, Gösta Rehn and Rudolf

Meidner, in the late 1940s and early 1950s.<sup>3</sup> They saw active labour market policy as a necessary ingredient in a policy mix designed to combine low inflation, full employment and wage compression. They worried that an anti-inflationary demand-management policy would cause unemployment in low-productivity sectors. To avoid that, they recommended labour market re-training and other mobility-enhancing measures, so that workers threatened by unemployment in low-productivity sectors could be transferred to high-productivity sectors, relieving labour shortages there.

The original focus in post-war Swedish labour market policy was thus on increasing labour mobility. However, over time in the 1960-1990 period the emphasis gradually shifted in the direction of counteracting all types of unemployment. In the late 1960s and early 1970s, the objective of eliminating remaining "islands of unemployment" through selective job creation programmes became more important (Meidner, 1969). Gradually, it also became a more important aim to hold down unemployment in general in recessions. This development seems to be explained by generally rising ambitions in employment policy (Lindbeck, 1975; Calmfors and Forslund, 1990).

The motive of holding down open unemployment in general came to dominate completely in the 1990s. In the early 1990s, Sweden entered its deepest recession in the post-war period with regular employment falling by 13 per cent between 1990 and 1994. In this situation, placement in ALMPs became the main short-run policy instrument to counteract the rise in open unemployment. Policy was also to a large extent guided by the social-policy objectives of providing income support for the unemployed: formally, unemployment compensation could not be had for more than 14 months for the majority of the work force, but eligibility could be renewed through participation in ALMPs. There is ample evidence that programme placements were systematically used to this end (e.g. Carling et al., 1996; Sianesi, 2001).

An important side objective of Swedish active labour market policy has always been to mitigate the moral hazard problems of a generous unemployment insurance: by making payment of unemployment compensation conditional on accepting regular job offers or placement offers in ALMPs from the public employment offices, active labour market policy has been used as a work test for the recipients of unemployment compensation.

## 2.2 The various programmes

Originally, *labour market training* mainly consisted of vocational training programmes, but over time schemes containing more general education have become more important. In recent years, also education in *Swedish for immigrants* has formed part of labour market training. *Computer activity centres*, which were introduced in 1995, represent another innovation; in addition an IT program (*Swit*) was launched by the government in 1998 in cooperation with the Confederation of Swedish Industries. The duration of training programmes has usually been six months. Participants have received training grants equivalent to unemployment compensation. From the second half of the 1980s, it has been possible for unemployed individuals to requalify for unemployment compensation through participation in training programmes. In 2000, this possibility was abolished for all labour market programmes.

There have been many types of subsidised employment schemes over the years. The classical measure has been *relief works*. They consisted of temporary jobs (around six

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<sup>3</sup> The main reference is *Fackföreningsrörelsen och den fulla sysselsättningen* (1951).

months), which were usually arranged in the public sector, but to some extent also in the private sector, and where employers obtained a subsidy for employing individuals chosen by the public employment offices. The participants were paid wages according to collective agreements. Relief works were used up to 1998, when they were abolished.

In the 1990s, relief works were largely replaced by so-called *work experience schemes*. These consisted of activities that "would otherwise not have occurred" and were often arranged by various non-profit organizations. The aim was to organise activities that would not crowd out regular employment. Participants in work experience schemes received unemployment compensation. *Recruitment subsidies* and (more recently) *employment subsidies* are programmes that are more similar to regular employment. Both programmes have entailed wage subsidies to employers for hiring unemployed (mainly long-term unemployed). Participants have been paid regular wages according to collective agreements.

Another type of subsidised employment is *self-employment grants*. These grants, which consist of unemployment benefits for up to six months, are given to unemployed persons to start their own businesses after scrutiny by the employment offices. These have also arranged entrepreneurial training for the participants.

Other programmes can be characterised as *work practice programmes*. In our survey of empirical results, we include these in job creation activities, but work practice programmes are supposed to have also a training content. Various types of *youth schemes* belong to this category. The first youth programme was *youth teams* introduced in 1984. They were followed by "schooling-in slots" (*inskolningsplatser* in Swedish). During 1992 *youth practice* was introduced. This programme rapidly reached large volumes. The programme was targeted at youth below the age of twenty-five. The aim was to provide the participants with both work practice and work experience. As was the case for work experience schemes, there were clear instructions to avoid displacement effects (youth practice was not to replace ordinary recruitment and was seen as a measure of last resort).

Other examples of work practice programmes were *practice for immigrants* and *practice for academic graduates*, which were similar in spirit to youth practice, but with different target groups. Yet another work practice programme was *work placement schemes*, which replaced practice for immigrants, practice for academic graduates and youth practice in 1995.

*Resource jobs* were introduced in 1997 and entailed subsidies (corresponding to the amount the participant would receive from the unemployment insurance) to employers for temporarily (six months with an option to prolong it by three months) hiring unemployed persons. The participants were mainly supposed to work, but were in addition supposed to take part in training and to actively search for jobs. The wage rate was capped at what roughly corresponds to 90 percent of the participant's previous income.

*Trainee replacement schemes* involved subsidies during at most six months to employers, who paid for training for an employee and hired a replacement (who received a wage according to collective agreements). Hence, trainee replacement schemes can be classified as both training and job creation.

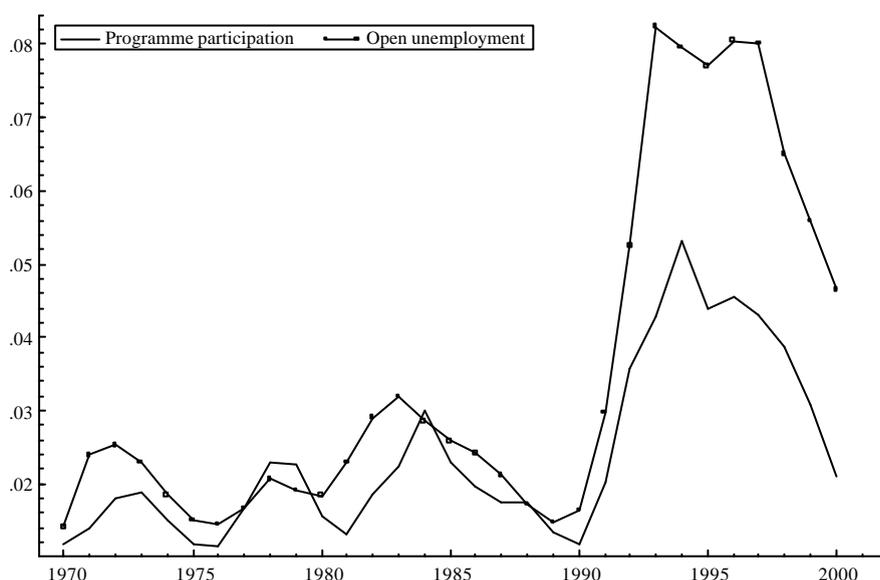
The only programme that has been used over the entire period under study is labour market training. All other programmes have either been instituted during the period and/or ended during it. Relief works were abandoned in 1998, recruitment subsidies were used between 1981 and 1997, work experience schemes were used between 1993 and 1998, work placement schemes between 1995 and 1998, trainee replacement schemes between 1991 and 1997, resource jobs between 1997 and 1999, and practice for academic graduates and practice for immigrants between 1993 and 1995. Self employment grants were

introduced in 1984, youth programmes in 1984, computer activity centres in 1995, and employment subsidies in 1997.

Finally, a reform took place in 2000, when an *activity guarantee* was introduced. This programme is targeted at persons who are or are at risk of becoming long-term unemployed (or, more precisely, long-term registered at the public employment service). Within the programme the participants are given some full-time activity, e.g. job search, until they find a job or enrol in regular education. This reform was made in connection with the abolishment of the earlier possibility to renew benefit eligibility by participating in ALMPs.

### 2.3 The empirical picture

Figures 1-3 illustrate how the programme volumes have developed over time.

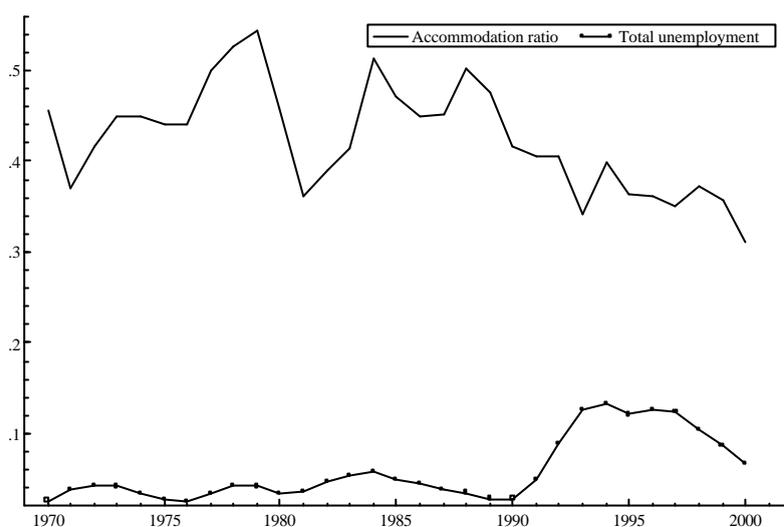


**Figure 1:** Open unemployment and programme participation (shares of labour force), 1970 – 2000

Sources: Unemployment and labour force: Statistics Sweden, Labour Force Surveys; Programme participation: the National Labour Market Board.

Figure 1 shows open unemployment and total participation in ALMPs. The picture is one of a slow trend-wise growth in the size of ALMPs in the 1970s and 1980s, but also reveals a cyclical pattern. The large expansion in the 1990s in connection with the steep rise in unemployment also stands out. In recent years, the programme volumes have been reduced again when unemployment has been coming down.

Figure 2 depicts *total unemployment* (the sum of open unemployment and participation in ALMPs) and the *accommodation ratio* (the ratio between programme participation and total unemployment). In the 1970s and 1980s, the accommodation ratio was of the order of magnitude of 0.4–0.5, but it fell in the 1990s. Although programmes expanded strongly then, they did not increase proportionally to the rise in unemployment. In 2000, the accommodation ratio was around 0.3, which implied that every third job seeker participated in a labour market programme

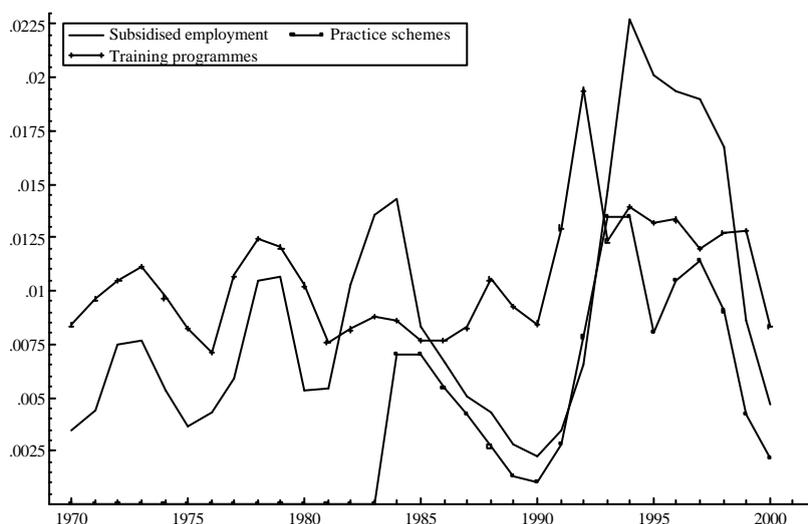


**Figure 2:** The accommodation ratio and total unemployment, 1970 – 2000

*Notes:* Total unemployment is defined as the sum of open unemployment and total participation in ALMPs. The accommodation ratio is defined as the ratio of programme participation to total unemployment.

*Sources:* Participation in ALMPs: the National Labour Market Board; Unemployment and the labour force: Statistics Sweden.

*Figure 3* shows the development of various programme types. In the 1970s and 1980s, training encompassed more persons than subsidised employment. The only exception was the recession in the first half of the 1980s. The steep increase in unemployment in 1991-92 was first met by a large expansion of training programmes, but later there were large increases in schemes of subsidised employment and practice. Recently, training programmes have again become quantitatively relatively more important.



**Figure 3:** Participation in different kinds of labour market programmes, 1970 – 2000

*Note:* The programmes are generally classified as in the main text. Trainee replacement schemes and resource jobs are classified as subsidised employment.

*Source:* The National Labour Market Board.

## 2.4 Swedish ALMPs in an international perspective

Tables 1–3 provide an international perspective on Swedish ALMPs. Table 1 shows the expenditures on active labour market policy as a fraction of GDP. In both the 1986–90 and 1991–95 periods, Sweden spent more on active labour market policy than any other country in the table. The difference is especially marked in the 1991–95 period, when expenditures in Sweden amounted to 1.79 percent of GDP, one percentage point higher than the EU average. Expenditures in Sweden were reduced in 1996–99 when unemployment fell, but still amounted to as much as 1.14 percent of GDP, which was well above the EU and OECD averages. In this period, both Denmark and Finland, however, spent slightly more on active labour market policy.

