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# A MICROECONOMETRIC EVALUATION OF THE ACTIVE LABOUR MARKET POLICY IN SWITZERLAND*

*Michael Gerfin and Michael Lechner*

In the late 1990s, Switzerland introduced an ambitious active labour market policy (ALMP) encompassing several programmes. We evaluate their effects on the individual employment probability using unusually informative data from administrative records. Using a matching estimator for multiple programmes, we find positive effects for one particular programme unique to the Swiss ALMP. It consists of a wage subsidy for temporary jobs in the regular labour market that would otherwise not be taken up by the unemployed. We also find negative effects for traditional employment programmes operated in sheltered labour markets. For training courses, the results are mixed.

In the 1990s, substantial active labour market policies (ALMP) were enacted in many continental European countries. Many policy makers as well as economists considered ALMP as important tools to reduce Europe's notoriously high levels of unemployment, without having to go through the painful side-effects of substantial reforms of the labour markets. Recent evaluation studies surveyed for example by Fay (1996) and Heckman *et al.* (1999), however, do not appear to develop any consensus whether these hopes are justified. Quite to the contrary, many studies raise serious doubts. In addition, the available data used were typically far from being ideal, adding additional uncertainty about the 'true' effect of Europe's diverse ALMP.

After Switzerland experienced a continuous increase in unemployment in the beginning of the 1990s, a revision of the law regulating unemployment insurance and ALMP was enacted in 1997. It expanded ALMP considerably. Furthermore, it made benefit payments conditional on participation in a labour market programme after being unemployed for more than seven months. This activation principle, in conjunction with compulsory programme participation after some defined duration of unemployment, had previously been introduced in Denmark and since then only in the New Deal in the UK (OECD, 2001).

The Swiss ALMP consists of many different training and employment programmes similar to the kind of programmes found in other European countries.

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In addition, there is a unique large programme that we call *Temporary Wage Subsidy*. It encourages the unemployed to accept job offers that pay less than their unemployment benefit by (over-) compensating the difference with a subsidy. These jobs are temporary, and participants are still registered as job seekers. In this sense, this wage subsidy programme differs from the employment subsidy programme in the British New Deal and most other countries.

We perform a microeconometric evaluation of the different programmes of the Swiss ALMP. We focus on the differences of individual success in the labour market that are due to these programmes. The Swiss government made available a novel, informative and large database consisting of administrative records from the unemployment insurance system, as well as from the social security system for the population of unemployed persons in December 1997.[1]

For any evaluation study, there is the question of identification strategies and estimation methods suitable for the specific situation. Angrist and Krueger (1999), Heckman and Robb (1986) and Heckman *et al.* (1999) provide excellent overviews of available strategies. Because we argue that we observe the major variables influencing selection as well as outcomes, we assume that labour market outcomes and selection are independent conditional on these observables (conditional independence assumption, CIA). Therefore, for the present situation characterised by rich data, an estimator that exploits CIA and avoids other assumptions would be called for. In addition, that estimator should avoid restricting the effects to be the same in specific sub-population because there is substantial *a priori* evidence that those programmes could have different effects for different individuals. Finally, this estimator has to take account of the different programmes of the Swiss ALMP.

An estimator that fulfils these requirements is *matching*. The idea is to construct an artificial comparison group and compare their labour market outcomes to those of the programme participants. Under CIA, this estimator is consistent when the comparison group has the same distribution of observables determining outcomes and participation as the group in the programme. Angrist (1998), Dehejia and Wahba (1999), Heckman, Ichimura and Todd (1998), Heckman, Ichimura, Smith and Todd (1998) and Lechner (1999, 2000), among others, recently apply and discuss matching. Imbens (2000) and Lechner (2001*a*) extend the binary matching approach (in the programme vs not in the programme) to allow for multiple programmes.

Our results indicate considerable heterogeneity with respect to both the effects of the different programmes and the effects for different subpopulations within a programme. *Employment programmes* perform poorly, *Vocational training programmes* show a rather mixed performance depending on the specific subprogramme considered, whereas *Temporary wage subsidy* is a successful programme in terms of increasing the chances on the labour market. With respect to the heterogeneity by sub-population, it appears that participating in a programme in the early stages of the unemployment spell is less effective than participating in later stages.

[1] This study is part of a series of evaluation studies commissioned by the Swiss State Secretariat of Economic Affairs (seco), that use diverse empirical methods; for an overview in German, see http://www.seco-admin.ch/wirtpol/amp/d_ForschungALV.html.

The plan of the paper is as follows: Section 1 gives the stylised facts of the Swiss labour market and explains the institutional arrangements of the unemployment insurance system. Furthermore, it gives the details of the active labour market policies. Section 2 discusses data issues, presents descriptive statistics and empirically characterises the selection processes into the programmes. Section 3 discusses our identification and estimation strategy. Section 4 contains the results and Section 5 concludes. Two Appendices contain more information on the data, as well as the results of the estimation of a multinomial probit model used to explain participation in the programmes. Finally, an Appendix that can be downloaded from the internet[2] contains additional information.

## 1. The Swiss Labour Market

### 1.1. *The Economic Situation*

Switzerland is a federalist state with three major language regions. The German speaking region is by far the largest and economically most prosperous part. Generally, the Swiss labour market operates on broadly similar terms as, for example, the German labour market. It is even more geared towards consensus between union, employers and government. The female labour market participation rate is relatively high at about 70% (with about 55% of them working part-time). The share of foreigners in the work force is about 20%.[3]

Switzerland has a unique unemployment experience. Before 1990, unemployment never was a major problem. In the 1970s, the unemployment rate never exceeded 1% and, in the 1980s, the highest rate was 1.1%. After a long period of economic growth, the first seven years of the 1990s proved to be a period of stagnation and the unemployment rate increased markedly and reached a maximum of 5.2% in 1997. Since then, unemployment fell again to 2.8% in 1999.[4] The main reason why recessions before 1990 did not translate into a large increase in unemployment, as in most other Western European countries, seems to be the cyclical responsiveness of the foreign labour force. About 75% of the employment reduction in the 1975/6 recession was absorbed by a reduction in the non-permanent foreign labour force.[5] The situation was different in the 1990s, with more than 60% of the foreign labour force having a permanent work permit, implying a much reduced responsiveness. In addition, female labour supply also became much less elastic with respect to labour demand.

Foreigners are over-represented among the unemployed with a share of roughly 40%. This is also the case for women whose unemployment rate is about 1%-point higher than the overall unemployment rate. There is also a disparity in unemployment rates between the German speaking cantons and the non-German speaking cantons (3.4% vs 7.1% in 1995).

[2] www.siaw.unisg.ch/lechner/gl_ej.

[3] The source for most of the numbers presented in this section is OECD (1996).

[4] The OECD standardised unemployment rates are somewhat lower then those based on the Swiss official statistics.

[5] Non-permanent work permits are only renewed in case of successful employment.

1.2. *Unemployment Insurance and Active Labour Market Policies*

Switzerland had no compulsory unemployment insurance until the late 1970s. The national unemployment insurance law (AVIG) was enacted only as late as 1984. Active labour market policies were provided for in the AVIG but they were of no importance before its last major revision in 1996. The main feature of that revision is a change from so-called passive unemployment benefits towards an active system in which benefit payments are conditional on participation in labour market programmes.[6] Benefit entitlement was prolonged to two years. The entitlement period is separated into two parts: the first 30 weeks are unconditional on programme participation whereas the remaining entitlement is conditional on some participation. However, in practice, these rules are not strictly enforced; it is not unusual to participate in programmes in the first 30 weeks of the unemployment spell. In this case, the unconditional benefit payments can be received later. Frequently unemployed receive the same benefits in the conditional period as before without any participation in ALMP, because no programme has been offered.

Entitlement is conditional on having contributed to the unemployment insurance for at least 6 months in the previous two years. After the entitlement period has expired, a new entitlement period is conditional on being employed for at least another 12 months within three years after the previous unemployment spell. The replacement ratio is, in general, 80% of the insured earnings.[7] The maximum of the monthly benefit is limited to about CHF 7,000.

The cantons are obliged by law to supply a minimum of places in labour market programmes per year. Until January 2000, this minimum was 25,000 distributed across cantons according to their unemployment. This minimum was exceeded in 1998 by roughly 6,000. By comparison, there were about 190,000 registered jobseekers in 1997 and 140,000 in 1998.

The active labour market programmes (ALMP) can be grouped into three broad categories:

(a) Training courses
(b) Employment programmes
(c) Temporary employment with wage subsidy (*Temporary wage subsidy*)

The difference between (b) and (c) is that employment programmes take place outside the 'regular' labour market (see below). By contrast, *Temporary wage subsidy* must be a regular, but temporary, job.

Training programmes consist of a wide variety of courses, ranging from basic courses to specific work-related training. The decision to participate in a training course is made by the placement officer according to his impressions obtained mainly from the monthly interviews. The unemployed can also apply for training courses. The law requires that courses must be necessary and adequate with the goal to improve individual employment chances. Criteria for the decision include age and motivation of the unemployed, and the duration of the course and its

[6] The amount of benefits is the same for the active as well as passive ones.
[7] A reduced replacement ratio of 70% applies to able-bodied persons without parental obligations or with previous monthly earnings above about CHF 3,500.

relevance for the occupation. Occupational retraining is specifically not considered to be a task of the ALMP. The duration of training courses varies between one day and several months.

Employment programmes usually last for six months. They should be as similar as possible to regular employment, but they should also be *extraordinary*, i.e. employment programmes should not be in competition with other firms. Employment programmes are organised by both public and private institutions.[8] During an employment programme, the unemployed have to continue their job search and must accept any suitable job offer. While in an employment programme, an unemployed person receives a wage which can be larger than the unemployment benefit. However, in practice, this would be an exception. Participation in training courses and employment programmes does not extend the benefit entitlement period. By contrast, *Temporary wage subsidy* (*TEMP*) can extend the entitlement period if its cumulated duration exceeds 12 months.

The objective of *TEMP* is to encourage job seekers to accept job offers that pay less than their unemployment benefit by compensating the difference. The income generated by this scheme is larger than unemployment benefits.[9] Thus, this programme is financially attractive for both the unemployed and the placement office. Persons in *TEMP* are registered as job seekers and must obey the same rules as unemployed receiving benefits.

*TEMP* does not belong to the ALMP officially but there is compelling evidence that the placement offices use them as an active labour market policy instrument. This is documented in Bauer *et al.* (1999) and in interviews we conducted at selected placement offices. Not surprisingly, it is the largest programme of the ALMP. In 1998, roughly 20% of the unemployed were at some point in *TEMP*. Thus, we treat it as part of the ALMP. Bauer *et al.* (1999) report that only about 20% of the jobs in *TEMP* are arranged by the placement office. The OECD (1996) states that *TEMP* can be a powerful instrument to bring the unemployed back to employment. However, there is concern that it may lead to serious distortions in the labour market if not tightly monitored. For example, workers might be laid-off and recalled in the *TEMP* scheme. Furthermore, firms might use *TEMP* to avoid dismissal protection rules to have a more flexible work force; or *TEMP* might be used to avoid the wage levels set out in collective wage bargaining agreements. However, there is (yet?) no evidence of these negative effects in practice.

In Switzerland, the cantons and even the placement offices within a canton are fairly free regarding their policy of allocating unemployed to programmes. They just have to conform to the rather vague guidelines set out in the federal law. This introduces regional heterogeneity into the allocation process that is evident from

[8] There is no substantive difference between the type of 'jobs' offered by the two different groups of providers of these programmes.

[9] The compensation payment is the replacement ratio applied to the difference between the earnings in the temporary job and the previous earnings, which will always be larger than the difference between the unemployment benefit and the earnings in the temporary job. At the same time, the unemployment insurance system 'saves money' by always paying less than the regular unemployment benefit.

both the interviews we conducted and the estimation results of the factors explaining the selection process presented below.

### 1.3. *The Programmes of the Swiss ALMP as Defined in this Study*

There are 16 different types of training courses that we aggregate into five fairly homogenous groups:

(a) Basic courses (aiming at improving the effectiveness of job search and self-esteem)
(b) Language courses
(c) Computer courses
(d) Further vocational training
(e) Other courses (including courses for specific occupations).[10]

The employment programmes are differentiated according to whether they are offered by public or private institutions. *Temporary wage subsidy* is a programme of its own, and the final group consists of those who never participated in any programme.[11] Thus, in total, we have nine groups of ALMP participation status to which we allocate the individuals in our data.

