1 The Effects of Active Labor-Market Policies in Sweden: What Is the Evidence?

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During the last decade there has been an increasing international interest in active labor-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) labor-market training, and (iii) subsidized employment (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 1997, the European Council agreed on an employment strategy that includes active labor-market policy as a key ingredient,[2] and many member states have followed these recommendations.[3]

The recent interest in active labor-market policies motivates a thorough evaluation of how successful the active labor-market programs (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 labor-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 percent of the labor force and expenditures accounted for more than 3 percent 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 Labor 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 labor-market policy. The focus is on how ALMPs affect regular employment, i.e., employment excluding participation in programs. The motivation for this focus is that employment generation is widely considered to be the primary aim of active labor-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 labor-market policy as a means of raising employment.

Section 1 gives a background picture of how ALMPs have been used in Sweden. Section 2 identifies a number of theoretical mechanisms. Section 3 surveys Swedish microeconometric studies of the effects on the individuals participating in ALMPs. Section 4 surveys Swedish macroeconomic studies of the general-equilibrium effects. Section 5 reviews the studies based on cross-country or panel data for OECD countries. Section 6 sums up the results and draws policy conclusions.

1 Active Labor-Market Policy in Sweden

There is a long tradition of active labor-market policy in Sweden. In the beginning of the twentieth century, municipal employment offices were established (Thoursie 1990). In the depressions of the interwar years, the government organized relief works and special youth jobs. In 1948, the foundations of modern labor-market policy were laid when the National Labor Market Board was instituted.

1.1 The Thinking behind Labor-Market Policy

The thinking behind Swedish labor-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 the early 1950s.[4] They saw active labor-market policy as a necessary ingre-

dient 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 labor-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 labor shortages there.

The original focus in postwar Swedish labor-market policy was thus on increasing labor mobility. However, in the period 1960–1990 the emphasis gradually shifted in the direction of counteracting all types of unemployment. In the late 1960s and the early 1970s, the objective of eliminating remaining "islands of unemployment" through selective job-creation programs 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 postwar period, with regular employment falling by 13 percent 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 program placements were systematically used to this end (e.g., Carling et al. 1996; Sianesi 2001).

An important side objective of Swedish active labor-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 labor-market policy has been used as a work test for the recipients of unemployment compensation.

1.2 The Various Programs

Originally, *labor-market training* consisted mainly of vocational training programs, but over time schemes containing more general education

have become more important. In recent years, *education in the Swedish language for immigrants* has been a part of labor-market training. *Computer activity centers*, 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 programs has usually been six months. Participants have received training grants equivalent to unemployment compensation. In the second half of the 1980s, it became possible for unemployed individuals to requalify for unemployment compensation through participation in training programs. In 2000, this possibility was abolished for all labor-market programs.

There have been many types of subsidized employment schemes over the years. The classical measure was *relief works*. They consisted of temporary jobs (around six 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 selected 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 *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 organize activities that would not crowd out regular employment. Participants in work-experience schemes received unemployment compensation. *Recruitment subsidies* and (more recently) *employment subsidies* are programs that are more similar to regular employment. Both programs 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 subsidized 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 programs can be characterized as *work-practice programs*. In our survey of empirical results, we include these in job-creation activities, but work-practice programs are supposed to have also a training content. Various types of *youth schemes* belong to this category. The first youth program was *youth teams*, introduced in 1984. They were

followed by "*schooling-in slots*." During 1992, *youth practice* was introduced. This program expanded rapidly. The program was targeted at those below the age of 25. As was the case for work-experience schemes, there were clear instructions to avoid displacement effects.

Other examples of work-practice programs 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 program was *work placement schemes*, which replaced practice for immigrants, practice for academic graduates, and youth practice in 1995.

Resource jobs, introduced in 1997, entailed subsidies 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 program that has been used over the entire period under study is labor-market training. All other programs have 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 programs in 1984, computer activity centers in 1995, and employment subsidies in 1997.

Finally, a reform took place in 2000, when an *activity guarantee* was introduced. This program 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). Participants are given some full-time activity (e.g., job search) until they find a job or enroll in regular education. This reform was made in connection with the abolition of the earlier possibility to renew benefit eligibility by participating in ALMPs.

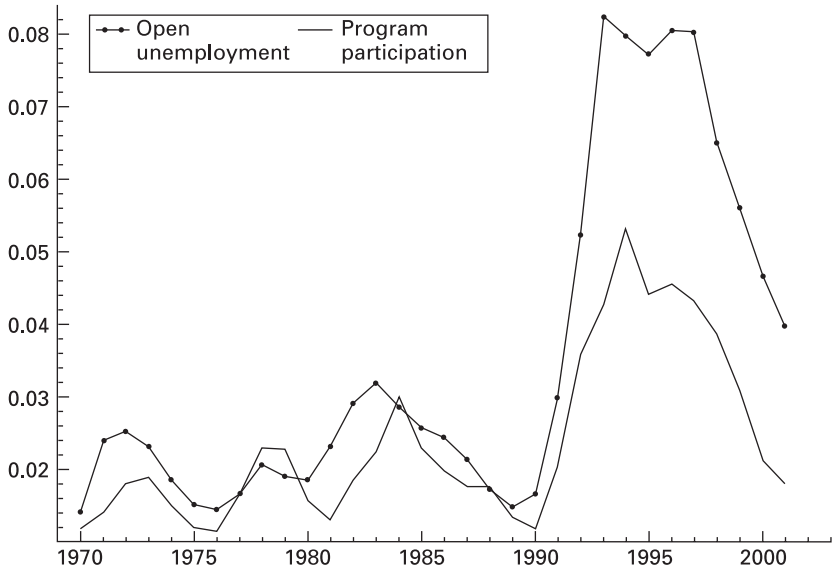

Figure 1.1
Open unemployment and program participation (shares of labor force), 1970–2001. Sources: Unemployment and labor force: Statistics Sweden, Labour Force Surveys; Program participation: National Labour Market Board.

1.3 The Empirical Picture

Figures 1.1–1.3 illustrate how the program volumes have developed over time.

Figure 1.1 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 the 1980s, but there is also a cyclical pattern. The large expansion in the 1990s in connection with the steep rise in unemployment also stands out. Towards the end of the 1990s, when unemployment came down, the program volumes were reduced again.

Figure 1.2 depicts *total unemployment* (the sum of open unemployment and participation in ALMPs) and the *accommodation ratio* (the ratio between program participation and total unemployment). In the 1970s and the 1980s, the accommodation ratio was approximately 0.4–0.5, but it fell in the 1990s. Although programs expanded strongly then, they did not increase proportionally to the rise in unemployment. In 2000, the accommodation ratio was around 0.2.

Figure 1.3 shows the development of various program types. In the 1970s and the 1980s, training encompassed more persons than sub-

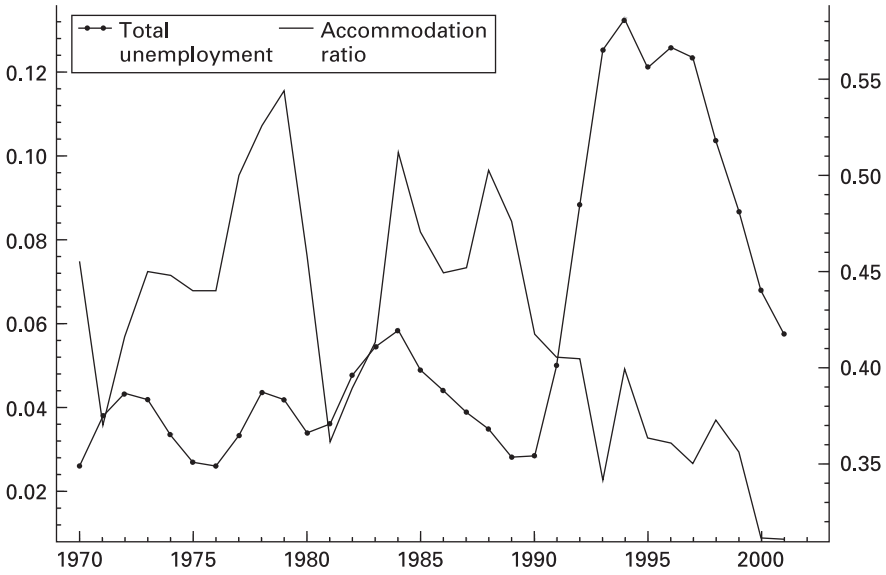

Figure 1.2
Accommodation ratio (right-hand scale) and total unemployment, 1970–2001. Total unemployment is defined as the sum of open unemployment and total participation in ALMPs. Accommodation ratio is defined as ratio of program participation to total unemployment. Sources: Participation in ALMPs: National Labour Market Board. Unemployment and the labor force: Statistics Sweden.

