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EVALUATIONS OF SWEDISH LABOR MARKET POLICY*

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Sweden has for a long time spent large resources on labor market policies targeted to the unemployed. In the Anglo-Saxon labor economics literature new methods have been developed for the purpose of estimating the effects of such policies. This paper presents the new methods in this field and describes the main studies of Swedish labor market policies. The conclusion is that in spite of new methodological insight there remains much uncertainty about the effects of the Swedish policy experiment.

1. Introduction

Sweden is famous for its low open unemployment and its ambitious labor market policies. Since the early 1960s, when the first labor force surveys were done, the record high unemployment rate has been 3.5 percent in 1983; the record low was 1.2 percent in 1965. Taking the cyclical fluctuations into account, a slight upwards trend can be found in the data. Despite this trend, the development during the 1980s, when unemployment exploded in most other European countries, stands out as a remarkable performance by the Swedish labor market.

On the other hand, labor market policies to provide jobs and training for the unemployed have been extensive both in terms of expenditures and persons involved. During the cyclical downturns 1977/78 and 1982/83, total expenditures were around four percent of GNP, and close to five percent of the labor force were employed by means of some of the labor market policies.[1] The total policy package consists of a large variety of measures, the most important of which are temporary relief works (»beredskapsarbeten» in Swedish), training, mobility grants, employment service and special jobs (subsidies) for handicapped persons.

Relief works have been the most important counter-cyclical measure. In the cyclical peaks 1975, 1980 and 1987/88, less than one-half a percent of the labor force were employed by means of such jobs. When the business cycle turned down, the number of relief works increased very quickly to 1.5 – 2.0 percent. In the upturns the program has been reduced quite quickly too. A relief job typically lasts five or six months. From the beginning most jobs were in the construction industry, but

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[1] *The most informative presentation of Swedish labor market policy is Johannesson (1988).*

when youth unemployment increased during the seventies more of the jobs were provided by the public sector, in particular by local governments.

Labor market training has been used more as a »structural» measure, so the cyclical fluctuations have been smaller. During the last decade on average about one percent of the labor force has participated in this type of government supported training. The training courses typically last slightly less than half a year, so that more than 2 per cent of the labor force participate during a year. In general unemployed workers who need training have had good access to training courses during the last decade.

Various types of *mobility grants* can be paid by the labor market authorities to the unemployed. First, their travelling costs to visit employers at other places can be subsidized. Second, those who decide to move and accept a job in another region can receive grants which cover their costs to move plus an extra amount as a pure stimulus to move (»starthjälp»). The latter stimulus can be regarded as a compensation for the non-monetary costs of moving. It was introduced in 1959 and was increased gradually during the sixties. On July 1, 1984, the size of the stimulus was increased to SEK 15,000 (approximately $ 2,400) for households and SEK 4,000 (approximately $ 600) for single persons. All the grants were tax-free. From July 1, 1987, the extra stimulus is no longer paid. One of the stated arguments for abolishing this measure was that mobility grants often were given to persons who would have moved anyway.

The eligibility criterion for these programs is, with some minor exceptions, being unemployed or at the risk of becoming unemployed. Therefore both displaced workers (affected by structural changes in the labor market) and disadvantaged workers (low-skilled workers, often with some kind of a handicap) participate in these programs. The distinction between displaced and disadvantaged workers is unusual in Swedish labor market policy.

The expenditures spent on services provided by the *employment exchange offices* have gradually increased. These offices play a very central role in Swedish labor market policy. They are notified in advance when layoffs are planned, the unemployed who collect benefits are obliged to search via the offices, the work test in the unemployment compensation system is enforced there, and most labor market policy measures are implemented by the offices. Since the late seventies it is also compulsory for firms to notify the employment exchange of all new vacancies.

The purpose of this article is to summarize the available empirical evidence of the effects of these policies. The survey is confined to the studies of the effects for the program participants, whereas more general cost-benefit issues are omitted. Section 2 outlines the empirical methods used by labor economists to estimate effects of labor market interventions. A short presentation of the Swedish studies is offered in Section 3. The limitations of the leading research paradigm are briefly discussed in Section 4 and it is followed by a concluding section.