With respect to programme heterogeneity, there is a fundamental problem we have to address: how do we deal with multiple programme participation of the same unemployed? In principle, it is possible to observe individuals with programme 'careers' where participation in a later programme is, of course, not independent of prior participation. This creates an endogeneity problem, because more successful programmes will increase the likelihood of employment for their participants, while less successful programmes may just lead to yet another programme participation. For this reason, we evaluate only the *first* major programme. This approach implies that further programme participation is an indicator of failure of the first programme because it does not bring the unemployed back into employment. In practice, this approach is less restrictive than it appears. Only about 30% of all participants enter a second programme, and the majority of these successive programmes are of the same type as the first programme.

Another problem concerns the group of non-participants. For this group, important time varying variables like 'unemployment duration prior to the programme' are not defined. To make meaningful comparisons to those unemployed entering a programme, we use an approach suggested in Lechner (1999) which consists of drawing a hypothetical programme starting date from the sample distribution of starting dates for each non-participant. Persons with simulated starting date later than their actual exit date from unemployment are excluded from the data set.[12]

To summarise, we evaluate the first major programme starting after 1 January 1998. A major programme is defined as having a duration of at least two weeks.

[10] See Table A1 in Appendix A1 for details on the aggregation of the groups.
[11] 'Never' means in this case the period between 1 January 1998, and 31 January 1999, because we have information about participation only for this period.
[12] Lechner (2001*d*) contains a sensitivity analysis of this procedure by using starting dates predicted by relevant information available in December 1997. The results appear to be robust.

The reason not to consider programmes starting before 1998 is that the data do not contain sufficient information on the type and the duration of programmes prior to 1998.[13]

Table 1 shows the descriptive statistics of several important variables across the different groups defined by programme participation. The largest group is *Non-participation* (about one third) which is interesting given that programme participation is *in principle* compulsory after 30 weeks. The largest programme group is *Temporary wage subsidy*. All other programmes are of similar size except *Further vocational training* and *Other courses*. The final column shows that the employment rate at the final day in our data varies between 48% and 25%. Of course, this is not indicative for programme success because participants differ substantially with respect to variables influencing future employment. Hence, we expect differences in the employment rates for these different groups of unemployed, even without any programme participation. The table shows that important variables like qualification, nationality and duration of unemployment also vary substantially. The proportion of those starting a programme in the first 150 days of the unemployment spell (recall that programme participation is, in principle, compulsory after 150 days) is interesting, since it is low for employment programmes with 18% and about twice as large for most other programmes (except *Other training courses*). This indicates that especially training courses and TEMP often start earlier than required by law.

Important information is missing in Table 1 because, at the moment, there are no detailed information available on programme costs. Therefore, a cost, benefit analysis is not possible.

Data that are informative about the outcome and selection processes are important in evaluation studies, because they allow us to disentangle differences between participants and non-participants due to selective participation from differences *caused* by the specific programme. Since we use data from the administrative unemployment register, in principle, we know (almost) as much about the unemployed as the placement officer. We also need to know everything about the type, timing and duration of the programmes. In addition, we must measure the success of the programmes, eg by information on successful employment. Again, by using the administrative data, we have this information. In addition, there is evidence in the literature – see the survey by Heckman *et al.* (1999) – that it is important to control for individual labour market histories so as to capture individual heterogeneity. We can do this because it was possible to merge data from the social security records with the unemployment registrar data. Hence, we have retrospective data on labour market status and earnings covering at most ten years prior to the current unemployment spell. In conclusion, we have good data for a comprehensive evaluation of the Swiss ALMP. The data are described in more detail in Section 2.

[13] Comprehensive coverage of labour market programmes in the official statistics was only introduced in 1998.

Table 1

*Number of Observations and Selected Characteristics of Different Groups*

| Group | | Observations (persons) | Duration of programme (mean days) | Unemployment before ... Mean days | Share of duration < 150 days | Qualification (mean) | Foreign (share in %) | Employed March 1999 (share in %) |
|---|---|---|---|---|---|---|---|---|
| *Non-participation* | (*NONP*) | 6,918 | 0 | 240 | | 1.8 | 47 | 39 |
| *Basic courses* | (*BAC*) | 1,491 | 46 | 236 | 36 | 1.8 | 45 | 32 |
| *Language courses* | (*LAC*) | 1,719 | 71 | 225 | 36 | 2.2 | 72 | 29 |
| *Computer courses* | (*COC*) | 1,394 | 36 | 214 | 40 | 1.3 | 22 | 44 |
| *Further vocational training* | (*FVT*) | 424 | 74 | 231 | 35 | 1.6 | 38 | 42 |
| *Other training courses* | (*OTC*) | 497 | 94 | 263 | 23 | 1.8 | 43 | 42 |
| *Employment programmes (public)* | (*EP-PU*) | 1,124 | 153 | 302 | 18 | 1.7 | 41 | 28 |
| *Employment programmes private)* | (*EP-PR*) | 1,349 | 142 | 299 | 18 | 2.0 | 51 | 25 |
| *Temporary wage subsidy* | (*TEMP*) | 4,390 | 114 | 228 | 35 | 1.7 | 46 | 48 |

*Note*: Qualification is measured as skilled (1), semiskilled (2), and unskilled (3).

## 2. Data and Empirical Analysis of the Selection Process

### 2.1. *Data Base*

Our empirical analysis is based on administrative data. We obtained access to the information system for placement and labour market statistics (AVAM) and the unemployment offices payment systems (ASAL). From there, we obtained data from January 1996 to March 1999 for all persons who were registered as unemployed on 31 December 1997 (about 180,000).[14] These data provide detailed information about the unemployment history, ALMP participation and personal characteristics. In addition, we received data from the social security records for the period 1988–97, albeit only for a random subsample of about 25,000 observations. The merged sample contains information on the individual labour market histories and earnings on a monthly basis for ten years prior to the current unemployment spell. In addition, we have detailed information concerning several aspects: socio-demographics (age, gender, marital status, native language, nationality, type of work permit, language skills); region (town/village and labour office in charge); subjective valuations of placement officer (qualifications, chances to find job); sanctions imposed by the placement office; previous job and desired job (occupation, sector, position, earnings, full-/part-time); and a short history of labour market status on a daily basis.

The subjective valuations of the placement officers and the benefit sanctions can be particularly informative since they will capture characteristics like motivation and personal appearance that are usually unobservable. We are confident that, after controlling for this wealth of information, there is little unobserved heterogeneity left that is systematically correlated with labour market outcomes and programme participation.

We applied a series of sample selection rules to the data given in Appendix A in detail. The most important criteria are to consider only individuals unemployed for less than 12 months on 31 December 1997, who did not participate in any major programme in 1997, and who are aged between 25 and 55 years old. The reasons for these selection criteria are that, given the two-year entitlement period, we want to make sure that there is sufficient time left to participate in a programme after 31 December 1997. Furthermore, given our concentration on the first programme, we exclude those who participated in a major programme before. In addition, given the variety of options for young and older unemployed (schooling and early retirement), we exclude them from our analysis. The final sample has 19,307 observations.[15]

[14] Thus we use a stock sample that is representative for the population of unemployed in December 1997, but not necessarily for the inflows into unemployment in 1997.

[15] The full set of descriptive statistics is contained in Appendix www.siaw.unisg.ch/lechner/gl_ej.

2.2. *Empirical Analysis of the Participation Process into the Programmes*

This Section describes the estimation results of a multinomial probit model (MNP) for the selection of the individuals into the programmes.[16] Full results are presented in Tables B1 and B2 in Appendix B.

The results indicate that the main determinants of the selection process into the programmes are gender, region of residence, unemployment history, qualification, knowledge of language and previous occupation. The results correspond to what we would expect from the legal requirements and our interviews at selected placement offices. It seems that the unemployed are sent into programmes that are adequate with respect to their skill levels and the formal requirements for improving their employment chances. For example, foreigners whose mother tongue is not the language spoken in the canton they live in usually enter *Language courses* and are much less likely to go to *Computer courses, Further vocational training* or *TEMP*.

Low-skilled unemployed are likely to be selected into *Language courses* and both types of *Employment programmes*. On the other hand, unemployed with a higher skill level tend to go to *Computer courses, Further vocational training* and *TEMP*. Women are more likely to enter *Basic courses, Language courses* and *TEMP*. The probability for entering *Employment programmes* increases with age.

A main determinant of *Basic training* is residential location. This is also a result of the federalist system allowing placement offices some discretion with respect to the classification of actual programmes in the official groups (here mainly between *Basic* and *Language courses*).

The employment histories indicate the proportion of time unemployed in the previous ten years has a positive effect on the probability of entering an *Employment programme*. For all other programmes, this effect is negative (compared to non-participation). On the other hand, the proportion of time being employed in the last ten years has only a positive impact on the probability of entering *Temporary wage subsidy*. This finding suggests that the unemployed in *TEMP* seem to have a stronger attachment to the labour market in terms of past employment.

Finally, we find large positive correlations between the unobserved error terms in the MNP relating to the two employment programmes, whereas the corresponding correlations between training programmes are mainly negative. Not surprisingly, the negative correlation between *Language courses* and *Computer courses* is the largest one. However, the estimation of these correlations appears to be rather imprecise.

The correlations of the estimated probabilities (Table B3 in Appendix B) are negative with the two exceptions of *Computer courses* and *Further vocational training* and the two *Employment programmes* implying that participants in these two pairs of programmes are fairly similar.

[16] The MNP has been estimated by maximum simulated likelihood using the GHK simulator (100 replications for each individual observation in each equation). *Non-participation* is chosen as the reference group. We chose the MNP as opposed to the multinomial logit because it does not impose the restrictive independence of irrelevant alternatives assumption.

## 3. Econometric Estimation of the Effects of ALMP

### 3.1. *Notation and Definition of Causal Effects*

The prototypical model of the microeconometric evaluation literature is as follows: an individual chooses between two states, like participation in a training programme or non-participation in such a programme. The potential participant in a programme will receive a hypothetical outcome (eg earnings) in both states. This model is known as the (Roy, 1951; Rubin, 1974) model of potential outcomes and causal effects.[17]

Consider the outcomes of $(M+1)$ different mutually exclusive states denoted by $\{Y^0, Y^1, \ldots, Y^M\}$. The different *states* will to be called *treatments* to stick to the terminology of that literature. It is assumed that each individual receives exactly one of the treatments (typically, category '0' denotes treatment type *no treatment*). Therefore, for any individual, only one component of $\{Y^0, Y^1, \ldots, Y^M\}$ can be observed in the data. The remaining $M$ outcomes are counterfactuals. Participation in a particular treatment $m$ is indicated by the variable $S \in \{0, 1, \ldots M\}$.

The definitions of average treatment effects used for the case of just two treatments need to be extended.[18] In the following equations, the focus is on a pairwise comparison of the effects of treatments $m$ and $l$ :

$$\gamma_0^{m,l} = \mathrm{E}(Y^m - Y^l) = \mathrm{E}Y^m - \mathrm{E}Y^l \tag{1}$$

$$\theta_0^{m,l} = \mathrm{E}(Y^m - Y^l | S = m) = \mathrm{E}(Y^m | S = m) - \mathrm{E}(Y^l | S = m) \tag{2}$$

where $\gamma_0^{m,l}$ denotes the expected (average) effect of treatment $m$ relative to treatment $l$ for a participant drawn randomly from the population[19] and $\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of participants in treatment $m$ only. Note that both average treatment effects are symmetric in the sense that $\gamma_0^{m,l} = -\gamma_0^{l,m}$. Note also that, if the participants in treatments $m$ and $l$ differ in a way that is related to the distribution of $X$, and if the treatment effects vary with $X$, then $\theta_0^{m,l} \neq -\theta_0^{l,m}$, ie the treatment effects on the treated are not symmetric.

### 3.2. *Identification*

#### 3.2.1. *The conditional independence assumption*

The causal model clarifies that the average causal treatment effect is generally not identified. The lack of identification has to be overcome by plausible, untestable assumptions. Their plausibility depends on the problem analysed and the data available. Angrist and Krueger (1999), Heckman and Robb (1986) and Heckman *et al.* (1999) provide excellent overviews about available identification strategies. Here, as already noted in the previous Section, the data are so rich, that it seems

[17] See for example Holland (1986) for an extensive discussion of different concepts of causality.