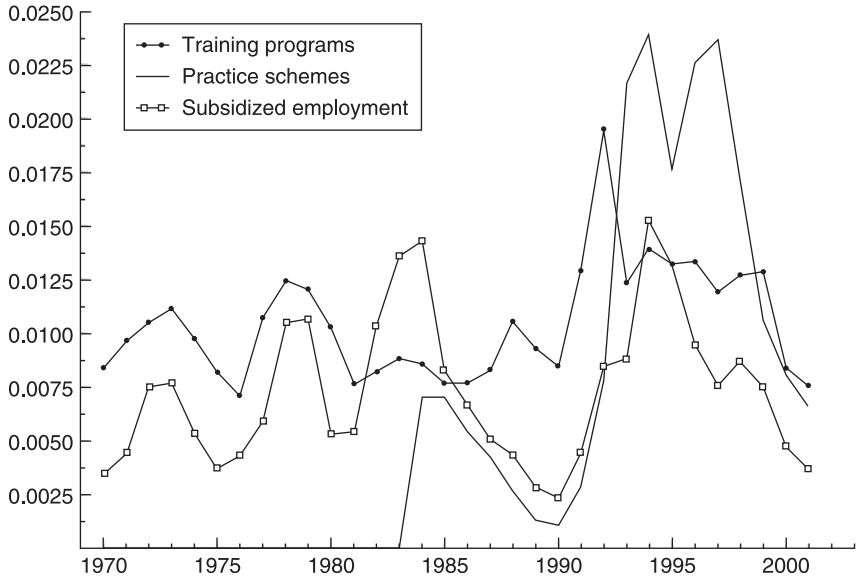

Figure 1.3
Participation in different kinds of labor-market programs, 1970–2001. The programs are generally classified as in the main text. Trainee replacement schemes and resource jobs are classified as subsidized employment. Source: National Labour Market Board.

Table 1.1
Swedish ALMPs in an international perspective. Source: OECD Employment Outlook, various issues.

	Expenditures on ALMPs (percent of GDP)			Expenditures on ALMPs (fraction of total unemployment expenditures)			Allocation of expenditures on ALMPs in 1986–2000			
	1986–90	1991–95	1996–2000	1986–90	1991–95	1996–2000	Public employment services and administration	Labor market training	Youth measures	Subsidized employment
Austria	0.26	0.28	0.38	0.21	0.18	0.23	0.39	0.37	0.04	0.16
Belgium	1.06	0.99	1.13	0.27	0.26	0.31	0.19	0.14	0.00	0.67
Denmark	0.82	1.15	1.19	0.17	0.19	0.25	0.10	0.48	0.21	0.21
Finland	0.82	1.39	1.11	0.37	0.27	0.29	0.12	0.32	0.04	0.50
France	0.50	0.85	1.04	0.2	0.3	0.37	0.20	0.42	0.13	0.25
Germany	0.72	1.16	1.03	0.36	0.35	0.31	0.25	0.37	0.05	0.33
Greece	0.16	0.23	0.29	0.29	0.34	0.39	0.45	0.08	0.03	0.44
Ireland	1.06	0.70	1.37	0.26	0.29	0.36	0.15	0.26	0.23	0.35
Italy	—	0.89	0.44	—	0.47	0.36	0.16	0.03	0.42	0.35
Luxembourg	0.16	0.12	0.18	0.17	0.16	0.21	0.23	0.12	0.38	0.27
Netherlands	0.56	0.85	0.95	0.16	0.22	0.25	0.30	0.43	0.05	0.21
Portugal	0.26	0.41	0.37	0.45	0.36	0.30	0.33	0.10	0.26	0.26
Spain	0.71	0.59	0.54	0.22	0.17	0.25	0.16	0.18	0.11	0.52
Sweden	1.10	1.79	1.30	0.59	0.47	0.41	0.19	0.42	0.08	0.31

United Kingdom	0.50	0.38	0.21	0.26	0.22	0.20	0.46	0.29	0.01	0.18
EU average	**0.62**	**0.79**	**0.80**	**0.28**	**0.28**	**0.35**	**0.24**	**0.27**	**0.14**	**0.33**
Australia	0.25	0.45	0.46	0.19	0.21	0.28	0.40	0.17	0.04	0.33
Canada	0.52	0.57	0.45	0.24	0.23	0.30	0.38	0.47	0.03	0.07
Japan	0.09	0.10	0.26	0.22	0.27	0.38	0.27	0.20	0.00	0.49
New Zealand	0.81	0.77	0.56	0.43	0.31	0.29	0.18	0.47	0.04	0.27
Norway	0.64	1.28	0.34	0.52	0.72	0.37	0.32	0.32	0.11	0.20
Switzerland	0.08	0.18	0.48	0.32	0.15	0.32	0.61	0.20	0.00	0.14
United States	0.20	0.17	0.14	0.3	0.26	0.34	0.37	0.34	0.17	0.06
OECD average	**0.54**	**0.70**	**0.66**	**0.29**	**0.29**	**0.34**	**0.28**	**0.28**	**0.11**	**0.30**

sidized 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 programs, but later there were large increases in schemes of subsidized employment and practice. Recently, training programs have again become relatively more important.

1.4 Swedish ALMPs in an International Perspective

Table 1.1 provides an international perspective. In 1986–1990 and in 1991–1995, Sweden spent more on active labor-market policy than any other country. The difference is especially marked in 1991–1995, 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 1996–2000, when unemployment fell, but still amounted to as much as 1.3 percent of GDP, which was well above the EU and OECD averages. In this period, Ireland, however, spent slightly more on active labor-market policy.

The table also shows that Sweden had the largest share of active expenditures (relative to total expenditures on the unemployed) in 1986–1990, when it was 59 percent, more than double the EU and OECD averages. The share subsequently fell, but it remained substantially above the EU and OECD averages.

What finally stands out is the larger emphasis in Sweden than in most other countries on labor-market training: 42 percent of the expenditures on ALMPs in Sweden have been on training, compared to EU and OECD averages of 27 and 28 percent, respectively. Only a few countries (New Zealand, Canada, Denmark, and the Netherlands) have spent larger fractions of active expenditures on training than Sweden.

2 A Theoretical Framework

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

In figure 1.4, a downward-sloping *employment schedule* shows how regular labor demand (labor demand excluding participation in ALMPs) depends negatively on the real wage. An upward-sloping

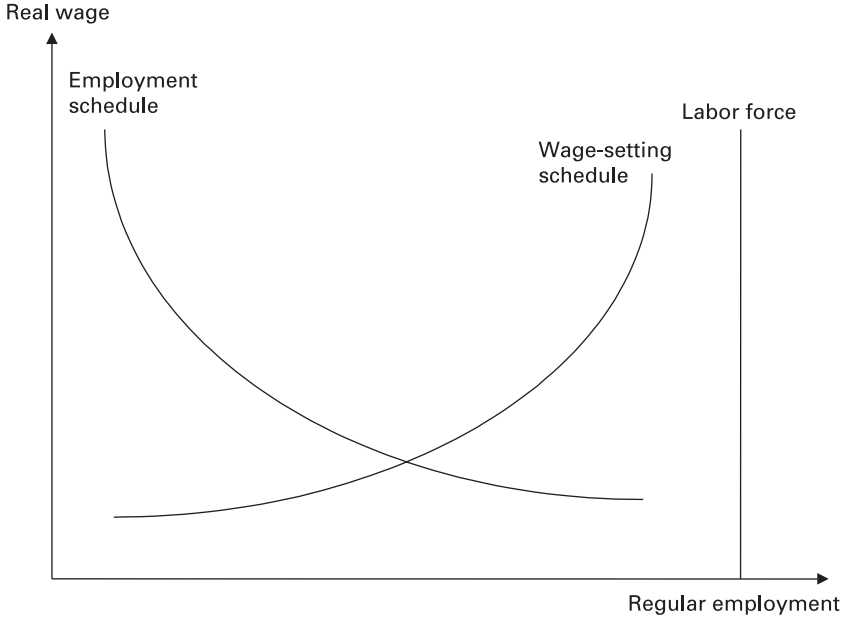

Figure 1.4
Wage setting and employment.

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. This gives employees a better outside option when bargaining with the present employer, 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 labor force. By deducting participation in ALMPs from the labor force, and comparing the outcome with regular employment, one obtains open unemployment.