2. *The analytical approach*

Estimation of effects of labor market interventions like those in Swedish labor market policy has a long tradition in the Anglo-Saxon labor economics literature. The analytical approach relies on a comparison of the post-program labor market outcome between a group of program participants and a group of non-participants. The most common variables describing the labor market outcomes are earnings, employment, hourly wage rates, and unemployment durations.

The first studies from the late 1960s to the early 1970s were often labelled quasi-experimental. This was a proper label, because the attempt was to find a surrogate for a randomly selected control group with non-participants. The underlying idea was that the ideal evaluation methodology is a classical experiment, where both an experiment group and a control group are randomly selected among prospective program participants. Typical examples, of surrogates for a pure control group were groups of eligible persons, drop-outs from programs and individuals who had applied for but not were allowed to participate in the program. Any differences in the labor market outcome between the participants and the selected comparison group were attributed to the program and considered as the effect of the program.

This quasi-experimental approach has

gradually been replaced by methods, which are more elaborate both from an econometric and an economic point of view. In the early seventies it became more common to use regression analysis to control for differences between program participants and non-participants. A typical specification based on micro data is:

(1) $Y_i = \beta X_i + \alpha P_i + \varepsilon_i$,

where Y_i is the labor market outcome of individual i (earnings, employment etc.), X_i is a set of personal characteristics which are controlled for, P_i is a dummy for program participation and ε_i is an error term.

Economic theory and empirical experience from previous studies could give some guidance as to which variables to include as controls. For studies of the effect of labor market programs on earnings and wages, human capital theory was a common framework. The approach proposed by Jacob Mincer[2] to derive earnings functions from human capital theory suggested both a log-linear functional form and that the effect of work experience on earnings could have both a linear and a quadratic term.

Later in the 1970s, panel data (or longitudinal data) became available and made it possible to extend the analysis. The extension was more statistical than economic. The main virtue of panel data is that it permits one to control for omitted unobserved variables which can bias the program coefficients. This is evident if outcome equations are specified both for a period before the program and for a period after the program and if the error term is decomposed into a permanent effect and a period-specific part:

(2a) $Y_{ib} = \beta X_{ib} + \varepsilon_{ib}$
(2b) $Y_{ia} = \beta X_{ia} + \alpha P_i + \varepsilon_{ia}$
(2c) $\varepsilon_{ia} = \mu_i + e_{ia}$, $\varepsilon_{ib} = \mu_i + e_{ib}$,

where b and a denote periods before and after the program took place, μ_i is a permanent individual effect (»fixed effect»), and e_{ia} and e_{ib} are period-specific error terms.

The permanent individual effect can represent intelligence, ambition and similar attributes which are difficult to observe. These effects are eliminated by differencing (2a) and (2b):

(3) $\Delta Y_i = \beta \Delta X_i + \alpha P_i + (e_{ia} - e_{ib})$

In this way one potential source of omitted variable bias can be avoided. Even though the first-difference equation (3) has been the most commonly used in labor economics applications in general and program evaluation in particular, alternative specifications based on panel data have been considered. The set of specifications, based on alternative structures of the dynamics of the outcome variable, include:

(i) Extension of 2a – 2c by allowing the period-specific error terms to be serially correlated:

$$e_{it} = \rho e_{it-1} + v_{it}$$

(ii) Extension of (2a) – (2c) by allowing each individual to have not only an intercept of its own, but also an individual growth component:

$$\varepsilon_{it} = \mu_i + t\,\phi_i + v_{it}$$

Heckman, Hotz and Dabos (1987) call this model the random growth model.

(iii) Respecification of equation (2b) to let past values of the outcome variable affect its present value:

$$Y_{ia} = \beta X_{ia} + \alpha P_i + \gamma Y_{ib} + \varepsilon_{ia}$$

This specification is known as the state-dependence model. It can be combined with individual permanent effects as in (2c).

The versions (i) and (ii) require more than two waves of panel data as does (iii) when it is combined with individual permanent effects. In general these models require more sophisticated estimation techniques than the basic first-difference model, which can be estimated by OLS.