[18] Assume for the rest of the paper that the typical assumptions of the Rubin model are fulfilled; see, for example Holland (1986) or Rubin (1974).

[19] If a variable $Z$ cannot be changed by the effect of the treatment then all what follows is also valid in strata of the data defined by different values of $Z$.

plausible to observe all important factors that jointly influence labour market outcomes and the process of selecting people into the nine states. Therefore, we assume that treatment participation and treatment outcome is independent, conditional on a set of observable attributes conditional independence assumption, CIA, Rubin (1977). Rosenbaum and Rubin (1983) show how this assumption could effectively be used for semi/nonparametric treatment evaluation, since it is not necessary to condition on a potentially high number of attributes, but only on the participation probability conditional on the attributes.[20]

Imbens (2000) and Lechner (2001*a*) consider identification under the CIA in the model with multiple treatments. The CIA defined to be valid in a subspace of the attribute space is formalised in (3):

$$Y^0, Y^1, \ldots, Y^M \coprod S|X = x \quad \forall x \in \chi. \tag{3}$$

This assumption requires the researcher to observe all characteristics that jointly influence the outcomes, as well as the selection into treatments. In that sense, the CIA may be called a 'data hungry' identification strategy.[21] In addition, it is required that all individuals in that subspace actually have the possibility of participating in all states (ie $0 < \mathrm{P}(S = m|X = x, \forall m = 0, \ldots, M, \forall x \in \chi)$. Lechner (2001*a*) shows that the CIA identifies all effects defined in this section. Lechner (2001*a*) also shows that depending on the effect of interest, this assumption can be weakened to involve fewer potential outcomes.[22]

We already argued in Section 2 that our database is exceptionally good, so we are confident that all factors that remain unobserved do not systematically influence the participation process in the programmes as well as the labour market outcomes.

3.2.2. *Reducing the dimension using balancing scores*

The basic ingredients of the final estimate are estimates of $\mathrm{E}(Y^m|X, S = m)$, since the CIA implies that $\mathrm{E}(Y^m|S = l) = \underset{X}{\mathrm{E}}[\mathrm{E}(Y^m|X, S = m)|S = l]$. However, nonparametric estimates cannot easily be obtained because of the high dimension of $X$ and the resulting curse of dimensionality in any nonparametric estimator.[23] In that respect, Lechner (2001*a*) shows also that some modified versions of the balancing score properties known from the binary treatment model (Rosenbaum and Rubin, 1983) hold in this more general setting as well. In the following, the basic results of Lechner (2001*a*) are repeated.

[20] See for example Section X in the paper by Heckman and Robb (1986) for the link between matching on the propensity score and classical selection models.

[21] Note that the CIA is not the minimal identifying assumption, because all that is needed to identify mean effects is conditional mean independence. However, the CIA has the virtue of making the latter valid for all transformations of the outcome variables. Furthermore, in this study, it would be difficult to argue why conditional mean independence should hold and the CIA might nevertheless be violated.

[22] In fact, the (particular) average treatment effect on the treated for treatment $m$ compared to treatment $l$, involves $Y^l$ only. For details, see Lechner (2001*a*). Nevertheless, since we are interested in all treatment effects, the more restrictive version given in (3) is appropriate.

[23] Since identification is obtained nonparametrically, it appears natural to avoid imposing a functional form on $\mathrm{E}(Y^m|X, S = m)$ for the purpose of ease of estimation (thus avoiding the danger of inconsistent estimates due to incorrect imposition of functional forms, that have carefully been avoided beforehand).

Denoting the choice probability of alternative $j$ conditional on $X$ as $\mathrm{P}(S=j|X=x)=\mathrm{P}^j(x)$, gives the following result for the effect of treatment $m$ compared to treatment $l$ on the participants in treatment $m$:

$$\begin{aligned}\theta_0^{m,l} &= \mathrm{E}(Y^m|S=m) - \underset{\mathrm{P}^{l|ml}(X)}{\mathrm{E}}\{\mathrm{E}[Y^l|\mathrm{P}^{l|ml}(X), S=l]|S=m\} \\ \mathrm{P}^{l|ml}(x) &= \mathrm{P}^{l|ml}(S=l|S=l \text{ or } S=m, X=x) = \frac{\mathrm{P}^l(x)}{\mathrm{P}^l(x)+\mathrm{P}^m(x)}.\end{aligned} \tag{4}$$

If the respective probabilities $\mathrm{P}^{l|ml}(x)$ are known or if a good estimator is available, ie a consistent estimator that converges at the parametric rate, the dimension of the (nonparametric) estimation problem is reduced to one. If $\mathrm{P}^{l|ml}(x)$ is modelled directly, no information from subsamples other than the ones containing participants in $m$ and $l$ is needed for the estimation of $\theta_0^{m,l}$ and $\theta_0^{l,m}$.

The equality

$$\mathrm{E}(Y^l|S=m) = \underset{\mathrm{P}^{l|ml}(X)}{\mathrm{E}}\{\mathrm{E}[Y^l|\mathrm{P}^{l|ml}(X), S=l]|S=m\}$$

suggests a similar strategy of identifying (and estimating) the population effect $\gamma_0^{m,l}$:

$$\begin{aligned}\gamma_0^{m,l} &= \mathrm{E}Y^m - \mathrm{E}Y^l = \sum_{j=0}^{M}[\mathrm{E}(Y^m|S=j) - \mathrm{E}(Y^l|S=j)]\mathrm{P}(S=j) \\ &= \sum_{j=0}^{M}\left\{\underset{\mathrm{P}^{m|mj}(X)}{\mathrm{E}}\mathrm{E}[Y^m|\mathrm{P}^{m|mj}(X), S=m]|S=j\right\} \\ &\qquad - \underset{\mathrm{P}^{l|lj}(X)}{\mathrm{E}}\Big\{\mathrm{E}[Y^l|\mathrm{P}^{l|lj}(X), S=l|S=j]\mathrm{P}(S=j)\Big\}.\end{aligned} \tag{5}$$

In this evaluation, it will be more straightforward from a modelling point of view to specify the complete discrete choice problem of choosing a particular treatment out of the complete list of treatments simultaneously (see Section 2). $\mathrm{P}^{l|ml}(x)$ could then be computed from that model. In this case, we have consistent estimates of all marginal choice probabilities $[\mathrm{P}^0(X),\ldots,\mathrm{P}^M(X)]$. Hence, it may be attractive to condition jointly on $\mathrm{P}^l(X)$ and $\mathrm{P}^m(X)$ instead of $\mathrm{P}^{l|ml}(X)$ . This also identifies $\theta_0^{m,l}$, because $\mathrm{P}^l(X)$ together with $\mathrm{P}^m(X)$ is 'finer' than $\mathrm{P}^{l|ml}(X)$, since

$$\mathrm{E}[\mathrm{P}^{l|ml}(X)|\mathrm{P}^l(X),\mathrm{P}^m(X)] = \mathrm{E}\left[\frac{\mathrm{P}(X)}{\mathrm{P}^l(X)+\mathrm{P}^m(X)}|\mathrm{P}^l(X),\mathrm{P}^m(X)\right] = \mathrm{P}^{l|ml}(X).$$

To summarise, in this paper, to identify the various effects of interest, we use the condition

$$\mathrm{E}(Y^l|S=m) \overset{!}{=} \underset{\mathrm{P}^m(X),\mathrm{P}^l(X)}{\mathrm{E}}\big\{\mathrm{E}[Y^l|\mathrm{P}^m(X),\mathrm{P}^l(X),S=l]|S=m\big\} \quad \forall l,m \tag{6}$$

which is a direct implication of condition (3) and the common support requirement.

Making use of (4) and (5) justifies the strategy of estimating $\mathrm{E}(Y^l|S=m)$ for all combinations of $m$ and $l$ based on (6), and then using these estimates to com-

pute the different treatment effects $\gamma_0^{ml}$ and $\theta_0^{ml}$. Such an estimator is proposed next.

### 3.3. *A Matching Estimator*

Given the choice probabilities, or a consistent estimate of them, the terms appearing in (4) and (5) can be estimated by any parametric, semiparametric or nonparametric regression method that can handle one-or two-dimensional explanatory variables. One of the popular choices of estimators in a binary framework is matching, for recent examples and discussions, see Angrist (1998), Dehejia and Wahba (1999), Heckman, Ichimura and Todd (1998), Heckman, Ichimura, Smith and Todd (1998), Lechner (1999, 2000) or Smith and Todd (2001). The idea of matching on balancing scores is to estimate $\mathrm{E}(Y^m|S=l)$ by forming a comparison group of selected participants in $m$ that have the same distribution of the balancing score (here $\mathrm{P}^{l|ml}(X)$ or $[\mathrm{P}^l(X), \mathrm{P}^m(X)]$) as the group of participants in $l$. By virtue of the property of being a balancing score, the distribution of $X$ will also be balanced in the two samples. The estimator of $\mathrm{E}(Y^m|S=l)$ is then simply the mean in that selected comparison group. Compared to kernel estimates, a major advantage of matching is clearly its simplicity and its intuitive appeal. Advantages compared to parametric approaches are its robustness to the functional form of the conditional expectations (wrt $\mathrm{E}(Y^m|X, S=m)$) and that it leaves the individual causal effect completely unrestricted and, hence, allows arbitrary heterogeneity of the effect in the population.

Lechner (2001*a, b*) proposes and compares different matching estimators that are analogous to the rather simple matching algorithms used in the literature on binary

Table 2

*A Matching Protocol for the Estimation of* $\gamma_0^{m,l}$ *and* $\theta_0^{m,l}$

| | |
|---|---|
| Step 1 | Specify and estimate a multinomial probit model to obtain $[\hat{\mathbf{P}}_N^0(\mathbf{x}), \hat{\mathbf{P}}_N^1(\mathbf{x}), \ldots, \hat{\mathbf{P}}_N^M(\mathbf{x})]$. |
| Step 2 | Restrict sample to common support: delete all observations with probabilities larger than the smallest maximum and smaller than the largest minimum of all subsamples defined by $S$. |
| Step 3 | Estimate the respective (counterfactual) expectations of the outcome variables. |
| | For a given value of $m$ and $l$, the following steps are performed: |
| | (a) Choose one observation in the subsample defined by participation in $m$ and delete it from that pool. |
| | (b) Find an observation in the subsample of participants in $l$ that is as close as possible to the one chosen in step (a) in terms of $[\hat{\mathbf{P}}_N^m(\mathbf{x}), \hat{\mathbf{P}}_N^l(\mathbf{x}), \tilde{\mathbf{x}}]$. $\tilde{\mathbf{x}}$ is a subvector of $\mathbf{x}$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again. |
| | (c) Repeat (a) and (b) until no participant in $m$ is left. |
| | (d) Using the matched comparison group formed in (c), compute the respective conditional expectation by the sample mean. Note that the same observations may appear more than once in that group. |
| Step 4 | Repeat Step 3 for all combinations of $m$ and $l$. |
| Step 5 | Compute the estimate of the treatment effects using the results of Step 4. |

*Note*: Lechner (2001*a*) suggests an estimator of the asymptotic standard errors for $\hat{\gamma}_N^{m,l}$ and $\hat{\theta}_N^{m,l}$ based on the approximation that the estimation of the probabilities in Step 1 can be ignored. A comparison to bootstrap standard errors as in Lechner (2001*d*) suggests that these standard errors are fairly accurate. Such a comparison is not possible in this case, because estimation of the MNP is too time-consuming to allow for sufficient bootstrap replications.

treatments. Here, the estimators are based on $[P^l(X), P^m(X)]$ that performed well in Lechner (2001*b*). The exact matching protocol is contained in Table 2.