The analytical framework in figure 1.4 can be motivated in several ways. The simplest possibility is to view the employment schedule as an ordinary stock demand for labor, 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 worthwhile 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 analyze various effects of ALMPs. Following Calmfors (1994), we distinguish (i) effects on the matching process, (ii) effects on the competition for jobs, (iii) productivity effects, (iv) effects on the allocation of labor between sectors, (v) direct crowding-out effects on regular labor demand, and (vi) accommodation effects on wage setting.

2.1 Effects on the Matching Process[5]

The aim of the job-broking and counseling 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 labor-market policy.

A more efficient matching process shifts the employment schedule in figure 1.4 to the right, which tends to raise both employment and the real wage. The explanation is that an increase in matching efficiency increases the probability of filling a posted vacancy at any point of time. Hence, the expected return to posting vacancies increases, and therefore more vacancies are posted. This results in higher employment.

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 that the higher is matching efficiency, the better is the firm's bargaining position vis-à-vis the employee, because a vacancy can then be more quickly filled if the employee quits because of disagreement over the wage. Hence, a higher matching efficiency means that the firm is able to negotiate a lower real wage at each level of employment.[6]

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 labor-market policy in the form of job broking and counseling activities as well as completed labor-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 programs working in the opposite direction if the participants do not exit from the programs 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.

2.2 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 labor force participation. In figure 1.4, the labor-supply schedule, showing the size of the work force, is then shifted to the right. The wage-setting schedule is also shifted to the right. The reason is that there are more workers competing for the same number of jobs: a certain level of regular employment is thus associated with a lower job-finding probability, which worsens the outside option of employees in wage bargaining.

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 the locking-in effects.

2.3 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 labor-market training as well as of various work experience programs, 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 1.4 upwards. Everything else 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.

2.4 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 subsection 1.1), the original goal of active labor-market policy in Sweden was to transfer labor from stagnating low-productivity sectors with high unemployment to expanding high-productivity sectors with low unemployment through training programs and other mobility-enhancing measures. Such policies will reduce wage pressures substantially in low-unemployment sectors, whereas wage pressures will increase only a little in high-unemployment sectors if the sector-specific wage-setting schedules are convex (Calmfors 1995; Fukushima 1998). The result will be a rightward shift of the *aggregate* wage-setting schedule.

2.5 Direct Crowding Out (Displacement)

An unintended side effect of ALMPs is that they may crowd out regular labor demand (see, e.g., Dahlberg and Forslund 1999). This is likely to apply mainly to schemes of subsidized employment. Such crowding out (displacement) presupposes that the unemployed who 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.

In terms of figure 1.4, direct crowding out means that the employment schedule (the regular labor demand schedule) is shifted to the left. This tends to reduce both the real wage and regular employment.

Direct crowding out should be seen in association with the competition effects in subsection 2.2. 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 may thus be necessary to reach the desired competition effects.

2.6 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. 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 Nymoen 1990; Calmfors and Forslund 1991).
- Participants in ALMPs may experience a higher degree of psychological well-being than the openly unemployed, because program participation is considered more meaningful (Korpi 1997).
- If program participation is expected to improve future labor-market prospects, it will increase the expected future welfare of participants (Calmfors and Lang 1995).
- If program 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 (the present activity guarantee in Sweden), 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. In terms of figure 1.4, 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.

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, program participation means a welfare loss because they can no longer allocate their time freely. Judging from the reactions of some of the unemployed, the activity guarantee in Sweden may to some degree work in this way (see subsection 1.2). To the extent that this is the case, the above effects are reversed, and the wage-setting schedule tends to be shifted downwards.

2.7 The Effects of ALMPs

Our analysis is summarized in table 1.2, which shows the expected direction of the various effects. We have put question marks where

Table 1.2
The expected effects of ALMPs—a summary of the theoretical discussion.

	Wage given employment (wage pressure)	Regular employment given wage	Net effect on regular employment
Matching	− (?)	+ (?)	+ (?)
Competition	− (?)	0	+ (?)
Direct displacement	0	−	−
Accommodation	+ (?)	0	− (?)
Productivity of job seekers	(+)	+	+/(0)
Allocation of labor force	−	0	+

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.

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 allocation of labor. Knowledge on these effects can also be obtained by examining how program participation affects the mobility of job seekers, their search behavior 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 by 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 summarize the studies of these types that have been made in Sweden.

3 Microeconomic Studies

This section surveys the evidence from microeconometric 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 labor-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 labor-market policy differs from the one usually considered in the evaluation literature. As was discussed in subsection 1.2, there is a wide array of continuously ongoing programs 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 an ALMP. The choice for an unemployed person is to participate in a program now or *later*, rather than now or *never* (see Carling and Larsson 2000a and Sianesi 2001a for further discussions). As a consequence, the mere existence of programs may influence the behavior of non-participants also.

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. 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). Third, as every

Table 1.3
Treatment effects of labor-market training (LMT).

Study	Program and timing	Sample	Dependent variable	Results
Edin and Holmlund 1991	LMT, 1981–1984	Register and survey data on 800 16–24-year-old unemployed in the Stockholm area, 1981	Reemployment probability in subsequent unemployment spells	Significant, positive effect
Axelsson and Löfgren 1992	LMT, 1981	Register and survey data on 2,000 participants. Random selection and representative sample	(i) Yearly income 1982 and 1983; and (ii) income growth 1981–82 and 1981–1983	Significant, positive effects
Korpi 1994	LMT, 1981–1984	Register and survey data on 800 16–24-year-old unemployed in the Stockholm area, 1981	Duration of employment	Insignificant effect
Harkman, Jansson, and Tamás 1996	LMT, 1993	Register and survey data on 3,000 20–54-year-old participants. Random selection and representative sample	Regular employment 6 months and 2.5 years after program	Positive effect only if potential selection is not considered
Harkman 1997	LMT, 1994	Register and survey data on 3,000 20–54-year-old participants. Random selection and representative sample	Regular employment 2 years after program	Significant, negative effect of training $\leq$100 days; no significant effect of training $\geq$100 days; the *difference* of 4% between short and long programs is significant
Regnér 1997	LMT, 1989–1991	Register data on 9,000 participants. Non-participating comparison group through matching. Random selection and representative sample	Yearly income 1990–1992	Significant, negative effect 1 year and insignificant effect 3 years after program
Harkman, Johansson, and Okeke 1999	LMT and computer activity centers, 1996	Register and survey data on 3,000 20–54-year-old participants. Random selection and representative sample	Regular employment 1 year after program	Positive effect of LMT only if potential selection is not considered; no significant effect of computer activity centers

Larsson 2000	LMT, 1992–93	Register data on 600 20–24-year-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 program	Significant, negative effects
Johansson and Martinsson 2000	Swit, 1999	Register and survey data on 4,000 Swit participants. Comparison group = 7,000 participants in similar traditional IT training	Regular employment 6 months after program	Significant, positive effect
Okeke 2001	LMT, 1998–99	Register and survey data on a stratified subsample of participants. Non-participating comparison group through propensity score matching	Regular employment 6 months after program	Significant, large positive effect
Richardson and van den Berg 2001	LMT, 1993–2000	Register data on a 1% random subsample of all who became openly unemployed, 1/1/1993–6/22/2000 (5,000 individuals, of whom 665 participated in LMT). Bivariate duration model with individual heterogeneity	Unemployment duration	Significant, negative effect that vanished within two months after the training ended if unemployment duration is measured from the end of LMT; insignificant effect if unemployment duration is measured from the start of program participation.
Sianesi 2001b	LMT, 1994–1999	Register data on 30800 adult individuals, entitled to unemployment benefits, who registered with employment offices for the first time in 1994 (1387 in LMT)	(i) Employment rate; and (ii) benefit collection	(i) Significant, negative effect on employment rates up to 30 months, then insignificant effect; (ii) significant, positive effect on benefit collection. The comparison is between participation now and "waiting in open unemployment"

long-term unemployed is likely sooner or later to participate in an ALMP, the problem of sample selection bias is exacerbated: job seekers with large difficulties of finding a job tend to be over-represented among ALMP participants (Sianesi 2001b).

The evaluation literature on Swedish ALMPs since the mid-1980s must therefore be interpreted with caution. It is possible that these evaluations analyze the effect of participating at *a specific point in time rather than later* or in *a certain program rather than in 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 based on survey data and/or information from personal files kept at the employment 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 behavior and employer attitudes are based on survey data.

Subsection 3.1 looks at treatment effects of labor-market training (LMT), whereas subsection 3.2 focuses on the effects of subsidized employment. Subsection 3.3 summarizes the evidence on the effects of ALMPs on the search behavior of participants. Subsection 3.4 reviews the studies of effects on employers' attitudes.

3.1 Labor-Market Training

The research on the effects of labor-market training is summarized in table 1.3.

