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**Does subsidised temporary employment get
the unemployment back to work?
An econometric analysis of two different
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Abstract

Subsidised employment is an important tool of active labour market policies to improve the reemployment chances of the unemployed. Using unusually informative individual data from administrative records we investigate the effects of two different schemes of subsidised temporary employment implemented in Switzerland: non-profit employment programmes (EP) and a subsidy for temporary jobs (TEMP) in private and public firms. Econometric matching methods show that TEMP is more successful than EP in getting the unemployed back to work. Compared to not participating in any programme EP and TEMP are ineffective for unemployed who find jobs easily anyway or have a short unemployment spell. For potential and actual long term unemployed both programmes may have positive effects, but the effect of TEMP is larger.

Keywords

Subsidised temporary job, employment programme, temporary work contracts, active labour market policies, matching on the propensity score, Switzerland

JEL classification: J38, J68

1 Introduction

Subsidised employment is an important tool of labour market policy in many developed countries. It exists not only in countries using the 'European' type of a more interventionist approach to labour market policy (like France, Germany, Sweden, ...), but it is also used by countries firmly based on the Anglo-Saxon model of the labour market, like the USA (Earned Income Tax Credit, EITC), Great Britain (as part of the *new deal*), and Canada (the Targeted Wage Subsidies and the Self-sufficiency Project).

There are considerable differences in the design of the subsidy schemes. The most common form is a wage subsidy with possibly some additional allowance for the employers and / or employees fixed cost. It may be paid either to the employer, the employee, or to both. The subsidy itself may be permanent (conditional on low earnings, like the EITC), or it may have a maximum eligibility period. The programmes may be directed at subsidising strictly temporary employment or just decreasing the initial wage to be paid by the employer for a job that is supposed to become a permanent one. Furthermore, within the subsidy schemes for temporary jobs: It could be a 'real' job in a firm operating in competitive markets, or the subsidised job may be in some specialised non-profit operation active in some sheltered part of the economy. It is this difference that is the main focus of this paper. Finally, the actual direct cost of programme participation to individuals, the unemployment insurance system, and society as a whole may also differ substantially between the different schemes.

Even in the case of subsidies for temporary jobs different ways to implement the subsidy may influence its effects. These types of subsidies offer temporary employment that otherwise would not be accepted by the unemployed or would not be created by firms. The reasons could be the access to unemployment and welfare benefits for the unemployed or minimum employment costs (minimum wages, unionised sector, or other restrictions increasing wage costs) on the side of the employer. So far few theoretical and comparative empirical research has been devoted to analyse differential effects of different employment subsidies.¹ A major reason is probably that usually cross-country studies would be needed to compare the different schemes. However, cross-country studies face the substantial problem of comparing two programmes under potentially very different labour market conditions. Therefore, it would be useful to 'partial out' the effect of local labour market conditions by comparing different programmes within the same country that are accessible to the same group of unemployed. Furthermore, a large and informative data base is necessary to address the selection issues that pop up in every evaluation study. This is a particularly

¹ An exception are experimental studies in the USA which compared wage subsidies paid to the employee and the employer, respectively (Burtless, 1985, Woodbury and Spiegelman, 1987) and two recent studies of the Swedish active labour market policy (Carling and Richardson, 2001, and Sianesi, 2002) that however address somewhat different issues.

demanding task when one concentrates on the more subtle differences between two programmes that may be only small parts of the usually diverse active labour market policy a particular country runs.

Switzerland can be used to study the differences between two versions of employment subsidies that both operate on a larger scale and both are targeted at more or less the same population of unemployed. Both programmes use subsidised temporary employment to increase the reemployment chances of their participants. The crucial difference is that one programme operates as a non-profit employment programme, whereas the other one subsidises temporary jobs in firms operating in a competitive environment. Furthermore, in the Swiss case a large and exceptionally informative individual data base (coming from various administrative registers) is available that was previously used by Gerfin and Lechner (2002, GL hereafter) for a microeconomic evaluation study of several active labour market policies. Although not of primary interest in their study, GL already note a substantial difference between the effects of the two types of temporary employment subsidies. Recent evaluation studies of the Swedish active labour market policies, for example, also draw the general conclusion that programmes most closely attached to a 'real' job in a competitive environment dominate other programmes (Carling and Richardson, 2001, and Sianesi, 2001). The main difference to the Swedish subsidised jobs programme is that subsidised jobs are expected to become permanent in Sweden, while in Switzerland they are expected to be temporary.