Several comments are in order. Step 2 ensures that we estimate effects only in regions of the attribute space where observations from two treatments *could* be observed having a similar participation probability (common support requirement).[24]

Table 3 shows the distribution of the deleted observation due to the common support requirement across the different subsamples. Overall, the loss of observations is about 14%. Language courses lose about 20% of their participants, whereas vocational training courses lose only close to 7%. For the other groups, the loss is close to the mean loss. Given the results for the language courses, it is not surprising that a comparison of the means of the deleted and the remaining samples reveals, that in general, the share of unskilled people, foreigners and women is somewhat higher in the deleted sample. The differences appearing in the distribution of sectors and occupations are probably related to those characteristics.[25]

A second remark with respect to the matching algorithm concerns the fact that the same comparison observation is used repeatedly in forming the comparison group (*matching with replacement*). This modification of the 'standard' estimator is necessary for the estimator to be applicable at all when the number of participants in treatment $m$ is larger than in the comparison treatment $l$. Since the role of $m$ and $l$ could be reversed in this framework, this will always be the case when the number of participants is not equal in all treatments. This procedure has the potential problem that few observations may be heavily used although other similar observations are available. This may result in a substantial and unnecessary inflation of the variance. Therefore, the potential occurrence of this problem should be monitored. Comparing the results with the limited evidence available from other studies (Lechner, 2001*b*), values between 17% and 55% for the share of the largest 10% of the weights relative to the sum of the weights in the respective comparison group do not appear to be exceptionally large.[26]

Comparing the means of the probabilities used for matching in the respective matched comparison sample to the respective numbers in Lechner (2001*b*) reveals that they are somewhat higher.[27] This is not surprising because, in this study, four additional variables are used. Hence, the quality of the match with respect to the

[24] Note that, if we would only be interested in pair-wise average effects on the treated, the current implementation would be unnecessarily strict, because making sure that there is an overlap for each pair would be sufficient. In that case, each effect would be based on a different population defined by the corresponding definition of the support. A comparison of the estimates could thus not disentangle differences coming from different supports and differences coming from different effects. For a detailed discussion of the common support issue, see Lechner (2001*c*). The values of the estimated probabilities used to ensure common support are given in Appendix www.siaw.unisg.ch/lechner/gl_ej.

[25] The detailed results are available in Appendix www.siaw.unisg.ch/lechner/gl_ej.

[26] The detailed results are available in Appendix www.siaw.unisg.ch/lechner/gl_ej.

[27] The detailed results are available in Appendix www.siaw.unisg.ch/lechner/gl_ej. To ensure that the match is not only adequate with respect to the balancing score but also with respect to other important variables, such variables which are denoted as $\tilde{\mathbf{x}}$ in Table 2 are also included in the matching. $\tilde{\mathbf{x}}$ contains the following four variables: *Native language not a Swiss language, Gender, Begin of programme* and *Duration of unemployment until begining of programme*. The weight in the Mahalanobis distance for the two probabilities is increased by a factor of 5 compared to $\tilde{\mathbf{x}}$.

Table 3

*Loss of Observations due to Common Support Requirement*

| | Non-part. | Basic courses | Language course | Computer course | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temp. wage subsidy |
|---|---|---|---|---|---|---|---|---|---|
| Observations before | 6,918 | 1,491 | 1,719 | 1,394 | 424 | 497 | 1,124 | 1,349 | 4,390 |
| Observations after | 5,987 | 1,290 | 1,380 | 1,237 | 395 | 433 | 978 | 1,133 | 3,700 |
| Percentage deleted | 13.5 | 13.5 | 19.7 | 11.3 | 6.8 | 12.9 | 13.0 | 16.0 | 15.7 |

*Note*: The total number of observations decreases due to the enforcement of the common support requirement from 19,306 to 16,533 (−14.3%).

probabilities must necessarily deteriorate. Nevertheless, the level of the mean differences in the various groups appears to be rather small.

## 4. Results

### 4.1. *Measures for the Effectiveness*

Our goal is to measure the micro effects of participation in the programmes with respect to the outcomes in the labour market. To do so, we rely on the data from the unemployment registrars only, because the social security data ends December 1997, just before the programmes start. Therefore, we can only observe individuals as long as they are registered as unemployed. When they leave the unemployment register we observe the destination state of the initial transition out of unemployment (employment, out-of-labour-force). The following transition is, however, only observed if its destination is unemployment. We do not observe earnings, or any other job characteristic, when people leave the unemployment register for employment. Furthermore, the potential period of observing programme effects cannot be longer than 15 months, since our latest observation dates from 31 March 1999. In that sense, the analysis is restricted to the short-run effects of the ALMP. Having in mind the above qualifications, the effects of the programmes will be measured in terms of changes in the average probabilities of unsubsidised employment.[28]

Effects are measured after the programme *begins*.[29] Hence, if somebody leaves a programme early to take up a job, this will influence our measure of effectiveness of the programme in a positive way. Such a measure could be disputed when one believes that being in the programme is a 'good thing' *per se*, but we concentrate solely on the success in the labour market. The alternative of measuring the effects after the programme *ends* entails an endogeneity problem (at least in the short run), because successful participants will leave their programme earlier.

Table 4 gives descriptive statistics for the outcome variables *employed* as well as for *not being registered as unemployed*. These outcomes are measured either with respect to a particular distance in time to the start of the respective programme (here one year), or at a particular day in calendar time (here the last day observed in the data, ie 31 March 1999). Table 4 shows that the concept of time does not really matter. However, the distinction employment–unemployment matters because the relative size of the flows into the remaining state 'out-of-the-labour-force' differ between programmes. In the following, employment will be used as outcome because we consider it to be the more relevant concept from an economic point of view.[30]

[28] The time in a programme, including a job subject to the temporary wage subsidy, is not considered as employment in that sense.

[29] In fact, *begin* is defined here as the official allocation to the programme to take account of behavioural change due to the knowledge that programme participation is certain.

[30] Note that the employment rates given in Table 4 do not exactly equal the ones given in Table 1. Table 4 is based on the subsample restricted by the common support requirement. However, all differences are smaller than two percentage points.

Table 4

*Mean Outcomes after the Beginning of the Programmes (in percentage points)*

| Sample | | 1 year after start | | | End of March 1999 | | |
|---|---|---|---|---|---|---|---|
| | | Employed | Not unemployed | *N* | Employed | Not unemployed | *N* |
| *Non-participation* | (*NONP*) | 41 | 61 | 4,169 | 39 | 61 | 5,987 |
| *Basic courses* | (*BAC*) | 35 | 48 | 834 | 32 | 45 | 1,290 |
| *Language courses* | (*LAC*) | 30 | 40 | 1,007 | 29 | 42 | 1,380 |
| *Computer courses* | (*COC*) | 46 | 57 | 898 | 44 | 56 | 1,237 |
| *Further vocational training* | (*FVT*) | 44 | 50 | 286 | 42 | 51 | 395 |
| *Other training courses* | (*OTC*) | 43 | 55 | 258 | 43 | 55 | 433 |
| *Employment programmes (public)* | (*EP-PU*) | 34 | 44 | 466 | 30 | 41 | 978 |
| *Employment programmes (private)* | (*EP-PR*) | 31 | 43 | 547 | 25 | 38 | 1,133 |
| *Temporary wage subsidy* | (*TEMP*) | 52 | 61 | 2,258 | 49 | 59 | 3,700 |

*Note*: See Appendix www.siaw.unisg.ch/lechner/gl_ej for more detailed statistics.

In addition to the means of the outcome variables, Table 4 contains the number of available observations. Note that this number is lower when the outcome is measured one year after the start of the programme. This is due to the fact that participants in programmes that start after March 1998 cannot be observed one year later. Nevertheless, the remaining sample appears to be sufficiently large to conduct reliable inference.

### 4.2. *Mean Effects*

Table 5 presents the results for the changes in the employment rates due to the programme one year after the individual programme participation starts. Since our last day of observation is 31 March 1999, this estimation is based on participants entering the programme before April 1998. The corresponding effects measured at the end of March 1999 are similar and will be omitted in the following discussion.[31]

The upper part of Table 5 displays the mean effects of the programmes on their respective participants; the lower part gives the estimated average effect for a person randomly selected from the population. The entries in the main diagonal show the employment rates in the nine groups in percentage points; the programme effects are off the main diagonals.[32] A positive number of, say, 10.4 indicates that the effect of the programme shown in the row compared to the programme appearing in the column is 10.4 percentage points higher rate of

[31] Details can be obtained from Appendix www.siaw.unisg.ch/lechner/gl_ej.
[32] To ease reading and writing in most cases we will call *Non-participation* a programme.

Table 5

*Average Effects Measured as Difference in Employment Rates One Year After Start of Programme (in percentage points)*

| | | | | | | | Employment programme | | |
|---|---|---|---|---|---|---|---|---|---|
| *m* | Non-part. | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Public | Private | Temporary wage subsidy |
| Average effect for participants in programme ($\theta_0^{m,l}$) | | | | | | | | | |
| *NONP* | 42 | 10.4** | | | | | 8.3* | 9.2** | −7.1** |
| *BAC* | −10.5** | 36 | | | | −18.4** | | | −14.8** |
| *LAC* | −8.7** | | 31 | | −15.6 | −16.1* | 9.1 | | −20.2** |
| *COC* | −4.8 | 11.1** | | 46 | | −14.1* | | | −9.1** |
| *FVT* | | 13.0** | | | 44 | | 13.8* | 13.8* | −11.9** |
| *OTC* | | 8.9 | | | | 43 | 12.7* | | −7.9 |
| *EP-PU* | −6.6* | | | | −10.6 | −14.3* | 33 | | −15.1** |
| *EP-PR* | −7.9* | | | | | −15.3* | | 31 | −22.6** |
| *TEMP* | 5.8** | 17.4** | 10.9** | 7.4* | | | 17.5** | 14.7** | 52 |
| Average effect ($\gamma_0^{m,l}$) | | | | | | | | | |
| *NONP* | 42 | 10.1** | 6.0* | | | | 9.9** | 8.5** | −7.6** |
| *BAC* | −10.1** | 36 | | −6.5 | −8.8* | −16.7** | | | −17.7** |
| *LAC* | −6.0* | | 31 | | | −12.5** | | | −13.5** |
| *COC* | | 6.5 | | 46 | | −10.2* | | | −11.2** |
| *FVT* | | 8.8* | | | 44 | | 8.6 | | −8.8* |
| *OTC* | | 16.7** | 12.5** | 10.2* | | 43 | 16.5** | 15.1** | |
| *EP-PU* | −9.9** | | | | −8.6 | −16.5** | 33 | | −17.4** |
| *EP-PR* | −8.5** | | | | | −15.1** | | 31 | −16.1** |
| *TEMP* | 7.6** | 17.7** | 13.5** | 11.2** | 8.8* | | 17.4** | 16.1** | 52 |

*Note*: Results are based on matched samples. ** indicates significance at the 1% level (2-sided test), * significance at the 5% level. Estimated coefficients not significant at the 10% level are omitted.

employment. In the upper part of the Table, this effect is valid for the population participating in the programme appearing in the rows of the Table. For example, the entry for the programme in the 5th row (*Further vocational training, FVT*) and the programme in the 8th column (*Employment programme, Private, EP-PR*) should be read as follows: 'For the population participating in *FVT, FVT* increases the probability of being employed one year after the programme on average by 13.8 percentage points compared to that population being in *EP-PR*.'

The effects presented in the lower part of the Table have a corresponding interpretation, but they do not refer to a specific subpopulation. Note these effects are symmetric ($\gamma_0^{m,l} = -\gamma_0^{l,m}$). The Table contains only an entry for a pair-wise effect when the estimate is statistically significant at the 10% level.

The results for the respective participants in the programmes (upper part of Table 5) indicate that *TEMP* is superior to almost all the other programmes. The mean gain is between about 6% and 22 percentage points. In particular, *TEMP* is the only programme that dominates *Non-participation (NONP)* (+6%). The pair-wise effects show clearly that *TEMP* is the most effective programme, whereas *Employment programmes* (*EP*) as well as *Basic* (*BAC*) and *Language courses* (*LAC*) have negative effects. The remaining courses are intermediate.

It might appear strange that a programme can harm individual employment chances. However, assume that the programmes themselves do not change the employment chances directly.[33] Nevertheless, an immediate indirect effect of all programmes will be a reduction in job search activities compared to non-participants. Furthermore, participants may receive fewer job offers from the labour office. In this case, we should expect an initial negative effect from any kind of participation in a programme but, in the longer run, the direct effect of a successful programme needs to overcompensate for this initial fall. (Note that we measure the effects from the start of the programme.) Indeed, the dynamics of the effects presented below show exactly that pattern.