**Table 1:** Expenditures on active labour market policies (percent of GDP)

	1986–90	1991–95	1996–99
Austria	0.26	0.28	0.36
Belgium <sup>a</sup>	1.06	0.99	1.12
Denmark	0.82	1.15	1.21
Finland	0.82	1.39	1.22
France <sup>a</sup>	0.50	0.85	1.04
Germany	0.72	1.16	1.04
Greece <sup>b</sup>	0.16	0.23	0.23
Ireland <sup>c</sup>	1.06	0.70	1.37
Italy	-	0.89	0.66
Luxembourg <sup>b</sup>	0.16	0.12	0.18
Netherlands	0.56	0.85	1.07
Portugal <sup>a</sup>	0.26	0.41	0.32
Spain	0.71	0.59	0.48
<b>Sweden</b>	<b>1.10</b>	<b>1.79</b>	<b>1.14</b>
United Kingdom <sup>a</sup>	0.50	0.38	0.26
<b>EU average</b>	<b>0.62</b>	<b>0.79</b>	<b>0.78</b>
Australia	0.25	0.45	0.48
Canada	0.52	0.57	0.46
Japan	0.09	0.10	0.10
New Zealand	0.81	0.77	0.60
Norway	0.64	1.28	0.56
Switzerland	0.08	0.18	0.51
United States	0.20	0.17	0.14
<b>OECD average</b>	<b>0.54</b>	<b>0.70</b>	<b>0.66</b>

Notes: <sup>a</sup> Data available until 1998. <sup>b</sup> Data available until 1997. <sup>c</sup> Data available until 1996.

Source: OECD Employment Outlook, various issues.

Table 2 provides another illustration of the focus put in Sweden on active labour market policy by relating expenditures to the total expenditures on the unemployed (the sum of expenditures on active labour market policy and expenditures on unemployment benefits and early retirement for labour market reasons). The table shows that Sweden had the largest share of active expenditures in 1986–90, when it was 59 percent, more than double the EU and OECD averages. The share subsequently fell, but remained 15–20 percentage

points above the EU and OECD averages. In 1991–95 and 1996–99, only Norway and Italy allocated larger shares of the unemployment expenditures on active measures than Sweden.

**Table 2:** Expenditures on active labour market policies as a fraction of total unemployment expenditures

	<b>1986-90</b>	<b>1991-95</b>	<b>1996-1999</b>
Austria	0.21	0.18	0.22
Belgium	0.27	0.26	0.30
Denmark	0.17	0.19	0.25
Finland	0.37	0.27	0.30
France	0.20	0.30	0.36
Germany	0.36	0.35	0.31
Greece	0.29	0.34	0.33
Ireland	0.26	0.29	0.36
Italy	-	0.47	0.47
Luxembourg	0.17	0.16	0.21
Netherlands	0.16	0.22	0.25
Portugal	0.45	0.36	0.27
Spain	0.22	0.17	0.10
<b>Sweden</b>	<b>0.59</b>	<b>0.47</b>	<b>0.42</b>
United Kingdom	0.26	0.22	0.20
<b>EU average</b>	<b>0.28</b>	<b>0.28</b>	<b>0.29</b>
Australia	0.19	0.21	0.28
Canada	0.24	0.23	0.29
Japan	0.22	0.27	0.19
New Zealand	0.43	0.31	0.31
Norway	0.52	0.72	0.52
Switzerland	0.32	0.15	0.32
United States	0.30	0.26	0.33
<b>OECD average</b>	<b>0.29</b>	<b>0.29</b>	<b>0.30</b>

Notes: <sup>a</sup> Data available until 1998. <sup>b</sup> Data available until 1997. <sup>c</sup> Data available until 1996.

Source: OECD Employment Outlook, various issues.

*Table 3*, finally, compares the allocation of expenditures on different programmes among countries for the whole 1986–99 period. What stands out here is the larger emphasis in Sweden than in most other countries on labour market training. 40 percent of the expenditures on ALMPs in Sweden have been on training, compared to EU and OECD averages of 26 and 28 percent, respectively. Only New Zealand and Canada have spent larger fractions of active expenditures on training.

**Table 3:** The allocation of expenditures on active labour market policies in 1986–99 (the expenditures on various programmes as shares of total expenditures on active labour market policy)

	Public employment services and administration	Labour market training	Youth measures	Job creation
Austria	0.41	0.38	0.04	0.17
Belgium <sup>a</sup>	0.18	0.14	0.02	0.67
Denmark	0.10	0.40	0.20	0.30
Finland	0.12	0.33	0.05	0.50
France <sup>a</sup>	0.19	0.40	0.12	0.30
Germany	0.24	0.37	0.05	0.33
Greece <sup>b</sup>	0.48	0.10	0.12	0.43
Ireland <sup>c</sup>	0.24	0.18	0.16	0.47
Italy	0.06	0.00	0.05	0.90
Luxembourg <sup>b</sup>	0.22	0.11	0.40	0.29
Netherlands	0.42	0.39	0.04	0.16
Portugal <sup>f</sup>	0.31	0.12	0.41	0.25
Spain	0.15	0.21	0.11	0.52
<b>Sweden</b>	<b>0.17</b>	<b>0.40</b>	<b>0.08</b>	<b>0.29</b>
United Kingdom <sup>a</sup>	0.53	0.30	0.02	0.18
<b>EU average</b>	<b>0.25</b>	<b>0.26</b>	<b>0.12</b>	<b>0.38</b>
Australia	0.43	0.19	0.06	0.34
Canada	0.41	0.46	0.03	0.09
Japan	0.26	0.24	0.00	0.51
New Zealand	0.18	0.51	0.05	0.27
Norway	0.35	0.34	0.12	0.20
Switzerland	0.58	0.22	0.02	0.34
United States	0.41	0.36	0.18	0.06
<b>OECD average</b>	<b>0.29</b>	<b>0.28</b>	<b>0.11</b>	<b>0.34</b>

Notes: <sup>a</sup> Data available until 1998. <sup>b</sup> Data available until 1997.

<sup>c</sup> Data available until 1996.

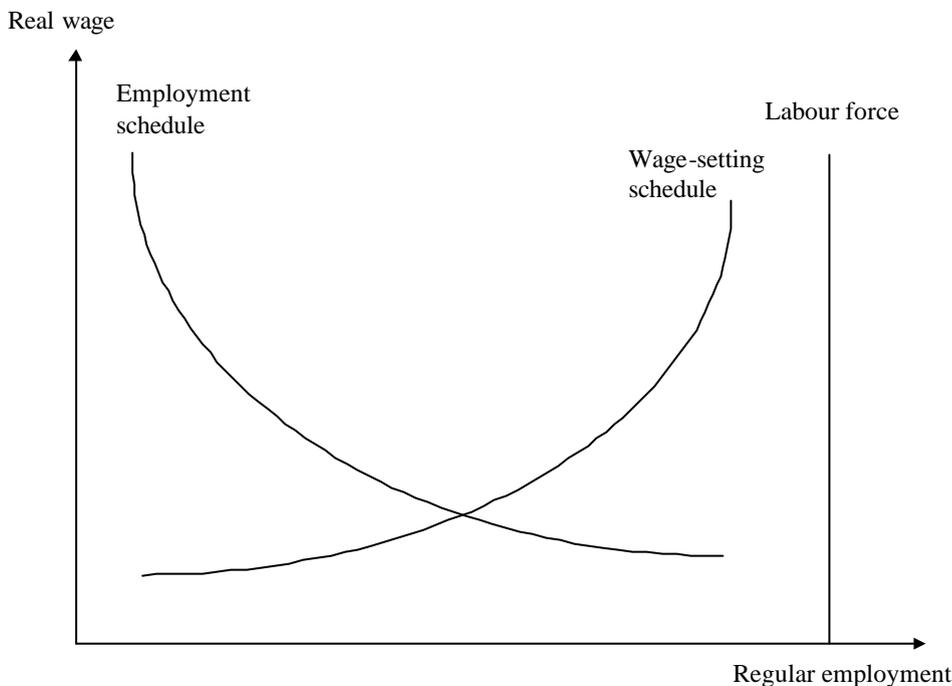
Source: OECD Employment Outlook, various issues.

### 3 A theoretical framework

ALMPs can have a number of effects on employment. Some of the effects are intended, whereas others are unintended side effects. To sort them out, we use a modified version of the Layard et al. (1991) theoretical framework for analysing equilibrium real wages and unemployment, as set out by Calmfors (1994).

In *Figure 4* we distinguish between three curves. A downward-sloping *employment schedule* shows how regular labour demand (labour demand excluding participation in ALMPs) depends negatively on the real wage. An upward-sloping *wage-setting schedule* shows how wage pressure depends positively on regular employment. (The underlying assumption is that higher regular employment is associated with a higher probability of

finding a job if an employee is separated from his present job.<sup>4</sup> This gives the employee a better outside option, which makes it possible to obtain a higher wage.) The intersection of the two curves gives the equilibrium levels of real wages and regular employment. In addition, a vertical line shows the supply of labour (the labour force). By deducting participation in ALMPs from the labour force, and comparing the outcome with regular employment, one obtains open unemployment.



**Figure 4:** Wage setting and employment

The analytical framework in *Figure 4* can be motivated in several ways. The simplest possibility is to view the employment schedule as an ordinary stock demand for labour, following from the usual marginal productivity condition. The wage-setting schedule may be viewed as the (steady-state) outcome of either collective wage bargaining or unilateral employer decisions on wages in an efficiency-wage framework. However, for some applications it is more worth-while to see the employment schedule as a (steady-state) reduced form derived from a framework where vacancies and unemployed need to be matched along the lines of Pissarides (1990) and Mortensen and Pissarides (1994). In this case, it is convenient to regard wage setting as the outcome of agreements between employers and individual employees.

With the help of the above framework, we shall analyse various possible effects of ALMPs. Following Calmfors (1994), we distinguish between:

<sup>4</sup> More precisely, we have

$$s = qN/(L - N), \tag{i}$$

where  $s$  is the probability for a job seeker (without a job) to find one,  $q$  is the rate of separations from existing jobs,  $N$  is the number of regularly employed persons, and  $L$  is the labour force. The probability to find a job is simply the number of job openings created by separations divided by the number of job seekers.

- effects on the matching process
- effects on the competition for jobs
- productivity effects
- effects on the allocation of labour between sectors
- direct crowding-out effects on regular labour demand
- accommodation effects on wage setting

### 3.1 Effects on the matching process<sup>5</sup>

The aim of the job-broking and counselling activities for the unemployed by the public employment offices is to make the matching process more efficient, i.e. to increase the number of successful matches at given numbers of vacancies and job seekers. This is often regarded as the primary function of active labour market policy.

A more efficient matching process shifts the employment schedule in *Figure 4* to the right, which tends to raise both employment and the real wage. The explanation is as follows. When deciding whether or not to post a vacancy, a firm compares the expected future revenues with the expected costs (hiring costs and future pay). The expected future revenues depend on how quickly the vacancy is expected to be filled. This depends in turn on the probability of filling a vacancy, which equals the number of aggregate matches divided by the number of aggregate vacancies in the economy. In a steady state with given regular employment (and a given number of job seekers), the number of matches in the economy is also given.<sup>6</sup> So, an increase in matching efficiency must mean that a given number of matches is associated with fewer vacancies. Hence, the probability of filling a posted vacancy increases. As a consequence, the return to posting vacancies increases, and therefore more vacancies are posted. This results in higher employment at each real wage level.

An increase in matching efficiency also shifts the wage-setting schedule to the right, which works in the direction of reducing the real wage and increasing employment. The reason is the following. Each match creates a surplus to share between the firm and the employed job seeker. The sharing will depend on the outside options of the firm and the employee, i.e. their alternative opportunities if they cannot agree. In that case, the employee quits and becomes a job seeker again, and the firm posts a new vacancy. The firm can expect to fill such a vacancy the quicker, the higher is matching efficiency. It follows that the firm has a better bargaining position vis-à-vis the employee, the higher is matching efficiency. Hence, a higher matching efficiency means that the firm is able to negotiate a lower real wage at each level of employment.<sup>7</sup>

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<sup>5</sup> The exposition builds on Pissarides (1990), Mortensen and Pissarides (1994), and Romer (1996), Ch. 10. See also Holmlund and Lindén (1993) and Fredriksson (1997) for direct applications to ALMPs.

<sup>6</sup> To simplify, we make the assumption that job seekers are either openly unemployed or participants in ALMPs. We also assume that the number of separations from jobs equals a fixed quit rate times (regular) employment (see footnote 1). In a steady state, the number of separations must equal the number of matches.

<sup>7</sup> One might think that an increase in matching efficiency should also have an effect working in the opposite direction because it will enable a quitter to find a new job more quickly. This is not, however, the case if employment is held constant. The probability for a job seeker to find a new job equals the aggregate number of matches divided by the aggregate number of job seekers in the economy. In a steady state with given employment (and a given number of job seekers), the number of matches is also given. It follows that at a given aggregate employment level, the probability for a job seeker to find a job is independent of matching efficiency.

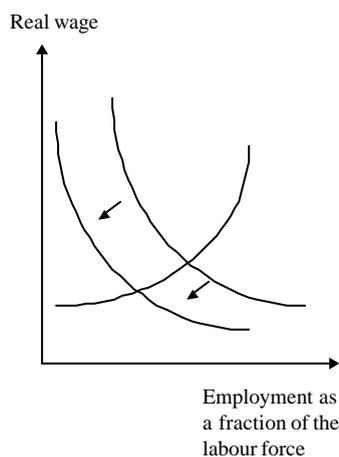
As a higher matching efficiency will shift both the employment and wage-setting schedules to the right, this effect must increase employment, whereas the effect on the real wage is ambiguous.

One should indeed expect active labour market policy in the form of job broking and counselling activities as well as completed labour market training to increase matching efficiency. This is the desired *treatment effect*. But there may also be a *locking-in effect* of training or job creation programmes working in the opposite direction if the participants are not willing to exit from the programmes before they are completed. This effect tends instead to shift the employment and wage-setting schedules to the left. The consequence is then a tendency to lower regular employment (whereas the impact on the real wage is still unclear). Whether or not the treatment effect dominates the locking-in effect is an empirical issue.