In the 1980s the methodology has been developed further. The development has mainly been economic but new econometric techniques have been necessary to estimate the new models. The background to the new method-

[2] *See Willis (1986) for a more than useful examination of human capital earnings functions.*

ology is the following: In the specifications presented above, program participation is exogenous, i.e. determined by some force which is »outside» with respect to the outcome variable. In a classical experiment with random assignment this is obviously the case.

From the perspective of an economist it is more appealing to assume that program participation is based on rational behavior. This is the idea behind self-selection models. The methodology is most straightforward in the case of a program which is open to all in the sample analyzed. This is the case for mobility grants in a sample of unemployed in Sweden. To simplify further we assume perfect foresight.

A rational unemployed person contemplating a possible move to another locality will compare the net benefits of the two alternatives. The prospective outcome of these two alternatives can be specified as[3]:

(4) $Y_i^s = \beta_i X_i + \varepsilon_i$

if the person stays and

(5) $Y_i^m = \beta_i X_i + \varepsilon_i + \text{Effect}$

if the person moves.

Rationality implies:

(6) Move (i.e. $P_i = 1$) if $Y^m - Y^s =$ Effect $>$ Costs

(7) Stay (i.e. $P_i = 0$) if $Y^m - Y^s =$ Effect $\leq$ Costs

To specify this model further, we have to formalize the effects and the costs. Obviously effects and costs cannot be constant and equal for all, because then all in the sample would either stay or move. In the real world we observe both stayers and movers. Hence either effects or costs or both must differ, i.e. some heterogeneity is required. The most general formulation is to allow both effects and costs to differ by an observable component and an unobservable:

(8) Effects: $\delta Z_i + u_i$

(9) Costs: $\eta W_i + w_i$

This gives the following complete model:

(10a) $Y_i = \beta_i X_i + \varepsilon_i + (\delta Z_i + u_i) P_i$
(10b) $P_i = 1$ if $\delta Z_i + u_i > \eta W_i + w_i$
(10c) $P_i = 0$ if $\delta Z_i + u_i \leq \eta W_i + w_i$

From an economic point of view this model is appealing because the impact of the program is estimated within a model which is compatible with the notion that those who voluntarily participate do so because it is good for them (high u_i's and low w_i's) and those who do not participate do so because it is good for them (low u_i's and high w_i's).

The concept of program effect can have three different meanings in the model. First, the effect on a randomly selected person in the sample which is δZ_i. Second, the average effect for those who actually have participated which is δZ_i plus the expected value of u_i conditional upon participation (in general positive because high effects persons are likely to be attracted to the program). Third, the marginal effect for new participants who would be attracted to participate if mobility grants or training stipends are raised. The marginal effect can be obtained by differentiating the outcome variable with respect to the costs (see Björklund and Moffitt (1987)).

Econometrically the model is rather complicated. First, it contains a random coefficient since the program coefficient in (10a) contains the error term u_i. Second, (10a) and (10b) constitute a zero-one model which require non-linear estimation. Third, there is a restriction between the equations since the Z-coefficients δ appear in both the outcome equation (10a) and the zero-one model (10b) and (10c). However, the full model can be estimated if distributional assumptions are made about the error terms ε_i, u_i, w_i and if an exclusion restriction is imposed so that at least one of the Z-variables is excluded from the W-variables.

It is important to emphasize that not only explicit self-selection models are compatible with rational behavior. Many regression models (with or without panel data) can also have this property given various assumptions. Heckman and Robb (1985a and 1985b) state these assumptions for a large number of models and for various types of data.

[3] The presentation will follow Björklund and Moffitt (1987). An alternative presentation can be found in Willis (1986).

Table 1. The percentage employed in 1971 and 1975 for movers and for the comparison groups.

	Employed in 1971	Employed in 1975
All movers	78 %	78 %
Non-return movers	86 %	83 %
Return movers	60 %	72 %
Comparison group 1	76 %	80 %
Comparison group 2	88 %	91 %

Source: From Figure 9.5 in Dahlberg (1978b).

An important conclusion is, though, that ordinary regressions models only provide the average effect for those who have participated. A complete self-selection model is necessary to make the distinctions between the effects for randomly selected participants, for those who have participated, and for marginal new participants.

