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## The relative efficiency of labor market programs: Swedish experience from the 1990's

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IFAU - OFFICE OF LABOUR  
MARKET POLICY EVALUATION

# **The relative efficiency of labor market programs: Swedish experience from the 1990's**

Kenneth Carling, Katarina Richardson

WORKING PAPER 2001:2

# **The relative efficiency of labor market programs: Swedish experience from the 1990's\***

by

Kenneth Carling and Katarina Richardson\*\*

March 9, 2001

## **Abstract**

This paper estimates the relative efficiency of eight Swedish labor market programs in reducing the unemployment duration for participants. The analysis uses a hazard regression model and a uniquely large and rich administrative data set that contains all adult workers who became unemployed during 1995-1997. We find that programs in which the participants obtain subsidized work experience and training provided by firms, have better outcome than classroom vocational training. The relative efficiency is similar across demographic and skill groups and independent of the timing of the placement. A careful examination of the assignment process to programs reveals no self-selection, but substantial administrative-selection. However, the administrative-selection appears to be unrelated to the outcome and, thus, should not bias the results.

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# 1. Introduction

Active labor market programs (hereafter programs) constitute a cornerstone of the labor market policy in many countries. For instance, the European Union and the OECD-countries have in recent years emphasized programs as an important means to reduce long-term unemployment, see EC (1998) and OECD (1996). One main purpose of programs is to increase unemployed workers' employment prospects either by facilitating their job search, improve their work habits or augmenting their human capital (Heckman, Lalonde and Smith, 1999). Despite the increasing use of various programs, essential knowledge of what characterize successful programs is lacking. This paper offers some evidence of programs' relative success in decreasing the participants' unemployment duration by use of Swedish micro data.

We focus on two issues. The first one is what type of programs reduces the participants' unemployment duration the most? Does the relative efficiency vary between different demographic and skill groups? For example, are some programs more useful for low educated workers than for others? Do some programs stand out as more beneficial to women than to men? Knowledge concerning these questions could be of importance to policy-makers who design the programs and take decisions on the program mix. Other decision-makers, as caseworkers and unemployed workers, may utilize this information in order to choose the program that increases the worker's employment prospect the most.

Most previous evaluation studies concern the effects of participation in one program compared with non-participation: a question we do not address in this paper. It is difficult to draw conclusions on programs' relative success from these studies since they often are not comparable to each other in several important aspects. The studies stem from different countries or time periods, and may concern different demographic or skill groups. Further, they may also use different outcome variables and estimation techniques.<sup>1</sup>

The second issue of this paper concerns whether the relative efficiency is affected by the timing of placement in programs. The issue of timing of placement in programs has not received much attention in the evaluation literature. There are pros and cons of late placement, (Calmfors, 1994). One advantage is

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<sup>1</sup> We are aware of three previous studies on programs' relative efficiency in increasing the participants' employment prospects, namely Bonnal, Fougère and Sérandon (1997), Gerfin and Lechner (2000) and Ridder (1986). Below, we compare our result to theirs.

that late placement makes it easier to avoid participants that can easily find a job on their own. A possible disadvantage of late placement is that the treatment effect might be smaller, which would be the case if it becomes more difficult to restore the unemployed persons' competitiveness in the labor market the more it has been allowed to deteriorate, (Layard *et al*, 1991). It is likely that the disadvantages of late placement vary between programs since they aim at giving the participants different treatments. Hence, we investigate whether the relative efficiency of the programs is affected by elapsed unemployment duration. However, we cannot infer from this information whether there is an optimal point of time to leave open unemployment to program participation

Sweden is, for several reasons, a suitable country for addressing the two issues of programs' relative efficiency. First, Sweden has a long and large-scaled experience of programs. In each year during the last decades, between 2 and 5 percent of the labor force participated in programs, or put differently: roughly one third of the stock of unemployed were in labor market programs.

Second, the Swedish experience of programs relative efficiency might be of interest to policy-makers in other countries as well. Programs that are targeted at adult workers have simple eligibility restrictions; in order to be eligible to the programs the worker should be registered as unemployed at a local Employment office. As a consequence, all demographic and skill groups participate in all programs and we are able to infer the relative efficiency of programs for each sub-group.

The third reason is the existence of large and rich administrative data in Sweden. Few other countries are comparable to Sweden in this respect. In this study we utilize non-experimental longitudinal micro data produced and provided by the Swedish National Labor Market Board. The database contains the event history of all those who have registered at an Employment office since August 1991. As a consequence, it contains information on all workers who since then have been eligible to publicly financed programs. For these workers, we know the dates of transition between regular employment, unemployment and programs. The database also contains information on several worker characteristics and from another database we obtain the worker's pre-unemployment wage and working hours.

The comparison focuses on the eight large-scale programs that were available for adult workers between June 1995 and December 1997. We restrict the analysis to workers who entered their first program and we follow them until they found a job or at the most until March 1999. With these restrictions we

end up with 25 280 individuals. We utilize hazard regression analysis to investigate the questions of interest.

The three main conclusions from the analysis are: First, we find that programs in which the participants obtain work experience and training provided by firms, have better outcomes than classroom vocational training. The more regular work the participants are allowed to do the better the program is relative to other ones. Second, we find that the timing of placement in programs does not affect the relative efficiency of the programs. Third, the results are the same across demographic and skill groups. It can be noted that our findings are similar to the ones in previous studies<sup>2</sup>. The similarity in the results across studies, carried out in various countries having different labor market structures and policies regarding labor market programs (i.e. eligibility rules, program mix) indicate a general validity of the conclusions.

A crucial issue in all evaluation studies is the selection of participants into programs and the consequential selection bias in the results. One strand of the literature abstracts from the problem by assuming that the selection of participants is not based on unobserved characteristics. Another strand allows that workers are selected into programs based on unobserved characteristics. It aims to take such unobserved heterogeneity into account at the expense of invoking model-specific assumptions. Common to most studies in the evaluation literature is however that the selection process *per se* is not discussed. It is therefore difficult to determine how reasonable different claims about the selection process are.

In this paper we take a third approach that we believe is fruitful for addressing the issue of unobserved heterogeneity and the relative efficiency of programs. We discuss the selection process into different programs with available data and, specifically, we investigate what factors that determine in which program the worker ends up. Harkman (2000) finds that unemployed workers tend to value various programs equally. We find that a worker's observed characteristics are relatively unimportant in determining which program he ends up in, even though we observe many potentially important characteristics. It is far more important to know which Employment office the worker belongs to. Our interpretation of the results is that the caseworkers use similar selection criteria

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<sup>2</sup> Bonnal *et. al* (1997) use French data from the mid-80's on young unemployed workers, and Ridder (1986) use Dutch data (Rotterdam) from the late 70's. Gerfin and Lechner (2000) use a very large and recent Swiss data set on unemployed adult workers.

within an office but not across offices. It is possible that these office specific criteria are based on unobserved characteristics that in turn are correlated with unemployment duration. However, we do not find that the relative efficiency of programs vary between offices as would be expected in this case and hence we are led to believe that the potential selection bias in the result is small.