Although results vary a lot between studies, some conclusions can be drawn. The estimated effects of labor-market training differ between the 1980s and the 1990s. Evaluations of training acquired during the first half of the 1980s suggest positive effects on participants' employment and/or income. Evaluations of training that took place in the 1990s usually find instead insignificant or significantly negative effects.

Both Edin and Holmlund (1991) and Korpi (1994) estimated, using the same sample, the effects of labor-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) analyzed the impact of labor-market training in 1981 on the (growth and level of) incomes obtained one to

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 sizable—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 (1997) also analyzed 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 labor-market training in the 1990s have been published by the National Labor 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 time.

Harkman (1997) analyzed 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, analyzed the effect of training in 1996 on the employment probability one year later. Besides "traditional" labor-market training, this evaluation looked also at the effect of training in computer activity centers. The results are hard to interpret, as participants (on average) have different prior unemployment and program participation experiences than non-participants. The analysis of the effect of a single program is further complicated by the fact that

participants as a rule have participated in several programs earlier. One way to handle this problem is to consider only persons without earlier program participation. Such estimates indicate that computer activity centers had no significant effect, while participation in traditional training had a positive effect on the employment probability one year after program completion. But these results must be interpreted cautiously because the effect is measured from the time of program completion. This implies that the estimated effect will be positively biased.[7]

Larsson (2000) estimated the effect of labor-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.[8] In addition, the transition probability to regular education was lower among participants in training than among nonparticipants.

Johansson and Martinsson (2001) studied a program called Swit, which was a joint project between the Swedish government and the Confederation of Swedish Industries. The program was initiated as an experiment during a two-year period (1997–1999) 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 Labor 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.

Okeke (2001) studied the effect of participation in training in 1998–99. According to the study the effects on participants' employment were large, positive, and significant. But once again the results are hard to interpret, as it is not clear from the presentation how the control group was selected, but the procedure used might imply a positive bias.[9]

Richardson and van den Berg (2001) analyzed the effect of training undertaken in 1993–2000. They found a significant negative effect on the duration of unemployment when the duration was measured from the end of the program. When duration instead was measured from the start of the program, the effect was insignificant. This suggests that a negative locking-in effect of labor-market training more or less offsets a positive treatment effect once the program had been completed.

Sianesi (2001b) estimated the effect of participating in training in the period 1994–1999. The comparison made in the study is between "participation now" and "waiting in open unemployment." She found that the effect on the participants' employment rate was significantly negative until 30 months after the end of the program; later the effect was insignificant. Also, the probability of collecting unemployment benefits in the future was significantly higher for participants than for non-participants.

Another observation refers to the differences between short-run and long-run effects of labor-market training. The short-run effects are often 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 training increases the reservation wages of participants (see subsection 2.3). However, Richardson and van den Berg (2001) found a different pattern. According to their study, the treatment effect of training vanishes after two months. The authors suggest that the short-run treatment effect could be due mainly to extra placement efforts on the part of employment officers.

There is some evidence to suggest that income and employment effects are more favorable with longer training periods. But here the amount of research is very small.

3.2 Subsidized Employment

There are a number of studies on the effects of subsidized employment, of which a few look also at the effects of labor-market training. However, given the amount of different subsidized employment programs, less is known about the specific effect of single programs than about labor-market training. The studies of subsidized employment programs are surveyed in table 1.4.

Some of the studies listed in table 1.4 have tried to evaluate the effects on subsequent employment of participation in various programs as compared to open unemployment, whereas other studies have tried only to compare various programs with each other (but not with open unemployment) or to study the effect of participating in a program at a given point of time rather than later. The latter studies avoid the problem that most long-term unemployed will sooner or later end up in a program, which makes it hard to find a comparison group of non-participants. (See the discussion in the introduction to section 3.)

Table 1.4
Treatment effects of subsidized employment.

Study	Program and timing	Sample	Dependent variable	Results
Sehlstedt and Schröder 1989	Recruitment subsidies and relief work, 1984	Register and survey data on unemployed 20–24-year-olds, 1984	Labor market situation, 1987	Significant, positive effect of recruitment subsidies if part of an "action plan". No significant effect of relief work
Edin and Holmlund 1991	Relief work, 1977–1984	Register and survey data on 800 unemployed 16–24-year-olds in 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, negative effect on subsequent unemployment spells
Korpi 1994	Relief work, 1981–1984	Register and survey data on 800 16–24-year-old unemployed in the Stockholm area, 1981	Duration of employment	Significant, positive effect
Axelsson, Brännäs, and Löfgren 1996	LMT, work experience schemes, relief work, and youth practice, 1993	Register data on 10,000 unemployed 20–54-year-olds, 1993	Employment within 30 days after program	LMT, work experience schemes, and relief work are equivalent alternatives, but youth practice is better
Harkman, Johansson, and Okeke 1999	Recruitment subsidies, trainee replacement schemes, work placement schemes, relief work, and work experience schemes, 1996	Register and survey data on 3,000 20–54-year-old participants. Random selection and representative sample	Employment 1 year after program	Large significant, positive effects of recruitment subsidies, and significant, positive effects of trainee replacement and work placement schemes as well. No significant effects of relief work and work experience schemes
Carling and Gustafson 1999	Self-employment grants and recruitment subsidies, 1995–96	Register data on individuals with self-employment grants (9,000) or recruitment subsidies (14,000) in 1995 or 1996	The duration of employment	Significantly better employment results for self-employment grants than for recruitment subsidies

Okeke 1999	Self-employment grants, 1994	Register and survey data on 7,000 enterprises (entrepreneurs)	Enterprise survival rate, 1997	No significant difference between enterprises with and without self-employment grants
Larsson 2000	Youth practice, 1992–93	Register data on 600 20–24-year-old participants. Non-participating comparison group through propensity score matching	(i) Yearly income (ii) employment (iii) regular education 1–2 years after program	Significant, negative effect on yearly income and employment; no significant effect on education
Okeke and Spånt Enbuske 2001	Self-employment grants, 1995	Register and survey data on 8,000 entrepreneurs	(i) Enterprise survival rate; and (ii) probability to earn a living through the enterprise, 1998	No significant difference between enterprises with and without self-employment grants. Self-employment grants have, according to self-reported averages, a positive effect on the probability to earn a living through the enterprise
Carling and Richardson 2001	Work experience schemes, LMT, work placement schemes, relief work, computer activity centers, recruitment subsidies, self-employment grants, trainee replacement schemes, 1995–1997	Register data on 25,000 individuals who became unemployed and began their first program in 1995–1997	Unemployment duration	Significantly better results for recruitment subsidies, self-employment grants, trainee replacement schemes, and work placement schemes than for LMT, computer activity centers, work experience schemes, and relief work
Sianesi 2001b	"Work practice" (work experience *plus* work placement schemes), LMT, relief work, recruitment subsidies, and trainee replacement schemes, 1994–1999	Register data on 30,800 25–54-year-olds who became unemployed for the first time in 1994 and were entitled to unemployment benefits. Comparison group through propensity score matching	Employment rate over time	Significantly better employment results for recruitment subsidies and trainee replacement schemes than for LMT, "work practice" and relief work. Subsidies are also better than "waiting in open unemployment."

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

Edin and Holmlund (1991) analyzed, in addition to labor-market training (see subsection 3.1), 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. When Korpi (1994) used the same sample of youth to analyze the effects of relief work on the duration of employment, he found a significant positive effect.

Axelsson et al. (1996) analyzed the relative efficiency of labor-market training, work-experience schemes, relief work, and youth practice in terms of the employment probability within thirty days after the program ended. There were no significant differences between labor-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) analyzed (in addition to labor-market training and computer activity centers (see subsection 3.1)) the effect of job-creation schemes in 1996 on the probability of having a job one year later. Also here the results must be interpreted with caution because of the risk of biased estimates (see subsection 3.1). Another problem is that the number of first-time participants in other forms of subsidized employment than work-experience schemes and work placement schemes was small. With these caveats, the results suggest that recruitment subsidies had the most favorable effects. Also work placement schemes and trainee replacement schemes seem to have had positive effects, while this was not the case for work-experience schemes and relief work.

Three evaluations of self-employment grants were published in 1999–2001. Okeke (1999) analyzed 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 alterna-

tive 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.

In a more comprehensive analysis, Carling and Gustafsson (1999) 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 1995–1997. This also supports the favorable impression of self-employment grants, as other evaluations (discussed earlier in this section) indicate that recruitment subsidies have had positive effects.

Okeke et al. (2001) compared the rate of survival in 1998 for firms started with and without self-employment grants in 1995. The difference in survival was insignificant, but a larger fraction of those who had received self-employment grants reported that they could make a living from the returns from their firms three years after the launching of the firm.