Our empirical findings based on matching methods strongly confirm the positive effects of the subsidy for temporary jobs (TEMP) as compared to the employment programme (EP) type of the subsidy on average. We dismiss the concern that the positive effects are due to the participants of TEMP taking up inferior, i.e. jobs paying less than the jobs subsequent to the current unemployment spell. With respect to possible differential effects of the programmes for different groups of unemployed we find both programmes to be more effective in raising reemployment probabilities for the unemployed having substantial difficulties in the labour market. However, even for the 'better risks' TEMP seems to have some, albeit small positive effects. It appears that TEMP and EP are adding human capital, although with different effectiveness. However, we cannot rule out that signalling effects also play a role in explaining our results as well. In terms of direct costs of the programmes TEMP is much less costly than EP (and not participating in any programme), which adds to the positive assessment of TEMP.

In summary, this paper contributes to the literature in several ways: First, it considerably extends the data base used by GL allowing a more detailed analysis of the outcomes achieved by the programmes. The time horizon is extended and other variables measuring the quality of employment are now available. Second, by focussing on two specific and similar types of programmes we are able to analyse their difference in participant selection and the resulting outcomes in a much more profound and informative way. We relate these differences to the different institutional set-ups and discuss theoretical implications. We check

whether the expected effect heterogeneity resulting from these considerations can actually be found in the data.

The structure of the paper is as follows: The following section describes the Swiss programmes in some depth. Section 3 briefly reviews theoretical concepts on why these different types of programmes may have different effects for different individuals. Section 4 as well as Appendix A describe the data and present some descriptive results for the differences between the different groups of participants. Section 5 gives a summary of the econometrics used, which is a multiple treatment evaluation framework using a 'matching on the propensity score' estimator. Section 6 presents the results and Section 7 concludes. Appendix B contains the results of the estimation of the propensity scores in a multinomial probit framework. Appendix C describes the extent of the common support problem and our remedies. Appendix D adds results concerning the subgroup heterogeneity of the effects.

2 Subsidised employment as part of active labour market policies: the Swiss case

As already noted subsidised employment can take many forms. Switzerland uses two different types of subsidies to foster reintegration of the unemployment into the labour market. To understand the effects of the programmes and the composition of the different groups of participants, it is necessary to understand the specifics of the programme as well as the institutional environment in which they operate.

Swiss unemployment insurance

The basic rules of the Swiss unemployment insurance (UI) in the period of interest are as follows: Benefit entitlement lasts for a maximum of two years (conditional on employment history). The entitlement period consists of two parts: in the initial 30 weeks benefits are unconditional on programme participation, the remaining entitlement is in principle conditional on some participation. The benefit level in the two periods is the same. However, in practice these rules are not strictly enforced: It is not unusual to participate in a programme in the first 30 weeks. More frequently, unemployed receive the benefits in the conditional period without any participation in ALMP, because no programme is offered. The entitlement is conditional on a previous contribution to the unemployment insurance for at least 6 months within the past two years. After the two year entitlement period expires, receiving a new entitlement period is conditional on being employed for at least 12 months within three years after the end of the previous unemployment

spell. The replacement ratio is usually 80% of the insured earnings, depending on socio-demographic characteristics.² The maximum monthly benefit is about CHF 7000.

Switzerland runs a substantial and diverse active labour market policy.³ The active labour market programmes (ALMP) in Switzerland can be grouped into three categories: a) training, b) employment programmes, and c) subsidised temporary jobs. Training consists of a wide variety of courses, ranging from basic courses to specific work-related training. The differences between b) and c) are discussed below in detail.

A specificity of the Swiss system of active labour market policy is that the cantons are obliged by law to fill a minimum number of places per year. Until January 2000 the nation-wide minimum was 25'000 yearly places distributed across cantons according to their unemployment rates in the previous year. By comparison, the number of registered job seekers was about 190'000 in 1997 and 140'000 in 1998.

Employment Programmes (EP)

Employment programmes are offered by both public and private institutions. There is no substantial difference between the type of 'jobs' offered by the two different groups of providers of these programmes. The usual individual participation duration is about six months. There are two different types of programmes: either it is a single position, i.e. a special job in a public organisation (e.g. administration or hospitals), or it is a collective programme. Collective programmes are carried out by specialised non-profit organisations. The jobs should be as similar as possible to regular employment, but they should be *extraordinary*, i.e. the organisers of employment programmes should not be in competition with other firms. However, in practice some organising firms may operate on the same market as other private firms with comparable products (e.g. in the repair and restoration sector). Collective employment programmes are regulated by the cantonal unemployment offices in consultation with the employer and the employee organisations.⁴ In conclusion, employment programmes can be seen as fully subsidised labour in a non-profit organisation. In most cases the subsidy even exceeds 100%, because some of the costs of capital, overhead costs, and so on may be reimbursed as well.