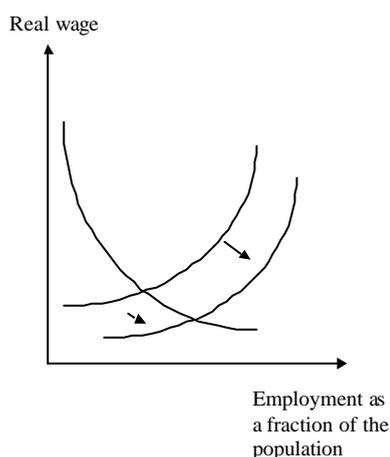
### 3.2 Effects on the competition for jobs

Quite apart from their effect on matching efficiency, ALMPs may affect the degree of competition for the available jobs by making participants more competitive. This may result from several mechanisms (Layard et al., 1991; Nickell and Layard, 1999). Participation in an ALMP may help to maintain the motivation to seek actively for work, i.e. counteract the "discouraged-worker effect" of unemployment. The competition for jobs is also stimulated if ALMPs help to preserve or increase the skills of the unemployed. And employers may in general perceive participants in ALMPs as more attractive than the openly unemployed.

As a result, ALMPs may have a positive effect on labour force participation. In *Figure 4*, the labour supply schedule, showing the size of the work force, may be shifted to the right. This is likely also to affect equilibrium employment and real wages. This is illustrated with the help of *Figures 5a* and *5b*, where we measure regular employment relative to the labour force and relative to the population, respectively.



**Figure 5a:** Labour market policy and employment as a share of the labour force



**Figure 5b:** Labour market policy and employment as a share of the population

The reason for the positive slope of the wage-setting schedule in *Figure 4* is that higher aggregate employment increases the probability that a job seeker finds a job (see the introduction to *Section 3*). This probability is positively related to the ratio between em-

ployment and the labour force.<sup>8</sup> Given this ratio, the size of the labour force does not matter. This explains why the wage-setting schedule in *Figure 5a* remains unchanged even if the labour force increases. In contrast to this, an increase in the labour force must shift the employment schedule in *Figure 5a* to the left. This happens because a given real wage is associated with a given number of employed persons according to a conventional labour demand schedule: hence a larger labour force means a lower employment rate relative to the labour force. So, an increase in the labour force will lead to both a lower real wage and lower employment as a fraction of the labour force.

*Figure 5b* illustrates instead the effect of a larger labour force on employment as a fraction of the population. As an increase in the labour force (a higher participation of the population in the labour force) means a reduction of the employment/labour force ratio if the employment/population ratio is held constant, it follows that the consequence is a lower job-finding probability.<sup>9</sup> This tends to reduce the real wage, so that the wage-setting schedule in *Figure 5b* is shifted downwards. But now the employment schedule (employment as a ratio of the population) is left unaffected. Hence, *Figure 5b* shows that employment increases as a fraction of the population.

It thus matters how employment is measured when ALMPs are evaluated. ALMPs may at the same time reduce employment as a share of the labour force and increase it as a share of the population. As will be discussed in *Section 5*, these measurement issues may be important for judging the effects of labour market policy on employment.

The above discussion is, of course, a gross oversimplification, as the relevant issue often is how effectively a non-employed person searches for a job rather than one of whether or not the person is in the labour force and searches at all. If employed insiders dominate wage setting, it is the job finding probability of an unemployed insider rather than the average job finding probability of the unemployed that matters. If ALMPs raise the relative search efficiency of outsiders, the probability of finding a job for an insider falls, as competition for the available jobs is strengthened. This will also help shift the wage-setting schedule downwards and raise employment (Layard et al., 1991; Calmfors and Lang, 1995).

So, ALMPs may exert a positive employment effect by increasing the competition for the available jobs. But just as with matching efficiency, this requires that the earlier discussed treatment effects are stronger than any locking-in effects.

### 3.3 Effects on the productivity of job seekers

Another desired effect of ALMPs is to increase the productivity of job seekers (Calmfors, 1994). This is the aim of labour market training as well as of various work experience programmes, but such an effect may also arise because of on-the-job training in a pure job creation scheme.

An increase in the productivity of job seekers shifts the segment of the marginal product curve that applies to job seekers (non-employed workers), i.e. the segment to the right of the intersection with the wage-setting schedule, in *Figure 4* upwards. Everything else

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<sup>8</sup> If we divide both the numerator and the denominator in equation (i) in footnote 1 by  $L$ , we obtain:

$$s = qn/(1-n), \quad (\text{ii})$$

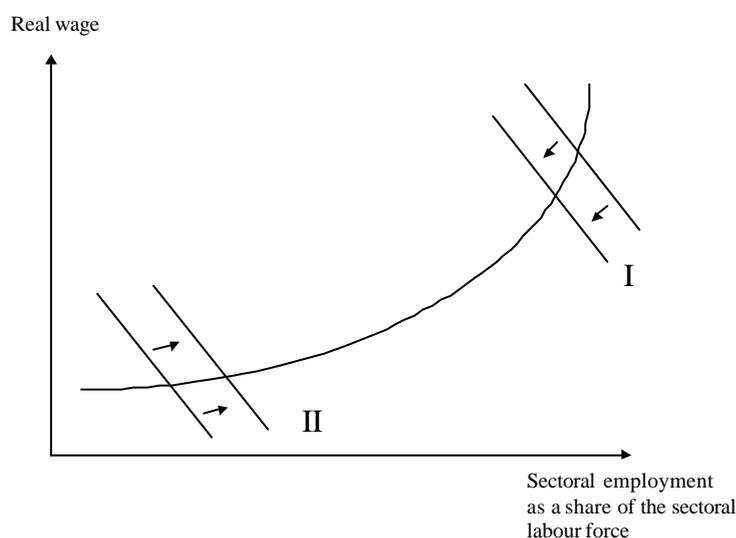
where  $n = NL$  is the employment rate measured in terms of the labour force.

<sup>9</sup> This is so because  $n = N / L = (N / M) \cdot (M / L) = \tilde{n} / l$ , where  $M$  is the population,  $\tilde{n} = N / M$  is the employment/population ratio and  $l = L / M$  is the participation rate in the labour force.

equal, this results in an increase in regular employment. But an increase in the productivity of job seekers may also cause their *reservation wages* to increase. If this occurs, the wage-setting schedule is also shifted upwards in this segment, which tends to offset the positive effect on regular employment. If the wage-setting schedule is shifted upwards by as much as the employment schedule, the net effect on regular employment is zero. Whether or not such effects are important is an empirical issue.

### 3.4 Effects on the allocation of the work force

A fourth intended effect of ALMPs can be to change the allocation of the work force between different sectors. According to the Rehn-Meidner model (see *Section 2.1*), the original goal of active labour market policy in Sweden was to transfer labour from stagnating low-productivity sectors to expanding high-productivity sectors through training programmes and other mobility-enhancing measures. This effect is illustrated in *Figure 6* (see also Calmfors, 1995; and Fukushima, 1998) with real wages and employment relative to the sectoral labour force on the axes.



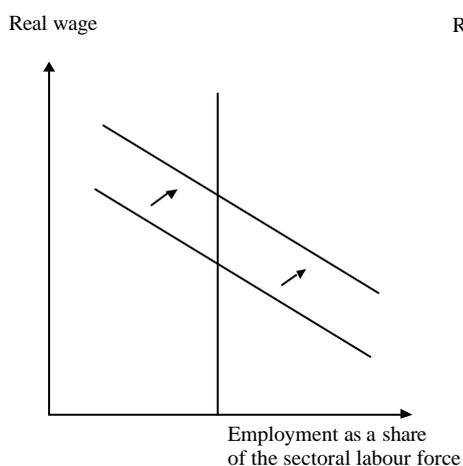
**Figure 6:** Reallocation of unemployed between a high-productivity sector and a low-productivity sector

Assume that there are two sectors in the economy: a high-productivity sector and a low-productivity sector. They have the same wage-setting schedule. The wage-setting schedule is steeper, the higher the employment rate in the sector (the share of the work force in the sector that is employed). Assume also that labour demand is higher in the high-productivity sector (curve I) than in the low-productivity sector (curve II), so that a larger share of the sectoral work force is employed in the high-productivity than in the low-productivity sector. A transfer of labour from the low-productivity to the high-productivity sector can be illustrated by a shift of the labour demand schedule to the left in the high-productivity sector and a shift to the right in the low-productivity sector: labour demand as a share of the sectoral work force at a given real wage falls in the high-productivity sector where labour supply increases, and rises in the low-productivity sector where labour supply decreases. Because of the convexity of the wage-setting schedule, the real wage increases only marginally in the low-productivity sector, but falls substantially in the high-productivity sector. As a consequence, the number of employed persons falls

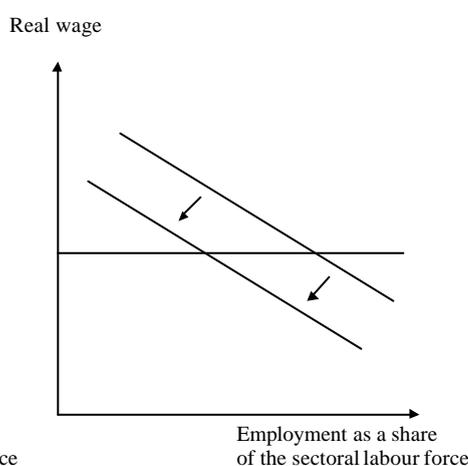
only a little in the low-productivity sector, whereas it increases by much in the high-productivity sector. The net result is that aggregate employment in the economy increases.

There is also another channel through which labour market training (and other types of education targeted on the unemployed, like the so-called *Kunskapslyftet – the adult education initiative* – in Sweden, a programme initiated in 1998 aiming at stimulating unemployed with low education to obtain general high-school education) can affect employment. The reason is that labour market training may affect the incentives of young people to obtain regular education. Fukushima (2000) has analysed this. As above, he distinguishes between a low-productivity and a high-productivity sector. Young people choose between (higher) education in the regular education system, which gives the possibility of a job in the high-productivity sector, and searching for a job in the low-productivity sector without education. In the latter case, there is the possibility of obtaining subsidised education, which will also qualify for high-productivity jobs, at a later stage in life in the event of unemployment.

In Fukushima’s model, young people make their education choices in such a way that an “arbitrage condition” holds: in equilibrium the expected return to regular education equals the expected return of not obtaining education and instead taking a job in the low-productivity sector. Greater educational possibilities for the unemployed in the low-productivity sector through ALMPs raise the expected return of starting to work in this sector without prior education. According to Fukushima’s analysis, this will cause a reduction of the work force and employment in the high-productivity sector as well as a reduction of aggregate employment in the economy.



**Figure 7a:** The effect on the high-productivity sector of training the unemployed



**Figure 7b:** The effect on the low-productivity sector of training the unemployed

Fukushima’s results follow from the assumptions that the wage-setting schedule is vertical in the high-productivity sector (see *Figure 7a*) and horizontal in the low-productivity sector (see *Figure 7b*). These assumptions seek to capture the stylised facts that the employment of high-skilled labour is relatively insensitive to variations in labour demand, whereas the employment of low-skilled labour is sensitive because the wage is given by some kind of minimum-wage restriction. If the expected return to work in the low-productivity sector rises because more unemployed persons are let into labour market

training (education) programmes, the return to regular education for young people must also rise in equilibrium. With a constant employment rate in the high-productivity sector, this can only occur if the wage rises in this sector. This requires in turn that the labour demand as a share of the work force in the sector rises at each wage level, i.e. that the labour-demand schedule in *Figure 7a* shifts to the right. This occurs if the work force in the high-productivity sector decreases, which happens if the total flow of workers into the sector (the sum of those who have received regular education as young and those who have received labour market education at a later stage) falls. An increase in labour market training (education) thus leads to (an even larger) fall in the volume of regular education. The work force increases in the low-productivity sector, which shifts the labour-demand schedule there (labour demand as a share of the work force) to the left. The combination of a higher wage in the high-productivity sector and an unchanged wage in the low-productivity sector causes aggregate employment in the economy to fall. Although this analysis is very stylised, it does show that the general-equilibrium effects of labour market training (education) programmes may be quite complex.

### **3.5 Direct crowding-out (displacement) effects**

An unintended side effect of ALMPs is that they may crowd out regular labour demand (see e.g. Dahlberg and Forslund, 1999). This is likely to apply mainly to job creation measures in the form of employment subsidies for hiring of unemployed. It could be the case that the same persons would have been hired also in the absence of such subsidies or that the subsidies lead employers to substitute one category of workers for another. In the former case one speaks of a *deadweight effect*, in the latter case of a *substitution effect*. Such crowding-out (displacement) effects presuppose that the unemployed that are hired are substitutes – and not complements – to other employees in production, so that the hiring of unemployed workers lowers the marginal product of regular employees (because similar tasks are performed).

In terms of *Figure 4*, direct crowding-out means that the employment schedule (the regular labour demand schedule) is shifted to the left. This tends to reduce both the real wage and regular employment.

The direct crowding-out effects need to be seen in association with the competition effects in *Section 3.2*. Even if there is complete crowding-out, there may be a positive employment effect to the extent that employment of long-term unemployed (outsiders) crowds out employment of insiders, so that the latter group meets more competition. The crowding-out effects may thus be necessary to reach the desired competition effects.

### **3.6 Accommodation effects on wage setting**

Participation in ALMPs may also give rise to unintended side effects on wage setting because the welfare of the unemployed is affected. To improve the welfare of the unemployed is often seen as an important objective of active labour market policy in itself. There are several possible effects:

- Participation in ALMPs may imply higher incomes for job seekers than would otherwise be the case, if compensation there is higher than the unemployment benefit (Calmfors and Nymoén, 1990; Calmfors and Forslund, 1991).
- Participants in ALMPs may experience a higher degree of psychological well-being than the openly unemployed, because programme participation is considered more meaningful (Korpi, 1994a).