This study has some limitations that ought to be mentioned. First, we choose to focus on programs' relative success in reducing unemployment duration. The reason is that the main purpose of programs in Sweden as well as in most other countries is to prevent long period out of employment (Prop 1997/98:1). However, a complete picture of the relative efficiency calls for several comparisons between programs. For example, various programs may have different effects on participants' subsequent employment spells as well as on wages. Moreover, the programs may affect non-participants differently as well, see Calmfors (1994).

A second restriction of the study is that we do not investigate whether participation in any program is better or worse than non-participation. The reason is that most Swedish, unemployed workers will end up in a program and hence it is difficult to find a comparison group that has not participated in any programs.

The structure of the paper is as follows. Section 2 describes the Swedish labor market programs targeted on adult unemployed workers. In section 3 we present the data, formalize the research question and discuss different effects of labor market programs. Section 4 is devoted to an in depth discussion of the selection process into programs. Section 5 sums up the results and offers a concluding discussion.

## 2. Description of the labor market programs

There were eight programs directed to adult unemployed workers that dominated during the investigated period. In *Table 1* the main features of these programs are described. The programs are presented in an order that reflects the degree of vocational training. The programs in the bottom of the *Table* have the highest degree of vocational training, whereas the ones in the top of the *Table* have the highest degree of on-the-job practice - in fact, they imply that a regular job is being carried out.

Some eligibility rules are common to all programs: for example, the worker must be registered as unemployed at the local employment office and be in a certain age group. All persons in our data set, which is described in *section 3*, qualify for participation on these criteria. A few programs have additional eligibility rules, which are indicated in *Table 1*. For example, ALU requires that the worker collects either UI-benefits or cash assistance. SUBE requires that the worker has been openly unemployed for at least six months.

The first category of programs in *Table 1* contains only one program, namely self-employment grants, SEMP. Workers with an approved business idea and financing plan may receive a self-employment grant for 6 month, where the grant usually is equivalent to the worker's UI-benefits.

The second category is *Subsidized On the Job Training Programs*, which contains SUBE and TRS. The purpose with SUBE is to increase the workers chances of receiving a job offer. Usually there is an agreement between the employer, worker and the caseworker that the engagement should continue after the program ends. In TRS the unemployed worker works as a deputy while the regularly employed person gets additional training.

Three programs are classified as *wage and employment subsidies*. One goal common to these programs is to induce employers to provide job-relevant skills to the worker.<sup>3</sup> However, there is a substantial within program variation in what type of skills, if any, that is provided. Participants in all three programs are not allowed to do work that a regular employed person otherwise would do. Compensation to the employer differs between programs: ALU is the cheapest and RW is the most expensive program. API is provided to both private and public sector. ALU and RW are usually provided to (non-profit) organizations that are not expected to employ new workers if they are not provided with a subsidy.

Finally, there are two programs, AMU and CAC, in the category *classroom training services*. CAC, which means basic computer training, was actually classified as an AMU-program prior to July 1995. More advanced courses in computer training are, however, still classified as AMU, at least up to and including 1997<sup>4</sup>. CAC usually runs for 3 months while AMU courses may vary from a couple of days to 30 or 40 weeks. There are both non-vocational and

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<sup>3</sup> It is actually very difficult to find more precise descriptions of the programs than the one provided in *Table 1*.

<sup>4</sup> In 1998, a partly new organization was build up to administer computer courses, the so-called Information technology training program.

vocational training provided under the AMU-label. Courses that provide non-vocational training do not aim to increase the participants' re-employment prospects directly after the program. Rather, non-vocational courses are viewed to help workers with weak education to increase their benefits from further education or programs. Hence, we exclude participants in non-vocational training from the analysis.

Participation in a program may influence the duration of unemployment benefits or the replacement ratio. In order to be eligible for UI-benefits<sup>5</sup> the worker needs to be a member of an UI-fund for at least 12 months. During this period, the worker must have worked at least part-time for six months (at least 70 hours each month). UI-benefits are paid out for a maximum of 300 working days or 60 weeks measured in calendar time. By participating in one program for at least six months, or several shorter programs, the worker is eligible for a new benefit period of 300 working days. In principal, participation in a program makes him eligible for a new benefit period only if he does not drop out of the program.

While participating in a program, the worker receives either the stipulated wage rate or receives an allowance equivalent to his unemployment compensation<sup>6</sup>, see *Table 1*. Workers who receive part-time UI benefits may become eligible for full-time UI-benefits by participating in a program running on full time. This opportunity was taken away in 1997. A second possibility to increase the UI-benefit has been to participate in a program that offers higher wage than the worker's pre-unemployment wage.

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<sup>5</sup> For a broader overview of the Swedish UI-system, see Carling *et al.* (2001).

<sup>6</sup> The benefit level is based on the worker's pre-unemployment wage and working hours. In 1995, the replacement ratio was 80 percent of pre-unemployment earnings up to a ceiling of 15 510 SEK in monthly earnings. The replacement ratio was cut from 80 to 75 percent in 1996 and at the same time the ceiling was increased to 16 544 SEK. The replacement ratio was restored to 80 per cent in September 1997. Workers that are not eligible for UI benefits may receive "cash assistance" (kontant arbetsmarknadsstöd, KAS). Compensation from KAS is much lower than the UI benefits and is not earningsrelated. The daily compensation was 245 SEK in 1995 and 230 SEK in 1996. These figures compares to approximately 40 percent of the maximum compensation from UI-benefits. KAS is paid out for a maximum of 30 weeks (150 working days). It is not possible for a worker with cash assistance to become eligible for UI-benefits by participating in a labor market program.

**Table 1:** Swedish labor market programs.

Program	Description of program	Employer type	Participant's compensation	Employer's compensation/contribution
<b>Selfemployment services</b>				
Self-employment grants (SEMP)	Purpose: Provide the worker with an opportunity to start up a new business. The worker needs approval of a business idea and a financing plan in order to be eligible for SEMP. ALU or AMU may precede SEMP with the purpose to help the worker prepare himself for self-employment. Costs*: 9 689 SEK.		Equivalent to the worker's UI-benefits. Business may generate additional income.	
<b>Subsidized on the job training:</b>				
Subsidized employment (SUBE)	Purpose: makes it easier for worker to receive a regular job offer. Subsidized regular employment of long-term unemployed (> 6 months in 1997) workers. The engagement is expected to continue after program ends. Costs*: 5 968 SEK.	Mainly private firms. Some sectors are excluded due to EC employment rules (AMS, 1997)	Wage and other benefits that comply with the collective agreement	Maximum of 350 SEK per day or 50 percent of the wage costs
Trainee replacement scheme (TRS)	Purpose: gives the worker experience of a regular job and help employers increase the employees' competence. Combines training for an employed worker with a possibility of a deputy ship for an unemployed worker. Costs*: 7 665 SEK.	Mainly work in the public sector and, in particular, health and related community service.	Wage and other benefits that comply with the collective agreement	Maximum of 350 SEK per day or 50 percent of the wage costs. The educational costs of the employee are subsidized.
<b>Wage and employment subsidies:</b>				
Work placement scheme (API)	Purpose: sustain work experience, give worker new employer references. The program may also be offered workers that wish to try a new occupation. The participant should only carry out work that would otherwise not have been done Costs*: 6 993 SEK.	Private and public sector.	Equivalent to the worker's UI-benefits	Employer pays 3 000 SEK. (2 000 prior to 1998) and 1 000 SEK for non Swedish citizens

\* Measured as monthly total cost per participant (AMS 1998a ).