Unemployed are placed in employment programmes by the labour office. Participation is compulsory. Interviews we conducted at the placement offices strongly suggest that it is not unusual that case workers use employment programmes as a test for the willingness to work. While participating in an EP the unemployed has to continue job search and must accept any *suitable* job offer (a job would not be considered *suitable* if it pays less than current unemployment benefits, the working conditions are unacceptable, or if

² The replacement ratio is reduced to 70% if the unemployed does not have dependent family members to support.

³ More details can be found in GL and in Lalive, van Ours, and Zweimüller (2000).

the workplace is too far away from home). Formally, the organiser of the employment programme acts as the employer and the participant as an employee (but the organiser cannot “hire” the employees, they are selected by the placement office). Duration of the programme (usually 6 months), the wage and the social security contributions are regulated in a temporary work contract between the organiser and the worker. In particular, the organiser has to send a monthly payroll account to both the employee and the placement office. The participant is paid by the placement office. The wage has to be no less than the minimum wage set for the region and sector (if there is a collective wage agreement). It may exceed the level of the unemployment benefits, but in practice this is rather an exception. For the placement office there are no direct savings by placing an unemployed into EP. In 1998, roughly 17'000 persons participated in an employment programme (about 10% of the registered job seekers).

Swiss employment programmes are pretty similar to employment programmes in Germany. Other similar programmes are also operated in several other European countries. As in the Swiss case these types of programmes – if used at all – are usually an important part of the active labour market policy of that specific country.

Subsidised Temporary jobs (TEMP)

The immediate objective of the subsidised temporary jobs programme is to encourage job seekers to accept job offers for “unsuitable” jobs (they pay less than their unemployment benefits) by overcompensating the difference with additional payments from the UI system. The income generated by this scheme is larger than unemployment benefits in case of not accepting the temporary job.⁵ Thus this programme is financially attractive for both the unemployed and the placement office. If the accumulated duration of temporary jobs within the entitlement period exceeds 12 months the unemployed becomes eligible for another 2-year entitlement period. However, insured earnings (to which the replacement ratio is applied to) are related to the wage earned in the temporary job which is below 80% of previous insured earnings (thus combining many such spells of TEMP would lead to a consistent decline in income). Mean duration of these temporary jobs is roughly 4 months, but there is considerable variation. The wording of the law regulating TEMP is not very specific. Rehiring laid-off workers in TEMP jobs by the same firm is usually not possible. But using TEMP as a subsidised screening device for firms is not ruled out and obviously

⁴ This so-called “three party commission” has the authority to decide whether an employment programme should be considered to be in competition with the private sector. It acts upon complaints by the private sector.

⁵ 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 benefits.

sometimes endorsed by the placement offices in order to improve job matches. However, TEMP jobs are not explicitly expected to become permanent after the subsidy runs out.⁶

Although TEMP is not part of the ALMP, roughly 20% (1998) of the unemployed participated at some point in TEMP. Bauer, Baumann, and Künzi (1999) report that only about 20% of the jobs in TEMP are arranged by the placement office. Employer and employee have a regular temporary work contract defining the conditions of the job (mainly duration of the contract, wage and contributions to future pensions). The wage cannot be below the above mentioned minimum wage. Since the wage has to be less than 80% of previous earnings (the unemployment benefit) to be eligible for a subsidised temporary job, many jobs in TEMP are below the qualification level of the unemployed.

OECD (1996) states concerns that TEMP may lead to distortions in the labour market if it is not tightly monitored. For example, workers might be laid-off and recalled in the TEMP scheme. Furthermore, firms might use TEMP to avoid the dismissal protection rules to increase the flexibility of their work force, or TEMP might be used to avoid the wage levels set in collective wage bargaining agreements. However, so far there appears to be no evidence of abuse of TEMP in these respects.

An important feature of subsidised temporary jobs compared to EP is that TEMP is not part of the official ALMP (probably for historical reasons). Thus, places provided by TEMP are not counted towards the minimum of ALMP places to be filled per canton. It is also important to recognise that the main difference between TEMP and EP is the type of job and work experience they generate. Ignoring any potential market distortions and assuming that EP does not produce public goods to a considerable extent, then from the point of view of the taxpayer EP is more expensive than TEMP. An interesting question we look at with our data is whether these programmes are systematically used by case workers in the labour office for different groups of people (case workers fully control access to EP, but only approve of participation in TEMP).