- If programme participation is expected to improve future labour market prospects, it will increase the expected future welfare of participants (Calmfors and Lang, 1995). This will, for example, be the case if ALMPs make it possible for those who become unemployed in low-productivity sectors to move to high-productivity sectors, where both the wage and the job-finding probability are higher, as was the main idea according to the traditional Rehn-Meidner model (see *Section 3.4*).
- If programme participation means that the participants renew their eligibility for unemployment compensation (the earlier Swedish system) or is used as a supplement to extend the period of income support beyond the maximum unemployment benefit period, this will also raise the future expected incomes of the unemployed.

All the above effects reduce the welfare difference between having and not having a job. Hence, they increase wage pressure both under collective and individual wage bargaining. Trade unions have an incentive to negotiate higher wages, if those who risk losing their jobs as a consequence of wage rises face better alternative opportunities. Individuals acquire a better bargaining position vis-à-vis their employers, the higher their expected welfare if they quit. In terms of *Figure 4*, the wage-setting schedule is shifted upwards. This means higher real wages and lower regular employment. This can be seen as an *accommodation effect*, which leads to *indirect crowding-out* of regular jobs. The effect is in principle the same as the effect of higher unemployment compensation.

However, there may also be a "control effect" working in the opposite direction (Jackman, 1994). Participation in ALMPs and active job search on part of the unemployed are requirements to receive unemployment compensation. So for some unemployed individuals, programme participation means a welfare loss because they can no longer allocate their time freely. Judging from the reactions of some of the unemployed, the so-called *activity guarantee* in Sweden, which was initiated in 2000, may to some degree work in this way (see *Section 2.2*). To the extent that this is the case, the above effects are reversed, and the wage-setting schedule tends to be shifted downwards.

### 3.7 The effects of ALMPs

Our analysis is summarised in *Table 4*, which shows the expected direction of the various effects. We have put question marks where the expected effects may theoretically be counteracted by other effects. This applies to matching efficiency and the competition for jobs, where treatment and locking-in effects work in opposite directions. It applies also to the accommodation effects on wage setting, where the wage-rising effects may be counteracted by control effects. We have indicated with parentheses that the positive productivity effects may be offset by increased reservation wages.

**Table 4** The expected effects of ALMPs – a summary of the theoretical discussion

	The wage given employment (wage pressure)	Regular employment given the wage	Net effect on regular employment
Matching	- (?)	+ (?)	+ (?)
Competition	- (?)	0	+ (?)
Direct displacement	0	-	-
Accommodation	+ (?)	0	- (?)
Productivity of job seekers	(+)	+	+ / (0)
Allocation of labour force	-	0	(?)

The net employment effect of ALMPs is obviously an empirical issue. The rest of the paper is devoted to a survey of the empirical research on the employment effects of ALMPs in Sweden. These studies are in principle of two types: microeconomic and macroeconomic. The microeconomic studies evaluate the effects of participation in ALMPs for the participating individuals, whereas the macroeconomic ones examine the aggregate general-equilibrium effects.

The microeconomic studies can benefit from data sets with a large number of observations. By examining whether participation in ALMPs implies larger employment chances as compared to non-participation, these studies can give indications of the effects on matching efficiency, the competition for jobs, the productivity of the participants and the re-allocation of labour. Knowledge on these effects can also be obtained by examining how programme participation affects the mobility of job seekers, their search behaviour and the attitudes of employers.

The microeconomic studies of the effects on individuals do not by definition capture the effects of ALMPs on non-participants. These general-equilibrium effects can only be examined in macroeconomic studies. This applies, for example, to the direct crowding-out effects and the accommodation effects on wage setting. Only the macroeconomic studies can give the full picture of the effects of ALMPs on employment and wages. But a problem with these studies is that the number of observations is often small.

The two types of studies complement each other. The two subsequent sections summarise the studies of these types that have been made in Sweden.

#### **4 Microeconomic studies**

This section surveys the evidence from microeconomic studies of the effects of ALMPs on the participants. We focus on the effects on regular employment, but look also at the effects on income (since income depends positively on employment).

The issue is how the labour market outcome of participants compares to the outcome that would have prevailed had they not participated in an ALMP. The crucial element in such an evaluation is to find a comparison group whose outcome equals the counterfactual needed to establish the treatment effects. Evaluations are plagued by potential problems of *sample selection bias*. There is a large literature on this evaluation problem, which was initiated by Heckman (1979) (see e.g. Heckman et al., 1999). However, the set-up of the Swedish labour market policy differs from the one usually considered in the evaluation literature. As discussed in *Section 2.2*, there is a wide array of continuously ongoing programmes for the unemployed. All unemployed may, theoretically, participate and most long-term unemployed do so repeatedly during their unemployment spell(s). Therefore, it is difficult to find a proper comparison group who neither *has participated* nor *will participate* in the future in an ALMP. The choice for an unemployed is to participate in a programme now or *later*, rather than now or *never* (see Carling and Larsson, 2000a; and Sianesi, 2001, for a further discussion). As a consequence, the mere existence of programmes may influence the behaviour of non-participants also. Such a case occurs, for example, if non-participants search less intensively for work, expecting a prolonged period of unemployment to increase the probability of treatment that will enhance their labour market prospects. If so, the outcome of non-participants will deteriorate compared to the outcome that would have prevailed in the absence of programmes (Calmfors, 1994). Consequently, estimated treatment effects may be positively biased.

Also, the fact that most long-term unemployed will ultimately participate in (several) ALMPs makes it difficult to evaluate the long-term effects. First, it is difficult to relate estimated effects to specific ALMPs and, second, the number of openly unemployed who have never participated, and can therefore be used as a comparison group, will be very small. This problem is genuine if treatment effects are not immediate and rapidly transient (Carling and Larsson, 2000b).

The evaluation literature on Swedish ALMPs since the mid-1980s should therefore to be interpreted with caution. It is possible that these evaluations analyse the effect of participating at *a specific point in time rather than later* or in *a certain programme rather than another* instead of the effect of participation compared to non-participation as such.

The early Swedish evaluation literature proceeds from small and “special” data sets (so that results cannot be generalised to the entire population) based on survey data and/or information from personal files kept at the unemployment offices. The research of the 1990s leans heavily on the event data-base Händel (which comprises information on all registered job-seekers since 1991) and sometimes combines this with register or survey data on employment and income. Statistics on search behaviour and employer attitudes are based on survey data.

This part is organised as follows. *Section 4.1* looks at treatment effects of labour market training (LMT), whereas *Section 4.2* focuses on the effects of job-creation programmes. *Section 4.3* reviews evidence on the effects of ALMPs as a whole. *Section 4.4* summarises the evidence on the effects of ALMPs on the search behaviour of participants, and *Section 4.5* reviews the effects on employer attitudes. *Section 4.6*, finally, sums up the picture obtained from the various microeconomic studies.

## 4.1 Labour market training

There has been quite a lot of research on the effects of labour market training. This research is summarised in *Table 5*, and the various studies are commented on in more detail below.

**Table 5** Treatment effects of labour market training

Study	Programme and timing	Sample	Dependent variable	Results
Edin & Holmlund (1991)	LMT, 1981–84	Register and survey data on 800 16–24 years old unemployed in the Stockholm area, 1981	Re-employment probability in subsequent unemployment spells	Significant, positive effect
Axelsson & Löfgren (1992)	LMT, 1981	Register and survey data on 2000 participants. Random selection and representative sample	(i) Yearly income 1982 and 1983 (ii) Income growth 1981–82 and 1981–83	Significant, positive effects
Korpi (1994b)	LMT, 1981–84	Register and survey data on 800 16–24 years old unemployed in the Stockholm area, 1981	Duration of employment	Significant, positive effect
Harkman, Jansson & Tamás (1996)	LMT, 1993	Register and survey data on 3000 20–54 years old participants. Random selection and representative sample	Regular employment 6 months and 2.5 years after programme	Positive effect only if potential selection is not considered

Harkman (1997)	LMT, 1994	Register and survey data on 3000 20–54 years old participants. Random selection and representative sample	Regular employment 2 years after programme	Significant, negative effect of training $\leq$ 100 days; no significant effect of training $\geq$ 100 days; the <i>difference</i> of 4 % between short and long programmes is significant
Regnér (1997)	LMT, 1989–91	Register data on 9000 participants. Non-participating comparison group through matching. Random selection and representative sample	Yearly income 1990–92	Significant, negative effect 1 year and insignificant effect 3 years after programme
Harkman, Johansson & Okeke (1999)	LMT & computer activity centres, 1996	Register and survey data on 3000 20–54 years old participants. Random selection and representative sample	Regular employment 1 year after programme	Positive effect of LMT only if potential selection is not considered; no significant effect of computer activity centres
Larsson (2000)	LMT, 1992–93	Register data on 600 20–24 years old participants. Non-participating comparison group through propensity score matching	(i) Yearly income; and probability to (ii) obtain a job; or (iii) proceed to regular education 1–2 years after programme	Significant, negative effects
Johansson & Martinsson (2000)	Swit, 1999	Register and survey data on 4000 Swit participants. Comparison group = 7000 participants in similar traditional IT training	Regular employment 6 months after programme	Significant, positive effect

Both Edin and Holmlund (1991) and Korpi (1994b) estimated, using the same sample, the effects of labour market training for young people in the early 1980s. Edin and Holmlund found that training increased the re-employment probability in subsequent unemployment spells. Korpi found that training increased the duration of subsequent employment.

Axelsson and Löfgren (1992) analysed the impact of labour market training in 1981 on the (growth and level of) incomes achieved one–two years later. Fixed-effect estimates, using the entire sample, indicated that the yearly incomes of participants increased by as much as 21 percent the first and 30 percent the second year following training. The authors also found that returns were higher the longer the training. But the estimated effects decreased considerably – but were still sizeable – as the sample and/or methods were changed. The results further indicated that participants were, on average, more ambitious and motivated than non-participants. As the selection thus established was considered, estimates became unstable, divergent and sometimes implausible.

Regnér (1991) also analysed the effect on yearly incomes. Irrespective of the method used, the effects were negative after one and insignificant after three years. However, specification tests yielded varying results, and models that were just about accepted by one test were rejected by another. But the qualitative conclusions remained basically the same.

Three different evaluations of labour market training in the 1990s have been published by the National Labour Market Board. Harkman et al. (1996) estimated the effect of training in 1993 on the probability of having a regular job later on. The results differ depending on the method used. According to one method, training had no effect after six months, but a significant and positive effect after three years. However, as selection problems were considered, the authors found no significant effect on the probability of having a job at either point in time.

Harkman (1997) analysed the effect of training undertaken in 1994 on the probability of having a job two and a half years later. Although selection problems were not considered, no effects of training in general were found. The analysis also compared the effects of short and long spells of training (with the limit set at 100 days). The short spells of training had a significant, negative impact on the employment probability, while long spells had a positive but insignificant effect. The *difference* in effect between short and long spells was, however, statistically significant, and amounted to 4 percent.

Harkman et al. (1999), finally, analysed the effect of training in 1996 on the employment probability one year later. Besides “traditional” labour market training, this evaluation looked also at the effect of training in computer activity centres. The latter type of training had no significant effect, and traditional labour market training had a significantly positive one only if selection problems were not considered.

Larsson (2000) estimated the effect of labour market training of young people in 1992–93. Her results convey a very discouraging picture. The effects on both future income and employment were significantly negative. In addition, the transition probability to regular education was lower among participants in training than among non-participants.

Johansson and Martinsson (2001) studied a programme called Swit, which was a joint project between the Swedish government and the Federation of Swedish Industries. It was initiated as an experiment during a two-year period (1997–99) with the aim of providing the Swedish industry with IT-skilled personnel. The set-up was non-traditional and combined traditional education and practice at host companies. The employment effects were compared with the effects of more conventional IT training run by the National Labour Market Board. The results indicated that Swit participants had a 20 percent higher probability to find a regular job than did participants in conventional IT training.

## 4.2 Job creation

Some of the evaluations of labour market training have looked also at the effect of job creation. A few other studies have focused on job creation schemes only. In general, less appears to be known about the different kinds of job creation programmes than about labour market training. The studies of job creation measures are surveyed in *Table 6* and commented below.

Sehlstedt and Schröder (1989) found that recruitment subsidies improved the labour market situation of young people, provided that programme participation constituted a part of a larger “plan” designed to improve their labour market prospects. But the effect of relief work was not significant.

**Table 6** Treatment effects of job creation

Study	Programme and timing	Sample	Dependent Variable	Results
Sehlstedt & Schröder (1989)	Recruitment subsidies & relief work, 1984	Register and survey data on 20–24 years old unemployed, 1984	Labour market situation, 1987	Significant, positive effect of recruitment subsidies if part of an “action plan”. No significant effect of relief work
Edin & Holmlund (1991)	Relief work, 1977–84	Register and survey data on 800 16–24 years old unemployed in the Stockholm area, 1981; and register data on 300 displaced workers in northern Sweden, 1977	Job finding probability in (i) the contemporary; and (ii) subsequent unemployment spell(s)	Significant, negative effect in the contemporary unemployment spell, but significant, positive effect in subsequent unemployment spells
Axelsson, Brännäs & Löfgren (1996)	LMT, work experience schemes, relief work & youth practice, 1993	Register data on 10 000 20–54 years old unemployed, 1993	Employment within 30 days after programme	Work experience schemes and relief work are equivalent alternatives, but youth practice is better
Harkman, Johansson & Okeke (1999)	Recruitment subsidies, trainee replacement schemes, work placement schemes, relief work & work experience schemes, 1996	Register and survey data on 3000 20–54 years old participants. Random selection and representative sample	Employment 1 year after programme	Large significant, positive effects of recruitment subsidies, but significant, positive effects of trainee replacement and work placement schemes as well. No significant effects of relief work and work experience schemes
Carling & Gustafson (1999)	Self-employment grants & recruitment subsidies, 1995–96	Register data on individuals with self-employment grants (9000) or recruitment subsidies (14 000) in 1995 or 1996	The duration of employment	Significant difference between those with self-employment grants and those with recruitment subsidies
Okeke (1999)	Self-employment grants, 1994	Register and survey data on (to) 7000 enterprises (employers)	Enterprise survival rate 3 years after start-up	No significant difference between companies with and without self-employment grants
Larsson (2000)	Youth practice, 1992–93	Register data on 600 20–24 years old participants. Non-participating comparison group through propensity score matching	(i) Yearly income (ii) Employment (iii) Regular education 1–2 years after programme	Significant, negative effect on yearly income and employment; no significant effect on education
Carling & Richardson (2001)	Work experience schemes, LMT, work placement schemes, relief work, computer activity centres, recruitment subsidies, self-employment grants, trainee replacement schemes, 1995–1997	Register data on 25 000 individuals who became unemployed and began their first programme in 1995–97	Unemployment duration	Significantly better results for recruitment subsidies, self-employment grants, trainee replacement schemes and work placement schemes than for LMT, computer activity centres, work experience schemes and relief work

Edin and Holmlund (1991) analysed also, in addition to labour market training (see *Section 4.1*), the employment effects of relief work, using a sample of young people and displaced workers. One result was that relief work participants found regular employment at a slower pace than non-participants. But re-employment probabilities in subsequent unemployment spells were significantly higher for former relief-work participants than for non-participants.