Larsson (2000) analyzed the effects of youth practice and labor-market training (see subsection 3.1) in 1992–93. The results of youth practice were very similar to those of labor-market training: incomes were 20–30 percent and employment probabilities 18–37 percent lower among former participants than among non-participants one or two years after the program.[10] But, in contrast to labor-market training, youth practice had no decreasing effect on the transition rates to regular education.

Carling and Richardson (2001) analyzed the relative efficiency of eight different programs in terms of employment probabilities. The results indicate that the programs 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 labor-market training and training at computer activity centers) and job-creation programs 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 subgroups of unemployed and is not affected by the timing of program entry. This suggests that the results are not due to selection bias.

Sianesi (2001b) compared the effects of participation in six different programs (including labor-market training; see subsection 3.1) in

1994–1999. She found that it was (significantly) better to "wait in open unemployment" than to participate in "work practice" (work placement schemes and work-experience schemes) or relief work in terms of subsequent probability of employment. Trainee replacement schemes and recruitment subsidies, however, were equally good and better, respectively, compared to waiting in open unemployment. A mirror image of this was that both work practice and relief work had a significant, positive effect on the probability to receive future unemployment compensation.[11] This indicates that the possibility to renew unemployment benefit eligibility may have been an important reason for program participation. Another aspect of this is that the most negative effects of program participation arose for those who started a program close to the time of benefit exhaustion (Sianesi 2001a).

3.3 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 behavior between program participants and openly unemployed. The studies are summarized in table 1.5.

Edin and Holmlund (1991) found that unemployed youths devoted fully seven hours a week to job search in the beginning of the 1980s. The corresponding figure for relief work participants was less than one hour. The same survey indicated that the number of search methods used was significantly higher among the unemployed (3.1 per week) than among relief workers (0.6 per week). The results of Sehlstedt and Schröder (1989) for youth search behavior in the mid 1980s convey a similar picture.

Ackum Agell (1996) and Regnér and Wadensjö (1999) studied search behavior among adults. Ackum Agell (1996) found that the unemployed are more active job seekers than program participants. The percentage of active seekers is much (and significantly) higher among the unemployed (95) than among program participants (57), and program participants use fewer search methods. Among the unemployed, 30 percent reported that they did not search at all during the survey week; the corresponding percentage for program participants was 68. This difference was statistically significant.

Regnér and Wadensjö (1999) found that the percentage actively searching jobs or being in contact with the public employment service

Table 1.5
Search activity.

Study	Program and timing	Sample	Dependent variable	Results
Sehlstedt and Schröder 1989	Relief work, 1984–85	Register and survey data on 500 20–24-year-old unemployed, 1984	Search activity and number of search methods	Significant, negative effect
Edin and Holmlund 1991	Relief work, 1977–1984	Register and survey data on 800 16–24-year-old unemployed in the Stockholm area, 1981	Search activity and number of search methods	Significant, negative effect
Ackum Agell 1996	LMT and job creation programs (work experience schemes, relief work, and trainee replacement schemes), 1993–94	Survey data on 4,000 20–54-year-old unemployed, 1991	Search activity and number of search methods	Significant, negative effect
Regnér and Wadensjö 1999	Program participation in 1998	Survey data on 700 19–65-year-olds, unemployed during the spring of 1998 ("stock sample") and 1,300 25–60-year-olds leaving unemployment during the spring of 1998 ("flow sample")	Search activity and search methods	Compared to the openly unemployed, program participants search less actively while in programs, but more actively after program completion

was significantly higher among the openly unemployed (72 and 79, respectively) than among program participants (53 and 63, respectively) during the four weeks preceding the interview. But the authors also found that persons who had participated in a program during the preceding twelve months searched significantly more than those who had not participated in any program during the same period.[12] This suggests a negative locking-in effect on search effort during program participation, but a positive treatment effect after completion of the program.

Ackum Agell (1996) emphasized that participants have less time to search for work than do non-participants. Also, it can be socially beneficial that participants do not look for work if the program forms part of a comprehensive plan to improve their labor-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 temporary locking-in effects.

3.4 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 summarized in table 1.6.

Agell and Lundborg (1995, 2002), in two survey studies, undertaken in 1991 and 1998, asked personnel managers about their attitudes to unemployed job seekers with and without prior participation in ALMPs. The percentage who considered a long-term unemployed potentially less productive than a similar job applicant without an unemployment history rose from 21 to 27 between 1991 and 1998. Eighteen percent of the respondents in the 1991 survey also considered applicants with prior participation in a labor-market program potentially less productive than a similar applicant without unemployment experiences. The questions in the 1998 survey were more specific about the effects of program participation. Almost 15 percent of the respondents considered a participant in labor-market training potentially less productive than a similar person without an unemployment history. The corresponding percentage for participants in relief work and work-experience schemes was 20. However, the differences between participants in various programs and the openly unemployed were never significant.

Table 1.6
Employer attitudes.

Study	Source of information	Sample	Dependent variable	Results
Agell and Lundborg 1995	Survey, 1991	Personnel managers at ∼150 companies	Share who believe that (i) unemployed non-participants; and (ii) participants are potentially less productive than other job seekers	Openly unemployed 21%; program participants 18%
Behrenz 2001	Survey, 1995	Company representatives at ∼800 companies	Share who automatically sort out (i) unemployed non-participants; and (ii) participants	Openly unemployed 4.2%; LMT participants 1.2%; participants in other programs 1.6%
Agell and Lundborg 2002	Follow-up survey, 1998	Personnel managers at ∼150 companies	Share who believe that (i) unemployed non-participants and (ii) participants are potentially less productive than other job seekers	Openly unemployed 27%; LMT participants 15%; participants in work experience schemes/relief work 20%
Klingvall 1998	Survey, 1998	Employers at ∼280 workplaces	Share who consider hiring various categories	Openly unemployed 2%; LMT participants 30%; participants in other programs 20%
Agell and Bennmarker 2002	Survey, 1999	Personnel managers at 885 workplaces	How long a spell of unemployment or training/unemployment is needed before a job applicant is considered less suitable	Labor market training increases the time required for stigma from unemployment to arise

In a survey reported by Klingvall (1998) only some 3 percent of the responding employers stated that they would consider hiring an openly unemployed job applicant, while 30 (20) percent stated that they would consider hiring a job seeker who had participated in labor-market training (any other labor-market program). The differences were statistically significant.

Also the results in the survey reported by Behrenz (2001) suggests that employers consider open unemployment more negative than participation in labor-market programs: 4.2 percent of about 800 respondents said that they automatically discard applications from openly unemployed job seekers; the corresponding percentages for participants in labor-market training and participants in other programs were 1.2 and 1.6, respectively. The differences between the figures for program participants and openly unemployed were significant; the difference between training and other programs was not.

Agell and Bennmarker (2002), in survey questions answered by personnel managers at 885 workplaces, found that participation in labor-market training prolonged the time required for a stigma from unemployment to arise.

The survey studies of employer attitudes are the studies giving the most favorable results for the effects of ALMPs on individuals. Although questions and estimated effects differ, these studies suggest that employers judge former ALMP participants more favorably than unemployed who have not participated in programs. This evidence also suggests that labor-market training is preferred to the other programs. It is notable that this favorable view of labor-market training has no counterpart in the econometric results reported in subsection 3.1. It is not clear exactly what this discrepancy reflects; perhaps it can be explained by the fact that the survey studies of employer attitudes do not control for other factors or potential selection, as the econometric studies do.

4 Macroeconomic Studies

In this section we survey the macroeconomic studies of the total (general-equilibrium) effects of ALMPs in Sweden, using our earlier classification in table 1.2.

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 econo-

metric 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 labor-market situation may also trigger political decisions to adjust the volume of ALMPs. This may give rise to simultaneity bias and identification problems. We will repeatedly return to how this issue has been handled in various studies below.

4.1 Beveridge Curves, Matching Functions, and Migration Relationships

A first type of macroeconomic studies directly shed light on the efficiency of the matching process. These are studies of *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; Calmfors 1993). Neither of these studies show any effects of ALMPs on matching efficiency.[13] But the main conclusion is that we largely lack knowledge of the Beveridge-curve effects as none of the studies covers the 1990s.

There are two studies of *matching functions*, which relate the number of hirings to the numbers of vacancies and unemployed, on Swedish data. Edin and Holmlund (1991) found that program participation contributes to matching, but that the effect is only half that of open unemployment.[14] This suggests that locking-in effects dominate treatment effects of these programs in the short run. Hallgren (1996) found that subsidized employment had a significant negative impact on matching, whereas the opposite was true for labor-market training. 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. Several studies have been made. They are summarized in table 1.7.