This short survey of the methodological development shows that during the last 20 years there has been a shift from rather mechanical quasi-experimental comparisons to more explicit behavioral models estimated by means of rather sophisticated techniques. Hence, evaluation researchers today are equipped with a whole set of models which under various assumptions are compatible with rational behavior.

3. *The studies*

In an international perspective the number of evaluation studies are very few in Sweden compared with the U.S. The studies in the labor economics tradition of the main programs are only six[4] and will now be briefly presented.

3.1 Mobility grants

Dahlberg (1978a and 1978b)

The only study of the effects of mobility grants which has been done in Sweden was confined to movers (with mobility grants) from six places in northern Sweden during 1969 and 1970. The sample size was 1600 movers.

The outcome in terms of employment and earnings of the movers was compared with the outcome of two comparison groups. Comparison group 1 consisted of persons who were unemployed any time or took part in any other labor market policy measures during 1969 –70 at the same places. Comparison group 2 consisted of other labor force participants from the six places. The first comparison group was considered as the most reliable one.

The labor market outcome and background characteristics of the movers and the comparison groups were described by means of surveys, one in 1971 and one in 1975. Data on yearly earnings during 1971 – 1974 were collected from official registers.

The effect on employment status in 1971 and in 1975 was analyzed in Dahlberg (1978b) by a quasi-experimental comparison (standardized by sex, age, and social background) between the movers and the comparison groups. The results are reported in Table 1. It appears that all movers did slightly better than comparison group 1 in 1971 (78 percent employed versus 76 percent) indicating a small positive effect. In 1975 the situation was reversed with a slightly higher employment figure for comparison group 1 than for all the movers (80 percent versus 78 percent).

It also appears from Table 1 that those movers who did not move back to their original place did much better than the comparison group 1, especially in 1971, whereas those who moved back again did markedly worse than the comparison groups.

In Dahlberg (1978a) regression analysis is used to study the effect on yearly earnings in the four years 1971 – 1974. This analysis is, however, confined to the non-return movers, i.e. the most successful migrants according to the analysis above. Yearly earnings is the dependent variable in the regression analysis and control variables describing age, sex, family status, education, unemployment before migration, occupation, and residence were used. The coefficients of the dummy variables for migrants were interpreted as measuring the effect of mobility grants on earnings. The estimates revealed significant and large positive effects on earnings in 1971 (SEK 4300), 1972

[4] *Three studies from the early seventies about training will not be presented here. Two of them used a methodology which differred from the research paradigm in labor economics and one of them did not report the estimated equations in a satisfactory way.*

Table 2. Some results from the experiment in Eskilstuna.

		Experimental group	Control group
1	The percentage with a job at the end of the experimental period	48 %	34 %
2	Weeks of unemployment from the start of the experiment until the follow up 9 months later	11 weeks	18 weeks
3	Average monthly earnings for the employed	SEK 3,588	SEK 3,386
4	Percentage with a permanent job	92 %	68 %
5	Percentage with negative attitudes to the quality of the work	12 %	27 %

Source: Delander (1978).

(SEK 2800), and 1973 (SEK 2800) whereas the effect in 1974 (SEK 900) was smaller and insignificant. The effect on earnings for all movers was not reported in any of the papers.

3.2 Intensified employment services

Delander (1978)

This is the only classical experiment in Swedish (labor market policy) evaluation research. It was done in the town of Eskilstuna in 1974. The study population, comprising up to 400 persons consisted of the unemployed job seekers who were registered at the employment agency's district office in Eskilstuna, Sweden, for three months or more. The office in Eskilstuna was given a personnel reinforcement for the period of the experiment, March 10 – June 6, 1975. The experimental group used the agency's services for an average of 7.5 hours during the experiment period compared with 1.5 hours on the average for the control group. The latter group received normally dimensioned service, which is why the study is aimed at measuring the effects of increased service in particular.

The randomly selected experimental and control groups included 216 and 194 persons, respectively. Information about them was gathered at the beginning of the experiment, at the end, and c. 9 months later with the help of a mail questionnaire, where the nonresponse rate was only 10 %.