Axelsson et al. (1996) analysed the relative efficiency of labour market training, work experience schemes, relief work and youth practice in terms of the employment probability within thirty days after the programme ended. There were no significant differences between labour market training, work experience schemes and relief work, while youth practice had a significantly larger impact on the short-run probability of leaving unemployment.

Harkman et al. (1999) analysed also (in addition to labour market training and computer activity centres (see *Section 4.1*)) the effect of job creation schemes in 1996 on the probability of having a job a year later. The largest effect was found for individuals benefiting from recruitment subsidises, but work placement schemes and trainee replacement schemes appeared also to have a positive (but smaller) effect. However, relief work and work experience schemes had no significant effects.

Two evaluations of self-employment grants were published in 1999. Okeke (1999) analysed the enterprise survival rate, which we interpret as an indicator of the employment effect. She found no significant difference in the survival rate between enterprises started with and enterprises started without self-employment grants three years after the start-up. This can be interpreted as a positive result if one assumes that enterprises started by unemployed (as an alternative to unemployment) have worse prospects than enterprises started by employees who choose voluntarily to leave their employment to exploit supposedly profitable opportunities. However, potential selection bias was not considered, and the estimates should therefore be interpreted with caution.

A more exhaustive analysis is presented in Carling and Gustafsson (1999). They found that the outflow to unemployment was half as large among those who had obtained self-employment grants as among those who had benefited from recruitment subsidies during 1995–97. This also supports the favourable impression of self-employment grants, as other evaluations (see the earlier discussion in this section) indicate that recruitment subsidies have had positive effects.

Larsson (2000) analysed the effect of youth practice, as well as of labour market training (see *Section 4.1*), in 1992–93. The results of youth practice were very similar to those of labour market training: incomes were 20–30 percent and employment probabilities 18–37 percent lower among former participants than among non-participants one–two years after the programme. But, in contrast to labour market training, youth practice had no decreasing effect on the transition rates to regular education.

Carling and Richardson (2001), finally, analysed the relative efficiency of eight different programmes in terms of employment probabilities. The results indicated that the programmes in which participants conduct regular work (recruitment subsidies, self-employment grants and trainee replacement schemes) or at least obtain practice at a regular workplace (work placement scheme) achieve the best results. Training (both ordinary labour market training and training at computer activity centres) and job creation programmes that do not constitute regular work (work experience schemes and relief work) perform less well in terms of subsequent regular employment. The same ranking holds for

different sub-groups of unemployed and is not affected by the timing of programme entry. This suggests that the results were not due to selection bias.

### 4.3 ALMPs as a whole

We have, so far, surveyed the evidence on the impact of *different types* of programmes. Sianesi (2001) analysed instead the impact of ALMPs as a whole, aggregating all programmes into a composite variable. She combined the longitudinal event data set of the National Labour Market Board (Händel) with a data set (on compensation, previous earnings and working hours) from the unemployment insurance system (Akstat). Around 116 000 individuals, who registered as unemployed in 1994, were followed for five years. The strength of the study is the large number of observations and the long follow-up period, but the results should be interpreted with caution as no distinction is made between different programmes.

The Sianesi study starts out from the Carling and Larsson (2000a) discussion of the possibilities to evaluate the effects of ALMPs in Sweden, as there are few long-term unemployed who never take part in programmes and can therefore serve as a comparison group (see the introduction to *Section 4*). It is therefore possible that evaluations of Swedish ALMPs should be interpreted as measuring the impact of joining an ALMP at a certain point of time rather than later. Given this interpretation, all those unemployed who have not joined an ALMP at a certain point of time can legitimately be used as a comparison group when evaluating the effect of joining at that point of time. This is the approach taken in the Sianesi study. The methodology used is propensity score matching.

The results in the study are contradictory. On the one hand, those who joined a programme (as opposed to waiting) had significantly higher employment probabilities (of the order of magnitude of five percentage points) over the first five years after the start of the programme. This holds for the whole period except for the first six months, which corresponds to the actual length of most programmes. On the other hand, Sianesi found that those who entered a programme at a given date remained unemployed for about two months longer than those who joined at a later date. She also studied the effects of programmes on the probability to retain a job when once found and on the probability to escape unemployment if falling back into it, but could not find any significant effects.

Finally, Sianesi also found that those who entered a programme at a given date remained unemployed for about two months longer than those who joined at a later date. The worst results were obtained for those who joined a programme close to the time of benefit exhaustion (after 14 months of unemployment), possibly mainly in order to renew benefit eligibility.

### 4.4 Search activity

The probability to obtain a job is influenced by the job applicants' search activity. It is therefore of interest to study whether or not ALMPs influence search activity. This is the topic of a number of survey studies, which have examined the difference in search behaviour between programme participants and openly unemployed. The studies are summarised in *Table 7*.

Selhstedt and Schröder (1989) found that young, unemployed non-participants searched both more frequently and in more ways than did relief work participants in the mid-1980s. Similar results were obtained by Edin and Holmlund (1991). According to their study, young unemployed non-participants used more than seven hours a week to look for work in the early 1980s, whereas young relief workers used less than an hour. The survey also

showed that the number of search methods was significantly higher among non-participants than among participants.

**Table 7** Search activity

Study	Programme and timing	Sample	Dependent Variable	Results
Sehlstedt & Schröder (1989)	Relief work, 1984–85	Register and survey data on 500 20–24 years old unemployed, 1984	Search activity & number of search methods	Significant, negative effect
Edin & Holmlund (1991)	Relief work, 1977–84	Register and survey data on 800 16–24 years old unemployed in the Stockholm-area, 1981	Search activity & number of search methods	Significant, negative effect
Ackum Agell (1996)	LMT & job creation programmes (work experience schemes, relief work & trainee replacement schemes), 1993–94	Survey data on 4000 20–54 years old unemployed, 1991	Search activity & number of search methods	Significant, negative effect

Ackum Agell (1996) compared the search behaviour of openly unemployed and participants in ALMPs. She found that participants in training and participants in job creation programmes (work experience schemes, relief work and trainee replacement schemes) behaved very similarly. A significantly larger fraction of non-participants than participants searched actively for work (95 versus 57 percent). She also found that non-participants used more search methods than did participants (3.1 versus 1.9). As much as 68 percent of the participants stated that they did not search for any job at all during the relevant week; the corresponding figure for non-participants was 30 percent. Again, the difference is significant.

Ackum Agell emphasised that participants have less time to search for work than do non-participants. Also, it can be beneficial to society at large that participants do not look for work if the programme forms part of a comprehensive plan to improve their labour market prospects. But this conclusion no longer holds if placement in ALMPs is used to renew eligibility for unemployment insurance. Either way, the studies of search activity do suggest that ALMPs cause locking-in effects.

#### 4.5 Employer attitudes

Employer attitudes towards different categories of job applicants is another factor that influences the possibility of finding a job. Several survey studies have examined the effect of ALMPs in this respect. The studies are summarised in *Table 8*.

**Table 8** Employer attitudes

Study	Source of information	Sample	Dependent variable	Results
Agell & Lundborg (1995)	Survey, 1991	Personnel managers at ~ 150 companies	Share who believes that (i) unemployed non-participants; and (ii) participants are potentially less productive than other job seekers	(i) Openly unemployed 21% (ii) Programme participants 18 %
Behrenz (1998b)	Survey, 1995	Company representatives at ~ 800 companies	Share who automatically sorts out (i) unemployed non-participants; and (ii) participants	(i) Openly unemployed 4.2 % (iia) LMT participants 1.2 % (iib) Participants in other programmes 1.6 %
Agell & Lundborg (1999)	Follow-up survey, 1998	Personnel managers at ~ 150 companies	Share who believes that (i) unemployed non-participants and (ii) participants are potentially less productive than other job seekers	(i) Openly unemployed 27 % (iia) LMT participants 15 % (iib) Participants in work experience schemes/ relief work 20%
Klingvall (1998)	Survey, 1998	Employers at ~ 280 workplaces	Share who prefers to hire one category rather than another	(i) Openly unemployed 2 % (iia) LMT participants 30 % (iib) Participants in other programmes 20 %

Agell and Lundborg (1995, 1999) found that the share of personnel managers who expected a prolonged period of unemployment to have negative effects on worker productivity increased from 21 to 27 percent between 1991 and 1998. In 1991, 18 percent of the respondents also regarded programme participants as potentially less productive than other job applicants. In the 1998 survey, questions were more specific. The answers indicated that 15 percent of the personnel managers asked considered participants in labour market training to be potentially less productive than an otherwise identical person; the corresponding figure for work experience schemes and relief work was 20 percent. However, neither the difference between (i) labour market training and work experience schemes/relief work, nor the differences between (ii) various programmes and open unemployment were statistically significant.

The survey evidence presented in Behrenz (1998b) indicated that potential employers regard open unemployment more negatively than programme participation. 4.2 percent of the company representatives asked stated that they automatically disregarded job applica-

tions made by openly unemployed. The corresponding figures for individuals who had participated in labour market training or some other programme were 1.2 and 1.6 percent, respectively. The differences between ALMPs, on the one hand, and open unemployment, on the other hand, was statistically significant, while the difference between labour market training and other programmes were not.

In the survey by Klingvall (1998), only 2 percent of the employers claimed that they would hire an openly unemployed person rather than a programme participant. 30 (20) percent stated that they would rather hire a former participant in labour market training (other ALMPs) than an openly unemployed. These differences are statistically significant.

Although questions and estimated effects differ, the studies of attitudes suggest that employers judge former ALMP participants more favourably than unemployed who have not participated in programmes. This evidence also suggests that labour market training is preferred to the other programmes.

#### **4.6 Conclusions from the microeconomic studies**

Although results vary a lot between different studies, a number of conclusions can be drawn.

One observation is that the estimated effects of labour market training seem to differ consistently between the 1980s and 1990s. Evaluations of training acquired during the first half of the 1980s show – at least over a few years – large positive effects on participants' employment and/or income. Evaluations of training that took place in the 1990s find instead insignificant or significantly negative effects. As will be discussed in *Section 7.3*, the less favourable results in the 1990s are likely to be explained by factors such as the large size of programmes, the use of programmes to re-qualify participants for unemployment benefits, and the large shortfall of demand.

Another observation refers to differences between short-run and long-run effects of labour market training. The short-run effects are usually insignificant or even negative. However, with a time horizon of a few years the estimated effects are more positive (1980s) or are, at least, no longer negative (1990s). A conceivable explanation is that education increases the reservation wages of participants (see *Section 3.3*).

Also, there is some evidence to suggest that income and employment effects increase with the length of training. Here, the amount of research is very small.

Less is known about the treatment effects of different job creation programmes. Conclusions here are therefore less certain. But the evidence seems to suggest that self-employment grants, recruitment subsidies, trainee replacement schemes and work placement schemes have had positive treatment effects in terms of employment and income. Work experience schemes and relief work do not seem to have had such effects.

The estimated effects of programmes for young people vary. Several earlier studies of the 1980s found positive effects, at least for some programmes in some circumstances and in the long run. But these results were based on small samples and cannot be generalised to the population at large. The one study of the 1990s (Larsson, 2000) found negative employment and income effects of both labour market training and youth practice. It is not clear whether these results should be interpreted to reflect specific characteristics of youth programmes or relate to the age group as such.

On the whole, the microeconomic evidence on the effects of ALMPs gives a rather disappointing picture. In addition, the studies of search activity point to important locking-in effects of ALMPs. However, the survey studies of employer attitudes give a consistently

positive picture of ALMPs. It is not clear how these results square with those in the other studies.

## 5 Macroeconomic studies

This section surveys the macroeconomic studies of the total (general equilibrium) effects, of ALMPs in Sweden. Doing this we follow our earlier classification in *Table 4*.

There are some general methodological problems in the macroeconomic studies. It may be difficult to obtain precise estimates of effects because the number of observations that can be used in the econometric analyses is often small. Another problem is two-way causality. It is not only the case that ALMPs may affect (un)employment, but changes in the labour market situation may also trigger political decisions to adjust the volume of ALMPs. This may give rise to simultaneity bias and identification problems. We repeatedly return to how this issue has been handled in various studies below.

*Section 5.1* reviews the macroeconomic evidence on the effects on the matching process. *Section 5.2* discusses direct crowding-out (displacement) effects and *Section 5.3* the effects on labour force participation. *Section 5.4* surveys the studies of aggregate wage-setting effects, which are the net of several of the effects discussed in *Section 3*. *Section 5.5*, finally, reviews reduced-form estimations of the total effects on (un)employment.

### 5.1 Beveridge curves, matching functions and migration relationships

A first type of macroeconomic studies directly shed light on the efficiency of the matching process. These studies concern *Beveridge curves*, *matching functions* and *geographical mobility*.