McCormick and Skedinger (1991) found that increased program volumes at the regional level give rise to higher unemployment. The results may be interpreted in several ways, but the authors conclude that ALMPs have reduced geographical 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 program 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

Table 1.7
Effects of ALMPs on geographical mobility.

Study	Data	Results
McCormick and Skedinger 1991	24 counties, 1968–1985	Locking-in effects
Nilsson 1995	24 counties, 1966–1993	Locking-in effects
Westerlund 1997	24 counties, 1970–1989	Locking-in effects or insignificant results
Heiborn 1998	24 counties, 1964–1993	Mixed results
Westerlund 1998	24 counties, 1970–1989	Mixed results
Widerstedt 1998	541 males, 1981–1991	No effects
Fredriksson 1999	24 counties, 1968–1993	Small locking-in effects

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, labor-market training, and relief work on mobility across county borders. Mobility grants had a marginally significant positive 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 programs 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) looked at regional adjustments to employment shocks at the county level. The main finding was that the bulk 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. However, none of the studies have considered the job broking activities of the employment offices. In addition, most of the studies do not distinguish between subsidized employment and labor-market training.[15]

4.2 Direct Crowding Out (Displacement)

As discussed in subsection 2.5, subsidized employment is likely to cause direct displacement. The studies of this fall into two categories: *survey* studies and *econometric* studies of labor demand.

4.2.1 *Survey Studies*

In a number of surveys, employers, program participants and employment officers have been asked whether (i) they believe that the work performed by program participant(s) would have been performed by *anyone* in the absence of the program (*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 programs 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 programs crowd out employment in other workplaces than that associated with the program.

A number of survey studies are summarized in table 1.8. Although the results vary considerably, all studies but one indicate that direct displacement occurs. In most cases the estimated displacement is substantial.

A way to summarize the information in table 1.8 is to compute the average displacement for each program according to the studies shown. The results are reported in table 1.9, where the programs have been ranked according to the size of the average displacement effect.[16] There is a clear tendency that the closer to the regular labor market a program 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.

In addition to the studies in table 1.8, 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.

4.2.2 *Econometric Studies of Direct Displacement*

The econometric studies identify the relationship between programs and regular employment by comparing actual employment with the employment that would have been realized in the absence of programs.

Table 1.8
Survey studies of direct displacement.[a]

Study	Method	Program	Results
Sehlstedt and 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[b]	Telephone interviews with participants, employers and employment officers	WES	Participants: 17% 1993, 12% 1994 Organizers: 3–7%
NUTEK 1994	Questionnaires to participants and employers	WES	About 30% according to both participants and employers
Ams 1995	Questionnaires to organizers	RS	36% (of which slightly more than half would have recruited the same person).
Hallström 1995	Interviews with participants	WES	20–25%
Anxo and 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%[c] Employment officers: 11%[d]

a. The following abbreviations are used in the table. GES: general employment subsidy. MYP: municipal youth programs. RJ: resource jobs. RS: recruitment subsidy. RW: relief work. TES: targeted employment subsidy TPJ: temporary public jobs. TRS: trainee replacement schemes. WES: work experience schemes. WPS: work placement schemes.
b. The Ministry of Labour commissioned the study and the results were reported in Ams 1997.
c. 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 program.
d. The fraction that answered "Yes, in most cases."

Table 1.9
Average direct displacement effects according to the studies in Table 1.8.

Program	Average displacement effect (%)	Number of studies
Temporary public jobs (TPJ)	1.0	1
Municipal youth programs (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

Most of the studies have estimated traditional labor demand schedules augmented with measures of the volume of programs.

A fundamental problem for econometric studies of direct displacement is that the relation between programs and employment goes both ways: employment may depend on program participation, but the size of programs is also likely to depend on (un)employment. This *simultaneity problem*, discussed in the introduction to section 4, 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 summarized in table 1.10.

A first econometric study of direct displacement was carried out by Gramlich and Ysander (1981) using aggregate data for the period 1964–1977. 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 (36–69 percent) in the construction sector, but no significant effects 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

Table 1.10
Econometric studies of direct displacement. 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 1.8. Here, LMT denotes labor-market training.

Study	Program, data	Results
Gramlich and Ysander 1981	RW; aggregate time-series data 1964–1977.	Road construction: 100%; health and welfare: 0%
Forslund 1996	WES, LMT, RW, youth programs, TRS; panel of the Swedish municipalities 1990–1994.	WES: 0%; LMT: 0%; RW: 84%; youth programs: 76%
Forslund and Krueger 1997	RW; panel of the Swedish counties 1976–1991, 1980–1991.	Construction workers: 69%; health and welfare 0%
Löfgren and Wikström 1997	WES, LMT, RW, youth programs, TRS; panel of the Swedish municipalities 1990–1994.	WES: 0%; LMT: 0%; RW: 0%; youth programs: 94%; TRS: 0%
Dahlberg and Forslund 1999	RW, LMT, subsidized employment; panel of the Swedish municipalities 1987–1996.	RW: 66%; LMT 0%; subsidized employment: 65%
Edin, Forslund, and Holmlund 1999	Youth programs; panel of the Swedish municipalities 1990–1994.	76%

where there should be no displacement.[17] 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 subsidized employment, relief work, and labor-market training.[18] Subsidized 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 Sjöstrand 1997 and found shortcomings in both studies. With the preferred specification of Löfgren and Wikström, only youth programs gave rise to displacement (94 percent).

Edin, Forslund, and Holmlund (1999) analyzed the effects of youth programs 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 percentages are well above 60. 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 programs. Hence, the effects are averages over several programs. As an example, both work-experience schemes and youth practice were included in "subsidized employment" in Dahlberg and Forslund 1999. The average displacement effect for subsidized employment in this study was 65 percent. This percentage would, for example, be consistent with youth programs crowding out significantly more than 65 percent and work-experience schemes crowding out significantly less.

Most of the studies of displacement effects have tried to handle the simultaneity problem discussed in the introduction to section 4 through various methods. The fact that the studies have not found displacement effects of labor-market training (although the size of training programs can be expected to change in response to the employment situation in a similar way as subsidized employment) also suggests that the relationships found reflect the effect of programs on employment rather than the other way around.

4.3 Labor-Force Participation

The effects of ALMPs on labor-force participation is yet another area where research efforts have been modest. We are aware of only three studies that deal directly with the issue: Wadensjö 1993, Johansson and Markowski 1995, and Johansson 2001. All studies indicate strong positive effects of ALMPs on labor-force participation.

One can also obtain indirect evidence on the labor-force effects of ALMPs from studies of direct displacement. If ALMPs have a positive effect on labor-force participation, the estimated crowding-out effects should be larger when employment is measured relative to the labor force than when they are measured relative to the population. This was indeed the case in Löfgren and Wikström 1997 and in Dahlberg and Forslund 1999.

However, the results on labor-force participation should be interpreted with caution. If program participation has been used as a means

to renew eligibility for unemployment benefits, the increase in labor-force participation has not necessarily meant an increase in *effective* labor supply.

4.4 Wage Setting

For a number of reasons discussed in section 2, ALMPs may affect wage setting. The mechanisms involve effects on matching, the competition in the labor market, the welfare and productivity of job seekers, and the allocation of the labor force across sectors. The net effect is theoretically unclear. Estimates of *wage-setting schedules* can throw light on this issue. A large number of such studies have been undertaken. In all cases, real wage equations including measures of unemployment and the volume of labor-market programs as explanatory variables have been estimated. The main results are summarized in table 1.11.

The table 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 may reduce wage pressure. Most studies do not distinguish between different programs. No consistent pattern emerges from the three studies (Löfgren and Wikström 1991; Forslund 1992; Edin, Holmlund, and Östros 1994) that estimate separate effects of labor-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 peaks 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 programs were lowered and the expected treatment effects on the probability of finding a job or on future income deteriorated, one should expect less unfavorable (or more favorable) wage effects of ALMPs. However, Johansson et al. (1999), Rødseth and Nymoen (1999), and Forslund and Kolm (2000) did not find any significant changes in the wage-setting behavior between earlier periods and the 1990s.