On the whole, this experiment produced very considerable, positive effects. An overview of the most important results is presented in Table 2. The first two lines give different quantitative measures of the effects of the experiment, such as employment and unemployment. All the measures indicate clear differences to the advantage of the experiment group, that is positive effects. It is interesting to note from lines 3 – 5 that also the quality of the placements were improved. Hence improvements regarding both quickness and the quality in the placements occurred.

For the most, these differences between groups were reported to be significantly different from zero, that is, it is unlikely that mere chance caused the difference between groups. Delander also carried out some cost/benefit calculations for the profitability of the efforts. These also showed strongly positive results.

Engström et al (1988)

In this study the effects of intensified employment services to support redundant workers who left the Swedish mining company LKABs plants in Lapland during 1983 are examined. A special delegation was formed to provide employment services for the redundant workers. The effects of this delegation was examined by comparing the labor market outcomes for LKAB workers with a group of other unemployed persons in the same region[5].

The effect of the delegation on the duration of unemployment spells was examined by estimating a hazard model of the probability of leaving unemployment. Four technically

[5] *Analysis of the effects of the delegation can also be found in Ohlsson (1988).*

different specifications were estimated, the simplest of which was (slightly changed for expositional purposes) as follows:

$$h(t)_i = \exp(\beta_1 X_i + \beta_2 \, LKAB_i)$$

where $h(t)_i$ is the hazard of leaving unemployment for a job, X_i is a set of personal characteristic's and $LKAB_i$ is a dummy for the LKAB workers who received service from the delegation.

The other specifications were extended to account for duration-dependent hazard rates, heterogeneous hazard rates and a combination of duration dependence and heterogeneity. The dummy coefficient for LKAB was small and insignificant in all four specifications, indicating little or no effects of the delegation. Interestingly the estimated coefficients were almost the same in all four specifications.

Engström et al. also examined whether the work of the delegation had any effects on the number of unemployment spells during the period July 1983 to October 1986. A similar equation was estimated but the dependent variable was the number of unemployment spells. Because of data limitations the number of spells is truncated upwards to four or more spells as well as downwards to zero. Hence a two-limit Tobit model is appropriate in order to take the value of the dependent variable into account.

The estimates suggest a large and strongly significant effect of the delegation; the LKAB group had markedly fewer spells of unemployment.

3.3 Temporary relief works

Sehlstedt and Schröder (1989)

This study is based on a sample of 500 individuals from the population of youths (20 to 25 years old) who registered as unemployed in four cities during the fall 1984. The design of the study was to identify the »strategy» chosen for each individual from the fall 1984 until the fall 1985 and compare the labor market outcome in September 1987 between the different strategies.

The following strategies were identified:

(i) temporary job »within a plan»[6] (88 persons),
(ii) temporary job »without a plan» (67 persons),
(iii) recruitment subsidy to an ordinary employer for a permanent or temporary job (28 persons),
(iv) relief work (40 persons),
(v) labor market training (17 persons),
(vi) education (39 persons),
(vii) unemployment (52 persons).

Those (105 individuals) who obtained a permanent job during the first year were deleted from the analysis.

The labor market outcome in 1987 was classified into four groups:

(i) permanent solution (a permanent job)
(ii) temporary solution »within a plan»,
(iii) continued turnover (between unemployment and temporary jobs without good prospects),
(iv) established in unemployment or labor market schemes.

The effects of the various strategies were estimated by a multiple logit model in (slightly simplified for expositional purposes) the following way:

(11) Prob(Outcome) = f(Background variables, Strategy),

where the four outcomes and the seven strategies defined above were used and where a set of variables describing the labor market history and various social characteristics were used as background variables.

The issue addressed was whether the outcomes of strategies (i) to (vi) produced significantly different (and better) outcomes than the strategy unemployment. The results are that the first three strategies produced (at conventional levels) significantly different and better outcomes, whereas the strategies relief work, labor market training and education did not produce significantly different outcomes.

3.4 Training

Edin (1988)

This study is confined to the effect of train-

[6] *A temporary job has a fixed duration and is not protected by the employment security legislation in the same way as a permanent job is. »Within a plan» means that the temporary solution is regarded as meaningful for the individual given his or her background and interests.*

ing for a sample of workers made redundant due to the closing of a pulp plant in Kramfors in northern Sweden in 1977. Edin collected data on earnings and labor market activities for the period 1969−80.