The population effects contained in the lower panel, by and large, confirm the results that appear in the upper panel of Table 5.[34] This is somewhat surprising because a well targeted programme could be expected to be more efficient for its participants than for participants in other programmes or non-participants. Howerer, a comparison of corresponding numbers above and below the main diagonal in the upper panel of Table 5 for the different groups of participants reveals no systematic pattern. In particular the effects of the programmes do not appear to be consistently more positive for their particular participants than for other groups of the population. This result suggests that selection into the programmes appears not to be correlated with realised gains from the programmes. This raises the question whether the allocation of individuals to specific parts of the active labour market policies could be improved in the future.

The following Figures show the dynamics of the effects by showing their development over time after the start of the programme. Figure 1 compares the state of

[33] They could, of course, decrease the employment chances if there is negative signalling, but ignore this for the sake of the argument.

[34] The term population refers to the population defined by selection rules explained earlier.

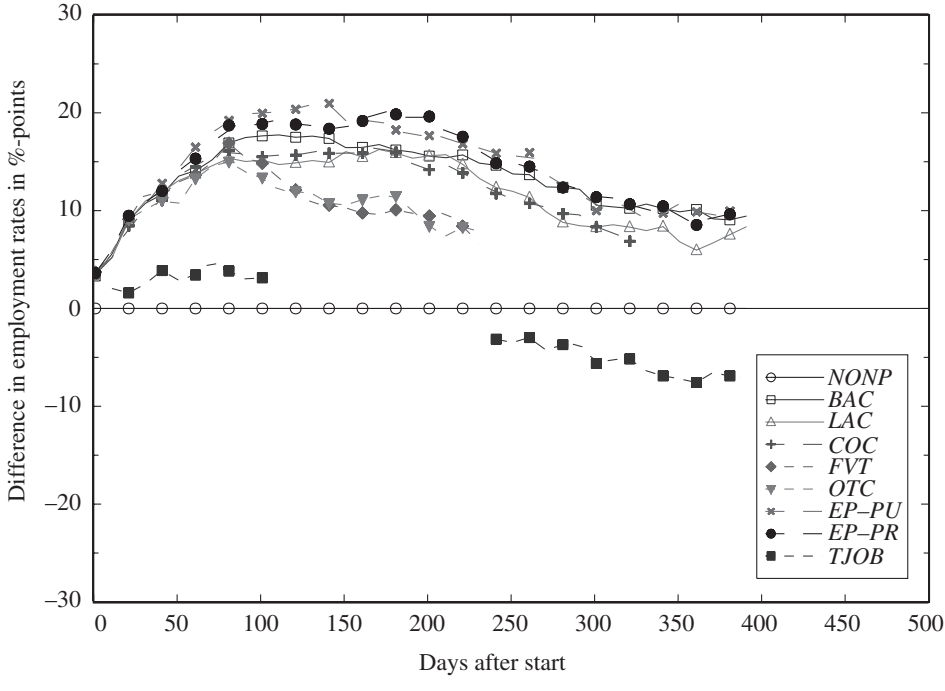


Fig. 1. *Average Effects for the Population: A Comparison to Nonparticipation*
*Note*: *NONP*: Nonparticipation, *BAC*: Basic training course; *LAC*: Language course; *COC*: Computer course; *FVT*: Further vocational training; *OTC*: Other training course; *EP-PU*: Employment programme (public); *EP-PR*: Employment programme (private); *TJOB*: Temporary wage subsidy.
Only estimated effects that are significant at the 5% level are reported.
A ten-day moving average is used.

non-participation as the reference state with all programmes.[35] In this case, all effects relative to *NONP* are estimated for the entire population. Note that a line above zero indicates that *NONP* would actually increase employment compared to the programme indicated by this particular line. The sample sizes decrease with increasing distance from the start (beginning month 3), since the last observation is 31 March 1999, and programmes may start until 31 January 1999. However, most programmes start in the first quarter of 1998, so that the sample size is potentially large enough to estimate the effects for about one year after the start of a programme with sufficient precision.[36]

The dynamic evolution of the effects has the expected profile: in the beginning, they are positive and increasing (probably a result of reduced job search activities in the programmes), but then they decline as participants leave their respective programmes and increase their job search activities. For all programmes, we find an initial negative effect compared to *NONP*. For *TEMP*, this effect disappears after

[35] Lechner (2001*b*) proposed composite effects to summarise the different pair-wise effects. For the sake of brevity, they are only contained in the Appendix www.siaw.unisg.ch/lechner/gl_ej.
[36] See Appendix www.siaw.unisg.ch/lechner/gl_ej for the detailed results.

about four months and becomes positive after about eight months. We observe a similar, but somewhat slower, reversal for *FVT, Other training courses* (*OTC*), and with a further delay also for *Computer courses* (*COC*). However, in all these cases the effect does not become significantly positive. The dynamics of the effects of *LAC, BAC* as well as *EP* have a similar shape, but remain significantly negative until the end of the observation period.

Figures 2–4 give more insights for the particular programmes using direct comparisons of the programmes with each other and with *NONP*.[37] Figure 2 displays the estimates of the effects of *FVT* for participants in *FVT*. A line above zero indicates that *FVT* has a positive employment effect relative to the programme associated with that particular line. Only effects significant at the 5% level are displayed.

With respect to *NONP*, the same pattern appears as before. Initially, there is a drop leading to a negative effect associated with the time in the programme. After about three months, there is a reversal and, after about 250 days, there is no statistically significant difference (at the 5% level). The time horizon might be too short to observe a statistically significant positive effect.

Such a reversal does not appear for the negative effect of *FVT* in the comparison to *TEMP*. The effect remains negative throughout. Taken literally, this

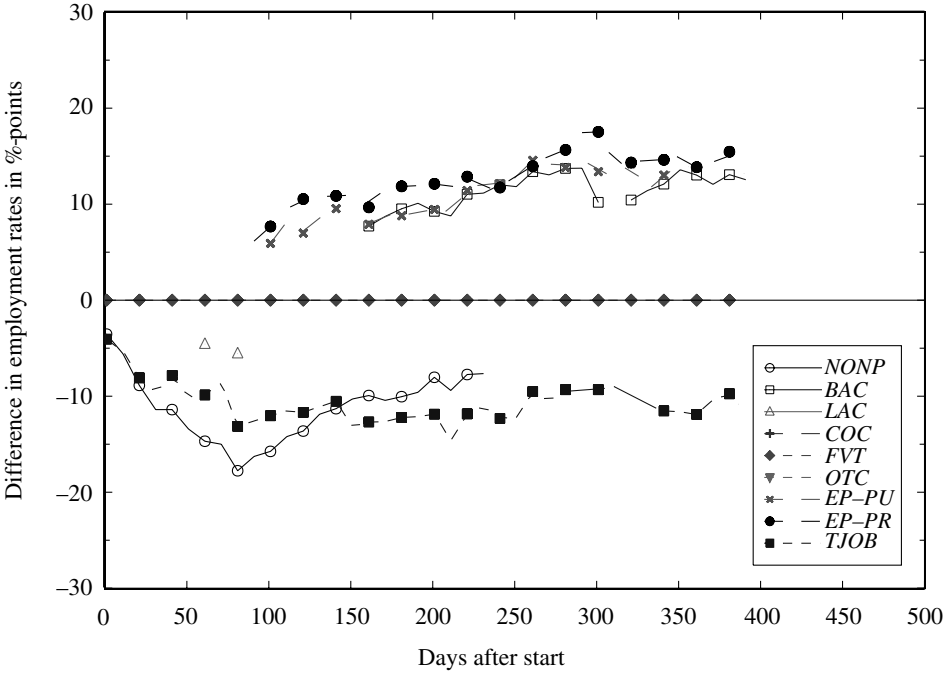


Fig. 2. *Average Effects of Further Vocational Training for Participants in FVT*
*Note:* See Fig. 1

[37] For the sake of brevity, we concentrate only on three different programmes. Corresponding results for the other programmes can be found in Appendix www.siaw.unisg.ch/lechner/gl_ej.

means that the participants in *FVT* would have been better off if they would have participated in *TEMP* instead. Comparisons with *EP* as well as with *BAC* show that these programmes are all dominated by *FVT* after about three months.

Whereas the results for *OTC* are similar to those for *FVT*, the results for *BAC* as well as for *LAC* differ substantially from the results for *FVT*. In both cases, we observe similar dynamic patterns: with respect to *NONP*, the initial drop is somewhat reversed over time, but remains significantly negative. They are always dominated by *TEMP* and, after some time, they are also dominated by other training courses (*FVT*, *OTC*). *COC* are between *FVT*, *OTC* and *BAC*, *LAC*. They are permanently dominated by *TEMP*, whereas the negative effect compared to non-participation disappears towards the end.

Figure 3 shows the estimates of pair-wise effects for *Employment programmes (Public) EP-PU*. The previous result that *EP-PU* is not a successful programme is confirmed: it is dominated throughout by *NONP* and *TEMP*. Again, the negative effect with respect to *NONP* increases initially and starts to decline after about three months. Note that this happens well before the average programme duration of about 150 days. Although the effect decreases continuously, afterwards, it remains significantly negative after about one year. It seems unlikely that it will become positive in the near future. After about 250 days, *EP-PU* is also dominated by *FVT* and *OTC,* which have an average programme duration of about half of that of *EP-PU*. *EP-PU* is the only programme that has no

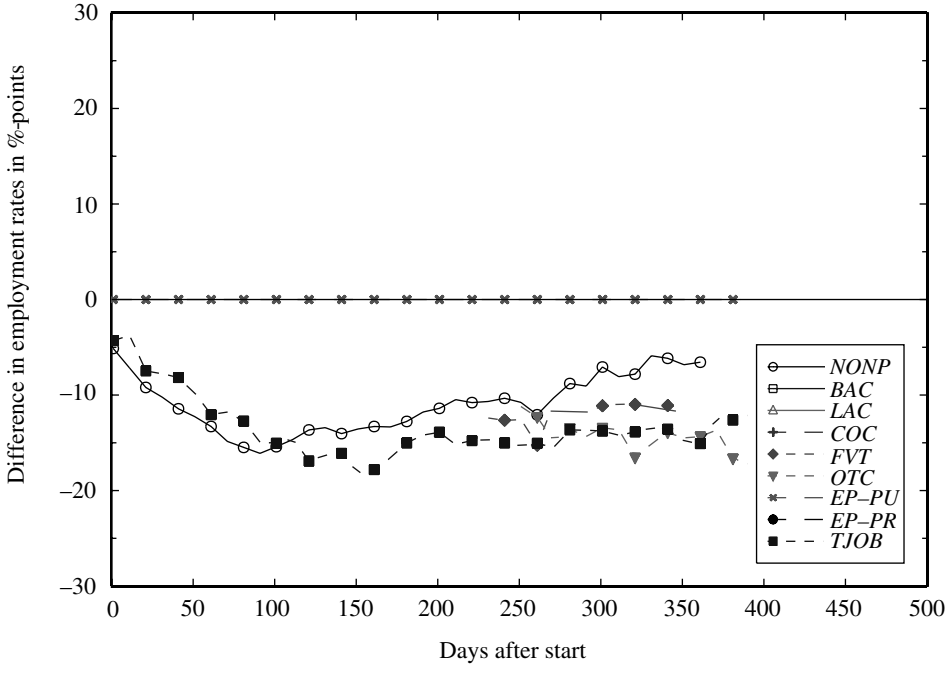


Fig. 3. *Average Effects of Employment Programme (Public) for Participants in EP-PU*
*Note*: See Fig. 1

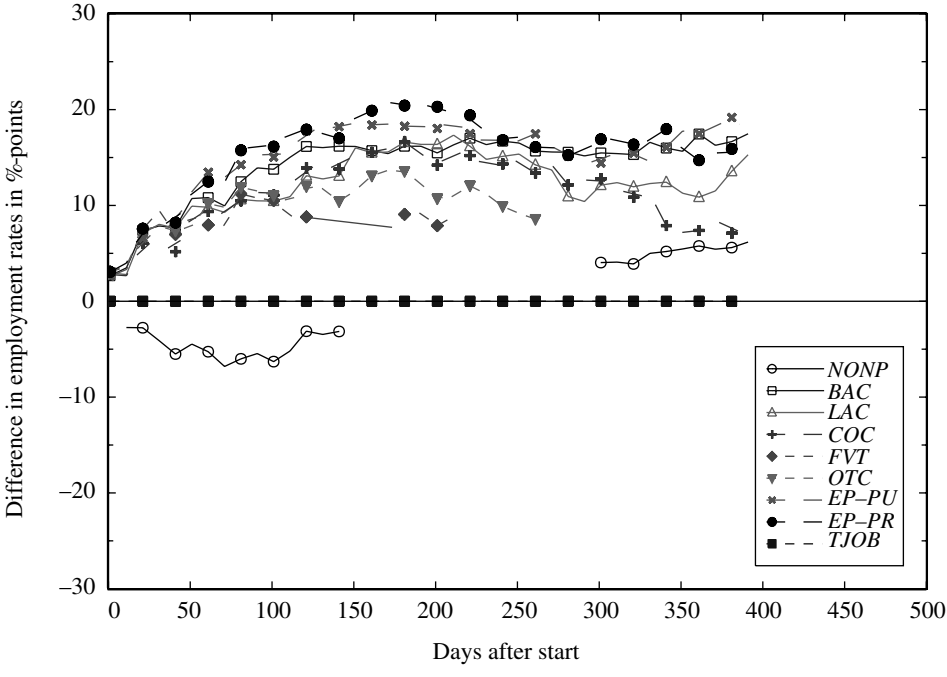


Fig. 4. *Average Effects of Temporary Wage Subsidy for Participants in TEMP*
*Note*: See Fig. 1

positive effect at all. The results for *EP-PR* lead to the same conclusions as the ones for *EP-PU*.