Arrangements subsidising jobs within firms competing in the market that are not expected to become permanent are not commonly used in European active labour market policies. One programme that is similar to TEMP is the Targeted Wage Subsidies scheme introduced 1996 in Canada. It is an employer based subsidy. A maximum of 60% of the wage is paid up to 78 weeks. Similar to the Swiss case the main goal of this programme is to offer work experience, not necessarily continuing employment, to the unemployed.

⁶ The original intention of policymakers was twofold: on the one hand there was the belief that working is better than not working, hence the provision of temporary jobs for the unemployed. On the other hand, the intention was to provide firms with a flexible workforce for temporary jobs, for which otherwise no suitable labour supply is available.

3 Why and for whom should these programmes work?

The main purpose of this paper is to answer the question whether and why the subsidised temporary job programme could be superior to the employment programme as indicated by previous results. In the following we discuss three main reasons why the different programmes may have different effects: a) human capital, b) signalling and stigma, c) improved job matching.

Human capital

Both programmes do not incorporate explicit training, except for on-the-job training. Bell, Blundell and van Reenen (1999) show that the only way that a temporary subsidy can have a permanent effect on the employability of low-skilled unemployed is to raise their productivity through work experience in the programme. It is possible that both programmes do have this effect on productivity. However, given the institutional differences it is possible that employment programmes generate human capital that is less valued by potential employers due to the requirement that these jobs have to be “extraordinary” and not in competition with “real” jobs. Furthermore, being a real job TEMP may have stronger effects on “soft” human capital such as important contacts and references, which can be very helpful in finding permanent jobs. This effect will be especially strong when the potential permanent job is in the same sector as the subsidised temporary job.

Signalling

Subsidised temporary jobs are often below the qualification of the unemployed (they usually pay less than unemployment benefits which are only 80% of previous earnings). Hence, it may be argued that human capital effects cannot be strong. However, it is possible that the programmes have a signalling value to employers. Because the subsidised temporary jobs are “real” jobs the employer may use this information to conclude that participants in TEMP are better in the sense of having a closer attachment to the labour force. The signal is especially valuable when the potential permanent job is in the same sector as the subsidised temporary job. In order to be a credible signal temporary subsidised jobs must be more costly to find for less productive workers. Since these jobs are limited and usually arranged by the unemployed themselves, hence requiring additional search efforts, this requirement appears to be fulfilled.

Signalling may also occur in terms of stigma effects. Suppose employment programmes are stigmatised in the sense that there is a common belief among employers that participants in employment programmes are on average less productive than their counterparts in subsidised temporary jobs. If the unemployed know this, more productive unemployed self-select themselves into the temporary subsidised jobs programme. There is anecdotal evidence that employment programmes indeed carry the described stigma. Given this,

all unemployed would want to participate in TEMP, but again finding a subsidised temporary job is more costly for less productive unemployed.

Improved job matching

Firms may use TEMP as a subsidised screening device. Hiring new workers is costly and involves uncertainty about the quality of the applicants, especially when they come from unemployment. TEMP reduces these costs and uncertainties. Similarly, the unemployed may use TEMP to gain knowledge about potential new employers or even new occupations. Taken together, these strategies may improve the matching process on the low-skill labour market. It is unlikely that EP will have a similar effect.

Discussion of the resulting effect heterogeneity

It is not possible to derive strict tests for the relative importance of these explanations. Nevertheless, we can think of hypotheses about effect heterogeneity for different groups of unemployed that are plausible under some explanations and not plausible under others. Examining the empirical evidence for these hypotheses may indicate some answers to the question why programmes have different effects. Our strategy is to use nonparticipation in any programme as a benchmark because nonparticipation will have neither human capital nor lock-in effects. Different effects with respect to nonparticipation for different groups of unemployed may give some indication on why and for whom the two programmes TEMP and EP work.

Assume for the sake of the following arguments that if human capital is generated by one of the programmes it is by replacing already lost or preserving human capital due to ongoing unemployment. Consider the expected effects of employment programmes and subsidised temporary employment compared to nonparticipation for unemployed with a short unemployment spell. For this group, we expect the human capital effects of the programme to be negligible.⁷ On the other hand, lock-in effects are particularly strong for this group because at the beginning of the unemployment spell the job offer arrival rate is relatively high. Hence, if a programme has a positive effect with respect to nonparticipation it should be primarily due to a signal. On the other hand, for the long term unemployed we expect human capital effects and much weaker lock-in effects. Signalling may be important as well but it is not possible to disentangle the reasons for the estimated effects.