*The Table continues on next page.*

**Table 1:** (Cont.) Swedish labor market programs.

Program	Description of program	Employer type	Participant's compensation	Employer's compensation/contribution
<b>Wage and employment subsidies:</b>				
Relief work (RW)	Purpose: Increase labor demand in periods with high unemployment. The program should offer the worker an opportunity to practice his/her skills and also to avoid UI-benefit exhaustion. Mainly temporary jobs in the public sector. The participant should only carry out work that would otherwise not have been done. Costs* : 9 201 SEK.	Mainly municipalities and state organizations	Wage and other benefits that comply with the collective agreement	Maximum of 50 percent of the wage costs or 7 000 SEK per month
Work experience scheme (ALU)	Purpose: avoid UI-benefits exhaustion and help the participant keep in touch with working life. Worker must have UI-benefits or cash assistance in order to be eligible. The participant should only carry out work that would otherwise not have been done Costs* : 9 294 SEK	Almost 90 % of ALU programs is run by non-profit organizations or public sector	Equivalent to the worker's UI-benefits	none
<b>Classroom training services:</b>				
Labor market training (AMU)	Purpose: Reduce bottlenecks in the labor market by providing skills necessary for particular jobs in excess demand. Costs* : 13 940 SEK.	Mainly targeted on technical, manufacturing and adm. occupations	Equivalent to the worker's UI-benefits	
Computer/activity centers (CAC)	Purpose: increase basic computer knowledge in the labor force. Training is usually combined with other activities. CAC was not available for workers older than 25 years in 1995-96. Costs* : 7 269 SEK.		Equivalent to the worker's UI-benefits	

\* Measured as monthly total cost per participant, see National Labor Market Board (1998).

### 3. Data, research question, and results

We utilize a data set containing event history data on all unemployed workers registered at the public Employment offices in Sweden since August 1991. Registration at an Employment office is compulsory for workers who receive unemployment compensation. For workers not entitled to unemployment compensation registration is voluntary but required for those who want full service at the office and access to labor market programs. The latter must be stressed because it implies that registration is necessary for being eligible to publicly financed programs.

The database contains detailed information on the worker's unemployment spells. For example, the date when he register at the employment office, the date he starts a program, what type of program, the date when he leaves the program, and the date when his case is closed at the employment office and for what reason (job, education etc.). We also have information on the individual's age, education, and immigrant status, in what occupations he searches for jobs, and whether he has education and experience that occupation. We also know if the worker receives UI-benefits or KAS. We also a potential indicator of unobserved heterogeneity. The caseworker classify the workers by their needs of service when the workers register at the employment office. We select all workers that register (directly) as openly unemployed: Within this group workers are put into three categories; *i*) can take a job immediately, *ii*) in the need of guidance, and *iii*) waiting for a program. The third category is very small and we do not separate it in the first one. However the second is larger and in *Table A1* the percentage of workers in the second category in each program (see variable *Assigned code 12*).

**Table 2.** Mean value for the key variables (Standard deviation in parentheses).

	SEMP	SUBE	TRS	API	ALU	RW	AMU	CAC
Unempl. dur. prior to program (days)	144 (144)	142 (117)	94 (96)	184 (163)	287 (159)	167 (159)	163 (148)	199 (149)
Program's duration (days)	211 (69)	140 (59)	116 (84)	120 (72)	138 (74)	148 (78)	102 (101)	78 (36)
Unempl. dur. after program (days) <sup>*</sup>	23 (96)	61 (188)	96 (171)	200 (248)	293 (274)	268 (332)	266 (287)	213 (207)
Number of programs	1.06	1.17	1.30	1.63	1.94	1.77	1.85	1.75
Number of participants	3 048	1 006	753	5 383	5 006	1 006	6 251	2 827

Notes: <sup>\*</sup> Includes time spent in (possible) further programs, no adjustment for censoring is made. Other (control) variables are presented in *Table A1* in Appendix A.

We restrict the analysis to those individuals aged 25-54, who became unemployed for the first time between 1 January 1995 and 31 December 1997 and who started one of the eight programs discussed in *section 2*.<sup>7</sup> We follow the participants until they find a job or for other reasons leaves the Employment office. The observation window is closed at 15 March 1999. 1 January 1995 is chosen as first sampling date because after this date we have more detailed information on the type of program each worker participates in. Only workers who are openly unemployed when they register at the Employment office are included in the sample.<sup>8</sup> A second restriction is that we focus on the effect of

<sup>7</sup> The sampling window is 1995-1998 (including early 1999) and thus the results are relevant for that specific time period. There are however some details concerning the officials' coding of the programmes that ought to be mentioned. SEMP was not coded as a program prior to May 1, 1995, nor was API and CAC prior to July 1, 1995. This means that the very small fraction of workers who became unemployed early 1995 and entered one of the three programs before the summer will not be in the data. Moreover, as of January 1, 1998 the three programs SUBE, TRS, and RW are collapsed into one (new) code. We have excluded these observations as we can not correctly determine which of the three programs those workers participated in. To confirm that the results are not altered by our choice of excluding this subset of workers, an additional analysis was performed where all workers who entered a program 1998 or later were excluded.

<sup>8</sup> Some workers register at the Office since they anticipate future unemployment. These workers are excluded from the sample.

the first program that the individuals participate in, see also discussion in subsections 3.1 and 3.2.<sup>9</sup>

In total we have 25 280 workers. The mean values of worker characteristics are presented in *Table A1* in appendix 1. *Table 2* gives descriptive statistics on some key variables. A few things are worth noting. Firstly, the unemployment duration prior to participation does not vary a great deal between programs. For most programs the duration of unemployment is about half a year. The exception is unsurprisingly ALU which is often used as a means to avoid exhaustion of unemployment benefits for eligible workers at risk (see *Table 1*).

It can also be noted that it is not uncommon to see a program followed by other programs. In general, the longer the duration of the post-program unemployment, the likelier the entrance into a new program, see further discussion in section 3.1.

The *Table* shows the duration of the program. This is, however, the observed duration, which might differ from the intended (or the maximal) duration. Nevertheless, most programs appear to run for about 4-5 months.

### 3.1 Definition of the effects of program participation

Our choice is to focus on the effect of programs on the participant's re-employment prospects.<sup>10</sup> We distinguish between three effects of participation, defined by their occurrence in time. The third and last effect is the *treatment effect*, which is the effect on the participant's re-employment prospect once the program is completed. This effect could for example be the result of improved search techniques, reduced depreciation of already acquired skills, or that the participant acquired new and more valuable skills. Some programs may also

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<sup>9</sup> If we would extend the analysis to cover all programs a person may participate in, the analysis would quickly become unfeasible. The reason is that the number of parameters would increase very quickly while the number of observations in each combination of programs would decrease.