Finally, Figure 4 shows that *TEMP* dominates all other programmes almost from the start. For most programmes, this picture persists over time, except for *OTC* and *FVT*. For those two programmes, the initial negative effect reverses after about half a year and becomes insignificant some time later. With respect to *NONP*, the familiar pattern appears but, in this case, we find a positive effect for *TEMP* after about nine months.

### 4.3. *Heterogeneity of the Effects*

The previous Section has shown that there is considerable heterogeneity with respect to the effects of the different programmes. In this Section, we investigate the question whether different groups of potential participants show heterogeneous effects. We perform this analysis by estimating the various effects in different strata of the sample.[38]

With respect to sex, there appears to be an interesting deviation from the overall mean effects concerning *EP-PU* for women. Instead of the negative result discussed above, *EP-PU* appears to have an effect like the more successful training courses

[38] The detailed results are found in Appendix www.siaw.unisg.ch/lechner/gl_ej.

(*FVT, COC, OTC*). This effect changes only for *EP-PU*, but not for *EP-PR*, for which it remains as negative as for men and women together.

Several interesting differences appear with respect to nationality (Swiss or non-Swiss). First, a puzzling result is that *LAC*, have a large negative effect for the non-Swiss population. This is seriously at odds with our priors. We suspect that the observable factors related to the participation of this population – such as the year of entering the social security system, mother tongue, type of work permit and knowledge of language – may not be sufficient selectivity controls.

Second, we find larger positive effects of *TEMP* and *COC* for the non-Swiss population. This might be explained by the hypothesis that only foreigners with good prospects are admitted to *COC*. However, we did not test whether these differences are significant.

Considering two subgroups defined by the duration of the unemployment spell before the start of the programme (more or less than 270 days), a major difference appears with respect to shape and level of the effect of *NONP*. For people with a short unemployment spell, there is the familiar large negative initial effect. However, for those being in unemployment for more than 270 days before the programmes start the corresponding curve is much flatter. This different shape is, of course, transferred to the results for all other programmes. For example, for *TEMP*, there is no initial negative effect. Furthermore, we find an earlier and larger significant positive effect. From an economic point of view, these results are perfectly compatible with the following consideration: assume that the average job offer arrival rate is higher in the beginning of an unemployment spell. One reason could be a sorting argument: the 'good' unemployed find jobs earlier so, in the population of long-term unemployed, there are predominantly people with bad chances on the labour market. In that case, the cost of any programme in terms of a reduction of the employment probability due to receiving less job offers during a programme are higher for the population that is at an early stage of their unemployment spell. Put differently, it appears to be a bad idea to 'lock away the unemployed in a programme' while they have a high probability of finding a job. After the returns to job search have fallen to a lower level, these indirect costs of programme participation are much lower. There is a discussion of this 'lock-in' effect in OECD (2001). Calculations for Finland, for example, clearly illustrate why job seekers should not be referred to ALMP early in the unemployment spell.

### 4.4. *Sensitivity*

We checked our specification in several directions that could influence our overall conclusions.[39] First, we used two more restricted versions of the covariance matrix of our multiple choice model:

(i) Only correlations between *EP* and *TEMP* allowed
(ii) No correlations and homoscedasticity

[39] Again, see Appendix www.siaw.unisg.ch/lechner/gl_ej for the detailed results.

Furthermore, the number of replications has also been reduced to only 20. In all cases, the final evaluation results are surprisingly robust to the different resulting balancing scores. The same holds true for matching with and without the variables that have been included in addition to the probabilities.

The second set of sensitivity checks relate to the outcome variables. When we use 'not unemployed' instead of 'employed' as the outcome we obtain different results compared to *NONP* because this group contains the largest share of unemployed who leave the labour force after being deregistered from the unemployment register. Therefore, using not unemployed as the outcome variable makes *NONP* the most successful strategy, whereas the relative effects between the other groups remain more or less the same.

Changing the start of measurement from the begin of a programme to the end of a programme can have serious consequences for some results (at least in the short run) because of the already discussed endogeneity problem (successful participants leaving the programme early into employment). The most interesting changes appear with respect to the effect of the *EP-PR*: after an initial positive effect compared to *NONP* for the first three months,[40] we obtain a negative effect after some time. In this case, it is more difficult to obtain precise estimates because the number of observations is necessarily lower than in the standard case. Furthermore, since matching takes only account of start dates and not of end dates (because end dates are considered to be endogenous), these results may be influenced by business cycles as well as seasonal effects. Due to the economic upswing after 1997, we would expect an upward bias in the effects of the *Employment programmes*, because the outcomes of the participants of the *Employment programme* are measured considerably later than the outcomes of the non-participants.

In another sensitivity check, the 'standard' results are compared to results that are based on calendar time instead of time counted from the start of a programme. The mean effect is computed using all observations that already have started a programme. Therefore, these effects have a different interpretation because, initially, they are based on observations shortly after the start of their programme. Over time more observations enter, but also the share of the 'older' (measured as distance from start) observations, as well as the total number of observations, increases. We find effects that are similar to the ones already presented.

There may be concerns that the unemployed with only temporary permits might leave the unemployment register and re-migrate to their home country. In that case, we would observe them as being out-of-the-labour-force. Another issue might be that we want to consider the effects only for the so-called full-time unemployed. Our results for the subsample of full-time unemployed who have either permanent permits or are Swiss citizens indicate, however, that the main conclusions do not change for that subsample. Yet, the negative effects for the *LACs* are somewhat smaller than for the full sample. The results for women in

[40] We use the starting date as the end date for non-participants, which means we require them to remain unemployed until the programme of the participant to whom he or she is matched ends.

that subgroup do not substantially deviate from the general findings for women presented above.

Additional sensitivity checks for a subgroup of the programmes can be found in Lechner (2001*d*).

### 4.5. *Summary of the Results*

Perhaps the most important result of our evaluation study is that we find different effects for two different types of temporary employment programmes. On the one hand, there are traditional employment programmes. These programmes usually take place in a sheltered labour market that is not supposed to be in competition with the regular labour market. For these programmes, we find significant negative effects corresponding to results of evaluation studies for other countries. Our conjecture is that the additional amount of human capital obtained in these programmes is too small to compensate for the initial negative effects due to reduced job search.

On the other hand, we find a successful temporary employment programme that is unique. It is based on a wage subsidy scheme where the unemployed are encouraged to accept jobs that pay less than the unemployment benefit. The difference is overcompensated by payments from unemployment insurance. Jobs in this programme are within the regular labour market. Here, our conjecture for the reasons for the positive effect is that working in a job in the competitive market is valued by potential future employers because the unemployed still keep work-habit specific human capital. The idea here is that working in a market environment is different from the work environment in an employment programme intended solely for the unemployed. Carling and Richardson (2001) report similar findings for Sweden. They conclude that 'the more regular work the participants are allowed to do, the better the program is relative to other ones' (p. 27). Furthermore, this programme could be used as a cheap screening device by future employers. They could use the programme to uncover otherwise unobserved characteristics and offer regular jobs to 'good' programme participants.

As stated in Section 1, the OECD considers *Temporary wage subsidy* as a potentially powerful instrument. Our results indicate that this is true. However, there is concern regarding potential negative incentive effects of *TEMP* in terms of underbidding of the wages set in collective agreements and avoidance of dismissal protection. At this point, we do not have evidence as to whether these considerations are important.

To date, the only other econometric evaluation of the Swiss ALMP is by Lalive *et al.* (2000). Within their duration analysis framework, they allow for unobserved heterogeneity. Any such generalisation of CIA has a price in terms of additional restrictions at another place of the model (by basically restricting the effect of observed heterogeneity). They need a more restrictive specification of both the selection process into the programmes and of the dependence of the labour market outcomes on time and personal characteristics. Furthermore, the heterogeneity of the effects among the participants and non-participants is tightly restricted compared to our approach. Given the good data available for this study, such assumptions seem to be an unnecessary high price to pay to allow for unobserved heterogeneity independent of observables. One of the main differ-

ences compared to our results seems to be a positive effect for *EP* for women after the programme ended that dominates the initial negative effect during the programme.[41]

## 5. Conclusions

This paper provides a microeconometric evaluation of the different programmes of the Swiss ALMP. We focus on the differences of the chances to find a job that are due to these programmes. The estimates are based on a rich database constructed from administrative records of the unemployment as well as from the social insurance system. We use a matching estimator allowing for unrestricted effect heterogeneity among the population as well as for heterogeneity with respect to the different programmes of the Swiss active labour market policy.

The results point to considerable effect heterogeneity both with respect to different programmes as well as with respect to different subpopulations. First, *Employment programmes* perform poorly. We attribute this to the fact that the resulting reduction of received job offers due to being in these comparatively long programmes by far outweighs the gains in terms of additional work experience provided by these programmes. An exception is women in *Public employment programmes*. Uncovering the reason for this exception will be left to future research.

*Vocational training programmes* show a rather mixed performance depending on the specific sub-programmes considered. Although none of the programmes have a positive effect, *Further vocational training, Other training courses*, as well as *Computer courses* (in particular for foreigners) do not look bad. Future research should disentangle these still heterogeneous groups of courses further, possibly to obtain a more precise picture of their effects. In contrast to these courses, *Language courses* as well as *Basic courses* perform rather poorly. However, in particular with respect to *Language courses*, we suspect that the adjustment for the selection process could still be plagued by some remaining selection on unobservables.

*Temporary wage subsidy* appears to be the one clearly successful programme in terms of increasing the chances in the labour market. However, there are some issues whether this programme has incentive effects leading to some undesirable distortions in the labour market. The British New Deal programme also features subsidised employment appearing to be similar to the *Temporary wage subsidy*. The main difference is that in the New Deal people in subsidised employment are expected to stay with their employer when the subsidy period of six months has expired. The Swiss *Temporary wage subsidy*, on the other hand, is intended to encourage temporary employment while looking for a better job.

With respect to general heterogeneity by subpopulation, it appears that participating in a programme in the early stages of the unemployment spell is less effective than participating in later stages. We argue that this is due to the fact that

[41] Lalive *et al.* (2000) treat the largest programme, *TEMP*, as a censored exit state and not as a programme.

participation reduces the number of offers received compared to non-participation. This appears to be particularly damaging for people that would be good matches to these offers. With increasing duration of unemployment, sorting will have already eliminated the best matches and hence the positive (human capital enhancing) effect of the programme will be more important than the temporary reduction of received job offers.

There are still shortcomings that could be improved in future work. The database could be extended in several immediate ways: first, it would be desirable to increase the observation period to also identify longer-term effects of the programmes, second, having some information from the social security system for the period after 1997 would allow us to take account of the quality of the jobs found in terms of earnings and job stability, and third, having some data about the direct costs of these programmes would be useful to perform some sort of cost–benefit analysis.