Next we consider effect heterogeneity with respect to the skill level of the unemployed. This is the case where the signalling models of McCormick (1990) and Ma and Weiss (1993) are most appropriate in our setting. Ma and Weiss (1993) show that in case of job loss it may be better to become unemployed than to take up a low-skill job. Taking up a low-skill (“lousy”) job may be seen as a bad signal by future employ-

ers. A similar argument is made in McCormick (1990). Most subsidised temporary jobs and temporary employment are below the qualification of the unemployed. In this case the theoretical models imply that for the better qualified unemployed it is optimal not to take up such a job because it would be a negative signal to do so. Hence we should find negative effects with respect to nonparticipation when signalling is important. For those with relatively low earnings (and presumably productivity), on the other hand, this negative signalling effect should not be important. Any effects we find could be due to human capital or signalling, where the signalling effect is different from the one above. It may be positive for TEMP (the unemployed shows motivation) and negative for EP (stigma). A similar argument applies to qualification measured by the case worker's evaluation of the chances to find a job.

To understand which group of unemployed should be expected in which programme, it is instructive to compare the different incentive structures generated by the two programmes for the direct actors, namely the unemployed as well as the local placement office. From the point of view of the latter it is obvious that subsidised temporary jobs are attractive. The direct costs are lower and they do not require assignment efforts as they are in many cases found by the unemployed. The case workers basic strategy appears to be to wait and see whether the unemployed finds a regular job quickly. If the unemployed finds neither a regular nor a subsidised temporary job the case worker tries to find a suitable programme. Again, our informal interviews suggest that the unemployed are sent to employment programmes when nothing else seems to be appropriate. As already mentioned, sometimes employment programmes are also used as a test for the willingness to work. This behaviour is indicative of a rather bad reputation the employment programmes may have with potential employers. Another reason to send unemployed to employment programmes is the requirement that each canton has to fulfil its quota of programme places (c.f. Section 2).

For the unemployed the situation is more complicated. The above considerations suggest the following pattern: at the beginning of the unemployment spell it is not optimal to do low skill jobs while looking for an adequate job, especially for better qualified unemployed. In addition, an indirect effect of participating in a programme could be a reduction in job search activities and job offers from the placement office compared to nonparticipants. The unemployed with good chances to find a job will want to avoid this. After some time in unemployment, however, it can become optimal to search for a temporary job. However, the fact that the majority of subsidised temporary jobs is arranged by the unemployed herself suggests that a search effort is needed to get into this programme. This in turn implies that it is costly for the unemployed to find these programmes. Both human capital and the signalling explanations of the effects imply that this cost is only taken when the expected return is higher. Another incentive to enter the temporary

⁷ It is well known from the research on duration dependence and hysteresis that one effect of ongoing unemployment is an increasing depreciation of human capital.

subsidised job programme is job shopping of the unemployed. In other words, the unemployed uses the subsidised jobs to improve his chances of a good job match.

How will these considerations affect the composition of the participants in both programmes? In fact, it seems that nobody has an incentive to get into an employment programme (except for case workers in order to fulfil their quota). The described strategy of the case workers suggests that participants in employment programmes have a relatively long unemployment duration when they enter the programme. Unemployed with sanctions regarding their benefit already imposed in the past may also end up in employment programmes, given that these are sometimes used as a test for the willingness to work. Furthermore, we would expect the unemployed with low skills and low chances to find a job to be overrepresented in the employment programme because it is difficult for them to find subsidised temporary jobs.

4 Data and descriptive statistics

4.1 Data base

Our empirical analysis is based on two matched sources of administrative data that have already been used by GL. By the usual international standards for observational evaluation studies, this data set is exceptionally informative. The first data source is the information system for placement and labour market statistics (AVAM) and the unemployment offices payment systems (ASAL). We have data from January 1996 to December 1999 for everybody who is registered as unemployed on December 31, 1997. These data provide detailed information about the unemployment history, ALMP participation and personal characteristics. For a random subsample of about 30'000 observations we obtained the social security records for the period 1988-1999. The merged sample contains information on the individual labour market histories and earnings on a monthly basis for 10 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 the 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. Particularly the subjective valuations of the placement officers and the benefit sanctions can be informative since they capture characteristics like motivation and personal appearance that are usually unobservable.