Somewhat surprisingly, only two studies of Sweden have looked at the effects of ALMPs on matching in a *Beveridge curve* context. Jackman et al. (1990) found that an expansion of ALMPs (measured as the *accommodation ratio*, i.e., the number of programme participants divided by the sum of openly unemployed and programme participants) shifted the Beveridge curve downwards, when it was defined as the relationship between vacancies and *open unemployment*. But this result does not highlight the effect on matching efficiency, as increased programme placements must for arithmetic reasons reduce open unemployment at a given number of vacancies even if matching efficiency is unchanged. The relevant question is instead whether the Beveridge curve in terms of *total unemployment*, i.e., the sum of open unemployment and programme participation, is affected. Calmfors (1993) recalculated the Jackman et al. results in this way and found no significant effect on the relationship between vacancies and total unemployment. Calmfors also reported own estimations giving the same result.

Neither of the two available Beveridge curve studies thus show any effects of ALMPs on matching efficiency. But the main conclusion is that we largely lack knowledge of the Beveridge curve effects as there are no studies of the 1990s.

Edin and Holmlund (1991) is the only study of *matching functions*, which relates the number of hirings to the numbers of vacancies and unemployed, on Swedish data. The authors found that programme participation contributes to matching, but that the effect is only half that of open unemployment.<sup>10</sup> This suggests that locking-in effects dominate

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<sup>10</sup> The authors could not reject the hypothesis that relief work and labour market training have the same effect (and that this effect is half that of open unemployment). However, when the effects of training and relief work were estimated separately, it could not be rejected that training and unemployment have the same effect.

treatment effects of these programmes in the short run. But again the main conclusion is the lack of empirical knowledge.

Geographical mobility is one important dimension of the matching process. Hence, the effects of ALMPs on this variable may serve as a proxy for the effects on matching. Here, several studies have been made. They are summarised in *Table 9* and are briefly commented below.

**Table 9** The effects of ALMPs on geographical mobility

Study	Data	Results
McCormick & Skedinger (1991)	24 counties, 1968–85	Locking-in effects
Nilsson (1995)	24 counties, 1966–93	Locking-in effects
Westerlund (1997)	24 counties, 1970–89	Locking-in effects or insignificant results
Heiborn (1998)	24 counties, 1964–93	Mixed results
Westerlund (1998)	24 counties, 1970–89	Mixed results
Widerstedt (1998)	541 males, 1981–91	No effects
Fredriksson (1999)	24 counties, 1968–93	Small effects

McCormick and Skedinger (1991) found that increased ALMP volumes at the regional (county) level gave rise to higher open unemployment rates. There are many possible interpretations of this result, but the explanation favoured by the authors is that ALMPs contributed to lower mobility.

Nilsson (1995), Westerlund (1997), and Heiborn (1998) all estimated models of migration between the Swedish counties. Nilsson found some evidence of locking-in effects: increased programme participation in a county led to a significant decrease in out-migration. Some of Westerlund's estimates also pointed to locking-in effects, while others gave insignificant results. Heiborn's results were not stable over different specifications, so it is hard to draw any firm conclusions from her study.

Westerlund (1998) studied the effects of mobility grants, labour market training, and relief work on mobility across county borders. Mobility grants had a marginally significant effect on total migration, while training and relief work gave significant locking-in effects. Looking separately at migration of the unemployed and the employed, all programmes had a positive effect on the mobility of the unemployed and a negative effect on other (potential) movers.

Widerstedt (1998) estimated models of individuals' mobility probabilities, but found no significant effects of ALMPs.

Fredriksson (1999), finally, looked at regional adjustments to employment shocks at the county level. The main finding was that the brunt of the adjustment burden falls on mobility: ALMPs lower mobility marginally, and, hence, locking-in effects seem to dominate.

The results concerning geographical mobility are thus mixed. But most of the evidence suggests that ALMPs have reduced mobility.

## 5.2 Direct crowding-out (displacement)

As discussed in *Section 3.5*, job creation programmes are likely to cause direct displacement. The studies of this fall into two categories: (i) *survey* studies; and (ii) *econometric* studies of labour demand.

### 5.2.1 Survey studies

In a number of surveys, employers, programme participants and employment officers have been asked whether they (i) believe that the work performed by programme participant(s)

would have been performed by *anyone* in the absence of the programme (*substitution effects*); and (ii) in some cases, if this question was answered in the affirmative, whether the same person(s) would have been employed (*deadweight effects*).

Such surveys suffer from a number of problems. First, participants may have an exaggerated view of their importance for the activity concerned. This could lead to an upward bias in the estimated displacement. Second, both employers and employment officers have incentives to avoid the impression that programmes are abused, which could give a bias in the opposite direction. Third, respondents are not likely to be able to evaluate the extent to which programmes crowd out employment in other workplaces than that associated with the programme.

A number of survey studies are summarised in *Table 10*. Although the results vary considerably, all studies but one indicate that direct displacement occurs. In most cases the estimated displacement is substantial.

**Table 10** Survey studies of direct displacement<sup>a</sup>

Study	Method	Programme	Results
Sehlstedt & Schröder (1989)	Interviews with participants and supervisors	RS for youth	Participants: 49 % Supervisors: 23 %
LO (1993a, 1993b, 1994a, 1994b)	Questionnaires to participants	WES	20–39 %
Temo (1993, 1994, 1995) <sup>b</sup>	Telephone interviews with participants, employers and employment officers	WES	Participants: 17 % 1993, 12 % 1994; Organisers: 3–7 %
NUTEK (1994)	Questionnaires to participants and employers	WES	About 30 % according to both participants and employers
AMS (1995)	Questionnaires to organisers	RS	36 % (of which slightly more than half would have recruited the same person).
Hallström (1995)	Interviews with participants	WES	20–25 %
Anxo & Dahlin (1996)	Questionnaires to employers	TES, GES	84 % (GES); 69 % (TES)
AMS (1997)	Questionnaires to participants	RW, WPS, MYP, WES, TRS, RS	RW: 24 %; WPS: 16 %; MYP: 10 %; WES: 8 %; RS: 48 %; TRS: 42 %
AMS (1998a)	Questionnaires to participants	WES, TRS, RS, MYP, RW, WPS	WES: 13 %; TRS: 51 %; RS: 40 %; MYP: 14 %; RW: 27 %; WPS: 21 %
AMS (1998b)	Questionnaires to participants and employers	RS, RW, WES, TRS, WPS, TPJ, RJ, MYP	RS: 35 %; RW: 14 %; WES: 0 %; TRS: 32 %; WPS: 8 %; TPJ: 1 %; RJ: 1 %; MYP: 3 %
Johansson (1999)	Questionnaires to participants and employment officers	RJ	Participants: 16 %, 26 % <sup>c</sup> ; Employment officers: 11 % <sup>d</sup> .

*Notes:*

- The following abbreviations are used in the table: WES – work experience schemes, WPS – work placement schemes, RW – relief work, GES – general employment subsidy, MYP – municipal youth programmes, TPJ – temporary public jobs, RJ – resource jobs, TES – targeted employment subsidy, RS – recruitment subsidy, TRS – trainee replacement schemes.
- The Ministry of Labour commissioned the study and the results were reported in AMS (1997).
- This refers to answers to the question whether the participant believes that the employer actually could have afforded to hire someone in the absence of the programme.
- The fraction that answered "Yes, in most cases".

A way to summarise the information in *Table 10* is to compute the average displacement for each programme according to the studies shown. The results are reported in *Table 11*, where the programmes have been ranked according to the size of the average displacement effect.<sup>11</sup> There is a clear tendency that the closer to the regular labour market a programme is, the larger is the estimated displacement. For recruitment subsidies, trainee replacement schemes, general employment subsidies, and targeted employment subsidies, the estimated displacement effects are between 39 and 84 percent.

**Table 11** Average direct displacement effects according to the studies in *Table 10*.

Programme	Average displacement effect (%)	Number of studies
Temporary public jobs (TPJ)	1.0	1
Municipal youth programmes (MYP)	9.0	3
Resource jobs (RJ)	14.3	3
Work placement schemes (WPS)	15.0	3
Work experience schemes (WES)	15.6	11
Relief work (RW)	21.7	3
Recruitment subsidies (RS)	38.5	6
Trainee replacement schemes (TRS)	41.7	3
General employment subsidy (GES)	69.0	1
Targeted employment subsidy (TES)	84.0	1

In addition to the studies in *Table 10*, a number of earlier studies (Peterson and Vlachos, 1978; AMS, 1981; AMS, 1983; AMS, 1985; RRV, 1989) used survey methods to estimate the total employment effects of temporary or permanent wage subsidies. The identified employment effects were generally small. So, these studies, too, suggest substantial displacement.

### 5.2.2 Econometric studies of direct displacement

The econometric studies identify the relationship between programmes and regular employment by comparing actual employment with the employment that would have been realised in the absence of programmes. Most of the studies have estimated traditional labour demand schedules augmented with measures of the volume of programmes.

A fundamental problem for econometric studies of direct displacement is that the relation between programmes and employment goes both ways: employment may depend on programme participation, but the size of programmes is also likely to depend on (un)employment. This *simultaneity problem*, discussed in the introduction to *Section 5*, may give rise to biased estimates of the effects of ALMPs. The problem is considered in different ways and to a various extent in the studies.

The econometric studies of displacement are much fewer than the survey studies. The results are summarised in *Table 12*.

<sup>11</sup> The table should be interpreted with caution, as the averages derive from studies using different methods, and some programmes have been subject to a large number of studies and others to only a few ones.

**Table 12** Econometric studies of direct displacement

Study	Programme	Results
Gramlich & Ysander (1981)	RW	Road construction: 100 %; Health and welfare: 0 %
Forslund (1996)	WES, LMT, RW, youth programmes, TRS	WES: 0 %; LMT: 0 %; RW: 84 %; youth programmes: 76 %
Forslund & Krueger (1997)	RW	Construction workers: 69 %; Health and welfare 0 %
Löfgren & Wikström (1997)	WES, LMT, RW, youth programmes, TRS	WES: 0 %; LMT: 0 %; RW: 0 %; youth programmes: 94 %; TRS: 0 %
Dahlberg & Forslund (1999)	RW, LMT, subsidised employment	RW: 66 %; LMT 0 %; subsidised employment: 65 %
Edin, Forslund & Holmlund (1999)	Youth programmes	76 %

*Note:* Only results that are significantly different from zero are shown. Where the authors have estimated several models, we show the results preferred by the authors. For abbreviations, see *Table 10*. In addition to the notation in the table, LMT denotes labour market training.

A first econometric study of direct displacement was carried out by Gramlich and Ysander (1981) using aggregate data for the period 1964–77. Their results were that relief work in road construction crowded out 100 percent regular employment, whereas there was no significant effect of relief work in health and welfare.

Forslund and Krueger (1997) used panel data for counties for a period encompassing the 1980s. Their results were similar to those of Gramlich and Ysander. Forslund and Krueger found significant displacement (36 percent – 69 percent) in the construction sector, but no significant results for health and welfare. The authors handled the simultaneity problem in two ways. First they ran vector autoregressions to check whether relief work “explains” employment or if it is the other way around. Second, they estimated “displacement equations” for a sector where there should be no displacement.<sup>12</sup> The results indicated that the effect is from relief work to employment and not the other way around.

Forslund (1996) and Dahlberg and Forslund (1999) used data at the municipal level. They distinguished between subsidised employment, relief work and labour market training.<sup>13</sup> Subsidised employment and relief work were found to give displacement effects of around 65 percent, while there were no significant effects of training. Dahlberg and Forslund treated the simultaneity problem in several ways, including IV (instrumental variables) estimations.

Sjöstrand (1997) claimed in a comment to Forslund (1996) that the analysis of the latter was built on a mis-specified model and that one finds no displacement effects with a correctly specified model. Löfgren and Wikström (1997) reviewed Forslund (1996) and

<sup>12</sup> The logic is the following. Forslund and Krueger estimated a displacement effect for the durable manufacturing sector, in which there were no relief works. Such estimates test whether or not the displacement results only are spuriously reflecting cyclical patterns in both employment and relief work.

<sup>13</sup> Relief work was singled out as a separate category to allow comparisons with earlier work.

Sjöstrand (1997) and found shortcomings in both studies. With the preferred specification of Löfgren and Wikström, only youth programmes gave rise to displacement (94 percent).

Edin, Forslund and Holmlund (1999) analysed the effects of youth programmes on youth employment and found large displacement effects (76 percent evaluated at the means of the variables). The simultaneity problem was handled mainly by means of IV methods.

Generally, the econometric studies give higher estimates of displacement than the survey studies. Typical figures are well above 60 percent. One possible explanation for the difference in results is that displacement is partly the result of distorted competition. Such effects are clearly difficult to assess for the respondents in survey studies. Another difference between the two types of studies is that many of the econometric investigations do not distinguish between different programmes. Hence, the effects are averages over several programmes. As an example, both work experience schemes and youth practice were included in “subsidised employment” in Dahlberg and Forslund (1999). The average displacement effect for subsidised employment in this study was 65 percent. This figure would, for example, be consistent with youth programmes crowding out significantly more than 65 percent and work experience schemes crowding out significantly less.

### 5.3 Labour force participation

The effects of ALMPs on labour force participation is yet another area where research efforts have been modest. We are aware of only two studies that deal directly with the issue: Wadensjö (1993) and Johansson and Markowski (1995). Both studies indicate strong positive effects of ALMPs on labour force participation. Wadensjö’s estimates imply that an increase in programme participation by 1 percentage point increases labour force participation by 1 – 1.5 percentage points in the following year. Johansson and Markowski found an effect that is roughly half the size of the effect found by Wadensjö.