Simultaneity problems of the same kind as for studies of displacement effects may be present also in the estimation of wage effects. However, because it probably takes time for wage changes to influence employment and for employment changes to trigger changes in program volumes, the problem is likely to be less severe in this case. A more serious problem may be that program participation covaries

Table 1.11
Effects of ALMPs on the real wage.[a]

Study	Effect of ALMPs	
	Short run	Long run
Newell and Symons 1987	0	0
Calmfors and Forslund 1990, 1991	+	+
Calmfors and Nymoen 1990	+	+
Holmlund 1990	NA	+
Löfgren and Wikström 1991[b]	+/0	0/+
Skedinger 1991[c]	+	+
Forslund 1992[d]	+/−	+/−
OECD 1993[e]	−	−
Edin, Holmlund, and Östros 1994[f]	0/0/0	0/0/0
Forslund and Risager 1994[g]	0	0
Forslund 1995	0	+
Blomskog 1997[h]	NA	+/−/0
Okeke 1998[i]	NA	−
Johansson, Lundborg, and Zetterberg 1999[j]	+/+	+/+
Rødseth and Nymoen 1999	0	+
Forslund and Kolm 2000	0	0
Thomas 2000	−	NA

a. A "+" sign indicates a significantly positive effect, a "−" sign a significantly negative effect and "0" no significant effect.
b. The first effect refers to relief work, the second to labor-market training.
c. Data pertain to different groups of employees in mining and manufacturing 1971–1988. The program studied is relief work.
d. The data refer to twelve unemployment insurance funds. The first effect refers to relief work, the second to labor-market training.
e. The regression covers the period 1985–1990 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.
f. The estimates pertain to individual wages for workers in engineering 1972–1987. The effects refer to total programs, labor-market training and relief work, respectively. The results in the table are IV estimates. OLS estimates gave significant, wage-reducing effects of total programs and labor-market training both in the short run and in the long run, and of relief work in the long run.
g. Separate estimates for industry and the rest of the business sector.
h. Different results in different model specifications.
i. 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.
j. Effects were estimated for the periods 1965–1990 and 1965–1998, respectively.

Table 1.12
Effects of ALMPs in reduced-form estimates.

Study	Period	Results
Ohlsson 1993, 1995	Vector autoregressions, aggregate time-series data, 1969–1990	Job-creation schemes crowd out regular employment and lower open unemployment. No significant effects on wages.
Skedinger 1995	Vector autoregressions, aggregate time-series data, 1979–1991	Youth programs crowd out regular youth employment (110% in short run).
Forslund 1995	Reduced form, aggregate time-series data, 1960–1993	No effect on open unemployment of aggregate ALMPs.
Calmfors and Skedinger 1995	Reduced form, panel data for counties, 1966–1990	Job-creation schemes crowd out regular employment; unstable results for labor-market training.

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 subsection 2.2).

4.5 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 2. Put differently, these estimations examine how the intersection between the wage-setting and employment schedules in figure 1.4 is affected by the size of ALMPs. The results from four reduced-form studies are summarized in table 1.12.

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 subsidized employment. The result was displacement in the order of magnitude of 50 percent during the first quarter. During later quarters the estimates are too imprecise to warrant any conclusions. Ohlsson also looked at the effects of both subsidized employment and labor-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 analyze the effects of subsidized 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 program volumes decline), but is statistically significant during the first five quarters.[19]

Forslund (1995) estimated a reduced-form unemployment equation. The results indicated that ALMPs lead 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) studied the relationship between total unemployment on the one hand and subsidized employment and labor-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 composition of the political majority in a county influences the volume of ALMPs, because the Social Democrats have generally been more in favor of these programs than the Liberal and Conservative parties). Subsidized 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 summarize, the reduced-form studies suggest that subsidized employment schemes tend to reduce regular employment, but also that they probably contribute to lower open unemployment.

4.6 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 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 subsidized employment schemes that most closely resemble regular employment, but not of labor-market training. Some evidence indicates that programs tend to raise wage pressure, whereas other evidence does not point in this direction. Reduced-form estimates seem to show that programs (at least subsidized employment) tend to reduce regular employment, even though they may help reduce open unemployment. The most favorable effects of ALMPs refer to labor-force participation, which seems to be increased by an expansion of programs.

5 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 labor-market institutions. The earlier studies explained cross-country variations in unemployment

Table 1.13
Effects of ALMPs on (un)employment in cross-section and panel data studies of the OECD countries.

Study	Countries and period	Effect on	Results[a]
Layard, Nickell, and Jackman 1991	20 OECD countries; 1983–1988; cross-section data	Open unemployment	− (−1.53)
		Total unemployment	0 (−0.53)
OECD 1993	19 OECD countries; 1983–1988; cross-section data	Open unemployment (Layard et al. measure of ALMPs)	−
		Open unemployment (ALMP expenditures as fraction of mean wage multiplied by labor force	0
Heylen 1993	18 OECD countries; second half of 1980s; cross-section data	Real-wage sensitivity to unemployment variations[b] (effect of expenditures on total ALMPs, employment service and labor market training, respectively)	−
		Real-wage sensitivity to unemployment variations (effect of job creation measures)	0
Zetterberg 1995	19 OECD countries; 1985–1991; panel data	Open unemployment (ALMP expenditures as fraction of total expenditures on unemployed)	− (−1.49)
		Total unemployment	− (−0.49)
Jackman, Layard, and Nickell 1996	20 OECD countries 1983–1988 and 1989–1994; 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; 1983–1993; panel data	Open unemployment	− (−0.51)
		Total unemployment	+ (0.49)
		Employment as fraction of population	+

Forslund and Krueger 1997	OECD countries; 1983–1988 and 1993; cross-section data	Open unemployment 1983–1988; (Zetterberg measure of ALMPs)[c]	− (−0.83)
		Total unemployment 1983–1988; (Zetterberg measure of ALMPs)[c]	0 (0.17)
		Open unemployment 1983–1988; (ALMP expenditure as fraction of GDP)	0
		Open unemployment 1993; (Zetterberg measure of ALMPs)[c]	+
Elmeskov, Martin, and Scarpetta 1998	OECD countries; 1983–1995; panel data	Open unemployment	− (−1.18)
		Total unemployment	0 (−0.18)
Nickell and Layard 1999	20 OECD countries; 1983–1988 and 1989–1994; panel data	Open unemployment	− (−0.18)
		Long-term open unemployment	−
		Short-term open unemployment	0
		Total unemployment	+ (0.82)
		Employment as fraction of population	0
Blanchard and Wolfers 2000	20 OECD countries; 1960–1995; panel data with five-year averages	Open unemployment	− (−1.43)
		Total unemployment	− (−0.43)
Fitoussi, Jestaz, Phelps, and Zoega 2000	19 OECD countries; panel data 1960–1998, cross-section data 1983–1988	Sensitivity of open unemployment to shocks	−
Okeke 2000	20 OECD countries 1986–1998, panel and cross-country data	Employment including ALMP participation; (Layard et al. measure of ALMPs)	+/0

a. 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 labor force.

b. 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).

c. See the entry for Zetterberg 1995 above in the table.

rates with cross-country variation in labor-market institutions. Later studies have used panel data to exploit both cross-sectional and time-series variations. Most of these studies have examined the influence of ALMPs. As these studies have usually been interpreted to give very favorable results for ALMPs, it may be of some interest to compare them with the studies of Sweden that we have surveyed.

5.1 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 participant in the labor force (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 program 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 we did this. The results are shown in table 1.13.

Most of the studies reported in the table support the hypothesis that an expansion of ALMPs contributes 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, as program placement can be used to interrupt long unemployment spells. However, looking at the calculated effects on total unemployment (the sum of open unemployment and program participation), the picture is different. Some studies indicate that total unemployment increases when ALMPs expand, others that it decreases. A couple of studies also find insignificant effects. One study (Okeke 2000) finds that ALMP spending according to some panel estimates contributes to higher employment (including subsidized employment), but that the effect is declining in the level of spending.[20]

5.2 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 labor force, and Forslund and Krueger (1997) substituted 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 reduce the problems of simultaneity, whereas Nickell and Layard (1999) divided ALMP expenditures by the number of unemployed persons in an earlier time period. According to the first study, ALMPs have an insignificant, negative effect on total unemployment, whereas they have a significant positive effect according to the second study.

On the whole, the conclusion seems to be that the results of ALMPs appear less favorable when simultaneity bias is addressed. One should also note that the reported results refer to unemployment as a share of the *labor force*. As was noted in subsection 4.3, results may be more favorable for ALMPs if unemployment is instead measured as a fraction of the population, as the programs may influence labor-force participation positively. Two of the studies on OECD data are consistent with 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 labor force. Scarpetta (1996) found that ALMPs contribute to a lower share of inactive persons in the population.

6 Conclusions

6.1 The Various Mechanisms of ALMPs

The empirical studies surveyed highlight the following mechanisms or complexes of mechanisms of active labor-market policy: (i) effects on the matching process and the competition for jobs, as well as on productivity and the allocation of labor, (ii) direct crowding out effects, (iii) effects on the wage pressure in the economy, and (iv) the net effect on regular employment (and open unemployment).