Here we concentrate on the effect of the first programme so as to avoid obvious endogeneity problems and ignore any further effects of a successive treatments. In fact, if there are any such effects, they are attributed to the first treatment. However, before the issues relating to the timing and spacing of various programmes as well as programme careers can be fruitfully evaluated, further methodological advances are necessary that are beyond the scope of this study. The same is true for evaluating the macroeconomic effects of such programmes.

*University of Bern*
*University of St Gallen,*
*Swiss Institute for International Economics and Applied Economic Research (SIAW)*

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## Appendix A: Data

### A.1. *Aggregation of Training Courses*

When defining the categories used in the estimation, we try to combine programmes that are similar. Furthermore, the resulting groups should have a sufficient size in terms of observations. Still there remains some heterogeneity, especially in *Other courses* and to lesser extent in *Further vocational training*. *Basic courses* also is a rather heterogeneous group. See Table A1.

### A.2. *Sample Selection*

The data selection process is performed in four steps as indicated in Table A2. The first step relates to personal characteristics. With the age restriction, we want to avoid problems with schooling and early retirement options.

We exclude people who are long-term unemployed on 31 December 1997 (step 2) and who have previously participated in employment programmes (step 3). Finally, we exclude all non-participants whose simulated programme starting point is later than their exit from unemployment (see Section 2 for details).

Table A1

*Aggregation of Training Courses*

| Programme | Code in ASAL data set (variable *PROJEKTART*) |
|---|---|
| *Basic courses* | 2: Basic programme |
| | 3: Courses to promote self-esteem and personality |
| | 4: Courses for acquiring basic skills |
| *Language courses* | 5: Language course |
| *Computer courses* | 6: General computer course |
| *training* | 7: Specific computer course |
| *Further vocational* | 8: Business and trade training (up to level of vocational degree) |
| | 9: Business and trade training (above level of vocational degree) |
| | 10: Manufacturing and technical training (up to level of vocational degree) |
| | 11: Manufacturing and technical training (above level of vocational degree) |
| *Other courses* | 12: Practice firms |
| | 13: Practical courses for young unemployed |
| | 14: Courses for jobs in the tourism sector |
| | 15: Courses for jobs in the health care sector |
| | 16: Other courses |

*Note*: ASAL: unemployment offices payment systems. *PROJEKTART* means type of programme.

The resulting data set is then merged with the social security records. After deleting several observations with missing information in the social security data, we end up with our estimation data with sample size 19,307.

## Appendix B: Estimates of the Multinomial Probit Model

Table B1 shows the estimation results of a multinomial probit (MNP) model using simulated maximum likelihood with the GHK simulator.[42] Although being fully parametric, the MNP model is a flexible version of a discrete choice model, because it does not require the Independence of Irrelevant Alternatives assumption to hold.

The variables that are used in the MNP model are selected by a preliminary specification search based on binary probits (each relative to the reference category *Non-participation*) and score tests against omitted variables. Entries for variables excluded from a particular choice equation show a 0 for the coefficient and '–' for the standard error. Based on that, the final specification contains a varying number of mainly discrete variables that cover groups of attributes related to personal characteristics, valuations of individual skill and chances on the labour market as assessed by the placement office, previous and desired future occupations, and information related to the current and previous unemployment spell, and past employment and earnings.

In practice, some restrictions on the covariance matrix of the errors terms of the MNP model need to be imposed, because not all elements of the covariance matrix are identified and to avoid excessive numerical instability. Guided by considerations of similarity of options and sample size, we allowed for free correlations between *Computer courses, Further vocational training, language courses* and *Basic training*, as well as *between Employment programme (Public), Employment programme (Private), and Temporary wage subsidy*. Furthermore, the variance of the error term related to *TEMP* is not restricted (for details, see Table B2).

[42] See, for example, Börsch-Supan and Hajivassiliou (1993) and Geweke *et al.* (1994).

Table A2

*Sample Selection Rules*

| Variable | Non-part. | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programmes: Public | Employment programmes: Private | Temp. wage subsidy |
|---|---|---|---|---|---|---|---|---|---|
| Initial number of observations | 70,310 | 7,515 | 10,453 | 6,900 | 2,357 | 3,210 | 11,317 | 13,885 | 48,931 |
| Personal Characteristics<br>(a) 25 < age < 55<br>(b) Not disabled<br>(c) Previous earnings > SFr. 100<br>(d) Mother tongue not missing<br>(e) Swiss citizen or foreigner with work permit B (annual) or C (permanent)<br>(f) Not working at home, student, apprentice | | | | | | | | | |
| Remaining observations | 48,694 | 5,669 | 7,698 | 5,452 | 1,929 | 2,362 | 8,044 | 9,793 | 36,359 |
| Unemployment spell<br>Duration of current unemployment spell < 365 | | | | | | | | | |
| Remaining observations | 27,793 | 4,017 | 5,803 | 4,110 | 1,267 | 1,525 | 4,622 | 5,562 | 20,158 |
| Programme experience<br>(a) No programme duration > 14 days in 1997 (ASAL database)<br>(b) No employment programme in 1997 (AVAM database)<br>(c) Programme start not 1 January 1998 (otherwise continuing progr.) | | | | | | | | | |
| Remaining observations | 13,797 | 3,067 | 4,035 | 2,887 | 772 | 957 | 2,037 | 2,479 | 8,190 |
| Inconsistent simulated programme start<br>Simulated programme start after end of unemployment spell (only non-participants) | | | | | | | | | |
| Remaining observations | 12,936 | 3,067 | 4,035 | 2,887 | 772 | 957 | 2,037 | 2,479 | 8,190 |

Table B1

*Estimated Coefficients of a Multinomial Probit Model for Participation in a Programme*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Age in years/10 | 0.06* | 0 | 0 | 0 | 0 | 0.11* | 0.12* | 0 |
| Older than 45 | 0 | −0.11** | 0 | 0 | 0 | 0 | 0 | 0 |
| Female | 0.10** | 0.20* | −0.11 | −0.55** | 0.09* | −0.16* | −0.20* | 0.15* |
| Marital status married | 0 | 0 | −0.19* | 0 | 0 | −0.19* | −0.23* | 0 |
| Marital status divorced | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.12** |
| Number of persons to support | 0 | 0.03** | 0 | 0 | 0 | 0 | 0 | 0 |
| Mother tongue | | | | | | | | |
| French | 0 | 1.13* | 0 | 0 | 0 | 0 | 0 | 0 |
| Italian | 0 | 0.74* | 0 | 0 | 0 | 0 | 0 | 0 |
| Not (German/French/Italian) | 0 | 1.18* | −0.47* | −0.57** | 0 | 0 | 0 | −0.31* |
| G/F/I, but not canton language | 0 | 0.39* | 0 | −0.65* | 0 | 0 | −0.13** | −0.11** |
| Foreign Languages | | | | | | | | |
| Other Swiss language | 0 | 0.15* | 0.24** | 0 | 0 | 0.08** | 0.12* | 0.14* |
| English, Spanish, Portuguese | 0 | 0.27* | 0.42** | 0 | 0 | 0 | 0 | 0 |
| Looking for ... job (reference category: no information) | | | | | | | | |
| Full-time | 0 | 0.09** | 0 | 0 | 0 | 0 | 0 | 0 |
| Part-time | 0 | −0.13* | 0 | 0 | 0 | 0 | 0 | −0.21* |
| Unemployment-status (reference category: part-time) | | | | | | | | |
| Full-time | 0.31* | 0.24* | 0.09 | 0.49** | 0.32* | 0.44* | 0.29* | 0.22* |
| In part-time employment | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1.26* |
| Nationality (reference category: Swiss) | | | | | | | | |
| Foreign with permanent permit | 0 | 0 | −0.50* | 0 | −0.20* | −0.22* | 0 | 0 |
| Foreign with yearly permit | 0 | 0 | −0.74* | 0 | −0.12* | −0.13** | 0.07 | 0 |
| Monthly earnings in last job (reference category: between 2000 and 6000) | | | | | | | | |
| Less than 2000 | 0 | 0 | 0.24** | 0.39* | 0 | 0 | 0 | 0 |
| More than 6000 | −0.15** | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

Table B1
*Continued*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Chances to find a job (reference category: medium) | | | | | | | | |
| No information | −0.13 | −0.09 | −0.16 | −0.35 | −0.19 | −0.25* | −0.33* | 0.13* |
| Very easy | 0.07 | −0.17 | −0.01 | −0.01 | −0.03 | −0.16* | −0.27* | −0.06 |
| Easy | −0.03 | −0.17* | 0.11 | −0.28 | −0.03 | −0.16* | −0.14** | 0.11** |
| Difficult | −0.05 | 0.12** | −0.25** | −0.36* | −0.16** | −0.09* | 0.02 | −0.34* |
| Special case | −0.14 | −0.24** | −0.79* | −0.93* | −0.08 | −0.20** | −0.04 | −0.87* |
| Qualification (reference categories: semi-skilled, unskilled) | | | | | | | | |
| Skilled | 0 | −0.15* | 0.62* | 0 | 0 | 0 | 0 | 0 |
| Previous industry sector (reference categories: agriculture, mining/energy/water, other services, health care, education, banking/insurance, real estate, transportation, news and communication, trade, repairs, food/tobacco, textiles, wood/furniture, paper/paper products, leather, chemical, non-ferrous minerals, machinery and equipment, electrical machinery/optics, watches/jewelry, other manufacturing) | | | | | | | | |
| Construction | −0.16* | 0 | 0 | 0 | −0.31* | 0 | −0.36* | 0 |
| Public services | 0 | 0 | 0 | 0 | 0 | 0 | 0 | −0.33* |
| Consulting | 0 | 0 | 0.32* | 0 | −0.17** | 0 | 0 | 0 |
| Restaurants, catering | 0 | 0 | 0 | 0 | 0 | 0 | −0.37* | 0 |
| Printing | 0 | 0 | 0.73* | 0 | 0 | 0 | 0 | 0 |
| Metals | 0 | 0 | 0 | 0.86** | 0 | 0 | 0 | 0 |
| Industry unemployment rate in %, 1/98 | −0.08* | −0.06 | −0.32* | −0.68** | 0.03 | −0.18* | 0.15 | 0.05 |
| Job position function (reference category: assistant) | | | | | | | | |
| Self-employed | 0 | 0 | 0 | 0 | 0 | 0 | 0 | −0.61* |
| High (management, etc.) | 0 | 0 | 0.28* | 0 | −0.24** | −0.41* | −0.45* | 0 |
| Medium | 0 | 0 | 0.40* | 0 | 0 | 0 | −0.13* | 0 |
| Previous occupation (reference categories: mining, wood and paper, chemical, minerals, artist) | | | | | | | | |
| Agriculture | 0 | −0.18 | −0.79** | −2.19* | 0 | 0 | 0 | 0 |
| Food, tobacco | 0 | −0.46** | 0 | 0 | 0 | 0 | 0 | 0 |
| Textiles | 0 | 0.40* | 0 | 0 | 0 | 0 | 0 | 0 |
| Metals | 0 | −0.3*1 | −0.20 | 0.72* | 0 | −0.29* | 0 | 0 |