In *Section 3.2* we discussed how a positive effect of ALMPs on labour force participation could lead to a fall in employment as a share of the labour force, at the same time as employment increases as a share of the population. For this reason, different ways of measuring employment (relative to the labour force or to the population) in other studies can give indirect evidence on the labour force effects. For example, Dahlberg and Forslund (1999) found that direct displacement was larger when employment was measured as a share of the labour force than when it was measured as a share of the population. This result is also consistent with a positive effect of ALMPs on labour force participation. The implied effect was rather large – evaluated at sample means, the results suggested that 100 extra programme participants increases labour force participation by 62 persons.<sup>14</sup>

However, the results on labour force participation should be interpreted with caution. If programme participation has been used as a means to renew eligibility for unemployment benefits, the increase in labour force participation has not necessarily meant an increase in *effective* labour supply.

### 5.4 Wage setting

For a number of reasons discussed in *Section 3*, ALMPs may affect wage setting. The mechanisms involve effects on matching, the competition in the labour market, the wel-

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<sup>14</sup> Löfgren and Wikström (1997) also found larger displacement effects when employment was measured relative to the labour force than when it was measured relative to the population.

fare and productivity of job seekers, and the allocation of the labour force across sectors. The net effect is theoretically unclear. Estimates of *wage-setting schedules* can throw light on this issue. Starting with Newell and Symons (1987), Calmfors and Forslund (1990, 1991), and Holmlund (1990), a large number of such studies have been undertaken. In all cases, real wage equations including measures of the unemployment and the volume of labour market programmes as explanatory variables have been estimated. Most of the studies have used data for industry or the private sector. The main results are summarised in *Table 13*.

**Table 13** The effects of ALMPs on the real wage<sup>a</sup>

Study	The effect of ALMPs	
	Short run	Long run
Newell & Symons (1987)	0	0
Calmfors & Forslund (1990, 1991)	+	+
Calmfors & Nymoen (1990)	+	+
Holmlund (1990)	na	+
Löfgren & Wikström (1991) <sup>b</sup>	+/0	0/+
Skedinger (1991) <sup>c</sup>	+	+
Forslund (1992) <sup>d</sup>	+/-	+/-
OECD (1993) <sup>e</sup>	-	-
Edin, Holmlund & Östros (1994) <sup>f</sup>	0/0/0	0/0/0
Forslund & Risager (1994) <sup>g</sup>	0	0
Forslund (1995)	0	+
Okeke (1998) <sup>h</sup>	na	-
Johansson, Lundborg & Zetterberg (1999) <sup>i</sup>	+/+	+/+
Rødseth & Nymoen (1999)	0	+
Forslund & Kolm (2000)	0	0
Thomas (2000)	-	na

*Notes:*

- A "+"-sign indicates a significantly positive effect, a "-"-sign a significantly negative effect and "0" no significant effect.
- The first effect refers to relief work, the second to labour market training.
- Data pertain to different groups of employees in mining and manufacturing 1971–88. The programme studied is relief work.
- The data refer to twelve unemployment insurance funds. The first effect refers to relief work, the second to labour market training.
- The regression covers the period 1985–90 for a cross-section of 19 OECD countries. A number of effects were assumed to be equal across countries, whereas the effect of ALMPs was estimated separately for each country.
- The estimates pertain to individual wages for workers in engineering 1972–87. The effects refer to total programmes, labour market training and relief work, respectively. The results in the table are IV estimates. OLS estimates gave significant, wage-reducing effects of total programmes and labour market training both in the short run and in the long run, and of relief work in the long run.
- Separate estimates for industry and the rest of the business sector.
- The estimated models are "wage curves" on micro data. Okeke did not consistently find that ALMPs have contributed to less wage pressure. The shown negative effect was, however, found in most specifications.
- Effects were estimated for the periods 1965–90 and 1965–98, respectively.

*Table 13* shows mixed results. Many studies find that larger ALMPs increase wage pressure, but many studies do not find any significant effect. Only three studies (OECD, 1993; Okeke, 1998; Thomas, 2000) suggest that ALMPs reduce wage pressure. Most studies do not distinguish between different programmes. No consistent pattern emerges from the three studies (Löfgren and Wikström, 1991; Forslund, 1992; Edin, Holmlund and Östros, 1994) that estimate separate effects of labour market training and relief work.

Most of the studies cover periods ending before the deep recession of the 1990s. As both unemployment and ALMPs reached peak levels during this recession, it is uncertain

to what extent the results from earlier studies apply to the 1990s. To the extent that compensation levels in programmes were lowered and the expected treatment effects on the probability of finding a job or on future income deteriorated, one should expect less unfavourable (or more favourable) wage effects of ALMPs. However, Johansson et al. (1999), Rødseth and Nymoer (1999), and Forslund and Kolm (2000) did not find any significant changes in the wage-setting behaviour between the 1990s and earlier periods.

Simultaneity problems of the same kind as for studies of displacement effects may be present also in the estimation of wage effects of ALMPs. However, because it probably takes time for wage changes to influence employment and for employment changes to trigger changes in programme volumes, the problem is likely to be less severe in this case. A more serious problem may be that programme participation covaries with long-term unemployment, so that adverse wage-setting effects of ALMPs could reflect that higher long-term unemployment reduces the competition for jobs that insiders meet (see *Section 3.2*).

## 5.5 Reduced-form estimates

A last type of studies is *reduced-form* estimates of the effects of ALMPs on (un)employment, i.e. estimates of the total net effects through all channels discussed in *Section 3*. Put differently, these estimations examine how the intersection between the wage-setting and employment schedules in *Figure 4* is affected by the size of ALMPs.

The results from four reduced-form studies are summarised in *Table 14*.

**Table 14** The effects of ALMPs in reduced-form estimates

Study	Period	Results
Ohlsson (1993, 1995)	Vector auto regressions, 1969–90	Job creation schemes crowd out regular employment and lower open unemployment. No significant effects on wages.
Skedinger (1995)	Vector auto regressions, 1979–91	Youth programmes crowd out regular youth employment (110% in the short run).
Forslund (1995)	Reduced form, 1960–93	No effect on open unemployment of aggregate ALMPs.
Calmfors & Skedinger (1995)	Reduced form, 1966–90	Job creation schemes crowd out regular employment; unstable results for labour market training.

Ohlsson (1993, 1995) estimated vector autoregressions (VARs) on aggregate quarterly time-series data. The estimated model was used to study the effects of an expansion of subsidised employment. The result was displacement in the order of magnitude of 50 per cent during the first quarter. During later quarters the estimates are too imprecise to warrant any conclusions. Ohlsson also looked at the effects of both subsidised employment and labour market training on wage setting, but found no significant effects. This was also the case for the effects of training on unemployment.

Skedinger (1995) estimated VARs to analyse the effects of subsidised employment for youth on regular youth employment. The results imply more than total displacement as

soon as after one quarter. The effect becomes smaller over time (partly because programme volumes decline), but is statistically significant during the first five quarters.<sup>15</sup>

Forslund (1995) estimated a reduced-form unemployment equation. The results indicated that ALMPs led to lower open unemployment in the short run, but not in the long run. This would indicate complete displacement in the long run.

Calmfors and Skedinger (1995), finally, studied the relationship between total unemployment on the one hand and subsidised employment and labour market training on the other hand using a panel of the 24 counties between 1966 and 1990. The authors tried to handle the simultaneity problem through instrumental variables methods (one assumption being that the political majority in a county influences the volume of ALMPs). Subsidised employment was found to cause large displacement effects (of the order of magnitude of 60–90 percent), whereas the results for training were very unstable.

To summarise, the reduced-form studies suggest that especially job creation schemes tend to reduce regular employment, but also that they probably contribute to lower open unemployment.

## 5.6 Conclusions from the macroeconomic studies

Just as in the case of microeconomic studies, the overall picture from the macroeconomic studies of ALMPs in Sweden is rather disappointing.

There is little evidence that ALMPs as a whole make the matching process more efficient; rather the studies of geographical mobility suggest the opposite. There is evidence of large direct displacement effects of those job creation schemes that most closely resemble regular employment, but not of labour market training. Some evidence indicates that programmes tend to raise wage pressure, whereas other evidence does not point in this direction. But there is little to suggest that ALMPs help reduce wage pressure. Reduced-form estimates seem to show that programmes (at least job creation schemes) tend to reduce regular employment, even though they may help reduce open unemployment.

The most favourable effects of ALMPs refer to labour force participation, which seems to be increased by large programmes.

## 6 Reduced-form studies on OECD data

Beginning with the influential study by Layard et al. (1991), a large number of studies have tried to explain unemployment differences among OECD countries by differences in labour market institutions. The earlier studies explained cross-sectional variations in unemployment rates with cross-sectional variation in labour market institutions. Later studies have used panel data to exploit both cross-sectional and time-series variations in the data. Most of these studies have examined the influence of the size of ALMPs. As these studies have usually been interpreted to give very favourable results for ALMPs, it may be of some interest to compare their results with results from the studies of Sweden that we have surveyed.

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<sup>15</sup> Holmlund (1995) criticised Skedinger's assumption that aggregate unemployment is exogenous, and showed that displacement falls to 40 percent if this assumption is dropped. Skedinger's analysis was also criticized by Sjöstrand (1996a). See also Sjöstrand (1996b) and Skedinger (1996a, b).

## 6.1 Main results

The results in the studies of the OECD countries cannot be directly compared with those in the studies of Sweden. The reason is that the former studies use measures of expenditures on ALMPs (the only comparable measures available for all OECD countries), usually spending per unemployed person as a fraction of GDP per capita (which was introduced by Layard et al., 1991), as explanatory variables, and open unemployment as the dependent variable. This does not allow direct estimates of how total (and open) unemployment is affected by programme participation, i.e., of how much displacement occurs. To derive these effects, the results in the studies on OECD data have to be recalculated using certain assumptions. The Appendix describes how this was done. The results are shown in *Table 15*.

**Table 15** The effects of ALMPs on (un)employment in cross-section and panel data studies of the OECD countries

Study	Countries and period	Effect on:	Results <sup>a</sup>
Layard, Nickell & Jackman (1991)	20 OECD countries; 1983–88; cross-section data	Open unemployment	- (-1.53)
		Total unemployment	0 (-0.53)
OECD (1993)	19 OECD countries; 1983–88; cross-section data	Open unemployment (Layard et al. measure of ALMPs)	-
		Open unemployment (ALMP expenditures as a fraction of mean wage multiplied by the labour force)	0
Heylen (1993)	18 OECD countries; second half of the 1980s; cross-section data	Real-wage sensitivity to unemployment variations <sup>b</sup> (effect of expenditures on total ALMPs, employment service and labour market training, respectively)	-
		Real-wage sensitivity to unemployment variations (effect of job creation measures)	0
Zetterberg (1993)	19 OECD countries; 1985–91, panel data	Open unemployment (ALMP expenditures as a fraction of total expenditures on labour market policies)	- (-1.49)
		Total unemployment	- (-0.49)
Jackman, Layard & Nickell (1996)	20 OECD countries 1983–88 and 1989–94; panel data	Open unemployment	0 (-0.06)
		Long-term open unemployment	-
		Short-term open unemployment	0
		Total unemployment	+ (0.94)
Scarpetta (1996)	17 OECD countries; 1983–93; panel data	Open unemployment	- (-0.51)
		Total unemployment	+ (0.49)
		Employment as a fraction of the population	+
Forslund & Krueger (1997)	OECD countries; 1983–88 and 1993; cross-section data	Open unemployment 1983 – 88; (Zetterberg measure of ALMPs) <sup>c</sup>	- (-0.83)

		Total unemployment 1983 – 88; (Zetterberg measure of ALMPs)	0 (0.17)
		Open unemployment 1983 – 88, (ALMP expenditure as a fraction of GDP)	0
		Open unemployment 1993; Zetterberg measure of ALMPs	+
Elmeskov, Martin & Scarpetta (1998)	OECD countries; 1983–95; panel data	Open unemployment	- (-1.18)
		Total unemployment	0 (-0.18)
Nickell & Layard (1999)	20 OECD countries; 1983–88 and 1989–94; panel data	Open unemployment	- (-0.18)
		Long-term open unemployment	-
		Short-term open unemployment	0
		Total unemployment	+ (0.82)
		Employment as a fraction of the population	0
Blanchard & Wolfers 2000	20 OECD countries; 1960–95; panel data with five-year averages	Open unemployment	- (-1.43)
		Total unemployment	- (-0.43)

*Notes:*

- The minus and plus signs indicate the signs of the effects on the respective variables. A zero indicates a non-significant effect. Numbers in parentheses indicate the calculated effect on the variable in question of an increase in the participation in ALMPs by 1 percentage point of the labour force.
- According to conventional theory, the sensitivity of the real wage to variations in unemployment is negatively related to equilibrium unemployment (Layard et al., 1991; Nickell and Layard, 1999).
- See the entry for Zetterberg (1993) above in the table.

Most of the studies reported in the table support the hypothesis that an expansion of ALMPs contribute to lower open unemployment. Two of the studies also show a larger effect on long-term than short-term open unemployment (Jackman et al., 1996; Nickell and Layard, 1999). This is, of course, to be expected, because programme placement can be used to interrupt long unemployment spells. However, looking at the calculated effects on total unemployment (the sum of open unemployment and programme participation), the picture is different. Some studies indicate that total unemployment increases when ALMPs expand, others that it decreases. A couple of studies seem also to find insignificant effects.

## 6.2 The interpretation of the results

There is reason to suspect that the problem of simultaneity bias in the studies reported above is quite serious. The reason is that the Layard *et al.* measure of ALMPs used in most of the studies, i.e. spending per unemployed person as a fraction of GDP, is likely to covary negatively with unemployment (OECD, 1993; Forslund and Krueger, 1997). Some of the studies have just neglected this problem. Others have tried to address it in various ways. OECD (1993) substituted ALMP expenditure as a fraction of the mean wage multiplied by the labour force, and Forslund and Krueger (1997) ALMP expenditure as a fraction of GDP, for the Layard et al. measure. In both studies the introduction of the alternative measure resulted in insignificant estimates of the effects on open unemployment.