6.1.1 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 labor (to the extent that ALMPs raise the productivity of the participants and re-allocate labor 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 labor-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 labor. Although the microeconomic studies of the effects of labor-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 subsidized employment, and here the results vary more.

The most favorable results for the effects of ALMPs on individuals are obtained in survey studies of employer attitudes. But on the other hand, participants in ALMPs seem to search less actively for jobs than the openly unemployed while in programs (although one study indicates that they are searching more actively after program completion).

There is also some evidence that ALMPs in Sweden may have raised labor-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 labor-force participation translates into effective supply rather than just raising the possibilities to collect benefits.

6.1.2 Direct Displacement

Both survey studies and econometric macro studies indicate that job-creation schemes have crowded out regular employment to a substantial degree. Labor-market training does not appear to have had such effects. The direct crowding-out effects are considerably larger in the econometric studies (often 60–70 percent) than in the survey studies (usually 15–40 percent).

6.1.3 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.

6.1.4 Net Effect on Regular Employment and Unemployment

The net effect of ALMPs on (un)employment in Sweden has 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 program 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 favorable picture of the employment effects of ALMPs (see, e.g., Layard et al. 1991; 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 similar to the one from the Swedish studies.

6.2 Relative Efficiency of Various ALMPs

What do the studies of Sweden say about the relative efficiency of different programs? A first issue concerns labor-market training versus subsidized employment. Here, the microeconomic studies of effects on individuals and the macroeconomic studies of general-equilibrium effects give inconsistent results. The microeconomic studies of labor-market training in the 1990s found no or negative employment effects. In contrast, some studies found positive effects of subsidized employment on later regular employment. But in the macroeconomic studies, there is a strong tendency that labor-market training gives more positive (or less negative) effects on regular employment than subsidized

employment. Only the latter programs seem to cause direct crowding-out effects.

Another issue concerns the relative efficiency of various subsidized employment programs. 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 programs 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 programs. 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.

6.3 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 labor-force participation that have not yet worked themselves out. We do not rule out such effects. Notwithstanding these caveats, our conclusion is still that the labor-market policy followed in Sweden in the 1990s was not efficient. 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 labor-market training. It is a problem to expand training programs 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 programs 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 labor shortages in the economy will appear. The upshot is that training programs should be kept rather small in a deep recession. There is certainly a strong case for not using ALMPs (especially training programs) 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 program participation and result in very large program volumes.

As to subsidized employment, 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.

Our survey also questions the use of large-scale youth programs, 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 programs during their first half-year of unemployment. This is an argument for much smaller youth programs than were used in Sweden in the 1990s.

One cannot, of course, analyze the proper role of ALMPs without corresponding evaluations of alternative policy instruments. Indeed, subjecting only some policies to critical scrutiny, but not others, could lead to a worse 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 programs, 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 judgment 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 labor-market policy.

Appendix

Many of the studies on data from a large number of OECD countries discussed in section 5 have estimated unemployment equations of the form

$$u = \alpha\gamma + \cdots, \tag{A1}$$

where

$$\gamma = b_r r / uy. \tag{A2}$$

u is open unemployment as a fraction of the labor force, γ is the measure of ALMPs, α is a parameter measuring the effect of ALMPs on open unemployment, b_r is the expenditure on ALMPs per program participant, r is program participation as a fraction of the labor force, and y is GDP per capita.

We are interested in computing $\mathrm{d}u/\mathrm{d}r$ and $\mathrm{d}(u+r)/\mathrm{d}r$ from the estimated equations. To do this, we substitute (A2) into (A1) and differentiate implicitly. This gives

$$\frac{du}{dr} = \frac{(b_r/y)\alpha u}{u^2 + (b_r/y)\alpha r}. \tag{A3}$$

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 (1995) provides information on γ. The database collected by Rødseth and Nymoen (1999) gives information on program participation and unemployment for the same countries. As $b_r/y = \gamma u/r$, we can compute this ratio. The average values for the period 1985–1991 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 (A3) is increasing in b_r/y, our "guesstimate" of 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 program participation. The effect on total unemployment (the sum of program participation and open unemployment) is

$$d(r+u)/dr = 1 + du/dr.$$

Zetterberg (1995) instead used the ratio between total ALMP expenditures and total expenditures on the unemployed (table 1.1) as the

measure of ALMPs in his unemployment equations. This measure, which we label λ, can be written

$$\lambda = b_r r / (b_r r + b_u u), \tag{A4}$$

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 program participant equals the spending per openly unemployed, i.e., $b_r = b_u$. Given this assumption, and given an estimated effect $\beta = du/d\lambda$, we have in this case that

$$\frac{du}{dr} = \frac{\beta u}{(u + r)^2 + \beta r}. \tag{A5}$$

In table 1.13, we have assumed throughout that $u = 0.07$ and $r = 0.03$.

Notes

1. Lars Calmfors is professor of international economics at the Institute for International Economic Studies, Stockholm University. Anders Forslund is a senior research fellow and deputy director of IFAU (the Swedish Office of Labor Market Policy Evaluation). Maria Hemström is a senior research fellow at IFAU. The authors are grateful for comments on previous versions from Jonas Agell, Susanne Ackum Agell, Jim Albrecht, Dan Andersson, Per-Anders Edin, Bertil Holmlund, Per Johansson, Katarina Richardson, Karl-Martin Sjöstrand, Alfons Weichenrieder, and Johnny Zetterberg.

2. See http://europa.eu.int/comm/employment_social/elm/summit/en/papers/guide2.htm.

3. This is evident from the national action plans on employment.

4. The main reference is Fackföreningsrörelsen och den fulla sysselsättningen 1951.

5. The exposition builds on Pissarides 1990, Mortensen and Pissarides 1994, and Romer 1996. See also Holmlund and Lindén 1993 and Fredriksson 1997 for direct applications to ALMPs.

6. 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 thus a given number of job seekers), the number of matches is also given, if we assume—as is conventionally done—that the number of separations from jobs equals a fixed quit rate times employment. It follows that at a given aggregate employment level, the probability for a job seeker to find a job is independent of matching efficiency.

7. Larsson (2001) discusses the problems arising when effects are computed from the time of program completion rather than from the program start. The control group in the former case can be chosen in two ways: Program participants can be compared to either (i) non-participants who became unemployed at the same time as the participants and

were unemployed both at the program start and completion; or (ii) non-participants who at the time of program completion had been unemployed for as long as the participants at the time of the program start. Both procedures are likely to give a positive bias in the estimation of the treatment effect. In the first case, non-participants with a small job-finding probability tend to be over-represented in the sample (because they did not find a job during the program period). In the second case, the participants have a longer time to search for jobs, because they can search also while participating in the program.

8. However, the analysis in Fredriksson and Johansson (2002) shows that the procedures used in this study result in a negative bias in the estimated program effects because the control group is selected in such a way that all spells end in employment.

9. See the discussion in note 7, which is applicable also to Okeke's study.

10. However, there is likely to be a negative bias in the estimated program effects, as discussed in note 8.

11. Recruitment subsidies was the only program associated with a significantly lower probability of future receipt of unemployment benefits.

12. This conclusion was based on estimates in which search behavior was related to different individual characteristics. The outcome measure was a "search index" constructed from answers to questions about search time and search methods. Information about program participation at (i) the time of the interview and (ii) the twelve months preceding the interview are two of the variables used to explain individual search behavior.

13. The relevant relationship to look at is the one between vacancies and *total* unemployment (the sum of open unemployment and program participation). Calmfors (1993) estimates how this relationship is affected by a change in the accommodation ratio (the ratio between program participation and total unemployment). Jackman et al. (1990) study instead the relationship between vacancies and *open* unemployment, but their results are recalculated in Calmfors (1993).

14. The authors could not reject the hypothesis that relief work and labor-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.

15. *A priori*, it seems likely that subsidized employment would be associated with geographical locking-in effects. Regarding training, the case is not so clear: on one hand, training can be a substitute for geographical mobility, on the other hand, training may provide the trainee with skills that are valuable outside the home region.

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

17. 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 are only spuriously reflecting cyclical patterns in both employment and relief work.

18. Relief work was singled out as a separate category to allow comparisons with earlier work.

19. Holmlund (1995) criticized 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.

20. The model includes ALMP spending as both a linear and a squared term among the explanatory variables; the estimated coefficient for the linear term is positive, whereas the estimated effect of the squared term is negative. However, the measure of employment includes subsidized employment, but not labor-market training, so the results are difficult to interpret, although it is clear that they are not compatible with complete crowding out of regular employment. Cross-section estimates show a zero effect on the employment measure, which indicates a negative effect on regular employment (excluding subsidized employment).

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