Table B1
*Continued*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Health care | 0 | 0 | 0 | 0 | 0.45* | 0 | 0 | 0 |
| Architecture, engineer | 0 | 0.32** | 0.84* | 1.74** | 0 | 0 | 0 | 0 |
| Construction | −0.09 | −0.24* | −0.72* | 0 | 0.00 | 0 | −0.03 | 0 |
| Transportation | 0 | −0.59* | −0.36 | −0.79 | 0 | 0 | 0 | 0 |
| Restaurants | 0 | −0.12** | 0 | 0 | 0.42* | 0 | 0 | 0 |
| Printing | 0 | −0.84* | 0 | 0 | 0 | 0 | 0 | −0.57* |
| Entrepreneurs, senior officials, justice | 0 | 0 | 0 | 0 | 0 | −0.36* | −0.38* | −0.76* |
| Painting, technical drawing | 0 | 0 | 0 | 0.75** | 0 | 0 | 0 | 0.20* |
| Office and computer | 0 | 0.22* | 0.50* | 0.82** | 0.31* | 0 | 0 | −0.25* |
| Retail trade | 0.15** | 0 | 0.36* | 1.10** | 0 | −0.19* | −0.17** | −0.33* |
| Security, cleaning, clerical, social work | 0 | 0 | −0.63* | 0 | 0 | 0 | 0 | 0 |
| Science | −0.40* | 0 | 0 | 0 | 0 | 0 | 0 | −0.49* |
| Education | 0 | 0 | −0.84* | 0 | 0 | 0 | 0 | 0 |
| News and communication | 0 | 0 | 0.87* | 0 | 0 | 0 | 0 | 0 |
| Body care | 0 | −1.55* | −0.99** | 0 | 0 | −1.16* | −0.96* | 0 |
| Other | 0.15* | 0 | 0 | 0 | 0 | 0 | 0.21* | 0 |
| Desired = previous job, 3-digit | 0 | 0 | −0.14** | −0.33* | −0.14* | 0 | 0 | 0 |
| Additional regional effects by canton | | | | | | | | |
| Berne | −0.49* | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Lucerne | 0 | 0 | 0 | 0 | −1.08* | 0 | 0 | 0 |
| Schwyz | 0.99* | 0 | 0 | 0 | −1.08* | 0.70* | 0 | 0 |
| Glarus | 0 | 0 | 0 | 0 | 1.52* | 0 | 0 | 0 |
| Zug | −1.55* | 0 | 0 | 0 | −1.99* | 0 | 0 | 0 |
| Freiburg | 0.51* | 0 | 0 | 0 | 0 | 0.24** | 0 | 0 |
| Solothurn | 0 | 0 | 0 | 0 | 0 | −0.91* | 0.41* | 0 |
| Basel-City | −0.51* | −0.28* | 0 | 0 | 0 | 0 | −0.56* | −0.23* |
| St Gall | 0 | 0 | 0 | 0 | 0 | −0.61* | −0.87* | 0 |
| Graubünden | 0 | 0 | 0.87** | 0 | 0 | −1.45* | 0 | −0.46* |
| Aargau | −0.25** | 0.55* | −0.90* | 0 | 0 | −0.41* | 0 | −0.20* |

Table B1
*Continued*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Thurgau | 0 | 0 | 0.73** | 0 | 0 | 0.43* | 0 | 0 |
| Ticino | 0.25 | −0.44** | −1.45* | −2.17** | 0.09 | −0.28* | 1.65* | −0.08 |
| Waadt | 0 | −0.53* | 0 | 0 | 0 | −0.66* | −1.00* | −0.51* |
| Neuenburg | −0.79* | −1.15* | −1.01* | −1.86** | 0 | 0 | 0 | −0.50* |
| Geneva | −1.20* | −0.41* | −0.25* | −0.70** | 0 | −1.47* | −1.83* | −0.68* |
| Jura | −0.59** | −0.64* | 0 | 3.67** | 0 | 0 | 0 | −0.75* |
| Cantonal unemployment rate | −0.28* | −0.01 | 0.28* | 0.38* | −0.06 | −0.08** | −0.28* | −0.02 |
| Region (reference category: Zurich) | | | | | | | | |
| Eastern | 0.09 | 0.31* | 0.28* | 0.14 | 0.03 | 0.09 | 0.64* | 0.38* |
| Central | 0.19* | 0.41* | 0.88* | 0.61* | 1.76* | 0.20** | 0.81* | 0.10 |
| South-west | 0.96* | −0.44* | −0.99* | −0.86* | 0.24* | 0.57* | 2.14* | 0.60* |
| North-west | 0.40* | −0.12 | 0.48** | −0.04 | −0.02 | −0.37* | 0.94* | 0.29** |
| West | 0.69* | 0.01 | 0.03 | −0.57 | −0.11 | 0.37* | 1.01* | 0.34* |
| Size of town where worked before (reference categories: $< 100,000$, $< 50,000$, $< 20,000$, $< 10,000$) | | | | | | | | |
| $> 200,000$ | 0.28* | 0 | −0.64* | 0 | 0.37* | 0 | 0 | 0 |
| $< 30,000$ | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.13* |
| $< 5,000$ | 0 | 0 | 0 | 0 | 0 | 0.10* | 0 | 0 |
| $< 2,000$ | −0.09* | 0 | −0.18* | 0 | 0 | 0 | 0 | 0 |
| Region of placement office (reference categories: small city, no information) | | | | | | | | |
| Large city | 0 | 0 | 0 | 0 | −0.31* | 0 | −0.16** | 0 |
| Rural | 0 | 0 | −0.42* | 0 | 0 | 0 | 0 | 0 |
| Long-term unemployment in regional placement office | | | | | | | | |
| Inflow to long-term unemployment | 0 | 0 | 4.29** | 0 | 0 | 0 | 2.65* | 0 |
| Outflow from long-term unemployment | 0 | 0 | 4.81* | 0 | 0 | 0 | 3.93* | 0 |
| No information | 0 | 0 | 1.59* | 0 | 0 | 0 | 0.99* | 0 |

Table B1

*Continued*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Sanction days without benefit payment | | | | | | | | |
| Number of sanction days during last unemployment spell | 0 | −0.07* | 0 | 0 | 0 | 0 | 0 | 0 |
| Positive number of sanction days (in %) | 0 | 0.07 | −0.24* | 0 | 0 | 0 | 0 | −0.11* |
| Unemployment history | | | | | | | | |
| First spell | 0.14* | 0 | 0.23* | 0 | 0 | 0.11* | 0.17* | 0 |
| Number of spells prior to current spell | 0 | −0.17* | 0 | 0 | 0 | 0 | 0 | 0 |
| Previous programme participation | | | | | | | | |
| Sum of short programmes between July and December 1997 | 0.15** | 0 | 0.42* | 0.72** | 0.26* | 0 | 0.12** | 0.08 |
| Employment programme before July 97 | 0 | 0 | 0 | 0 | 0 | 0.44* | 0 | 0 |
| Temporary wage subsidy before July 97 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.53* |
| Begin of programme/100 | 0.22* | 0.05** | 0.12** | −0.00 | 0.20* | 0.36* | 0.38* | 0.37* |
| Duration of unemployment spell at beginning of programme | | | | | | | | |
| Duration (days) | −0.02 | −1.20* | −1.40** | −1.00 | −0.19 | −0.88* | −0.56** | −2.88* |
| Less than 90 days | 0.03 | −0.15* | −0.33* | −0.46* | −0.27* | −0.47* | −0.28* | −0.14** |
| Less than 180 days | 0.11* | 0 | 0 | 0 | −0.15** | −0.27* | −0.39* | −0.17** |
| Less than 270 days | 0 | 0 | 0 | 0 | 0.15* | −0.15** | 0 | −0.18* |
| Less than 365 days | 0.21* | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Remaining days of 'passive regime' on 31.12.97 | 0 | 0 | 0.30** | 0 | 0 | 0 | 0 | −0.12* |

Table B1
*Continued*

| Variable | Basic courses | Language courses | Computer courses | Vocat. training | Other training | Employment programme: Public | Employment programme: Private | Temporary wage subsidy |
|---|---|---|---|---|---|---|---|---|
| Employment history from social security data (monthly, 1988 to 1997) | | | | | | | | |
| Never unemployed | 0 | 0 | 0.34* | 0 | 0 | 0 | 0 | 0 |
| Month of entry into social security system | 0 | 0.84* | 0 | 0 | 0.29* | 0 | 0 | 0 |
| Mean duration of employ. spell in months | 0 | 0.16** | 0 | 0 | 0 | 0 | −0.19* | 0 |
| Mean duration of unemploy. spell in months | 0 | 2.14* | 4.28* | 0 | 0 | 0 | 0 | 0 |
| Standard deviation of wages/1000 | 0.15* | −0.15* | −0.14** | −0.07 | −0.19* | −0.19* | −0.25* | −0.13* |
| Proportion of time unemployed, in % | −0.13 | −1.62* | −1.63** | −1.51* | −0.58** | 0.45** | 0.62** | −0.67* |
| Proportion of time employed, in % | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.79* |

*Note*: Simulated maximum likelihood estimates using the GHK simulator (100 draws in simulator for each observation and choice equation). Coefficients of the category *Non-participation* are normalised to zero. All equations include a constant. Inference is based on the outer product of the gradient estimate of the covariance matrix of the coefficients ignoring simulation error. $N = 19,603$. Value of log-likelihood function: −31744.08.
** indicates significance at the 1% level (2-sided test), * the 5% level. If not stated otherwise, all information in the variables relates to the last day in December 1997.

Table B2

*Estimated Covariance and Correlation Matrices of the Error Terms in the Multinomial Probit Model*

| | Non-part. | | Basic courses | | Language courses | | Computer courses | | Vocat. Training | | Other training | | Employment programmes | | | | Temporary wage subsidy | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | Public | | Private | | | |
| | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val | Coef | t-val |
| Covariance matrix* | | | | | | | | | | | | | | | | | | |
| *NONP* | 1 | | 0 | – | 0 | – | 0 | – | 0 | – | 0 | – | 0 | – | 0 | – | 0 | – |
| *BAC* | | | 1 | | −0.19 | 0.17 | −0.78 | 0.58 | −0.27 | 1.63 | 0 | – | 0 | – | 0 | – | 0 | – |
| *LAC* | | | | | 1.04 | | −1.61 | 0.64 | −0.50 | 1.23 | 0 | – | 0 | – | 0 | – | 0 | – |
| *COC* | | | | | | | 4.71 | | −1.44 | 1.59 | 0 | – | 0 | – | 0 | – | 0 | – |
| *FVT* | | | | | | | | | 8.24 | | 0 | – | 0 | – | 0 | – | 0 | – |
| *OTC* | | | | | | | | | | | 1 | | 0 | – | 0 | – | 0 | – |
| *TE-PU* | | | | | | | | | | | | | 1 | | 0.53 | 0.22 | 0.04 | 0.24 |
| *TE-PR* | | | | | | | | | | | | | | | 1.28 | | −0.29 | 0.25 |
| *TEMP* | | | | | | | | | | | | | | | | | 2.19 | 1.85 |
| Correlation matrix* × 100 | | | | | | | | | | | | | | | | | | |
| *NONP* | 100 | | 0 | | 0 | | 0 | | 0 | | 0 | | 0 | | 0 | | 0 | |
| *BAC* | | | 100 | | −19 | | −36 | | −9.6 | | 0 | | 0 | | 0 | | 0 | |
| *LAC* | | | | | 100 | | −73 | | −17 | | 0 | | 0 | | 0 | | 0 | |
| *COC* | | | | | | | 100 | | −23 | | 0 | | 0 | | 0 | | 0 | |
| *FVT* | | | | | | | | | 100 | | 0 | | 0 | | 0 | | 0 | |
| *OTC* | | | | | | | | | | | 100 | | 0 | | 0 | | 0 | |
| *TE-PU* | | | | | | | | | | | | | 100 | | 47 | | 2.9 | |
| *TE-PR* | | | | | | | | | | | | | | | 100 | | −17 | |
| *TEMP* | | | | | | | | | | | | | | | | | 100 | |

*Note*: * 10 Cholesky factors are estimated to ensure that the covariance of the errors remains positive definite. t-values refer to the test whether the corresponding Cholesky factor is zero (off-diagonal) or one (main-diagonal).

Table B3

*Correlations of Predicted Probabilities*

| | | | | | | | Employment programme | | |
|---|---|---|---|---|---|---|---|---|---|
| | Non-part. | Basic courses | Language course | Computer course | Vocat. training | Other training | Public | Private | Temporary wage subsidy |
| *NONP* | 1 | −0.33 | −0.21 | −0.10 | −0.09 | −0.05 | −0.28 | −0.31 | −0.32 |
| *BAC* | | 1 | 0.03 | 0.07 | 0.03 | −0.03 | 0.02 | 0.00 | −0.15 |
| *LAC* | | | 1 | −0.26 | −0.17 | −0.02 | −0.23 | −0.16 | −0.31 |
| *COC* | | | | 1 | 0.39 | −0.07 | −0.13 | −0.29 | −0.13 |
| *FVT* | | | | | 1 | −0.09 | −0.03 | −0.13 | −0.12 |
| *OTC* | | | | | | 1 | −0.03 | 0.02 | −0.19 |
| *TE-PU* | | | | | | | 1 | 0.20 | −0.04 |
| *TE-PR* | | | | | | | | 1 | −0.05 |
| *TEMP* | | | | | | | | | 1 |

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