The model estimated was — slightly simplified — the following:

$$\begin{aligned} Y_{it} = {} & \mu_i + \gamma_1 a_{it} + \gamma_2 a_{it}^2 + \beta_{11} U_{it-1} + \beta_{12} U_{it-2} \\ & + \beta_{13} U_{ip} + \beta_{21} T_{it-1} + \beta_{22} T_{it-2} + \beta_{23} T_{ip} \\ & + \beta_{31} R_{it-1} + \beta_{32} R_{it-2} + \beta_{33} R_{ip} + \beta_{41} H_{it-1} \\ & + \beta_{42} H_{it-2} + \beta_{43} H_{ip} + \beta_{51} S_{it-1} + \beta_{52} S_{it-2} \\ & + \beta_{53} S_{ip} + v_{it} \end{aligned}$$

where Y_{it} = log weekly earnings relative to average industrial weekly earnings for individual i in year t, μ_i = an unobserved individual permanent effect, a_{it} = age, U_i = weeks of unemployment, T_i = weeks of labor market training, R_i = weeks of public relief works, H_i = weeks of sickness, S_i = weeks spent in regular education, and v_{it} is an error term.

Time spent in unemployment, training etc. can effect earnings with lags of one and two years. All past time spent in one of those states is denoted with subscript p and is interpreted as the permanent effect on earnings. The sample excludes individuals who left the labor force during the period. Hence the coefficient on any of the variables in the equation should be interpreted as the effect of time spent in the specific state instead of being employed. Edin uses the panel data to eliminate the individual fixed effects and allows the error term v_{it} to be serially correlated.

All three training coefficients are negative, i.e. labor market training reduces weekly earnings compared to employment. The effect during the first year is −9 percent and significant, but the permanent effect and the effect after two years are smaller and insignificant. More relevant is to compare the training and the unemployment coefficients because unemployment is often considered as the alternative to participation in training programs. It turns out that the drop during the first year is stronger for training than for unemployment and significantly so. On the other hand, the long-term effects of training and unemployment are not significantly different. Edin also found a significant negative second year coefficient for

Table 3. Effects of training according to various models in Björklund (1989).

A. Effects of training during 1976−80 on hourly wage rates and employment in 1981

	State dependence model	First-diff model	Self-Selection model
Effect on log of hourly wage at the time of the survey 1981	−0.049* (0.028)	+0.051 (0.039)	0.105** (marginal effect negative)
Effect on percentage employed at the time of the survey	+0.055* (0.031)	+0.080** (0.039)	−

B. Effects of training during 1976−1982 (first six months of 1982) on employment and income in 1983

	State dependence model	Fixed effect model
Effect on percentage employed anytime during 1983	+0.009 (0.024)	+0.013 (0.034)
Effect on log of yearly earnings during 1983	+0.044 (0.080)	+0.186* (0.097)

Standard errors in parenthesis.

* Significantly different from zero at the 10 % level
** — » — 5 % level
*** — » — 1 % level

Source: Björklund (1989)

relief work. On the other hand, participation in relief work was not significantly worse than being unemployed.

Björklund (1989)

This study focuses on the effect of training during the period 1976–1980. The sample analyzed was representative for the whole country. In this respect the results are more general than those obtained by Edin. On the other hand, the time spent in various labor market states (employment, training, unemployment, out of the labor force etc.) was not equally well documented in the data which introduces some uncertainty about the interpretation of the training coefficient.

The research strategy was to estimate several models to see whether the results are sensitive to the specification of the model. Furthermore, effects on hourly wage rates, employment and yearly earnings were estimated. The results are presented in Table 3.

The general pattern is positive but weakly significant effects of training. The only exception is the effect on hourly wage rates using a state dependence model which is estimated to be negative (not significantly different from zero though). The self selection model provided a significant positive average effect for participants on wage rates whereas the effect for marginal new participants was negative.

Two problematic patterns can be found in the results. Firstly, the standard errors are very large and secondly the estimated effects are quite sensitive to the choice of model. The latter indicate the need for some model selection procedure which can discriminate between alternative model specifications.