Compared to GL there are important extensions to the data. We now have social security data for the years 1998 and 1999 which allows us to construct additional and more precise outcome variables for employment and earnings on a monthly basis. In GL the most important outcome variable used to measure the ef-

fects of the programme was *leaving unemployment towards employment* as recorded in the unemployment register. Now, we measure employment by the entries in the social security data. Hence, we construct variables measuring the quality of employment in terms of earnings and to some extent job duration. This allows us to address the question whether specific programmes lead to types of employment that may be of lower "quality" than the job prior to the current unemployment spell. Furthermore, we evaluate the effects on earnings per se. Given the new data we evaluate the effects up to 24 months after the programmes start. More details on the data can be found in Appendix A.

4.2 The definition of programmes used in the empirical analysis

We differentiate four groups of ALMP participation states to which we allocate all observations. Since we are not interested in courses per se we aggregated the 16 different training courses into one broad group. Employment programmes are not differentiated according to whether they are offered by public or private institutions as in GL, because our earlier study found no systematic differences of the effects of these two similar forms of employment programmes. The third programme category covers participants in subsidised temporary jobs, and the final (comparison) group consists of those who did not participate in any major programme between January and December 1998.⁸ A major programme is defined as having a duration of at least two weeks. Following the arguments in GL we evaluate only the *first* major programme starting between January and December 1998 (see that paper for details).⁹

For the group of nonparticipants 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 (2002b): For each nonparticipant a hypothetical programme starting date is predicted by relevant information available in Dec, 1997. Persons with predicted starting date later than their actual exit date from unemployment are excluded from the data set.

4.3 The sample

We apply a series of sample selection rules to the data. Full details are given in Appendix A.1. The most important selection criteria are that we consider only individuals unemployed (without any other part-time job) on Dec 31, 1997 with an unemployment spell at that time of less than 12 months who have not participated in any major programme in 1997 and who are 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 suffi-

⁸ The reason not to consider programmes starting before 1998 is that the data does not contain sufficient information on the type and the duration of programmes prior to 1998. Comprehensive coverage of labour market programmes in the official statistics was only introduced in 1998.

⁹ 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 programs are of the same type as the first programme.

cient time left to participate in a programme after December 31, 1997. Furthermore, since our focus is on the first programme we exclude those who participated in a major programme before 1998. In addition given the variety of options for the young (schooling) and the older unemployed (early retirement) we exclude them from our analysis. The final data set has 18'354 observations. For detailed descriptive statistics the interested reader is referred to Table A.3 in Appendix A.¹⁰

4.4 Descriptive comparison of programme groups

Table 1 shows selected descriptive statistics to compare participant groups in the different programmes. Our main interest is the comparison of employment programmes and subsidised temporary jobs; the selected variables show significant differences between the two groups of participants. Our previous arguments in Section 3 were based on two central attributes: skill level and unemployment duration. The participants in employment programmes are clearly the least skilled, measured by the chances to find a job, qualification, job position, and previous earnings. For the other two groups there are hardly any differences, with the exception of the chances to find a job which are favourable for the participants of the subsidised temporary job programme. Unemployment spell duration at the time of programme start is almost three months larger for participants in employment programmes reflecting case workers' tendency to send unemployed to these programmes when no other programme seems to be adequate. Hence, the numbers support our expectations derived from the above discussions: we find significantly more low skilled unemployed in employment programmes, and subsidised temporary jobs take place earlier in the unemployment spell. This is in accordance with the hypothesis that there is a limited supply of temporary jobs. The low skilled may have problems finding them and after some time the case workers allocate them to employment programmes. Furthermore, there is obvious regional and occupational heterogeneity in the composition of participants in the programmes. The political structure of Switzerland gives the cantons a considerable degree of autonomy. They may put different emphasis on the various programmes in their local implementation of the national ALMP.¹¹

¹⁰ Compared to GL the number of participants in TEMP is larger. This is due to a change in the definition of a major programme in the case of TEMP. In the earlier study the proportion of the time spent in TEMP relative to the month was set to 66% in order to be counted as a month in TEMP; In this study we reduced this threshold to 50%.

¹¹ This is of course only a rough descriptive comparison, not a complete analysis of the participants structure. The results of a multinomial probit for the selection procedure used in a later stage to estimate propensity scores can be found in Appendix B.