<sup>10</sup> We ignore wage effects and effects on employment duration participation in programs. We also ignore partial effects of ALMP on non-participants (for example, direct displacement effects) and "economy wide" or general equilibrium effects (For a more complete list of potential effects of ALMP, see Calmfors, 1994)). The analysis in this paper will not provide a measure of what would happen if a program were removed altogether (Carling and Larsson, 2000). Many studies measure the impact of ALMPs on income or hourly wage. In Sweden, increased income has not been an explicit goal of ALMP. Rather ALMP has been a measure to maintain a compressed wage structure that was enforced in the 1960s and 1970s by strong and coordinated unions. By reshuffling displaced workers from shrinking industries with excess supply of labor to expanding ones with excess demand, increased wages in the expanding industries were to be avoided.

reveal new information to a potential employer about the participant's productivity and thereby increase his employment prospect with that employer (or other employers). A second (negative) effect of the program, the *locking in effect*, arises if the participant's search efforts are reduced during the program. For example, a worker may reduce his search effort if he finds the program attractive due to a high program-compensation, a large treatment effect or, because participation does not leave much time to search for a regular job. Edin and Holmlund (1991) find evidence, using Swedish data on young persons and displaced workers, that persons who participate in relief jobs find regular jobs at a slower pace than those that are *openly* unemployed.

The first effect is the so-called *announcement effect* on the worker's re-employment prospect once he is informed that he is accepted to a program. Programs may have different announcement effects because of different locking-in- and treatment effects.

Obviously, if the goal of the programs is to reduce the time out of regular employment, programs should "ideally" have a large positive treatment effect and as small as possible negative locking-in and announcement effects on the probability to find and accept a new regular job. In order to identify all three effects we need information on the announcement date, when the program starts and ends, and whether the worker completed the program or dropped out.

The data set contains information on the date the worker becomes unemployed and when he enters a program as well as how long he stays in the program. Unfortunately, we do not know whether he dropped-out or completed it. We also have information on the unemployment duration after the first program and on participation in subsequent programs. Hence, we are able to compare net-effect of the locking-in and treatment effects of the various programs. We are not, however, able to estimate the relative differences in announcement effects because we do not know the date the workers are informed about the program. By assuming that the announcement effects are similar between program<sup>11</sup> we address the question; what would be the change in expected unemployment duration if an eligible individual was picked at random and moved from one program to another?

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<sup>11</sup> This may be a strong assumption. However, we have contacted several caseworkers in order to find out when the worker is offered a program. Usually the program starts within one or two weeks after the announcement day. However, the AMU program may start several weeks after the announcement day.

In the sample, more than 50 percent of the workers participate in only one program. Approximately 75 per cent participate in two programs or less, and 5 per cent of the sample participates in four programs or more. These figures may indicate a potential endogeneity problem if, for example, the unemployed worker and the caseworker decide upon a whole sequence of programs instead of just the first program. In this case the workers behavior prior to, during, and after the first program is affected by the knowledge that he will participate in a second program. However, we believe that such an agreement between the worker and the caseworker is unlikely. The main reason is that we focus on the effect of the first program, which usually starts after 4.5 month of open unemployment. The official policy is that all workers who have been unemployed for 6 months have the right to have a “plan of action.” The worker and the caseworker should jointly produce this document. Approximately 27 percent of workers who were unemployed for the first time in 1997 had such an agreement with their caseworkers (AMS, 1998b).

However, the fact that unemployed workers participate in more than one program calls for a careful interpretation of the results. Participants in programs that turn out to be the least helpful are more likely to be offered a second program which in turn may produce new announcement-, locking-in- and treatment effects. We include the time spent in additional programs in the unemployment duration after the first program, see *Table 2*. Hence, in our analysis the effect of potential additional programs is assigned the first program.

### 3.2 The empirical method

In this section we will be specific about the method we use to address the evaluation question. In the next subsection we present the results. In the interest of clarity, we postpone the critical discussion of the method to *section 5*.

First some notations, let  $T$  denote the duration of unemployment from the start of the program until a job is reported.<sup>12</sup> Let  $\lambda$  denote the hazard function for  $T$  and  $D$  the program state, of which there are eight indexed by  $j=1,...,8$ . For simplicity, assume a homogenous population of unemployed being assigned, after a fixed elapsed unemployment duration, to only one of the pro-

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<sup>12</sup> An individual is considered to exit unemployment to employment the date he leaves the Employment office with the reason that he found a job or if he is register as “on a temporary job”.

grams through randomization. The parameter corresponding to the evaluation question is

$$(1) \quad \frac{\lambda^j(t)}{\lambda^k(t)} = \alpha^{jk}(t) \quad \forall j \neq k .$$

A relative comparison of the eight programs contains  $7!$  such parameters (or more precisely, functions).<sup>13</sup>

Suppose the timing of the program matters for the relative comparison of the outcome and that the elapsed unemployment duration,  $T_e$ , varies at random, then the parameter in eq. (1) can be extended to

$$(2) \quad \frac{\lambda^j(t | t_e)}{\lambda^k(t | t_e)} = \alpha^{jk}(t, t_e) \quad \forall j \neq k .$$

Furthermore, the individuals are likely to come from a heterogeneous population, and the effect may therefore vary across participants. Let  $X$  denote a set of characteristics pertaining to the participant, where the set can contain individual-specific as well as environment-specific features. To incorporate the heterogeneous effects, we define

$$(3) \quad \frac{\lambda^j(t | t_e, x)}{\lambda^k(t | t_e, x)} = \alpha^{jk}(t, t_e, x) \quad \forall j \neq k .$$

The parameter in (3) implies a formidable evaluation problem, particularly if  $X$  is an infinite set. This is so since sufficient pre-stratification is not possible even in an ideal randomized setting, and post-stratification will not necessarily produce consistent estimates of the effects. Thus, by necessity we assume that  $X$  is a finite set and that we measure it satisfactorily.<sup>14</sup>

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<sup>13</sup> Now, one could consider other definitions of counterfactual states, but we prefer this one for reasons that will be made clear in *section 4*.

<sup>14</sup> It is well known that estimates of hazards models might be sensitive to omitted variables, i.e. unobserved heterogeneity: the estimates tend to be biased towards zero. Hence the relative order of the programs is not affected by the presence of unobserved characteristics unless workers are not selected into programs based on these characteristics. Nevertheless there exist methods that at-

The conditional hazard model for each program is on the following form,

$$(4) \quad \lambda^j[t | t_e, x] = \lambda_0^j(t) \exp(x\beta_j + t_e\gamma_j) .$$

By assuming a piece-wise linear hazard in each time-interval, the discrete time hazard can be written as:

$$(5) \quad h^j[t | t_e, x] = 1 - \exp\{-\exp[x\beta_j + t_e\gamma_j + \eta^j(t)]\} ,$$

where  $\eta^j(t) = \ln\left(\int_{t_e}^{t_e+\Delta_l} \lambda_0^j(w)dw\right)$  and  $\Delta_l$  is the length of the  $l$ :th time-interval. We consider intervals of 30 days ranging from zero to 1110 days. The log likelihood function, given the model in (5), for a sample of  $n$  random observations on  $T^j$  and  $c$  is:

$$(6) \quad \ln LL(\beta_j, \gamma_j, \eta^j) = \sum_{i=1}^{n^j} \{c_i \ln(1 - \exp\{-\exp[x_i\beta_j + t_{e,i}\gamma_j + \eta^j(t_i)]\}) - \sum_{s=1}^{t_i} \exp[x_i\beta_j + t_{e,i}\gamma_j + \eta^j(s)]\}$$

where  $c_i = 1$  if the unemployment was observed to be terminated for a spell of employment and zero otherwise. The function is maximized with respect to its arguments.<sup>15</sup>

The specification in (4) allows for a program-specific hazard as well as program-specific effects of the control variables. At this step we have about 1000 free parameters to estimate. We are, however, trying to balance parsimony and completeness, by arriving at an acceptable simplification of this general specification: The basic strategy is to compare the estimates, pertaining to the control variables, obtained from each hazard, based on specification (4), to a more restricted specification. This is done by comparison of the program-specific es-

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tempt to adjust for unobserved heterogeneity. However we prefer not apply them for reasons discussed in Narendranathan and Stewart (1993).