Elmeskov et al. (1998) used the average of the Layard et al. measure over the whole time period studied in order to reduced the problems of simultaneity, whereas Nickell and Layard (1999) divided ALMP expenditures by the number of unemployed persons in an earlier time period. It is noteworthy that both these studies seem to indicate that an expansion of ALMPs raises total unemployment. On the whole, the results of ALMPs seem to become less favourable when the problem of simultaneity bias is addressed.

One should also note that the reported results refer to unemployment as a share of the *labour force*. Our previous discussion indicates that results may be more favourable if unemployment is instead measured as a fraction of the population, as ALMPs may influence labour force participation positively (see *Sections 3.2 and 5.3*). Two of the studies reviewed could point to such an effect. Nickell and Layard (1999) did not find any significant decreasing effect of ALMPs on employment as a fraction of the population at the same time as their results imply an increase in total unemployment as a fraction of the labour force. Scarpetta (1996) found that ALMPs contribute to a lower share of inactive persons in the population.

## **7 Conclusions**

This section gives our conclusions. *Section 7.1* summarises the lessons regarding the various mechanisms through which active labour market policy can work and the evidence on net employment effects. *Section 7.2* draws conclusions on the relative efficiency of various ALMPs. *Section 7.3*, finally, makes a tentative evaluation of active labour market policy in Sweden and tries to draw some general policy conclusions.

### **7.1 The various mechanisms of ALMPs**

The empirical studies surveyed highlight the following mechanisms or complexes of mechanisms of active labour market policy (see *Section 3*):

- effects on the matching process and the competition for jobs, as well as on productivity and the allocation of labour
- direct crowding-out effects
- effects on the wage pressure in the economy, which are the net of the effects on matching efficiency, the effects on the competition for jobs, the accommodation effects, the effects of re-allocation of labour and productivity effects.
- the net effect on regular employment (and open unemployment)

#### **7.1.1 Matching efficiency and the competition for jobs**

The effects on matching efficiency and the competition for jobs are highlighted in both microeconomic and macroeconomic studies. These effects are likely to be correlated with the effects on the productivity of job seekers and the allocation of labour (to the extent that ALMPs raise the productivity of the participants and re-allocate labour from low-demand to high-demand areas, matching efficiency and the competition for jobs are also likely to increase). On the whole, there is little support for the view that the active labour market policy in Sweden in the 1990s had positive effects in these respects.

Macroeconomic studies of geographic mobility seem to imply that ALMPs have rather tended to lock in labour. Although the microeconomic studies of the effects of labour market training on individuals in the 1980s found positive employment and income effects, this does not apply to the 1990s: the studies of the later period have instead usually

found insignificant or negative effects. There are fewer studies of job creation measures, and here the results vary more (see *Section 7.2* below).

The most favourable results for the effects of ALMPs on individuals are obtained in survey studies of employer attitudes, which show that employers are more positive towards job seekers who have participated in such programmes than to those who have been only openly unemployed. But on the other hand, participants in ALMPs seem to search less actively for jobs than the openly unemployed.

There is also some evidence that ALMPs in Sweden may have raised labour force participation, which might potentially lead to more competition for jobs. But the number of studies is too small to warrant more definite conclusions. There is also the question to what extent such a "registered" increase in labour force participation translates into effective supply rather than just raising the possibilities to collect benefits. Nor is it possible to draw any firm conclusions on the effects of ALMPs on labour force participation from the panel studies on OECD data.

### **7.1.2 Direct displacement**

Both survey studies and econometric macro studies indicate that job creation schemes have crowded out regular employment to a substantial degree. Labour market training does not appear to have had such effects. The direct crowding-out effects are considerably larger in the econometric studies (usually around 60–70 percent) than in the survey studies (usually 15–40 percent). These differences may be explained by the fact that part of the displacement occurs through general equilibrium effects, i.e. through distortions of competition that crowd out jobs in other work places than those the respondents to the survey questions can judge.

### **7.1.3 Wage pressure**

The effect of ALMPs on wage pressure is the net of a number of effects that work in different directions: effects on matching efficiency, competition effects, accommodation effects, effects on reservation wages and re-allocation effects. A large number of Swedish studies of the wage-setting relationship has examined this net effect.

The results are not clear-cut. Many studies have found that an expansion of ALMPs has increased wage pressure. Nearly as many studies have found no significant effect at all. Fewer studies have found a wage-reducing effect. The conclusion is that Swedish ALMPs are unlikely to have reduced wage pressure, but it is unclear whether they have raised wages or had no effect at all.

The results from the macroeconomic wage-setting studies are consistent with the results from the macroeconomic studies of geographical mobility and the majority of microeconomic studies of the effects on individual participants discussed in *Section 7.1*. If ALMPs do not have positive effects on matching efficiency (mobility) and the competition for jobs, they should not be expected to reduce aggregate wage pressure.

### **7.1.4 The net effect on regular employment and unemployment**

The net effects of ALMPs on (un)employment in Sweden have been studied in macroeconomic estimations of reduced-form equations. Most of the studies imply that an expansion of ALMPs reduces *open unemployment*. But the studies also suggest that the sum of direct and indirect crowding-out effects is large. The estimates do not support the view that an expansion of ALMPs reduces *total unemployment* (the sum of open unemployment and programme participation). Some of the evidence rather suggests the opposite.

We compared Swedish reduced-form estimations with similar estimations on cross-country and panel data for the whole OECD area. The latter studies have often been interpreted to give a very favourable picture of the employment effects of ALMPs (see e.g. Layard et al., 1991; or Nickell and Layard, 1999). This is, however, partly a misunderstanding, which derives from the fact that these studies have usually focused on the effect on open unemployment rather than on regular employment or total unemployment. If one recalculates the estimates in these studies to effects on total unemployment, the effects vary between studies, but the overall picture is very similar to the picture from the Swedish studies: the overall results do not support the view that ALMPs reduce total unemployment (or increase regular employment).

When interpreting the reduced-form studies, one should, however, keep in mind that unemployment has been measured as a share of the labour force, so a positive employment effect could be concealed by a positive effect on labour force participation (see *Section 3.2*). But the studies on OECD data that have looked at the effects of ALMPs on employment (or the share of "inactive", i.e. unemployed and people outside the labour force) as a share of population do not provide any clear such indications.

## **7.2 The relative efficiency of various ALMPs**

In much of the discussion in *Section 7.1*, we evaluated active labour market policy as a whole in Sweden. We did not distinguish between different programmes. This is a crude simplification, as one should expect different programmes to have different effects.

What do the studies of Sweden say about the relative efficiency of different programmes? A first issue concerns labour market training versus job creation. Here, the microeconomic studies of effects on individuals and the macroeconomic studies of general-equilibrium effects give inconsistent results. The microeconomic studies of labour market training in the 1990s found no or negative employment effects. In contrast, some studies found positive effects of job creation schemes on later regular employment (although the number of studies is limited). But in the macroeconomic studies, there is a strong tendency that labour market training gives more positive (or less negative) effects on regular employment than job creation. Only the latter programmes seem to cause direct crowding-out effects.

Another issue concerns the relative efficiency of various job creation programmes. The few available microeconomic studies suggest positive employment effects on the participating individuals of self-employment grants, recruitment subsidies, work placement schemes and trainee replacement schemes, whereas it has proved difficult to find such effects of relief work and work experience schemes. But at the same time, there is much to suggest that these programmes have large crowding-out effects. Unfortunately, there is a strong tendency that the schemes close to regular jobs have both positive employment effects for the participating individuals and large negative crowding-out effects.

The empirical studies seem to be the most negative for youth programmes. Here, there appear to be large crowding-out effects, at the same time as it is uncertain whether there are positive employment effects on the participating individuals.

## **7.3 Policy conclusions**

Which policy conclusions can be drawn from the unique Swedish experiment in the 1990s of using large-scale ALMPs to fight high unemployment? Should the Swedish policy be followed by other countries in similar circumstances? It is true that enough time may not yet have passed to allow a final verdict: this may require an analysis of to what extent the

rise in unemployment in the early 1990s will lead to persistent effects, and of whether there are long-term employment effects of ALMPs on labour force participation that have not yet worked themselves out. We do not rule out such effects (not least because some microeconomic studies point to more positive – or less negative – employment effects of ALMPs after a few years than in the short term). Notwithstanding these caveats, our conclusion is still that the labour market policy followed in Sweden in the 1990s was not efficient, and that it should not be taken as a blueprint to follow by others. The Swedish experience shows clearly the limitations of ALMPs as a measure to fight unemployment. It is not a measure that should be relied on to the extent that was done in Sweden.

A main problem with ALMPs in Sweden in the 1990s was their size. This applies especially to labour market training. It is a problem to expand training programmes very rapidly in a situation when the appropriate infrastructure is not there. In such a situation, one should expect marginal returns to be decreasing, as is suggested by Björklund and Moffitt (1987), who found the average effect on the hourly wage to be decreasing with the volume of training. One should also expect training programmes to be ineffective in a situation with very low demand, when unemployment duration is long under all circumstances, and when it is difficult to know where future labour shortages in the economy will appear. The upshot is that training programmes should be kept rather small in a deep recession. There is certainly a strong case for not using ALMPs (especially training programmes) as an income support measure (either as an alternative to unemployment benefits or as a means to re-qualify the participants for such benefits) as was done in Sweden, because this is likely both to distort the incentives for programme participation and result in very large programme volumes.

As to job creation measures, we have pointed to the conflict between positive employment effects on the participating individuals and the macroeconomic crowding-out effects. This is a strong argument to target job creation measures on the long-term unemployed (and those who are threatened to become long-term unemployed): then competition effects may affect regular employment positively, even if there are large crowding-out effects. Moreover, the risk of adverse wage-setting effects is reduced.

Our survey also questions the use of large-scale youth programmes, as they seem to have large displacement effects, at the same time as it is unclear whether there are any positive employment effects for the participating individuals. Since those who have been unemployed for less than six months seem rarely to meet negative employer attitudes (Klingvall, 1998), there appear to be no strong reasons to place young people in programmes during their first half-year of unemployment. This is an argument for much smaller youth programmes than were used in Sweden in the 1990s.

One cannot, of course, analyse the proper role of ALMPs in the tool set of employment policy without corresponding evaluations of alternative policy instruments. Indeed, subjecting only some policies to critical scrutiny, but not others, could lead to a deterioration of the policy mix. But it is safe to conclude that the Swedish strategy of using ALMPs as the main policy instrument to fight unemployment in the 1990s was not founded on systematic ex ante knowledge of the effectiveness of the programmes, and that our ex post evaluation does not support the view that they were effective in maintaining regular employment. Rather, the policies that were pursued are likely to have reduced open unemployment at the cost of also reducing regular employment. It is a value judgement whether one should consider this to reduce or increase social welfare. But there is a lot to suggest that the Swedish example of the 1990s is not one to follow if one views high regular employment as the primary objective of labour market policy.

## Appendix

Many of the studies on data from a large number of OECD countries discussed in *Section 6* have estimated unemployment equations of the form

$$u = \mathbf{a}\mathbf{g} + \dots, \quad (1)$$

where

$$? = b_r r / uy \quad (2)$$

$u$  is open unemployment (as a fraction of the labour force),  $\mathbf{g}$  is the measure of ALMPs,  $\mathbf{a}$  is a parameter measuring the effect of ALMPs on open unemployment,  $b_r$  is the expenditure on ALMPs per programme participant,  $r$  is programme participation (as a fraction of the labour force) and  $y$  is GDP per capita.

We are interested in computing  $du/dr$  and  $d(u+r)/dr$  from the estimated models. To do this, we substitute (2) into (1) and differentiate implicitly. This gives

$$\frac{du}{dr} = \frac{(b_r/y)\mathbf{a}u}{u^2 + (b_r/y)\mathbf{a}r} \quad (3)$$

To calculate  $du/dr$  we need information on  $b_r/y$ . In our calculations we set  $b_r/y = 0.5$ . This parameter value is motivated in the following way. For Denmark, Finland, Norway and Sweden, Zetterberg (1993) provides information on  $\gamma$ . The database collected by Rødseth and Nymoen (1999) gives information on programme participation and unemployment for the same countries. As  $b_r/y = \mathbf{g}u/r$ , we can compute this ratio. The average values for the period 1985 – 91, are 0.41 for Denmark, 0.60 for Finland, 0.42 for Norway and 0.44 for Sweden. As the effect on unemployment of ALMPs in (3) is increasing in  $b_r/y$ , our guesstimate 0.5 does not seem to imply that we have underestimated the effect systematically. Given this assumption, we can compute  $du/dr$  at given values of open unemployment and programme participation. The effect on total unemployment (the sum of programme participation and open unemployment) is obtained as  $d(r+u)/dr = 1 + du/dr$ .

Zetterberg (1993) instead used the ratio between total ALMP expenditures and total expenditures on the unemployed (see *Table 2*) as the measure of ALMPs in his unemployment equations. This measure, which we label  $\mathbf{I}$ , can be written

$$\mathbf{I} = b_r r / (b_r r + b_u u), \quad (4)$$

where, in addition to the previously explained variables,  $b_u$  is the expenditure per openly unemployed person. Here, we proceeded by assuming that the spending per programme participant equals the spending per openly unemployed, i.e.,  $b_r = b_u$ . Given this assumption, and given an estimated effect  $\mathbf{b} = du/d\lambda$ , we have in this case that

$$\frac{du}{dr} = \frac{\mathbf{b}u}{(u + r)^2 + \mathbf{b}r} \quad (5)$$

In *Table 15* in the text, we have assumed throughout that  $u = 0.07$  and  $r = 0.03$ .

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