Table 1: Number of observations and selected characteristics of different programme groups

Group		Subsidised temporary job (TEMP)	Employment programmes (EP)	Nonparticipation (NONP)
Obs. (persons)		5365	2107	5461
<i>Pre-programme characteristics</i>				
Chances to find a job	good or very good	23	18	22
	difficult or special case (share in %)	13	24	18
Qualification (mean)		1.74	1.87	1.73
Job position	very low	37	47	36
	High	5	3	7
Unemployment duration before programme (mean days)		222	303	218
Female		41	38	43
Nationality	foreign with yearly permit	15	18	16
	foreign with permanent permit	31	29	31
	Swiss	54	53	53
Earnings before unemployment (mean per month in CHF)		3970	3660	3950
Region	Zurich	18	17	22
	West	21	29	16
	Eastern	10	7	9
	Central	5	7	5
	South-west	28	22	30
	North-west	11	9	9
<i>Post-programme outcomes</i>				
Earnings Sept. 1999 (if employed) (mean in CHF)		3672	3279	3702
Employed Sept. 1999 (share in %)		72	58	59

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

Comparing the outcomes of the participants in the programmes we find that earnings in September 1999 are almost identical for nonparticipants and participants in temporary subsidised job, but lower for participants in employment programmes. The employment share is on a similar level for all programmes except for temporary wage subsidies where it is more than ten percentage points higher. Of course, these figures for the outcome variables cannot be interpreted as the causal effects of the programmes.

5 Econometrics

We base our analysis of the prototypical model of the microeconomic evaluation literature with multiple treatments: An individual chooses between several states, like participation in an employment programme or non-participation in such a programme. The potential participant in a programme gets an hypothetical outcome (e.g. earnings) in both states. This model is based on the binary potential outcome model (Roy, 1951, Rubin, 1974) extended by Imbens (2000) and Lechner (2001) to multiple, mutually exclusive states. Here, we consider outcomes of four different states denoted by $\{Y^0, Y^1, Y^2, Y^3\}$. The different states are called *treatments* in the following to stick to the terminology of that literature. For any individ-

ual, only one component of $\{Y^0, Y^1, Y^2, Y^3\}$ is observable. Participation in a particular treatment m is indicated by the realisation of the random variable $S, S \in \{0, 1, 2, 3\}$. This notation allows us under the usual assumptions (see Rubin, 1974) to define average treatment effects for pair-wise comparisons of the effects of different states:

$$\gamma_0^{m,l} = E(Y^m - Y^l) = EY^m - EY^l; \quad (1)$$

$$\theta_0^{m,l} = E(Y^m - Y^l | S = m) = E(Y^m | S = m) - E(Y^l | S = m); \quad m \neq l; m, l \in \{0, 1, 2, 3\}. \quad (2)$$

$\gamma_0^{m,l}$ denotes the expected (average) effect of treatment m relative to treatment l for a participant drawn randomly from the population (average treatment effect, ATE).¹² ATE's are symmetric ($\gamma_0^{m,l} = -\gamma_0^{l,m}$). $\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of participants in treatment m only (ATE on the treated, ATET). ATET's are not symmetric, if 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 .

5.1 Identification

ATE's and ATET's are generally not identified so that additional assumptions are needed. We already noted that our data compiled from different administrative records are so rich that it seems plausible to assume that we observe all important factors that jointly influence labour market outcomes and the process selecting people into the four different states. Therefore, we assume that treatment participation and treatment outcome is independent conditional on a set of (observable) attributes (conditional independence assumption, CIA). CIA defined to be valid in a subspace \mathcal{X} of the attribute space is formalised in expression (3):

$$Y^0, Y^1, \dots, Y^M \perp\!\!\!\perp S | X = x, \forall x \in \mathcal{X}. \quad (3)$$

This assumption requires the researcher to observe all characteristics that jointly influence the outcomes as well as selection into treatments. In addition CIA requires that all individuals that are part of the evaluation could participate in all states (i.e. $0 < P(S = m | X = x), \forall m = 0, \dots, 3, \forall x \in \mathcal{X}$).

Equation (3) postulates that conditional on the observable attributes there remains no systematic selection on unobservables. In other words there are no exogenous variables left out that are both correlated with

potential outcomes and the participation decision. Candidates for such unobservables include variables like motivation, ability, and personal appearance. Our unusually informative data allows us to capture the major effects of these unobservables. For example, motivation can be measured by sanctions imposed by the placement office as well as by the employment history in the past ten years. Unobserved ability is captured by past earnings, and specific labour-related problems can be measured by past employment profiles (repeated movement between labour market states). Of particular importance is the variable “chances to find a job”, which is a subjective judgement by the placement officer. This judgement is based on interviews and the impressions the placement officer obtains in his interviews in the beginning of the unemployment spell. This variable should capture characteristics like motivation and personal appearance that are usually unobservable. After controlling for this wealth of information there should be little unobserved heterogeneity left that is systematically correlated with labour market outcomes and programme participation. For detailed arguments about identification the reader is referred to GL.