<sup>15</sup> Starting values are obtained from the Approximate Maximum Likelihood method (Carling, 1995), and used in conjunction with the BHHH algorithm (see Carling and Söderberg, 1998).

timate with the one obtained if the control variable is specified to have equal effect on all programs. If the deviance of these two estimates are within two standard errors, it is considered acceptable to use the latter, i.e. the restricted specification. The deviance (presented as a t-ratio) and the estimates are presented in *Table A2* in the Appendix.

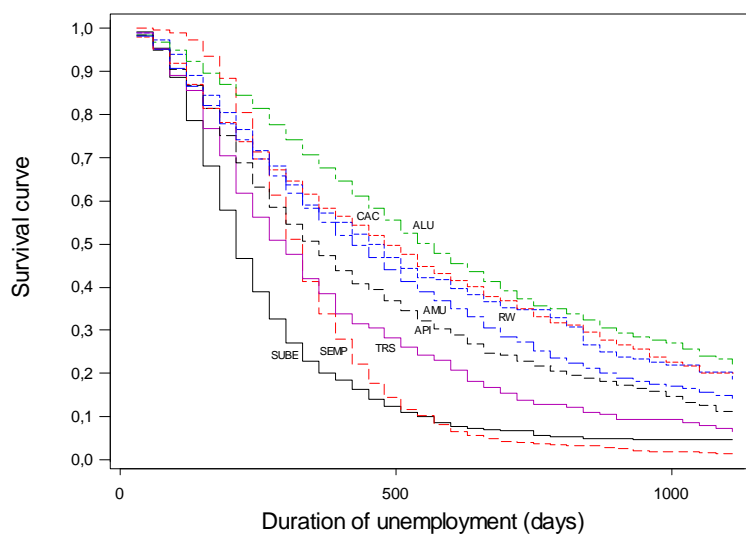
## 4. Results

We follow the terminology of Angrist and Krueger (1998) and make a distinction between causal variables and control variables. The estimates pertaining to the control variables are shown in *Table A2* in the Appendix. Below, we focus on the causal variables, i.e. program state and interaction terms.

Consider first the question of the relative efficiency of the programs. *Figure 1* shows the survival curves, i.e. the fraction that still has not left unemployment for a job, for the eight programs. The survival curve is evaluated for an average program-participant, i.e. an individual assumed to hold characteristics identical to the average in the data set.<sup>16</sup>

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<sup>16</sup> The effects of the programs are quite homogenous, and it is thus sufficient to make the comparison for an "average" worker.



**Figure 1.** The survival curve for an average program participant .

Clearly, the curves hold no simple relation and thus there is no simple statistic that can express the relative difference of two programs. In *Table 3* we present some statistics, 90th, 75th, 50th, 25th, and 10th percentile, for the programs that will aid in comparing the programs.

**Table 3.** Percentile-statistics (days until a job is found) derived from the model for an average participant.

Program	percentile				
	90	75	50	25	10
SEMP	171	228	303	406	543
SUBE	84	130	201	314	558
TRS	61	158	278	527	882
API	59	180	336	657	>1110
RW	80	219	448	869	>1110
ALU	89	294	543	1032	>1110
AMU	61	203	416	755	>1110
CAC	60	202	475	940	>1110

The percentile gives the fraction, derived from the model, that has not yet found an employment within the stated number of days.

Let the median be a reasonable statistic for making a comparison of the programs. It ranges from 201 days (SUBE) to 543 days (ALU). In other words, the model predict that, for a stock of eligible and homogenous workers, 50 % of the participants would have found a job within half a year if they entered in the program SUBE. Had they entered the program ALU, it would have taken them one and a half year.

*Figure 1* and the *Table 3* above refer to the basic model of no heterogeneous effect. Next we consider the second question of the timing effect of the program, i.e. we check whether the effect of the program depends on the unemployment duration preceding the program. In doing so, it has been necessary to introduce some restrictions on the model. Specifically, it is imposed that the baseline hazard is proportional across programs. The second column in *Table 4* gives the relative difference (%) between programs as the difference in the hazards (it can be compared to the difference in median duration as presented in *Table 3*). Extending the model to include interaction between program and timing does not affect the ranking of the programs. In fact, it hardly has any affect on the estimates at all. The timing is defined as the deviation from the average duration until a program is entered. The last column shows that the relative outcome of the program is insensitive to the timing. There are two exceptions, though: for CAC and ALU, the conditional probability of exiting to a job is decreasing the later the program is entered. This might follow as a consequence

of selection where less fortunate workers enters at a slower rate than more fortunate ones.<sup>17</sup> Nevertheless, it appears that the ranking of the programs is not affected by the timing.

**Table 4.** The effect of timing. The second column shows the relative efficiency of the programs before controlling for timing, the third after controlling for timing.

Program	Restricted model. No interaction	Restricted model with interaction with prior duration	Interaction effect
SEMP	-21.5	-23.8	-4.0
SUBE	0	0	5.2
TRS	-38.1	-37.7	-3.3
API	-68.1	-71.2	-6.1
RW	-96.3	-100.5	-11.9
ALU	-125.1	-119.9	-23.1
CAC	-101.8	-102.6	-25.9
AMU	-89.1	-92.4	-6.7

Assuming that the participants in the programs are made comparable by adjustment for the control variables, we can now draw three conclusions. First, we find that programs that imply practice at an employer (SUBE, SEMP, TRS and API) have a better outcome in terms of expected time out of regular employment than programs that are characterized by vocational training (AMU and CAC). The more similar a program is to a regular job the better it is relative to others. Second, we find that the timing of placement in programs does not affect the relative efficiency of the programs. Third, the results are the same across demographic and skill groups.

We note, finally, that we have performed several checks of the results. We have tried extended definitions of the outcome variable to include exit to regular education or attrition or both. Moreover, we have excluded non UI-

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<sup>17</sup> The explorative data analysis suggested that UI-benefit might affect the relative comparison of the programs. Additional analysis, allowing for interaction between program and UI-status, indicated, however, only minor changes in the pattern.

recipients, high- and low-income earners, and foreign citizens. These tests have not changed the conclusions.

## 5. Is the process of program assignment equivalent to randomization?

The identification of the causal effects hinges critically on the presumption that we observe and control for all the factors that jointly determine the program assignment and unemployment duration. In this section we will analyze the assignment process with available data.