4. *Limitations of the research paradigm*

The research paradigm presented above has a very strong position in labor economics. A large number of empirical studies have been done, new econometric techniques have been developed, and social experiments have been designed within its framework. Still it can be argued that it has some limitations, in particular that it is too partial an approach to evaluations of labor market interventions. The methods have in common the assumption that the program does not affect the outcome of the non-participants. If such indirect effects exist, the prevalent methodology is at best incomplete in that it does not capture the effects on non-participants. Even worse, it can also give rise to biased results because the comparison between participants and non-participants might not capture the total effects for the participants.

Two critical voices can be found in the literature. Johnson and Layard (1986) formulate a segmented model of the labor market with two types of labor, skilled and unskilled. Because of market imperfections there are queues in the sector for skilled labor and unemployment in the one for unskilled labor. By training an unskilled worker for a skilled job, there will be an earnings gain for the trained person. In addition, however, an unemployed person will get a job in the sector for unskilled. A (quasi-)experimental evaluation study based on a comparison between participants and non-participants would not be accurate in such a labor market.

Blau and Robins (1987) formulate a simple general equilibrium model with two sectors, one for trained and one for ordinary labor. A government training program can transfer labor from one sector to the other. The theoretical model predicts that the impact of the training program declines with the program size because wages of non-trained labor will be affected. An empirical illustration where the program effect is allowed to interact with the program size in the local labor market, supports the theoretical prediction of the model.

Defense against this criticism can follow two lines. First, it can be argued (Burtless and Orr (1986)) that the traditional methodology only captures the partial equilibrium effects. If it takes some time for the indirect effects on non-participants to appear, the studies based on the traditional methodology will appropriately capture the partial equilibrium effects. Furthermore it is argued, that favorable partial equilibrium effects are necessary for favorable general equilibrium effects.

A second line of defense is to rely on the results by Willis (1986) which are that the Mincer type of log wage specification has a solid general equilibrium foundation. Most studies of training programs have in actual practice been based on such wage or earnings specifications.

5. Conclusions

The evaluation methodology described above and typified with the main Swedish studies has developed markedly from rather mechanical comparisons between groups to more explicit behavioral models estimated by means of sophisticated econometric techniques. Despite this — in several respects impressive — development, the empirical studies have not been able to produce results about which there is consensus in the scientific community. Barnow (1987) who reviewed the American studies of the CETA programs[7] found a disturbing discrepancy between the estimates produced by marginally different methods using almost the same data. The standard errors of the estimates of the program effects are also large in many studies.

Our review of the Swedish studies showed clear positive effects only for intensified employment service; the two studies suggested rather strong positive effects. The studies of labor market training gave ambiguous results. The uncertainty was of the same kind as in the American studies of the CETA programs. The study of temporary jobs showed insignificant effects. The study of mobility grants, finally, must be interpreted with great care since it used the methodology prevalent during the early seventies.

In the literature there are two views about how to go on from here. One view (Ashenfelter and Card (1985)) advocates classical experiments as the only possibility to get reliable estimates of program effects. The other view argues that a testing procedure which can discriminate successfully between alternative models can be designed (Heckman, Hotz and Dabos (1987)). At issue is also whether classical experiments are feasible for all types of evaluations (Björklund (1988)).

In the journal literature the problems of data quality and non-response rates are seldom recognized. The large standard errors — which in general are understated because of the non-response — are probably caused by bad data quality. More attention must be paid to these — less glamorous — issues of data quality in order to get estimates of reasonable precision.

Taking these methodological problems into account one has to admit that there is little solid evidence about the effects of Swedish Labor Market Policy. Even though Sweden has managed to keep unemployment at a low level it is not possible to decide whether those participating in labor market training should be considered as disguised unemployed or not.

One of the greatest Swedish economists — the late Erik Lundberg — often argued that research makes us »confused at a higher level». Unfortunately this expressions is appropriate about the evaluations of the labor market policy. The methodological development has, no doubt, taken us to a higher intellectual level. However, as long as slightly different models produce qualitatively different results we have to admit that we are confused about the real issue: the effect of the policies!

[7] *CETA — Comprehensive Employment and Training Act — was the main federal training program in the U.S. during the 1970s.*

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