There are at least two agents involved in the choice of program - the unemployed worker and his caseworker. If these agents have access to and act on information that is unobserved by us, our analysis is subject to selection bias. Firstly, we discuss which of the agents that can be presumed to be the driving force in the assignment of the program. Secondly, we discuss what factors that may influence the assignment.

A recent study by Harkman (2000) indicates that unemployed workers value different programs equally. Harkman uses survey data in which openly unemployed, i.e. eligible, workers were interviewed about their interest in participating in different programs. A correlation analysis shows that if a worker is interested in participating in one program, he is equally likely to be interested in participation in another program as well.<sup>18</sup> The study by Harkman also shows that the motive for participating in a program is not primarily the potential increase in employability. Instead, the workers indicate social reasons to be the most important ones. Thus, it appears that workers' self-selection into different programs is a minor problem in this study. Harkman's study also shows that the decision — to participate in any of the programs or to not participate — appears to be based on factors unobserved by us. More specifically, a worker's subjective likelihood of employment turns out to be quite important in explaining the overall interest for program participation.<sup>19</sup>

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<sup>18</sup> The workers could choose between 5 alternative answers to the question ranging from very interested to not at all interested. Spearman's coefficient of rank correlation ranged from 0.4 to 0.6.

<sup>19</sup> We also control for elapsed duration of unemployment which potentially may cause selection bias in the results. Referring to Harkman's results above, we think of the participation-decision as a two stage decision problem. In the first one, the worker decides whether or not he wants to

The caseworkers, on the other hand, seem to have a clear view of what type of program is suitable to their clients (Harkman, 2000). In the survey, caseworkers were asked to judge how suitable six programs were to a worker they earlier had given consultation to. A correlation analysis of the caseworkers' judgements gave zero correlation between programs.<sup>20</sup> Thus, we are led to believe that the caseworkers are the driving force in selecting the program. This belief is further enforced by the caseworkers' subjective claim that the decision could be attributed to the individual in less than a quarter of the cases, rather they attribute the decision to themselves in the majority of cases.

What decision rules do caseworkers then use? We say that the caseworkers use the same decision rule if they, in similar circumstances, assign a given sample of workers identically to different programs. Briefly, caseworkers' decision rules are probably influenced by many factors, like his preferences, incentives, experiences, the colleagues' opinion on the relative efficiency, as well as instructions from regional and central decision-makers. Crucial to our study is the question of whether caseworkers coordinate their decision rules. If all caseworker act idiosyncratically, and if they are the driving force in the choice of program, we identify the causal effects (see further discussion in footnote 14).

Do caseworkers coordinate their decision rules? We show below that a worker's office affiliation is far more important than his observed characteristics in determining which program he ends up in, even though we observe many potentially important characteristics of the worker. This finding is consistent with the hypothesis that caseworkers do coordinate their decision rules based on worker characteristics within an office but not across offices. It is possible, though, that the decision rules within offices are partly based on unobserved characteristics that are correlated with unemployment duration. How-

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participate in a program. This decision is based on characteristics that are unobserved by us, see above and consequently elapsed unemployment duration is informative on unobserved heterogeneity. However, we do not analyse this decision. Instead our focus is on the second step, in which the choice of program is made (given a decision to participate in any program). We believe and argue above that this decision is based on unobserved heterogeneity to a less extent than the first step. Note further, that we allow for interaction effects between the programs and elapsed unemployment duration.

<sup>20</sup> The question is asked to caseworker after the workers that have escaped open unemployment to, for example, employment or program participation. Unfortunately, there was no way to examine the consistency of preferences across caseworkers. Further, the survey does not investigate whether the caseworkers believe participation is preferable to non-participation.

ever, the analysis in *Section 4* gave no significant differences between offices in the relative efficiency of programs. Such differences would be expected if offices use different decision rules based on unobserved characteristics which in turn are correlated with the time out of regular employment.

Why would caseworkers coordinate their decision rules within an office and not across offices? First, presumably caseworkers at the same office have greater opportunities to share experiences and jointly discuss what decision rules to use. Second, the instructions from the National labor market board are rather general and loosely formulated. Much discretion is therefore left to the Employment office and to the caseworker. Further, the offices are relatively autonomous; see discussion by Nyberg and Skedinger (1997). Third, there is no systematic and coherent information available on the relative efficiency of programs.

In the empirical analysis below we investigate the importance of (observed) worker characteristics, Employment office affiliation as well as local labor market conditions for the choice of program in a multinomial logit model. This method estimates the relative probability that a worker will participate in a program instead of a (comparison-) program. In the next step we calculate whether it is the worker's characteristics or his Employment office affiliation that contribute the most to the variance in the predicted relative probabilities, see *appendix B* for a more detailed description of the applied method.

We observe several characteristics of the worker that are potentially important for the choice of program. These characteristics are obtained at consultation talks between the caseworker and the worker. We also include the worker's wage (from the administrative AKSTAT register) although this information is not necessarily revealed to the caseworker.<sup>21</sup>

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<sup>21</sup> We have also made a complementary check for selection on unobserved characteristics. The idea is to test whether the past labour market situation can be explained by the present program state. If this is the case, then selection on unobservables seems important as it would imply a difference between programs in the participant's past labour market situation (and thus, most likely, a difference in the future situation even in the absence of any program effect). We can think of three potentially valid measures of past labour market situation; past employment history, past unemployment history, and previous wage. We have no access to the first one nor can the second one be used as we only include workers who are unemployed for the first time. Hence, we will use previous wage as a measure of the worker's past labour market situation. The wage is regressed on standard human capital variables like education, work experience, age et cetera, as well as present program state. We find the program state to be statistically insignificant and the point estimates to indicate a difference of at the most two percent. There is one exception however; The participants in the Self-employment grants (SEMP) have considerably higher previous

In our data 442 Employment offices are represented. Usually there is one Employment office in each municipality. In bigger cities there are several offices, specialized in certain sectors, like mediating work to health care or manufacturing industry. In the data, 60 percent (265 Offices) have had participants in at least seven of the eight programs. It would be unfeasible to estimate 442 (or 265 for that sake) separate office effects. Instead, we use a random sample of 20 % from the 265 Offices, in order to reduce the number of parameters to be estimated.<sup>22</sup> We also control for annual averages of the unemployment ratio as well as the ratio of program participants and total unemployment in the municipality.

We estimate a multinomial logit model (MLM), which provides estimates of the set of probabilities that a worker will end up in the eight programs. We switch the comparison program seven times in order to relate all programs to each other. The results are presented in *Table 5*. The comparison program is indicated in the first row of the *Table 5*. The entries in the *Table* report the relative contribution to the variance of the predicted probabilities from Employment office affiliation and worker characteristics, see *Appendix B* for more details. The ratio is greater than one if workers' office affiliation contribute more than the workers' characteristics to the variance in predicted probabilities. The results in *Table 5* show that office affiliation is far more important than the wide array of observed characteristics of the worker in determining which program he will end up in (the ratio is greater than one). Between CAC/API, SEMP/API, SEMP/TRS, and SEMP/CAC worker characteristics are more important than office affiliation (the ration is less than one). The analysis in *Section 4* gave that self-employment grants (SEMP) is the second best program in reducing unemployment times. Computer/activity center (CAC) on the other hand, came out as one of the worst programs. Hence it is possible that workers with relatively favorable observed labor market characteristics end up in SEMP, while workers with less favorable characteristics end up in CAC. All other comparisons between the programs indicates that office affiliation is more important than worker characteristics in determining which program the worker will enter.

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wage (about 10 %) than participants in the other programs. Hence, the estimate for this program is likely to be subject to selection bias. The magnitude of this bias is unknown however.

<sup>22</sup> In order to make the estimations converge we randomly re-coded some observations at offices with participants in 7 programs only. The conclusions are not sensitive to this re-coding.

**Table 5.** The relative contribution of variance from Offices and observed characteristics of the individuals. The columns indicate which program that is used as base. The first row indicates which program that is used as the comparison program in the estimations.

Comparison program =>	ALU	API	RW	SUBE	TRS	AMU	CAC	SEMP
ALU	-							
API	1.51	-						
RW	1.86	2.41	-					
SUBE	1.12	2.34	1.55	-				
TRS	2.37	1.24	2.31	2.07	-			
AMU	1.75	1.95	1.78	1.78	2.41	-		
CAC	2.87	0.91	3.13	2.08	1.70	2.67	-	
SEMP	1.55	0.50	1.34	1.49	0.98	1.31	0.84	-

Note: Number of observations are 4 076.

This analysis might have some limitations however. One would be if the unemployed workers are selected to programs based on unobserved heterogeneity in which case the MLM-analysis might be biased. The direction of the bias depends on the correlation between the observed and unobserved characteristics. If the correlation is negative, the effect of observed characteristics will be underestimated and hence worker characteristics will appear to be less important than they truly are. We cannot exclude this possibility.

Other factors may affect the analysis as well. For example, it is possible that the distributions of observed and unobserved worker characteristics vary across offices, or that the structure of industry varies between offices. Such differences may undesirably turn up as Employment offices' effects. Hence, in a sensitivity analysis we restrict the empirical analysis to three northern counties and three counties in the mid-Sweden respectively. In these analyses we excluded bigger cities. The results are similar to the one presented in *Table 5*.

## 6. Discussion

In this paper we estimate the relative success of eight Swedish programs in reducing the participants' unemployment times. The results show that programs

in which the participants obtain (subsidized) work experience and training provided by firms, have better outcomes than programs providing classroom vocational training. Further, the more regular work the participants are allowed to do, the better the program is relative to other ones. The relative efficiency is not affected by the timing of placement in programs. These results are the same across demographic and skill groups.

Why would classroom vocational training have a less favorable outcome than the programs providing firm-related work experience and training? One explanation is naturally that our results are biased due to selection of workers into programs based on unobserved characteristics. We perform a careful examination of the selection process but some uncertainty concerning this issue remains, as it always will in empirical analysis. However, we like to point at two facts that suggest this to be an inconsequential problem. Firstly, the magnitude of the estimated effects is quite large<sup>23</sup>. Secondly, the ranking of the programs, according to the obtained results, is coherent: pointing unambiguously at programs where the participants obtain work experience and training as the more successful ones. Nonetheless, we cannot exclude that our ranking of the programs are affected by spurious correlation. In particular, we found some indications that participants in *Self employment grants* (SEMP) may have better employment prospect due to unobserved characteristics than participants in other programs. We found that observed characteristics are more important than Employment office affiliation in explaining the relative probability to enter SEMP. This result may indicate the selection of participants to SEMP is also based on unobserved characteristics related to the outcome. We also found that the average pre-unemployment wage level is higher in SEMP than in programs.

A second explanation is that work experience and training at an employer actually is better than classroom vocational training. A possibility to show ones competence or an opportunity of on-the-job training might be more important in improving the prospects of receiving a job offer than “formal” vocational training. Presumably, valuable information is revealed to both the employer and the worker when the latter spends time in the program at the working site. Further, the more regular work the worker is allowed to do while in program, the more information on the match is revealed. A recent evaluation study on vocational training supports the conclusion that employer contacts are important for receiving a job offer. Johansson and Martinsson (2000) find that voca-

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<sup>23</sup> See Rosenbaum (1999) for a discussion why this provides strong evidence of an actual effect.

tional training (computer training) located at, and organized by, private firms have a much better outcome in terms of employment probabilities than traditional classroom vocational training.

Classroom vocational training is the most expensive program in Sweden. The high cost in combination with the poor relative performance of vocational training brings up the question whether vocational training is a social beneficial activity. An answer, however, demands that vocational training is evaluated in several other aspects as well. For example, we need to know whether vocational training shortens unemployment spells compared with non-participation and whether participation increases subsequent employment and earnings. Previous Swedish studies on the effects of vocational training compared with non-participation give mixed results, see Regnér (1997) for an overview. The study by Regnér, which is the most recent one, shows that vocational training has a negative effect on (annual) income (compared with non-participation) one year after participation. The negative effects vanish after three years. Korpi (1994) uses a sample of young unemployed Swedish worker and find that participation in classroom vocational training and relief work increase subsequent employment duration. Unfortunately, Korpi does not distinguish between the two types of programs.

Further, programs might have effects on non-participants as well, see Calmfors (1994). For example, programs must to be financed and may hence increase the tax burden. Programs may also affect the wage level and the wage structure in the economy. Further, the improved employment prospects of participants may come at the cost of reduced prospects of non-participants directly. These effects need not to be same across programs. There is only fragmentary evidence on the effects on non-participants of various programs. Dahlberg and Forslund (2000) use Swedish data over the period 1987-1996 and find that are direct displacement effect from programs that generate subsidized labor, but there seems to be no displacement effects from vocational training programs.

In order to understand the poor relative performance of vocational training more research is needed. Possibly vocational training is of poor quality or is not targeted on occupation in excess demand. On this issue there is, however, no evidence available.

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# Appendix A

**Table A1.** Mean values of workers' characteristics.

	SEMP	SUBE	TRS	API	ALU	RW	AMU	CAC
Age (years)	39.95	37.56	36.93	36.33	41.36	39.26	39.00	39.79
Women (%)	33.79	28.03	77.55	51.77	44.39	39.26	48.09	62.15
Immigrants from Eastern Europe (%)	1.67	9.15	0.92	20.71	1.52	13.22	8.72	4.70
Immigrants from Africa and Asia (%)	2.03	3.28	.092	11.29	1.60	9.05	3.20	3.33
<i>Education</i>								
Compulsory school (%)	27.69	34.09	26.82	36.69	41.55	40.06	30.41	36.29
Upper sec: (0-2 years) (%)	21.69	24.65	27.76	18.45	23.95	23.16	23.51	28.23
Upper sec. (>2 years) (%)	23.20	23.06	11.16	24.17	16.38	19.78	23.53	19.81
University (0-2 years) (%)	6.92	4.77	7.57	4.42	5.35	3.88	6.37	4.52
University (>2 years) (%)	20.50	13.42	26.69	16.27	12.76	13.12	16.17	11.14
<i>Work experience</i>								
Short work experience (%)	9.06	12.92	19.79	16.38	11.46	16.60	13.93	15.28
Long work experience (%)	82.32	73.16	66.53	54.48	77.80	61.13	70.61	66.89
Relevant education/training for the work (%)	73.22	64.51	67.87	64.90	58.90	69.18	65.06	58.33
<i>Benefits and wages</i>								
Receiver of cash assistance (%)	6.99	15.01	7.57	4.94	3.81	8.95	6.16	2.48
Receiver of UI-benefits (%)	67.42	54.08	64.41	44.94	88.13	53.68	63.89	67.32
Previous wage (SEK/day)	771	697	659	696	720	685	713	698
Assigned code 12	6.93	11.05	4.14	26.64	4.50	14.78	14.42	11.83
Number of participants	3 048	1 006	753	5 383	5 006	1 006	6 251	2 827

**Table A2.** Results. Estimates pertaining to the control variables. Total number of observations are 17 303 individuals. Offices are included if they contributed 100 or more participants. For each program the T-value is shown as the deviation from the overall estimate.

	Estimates overall	SEMP	SUBE	TRS	API	ALU	RW	AMU	CAC
work relevant training	0.052	0.29	0.35	0.03	0.45	0.57	0.38	0.07	0.94
Some work experience	0.055	0.05	0.75	1.08	1.96	0.96	0.93	0.61	1.33
long work experience	0.108	0.60	0.08	0.63	0.67	0.94	0.17	0.74	1.64
KAS	0.122	1.45	1.91	0.69	1.09	1.07	0.56	0.54	1.18
<b>UI-benefits</b>	<b>-0.015</b>	<b>2.18</b>	<b>0.46</b>	<b>1.07</b>	<b>2.69</b>	<b>2.02</b>	<b>0.91</b>	<b>1.05</b>	<b>3.20</b>
<b>Unempl. dur. prior to program (days)</b>	<b>-0.082</b>	<b>1.89</b>	<b>1.83</b>	<b>1.02</b>	<b>0.66</b>	<b>3.51</b>	<b>0.30</b>	<b>0.54</b>	<b>2.43</b>
Immigrants from East. Europe	-0.415	1.81	0.65	0.39	1.13	1.16	0.07	0.28	0.52
Immigrants from Africa and Asia	-0.401	1.01	0.42	0.59	0.21	0.02	2.11	0.20	1.74
30-34 years	-0.117	0.57	2.14	0.31	1.37	0.07	1.50	0.48	0.55
35-39 years	-0.166	1.47	1.03	0.82	2.11	0.22	0.29	0.23	0.84
40-44 years	-0.194	1.51	0.91	0.71	0.96	1.19	0.03	0.17	0.45
45-49 years	-0.286	1.84	1.34	0.66	0.02	1.47	0.17	0.01	0.67
50-54 years	-0.400	2.70	2.91	0.64	0.04	1.47	0.69	1.04	1.14
Upper sec:(0-2 years)	0.083	1.61	0.40	2.73	0.02	0.90	2.49	0.25	0.24
Upper sec:(>2 years)	0.087	1.50	0.17	1.44	1.06	0.42	2.49	0.16	0.53
University (0-2 years)	0.083	0.61	0.08	1.42	0.14	0.47	1.17	0.14	0.83
University (>2 years)	0.159	1.94	0.13	2.02	0.24	0.92	1.20	0.23	0.32
Women	-0.027	1.18	0.65	0.48	0.01	0.06	0.54	0.51	2.07
Local unemployment rate (log)	-0.149	0.34	0.85	0.96	0.64	0.71	2.13	0.55	0.77
Local program-to- unemployment rate	-0.121	0.04	1.78	0.19	0.46	0.68	0.13	0.24	1.03

*The Table continues on the next page.*

**Table A2** (Cont.). Results. Estimates pertaining to the control variables. Total number of observations are 17 303 individuals. Offices are included if they contributed 100 or more participants. For each program the T-value is shown as the deviation from the overall estimate.

Assigned code 12	-0.241	1.31	0.57	2.07	1.46	0.50	0.99	0.33	0.37
Wage prior to unemployment	-0.000	0.12	0.09	0.04	1.14	1.26	2.07	0.03	1.98
# of observations	17303	1689	552	414	3211	2695	496	6800	1446

Notes: Variable in bold whenever there is strong indications of heterogeneous effects. Not shown in *Table* is the Office factor (103 levels), the county factor (20 levels), and the code for the sector where work is sought (12 levels). Moreover, calendar time effects are accounted for by use of the date of entry to the program.

## Appendix B

This appendix gives a brief description of the multinomial logit model (MLM), see Greene (1993) for a more detailed description. MLM provides estimates of the set of probabilities that the worker enters the programs  $j=0, \dots, 7$ . The probability that worker  $i$  enters program  $j$  is given by:

$$\Pr(D = j) = F(X_i, Y_i, Z_i; \beta, \alpha) = \frac{e^{X_i\beta_j + Y_i\alpha_j + Z_i\eta_j}}{1 + \sum_{k=1}^7 e^{X_i\beta_k + Y_i\alpha_k + Z_i\eta_k}} \quad i=1, \dots, n, j=1, \dots, 7.$$

where  $X_i$  is the worker's observed characteristics. These characteristics are obtained at interviews between the caseworker and the worker. The  $X$ -vector includes all variables presented in *table A1* except indicators on membership in unemployment benefit fund, eligibility for KAS. The variables are described in more detail in *section 3*. The factor  $Y_i$  indicates the Employment office affiliation of the worker with a level for each office. The vector  $Z_i$  includes various indicators on local labor market conditions, as well as elapsed unemployment duration, indicators on membership in unemployment benefit fund, eligibility for KAS, and the worker's most preferred occupation (one digit-level), see *section 2*.  $F$  denotes the multivariate logistic cumulative distribution function. Finally,  $\beta_j$ ,  $\alpha_j$ , and  $\eta_j$  are the parameters to be estimated. The relative probability that worker  $i$  will enter program  $j$  instead of program 0 (the comparison program) is given by:

$$\frac{\Pr(D = j)}{\Pr(D = 0)} = \exp(\beta_j X_i + \alpha_j Y_i + \eta_j Z_i) . \quad (B1)$$

We decompose the variance of the log-transformed predicted relative probabilities into three categories: *i*) worker characteristics,  $X$  *ii*) Employment office affiliation,  $Y$  *iii*) other variables  $Z$ .

$$\text{var} \left( \ln \left( \frac{\Pr(D = j)}{\Pr(D = 0)} \right) \right) = \text{var}(\beta_j X) + \text{Var}(\alpha_j Y) + \text{var}(\eta_j Z) + R ,$$

where remainder  $R$  consists of possible covariance between  $X$ ,  $Y$ , and  $Z$ . We run eight estimations on (B1), switching the comparison program each time. By do-

ing so, we relate all eight programs to each other. One single estimation gives the relationship between the comparison program and one of the 7 other programs, but not the relation between the 7 programs. The measure presented in *Table 4* gives the relative contribution of office affiliation and observed worker characteristics respectively on the variance of predicted probabilities:

$$M = \frac{Var(\alpha_j Y)}{Var(\beta_j X)} .$$

If  $M > 1$  we conclude that worker's Office affiliation is more important than the worker's characteristics to explain why he enters program  $j$  instead of the comparison program  $0$ .