5.2 *A matching estimator*

Lechner (2001) shows that CIA identifies all effects defined in this section and that expression (3) implies independence not only conditional on X but also conditional on the marginal probabilities of the states (conditional on X), denoted by $[P^0(X), P^1(X), P^2(X), P^3(X)]$.¹³ Based on this insight, Lechner (2001, 2002a, b) propose and apply different matching estimators for that problem. Here we use the version implemented in the paper by GL (see Table 2 for details).

Several comments are in order: A discussion of the implementation as well as the results of the simulated maximum likelihood estimator of the multinomial probit model used in Step 1 is given in Appendix B. Step 2 ensures that we estimate only effects in regions of the attribute space where two observations from two treatments *could* be observed having a similar participation probability.¹⁴ Otherwise the estimator will give biased results (see Heckman, Ichimura, Smith, Todd, 1998). In total the common support criteria discarded only about 3.5% of the observations (see Appendix C for details).

¹² 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 .

¹³ Depending on the effect to be estimated we need to condition only on a subset or of functions of these probabilities. For all details the reader is referred to Lechner (2001).

¹⁴ This condition is also called the 'common-support requirement'. Note that if we would only be interested in pair-wise effects the current implementation would be unnecessarily strict, since making sure that there is an overlap for each pair would be sufficient. Our implementation has the advantage that we evaluate all programmes on the same support.

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 $[\hat{P}_N^0(x), \hat{P}_N^1(x), \hat{P}_N^3(x), \hat{P}_N^4(x)]$.
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	<p>Estimate the respective (counterfactual) expectations of the outcome variables.</p> <p>For a given value of m and l the following steps are performed:</p> <ol style="list-style-type: none"> Choose one observation in the subsample defined by participation in m and delete it from that pool. 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 $[\hat{P}_N^m(x), \hat{P}_N^l(x)]$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again. Repeat a) and b) until no participant in m is left. 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 estimates of treatment effects using the results of Step 4 by means in matched samples.

Note: Lechner (2001) suggests an estimator of the asymptotic standard errors for $\hat{\gamma}_N^{m,l}$ and $\hat{\theta}_N^{m,l}$ conditional on the estimated probabilities in Step 1.

A third remark about 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 (which means increasing the variance by reducing the bias) 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 is always the case when the number of participants is not equal in all treatments. For the sake of brevity we do not document the matching quality explicitly. Similarly to the already mentioned previous studies this estimator roughly balances the covariates in an appropriate way.

6 Empirical estimates of the effects

6.1 Measurement of the outcomes in the labour market

According to Swiss legislation the primary objective of the active labour market policy is to increase the reemployment probabilities. At least implicitly, the idea is also that the new job should be at least of similar quality as the previous one. We combine the two data sources to develop indicators that proxy these objectives. We compute indicators of successful employment from the social security data by using information whether there are payments from employment that can be related to a particular month. We define an employment spell as successful if it has a duration of at least 3 months. In addition we create an

indicator that measures the quality of employment (continuously employed for at least 3 months with earnings at least 90% of earnings in the previous job). Furthermore, as a crude proxy for individual productivity we include gross earnings coded as zero when an individual is not employed in the regular labour market. We also compute the months of unemployment within the next 12 months to get a measure on how many months of unemployment programme participation may save within a year. A final outcome variable (*seeking a job*) is based on the information whether somebody is registered with the labour office as job seeker. This outcome variable will pick up the institutional feature that subsidised temporary jobs can extend the unemployment benefit eligibility period. Thus the incentive to remain registered with the labour office is larger compared to other programmes, which do not affect the two-year eligibility period.¹⁵ Table A.2 in Appendix A describes the outcome variables more precisely. Table A.3 shows descriptive statistics.

We measure the effects of a programme in the month after the programme started (with simulated beginning dates for nonparticipants, see Section 4.2). In case an individual is known to be informed about programme participation prior to the month of the actual start of the programme we use this month of information as start date, because a programme may have an effect on individual behaviour from the very moment when an individual knows that she will participate. Furthermore, focusing on the begin of the programme rules out that programmes appear to be successful, just because they keep their participants busy by making them stay in the programme. We consider a programme to be most successful if everybody would leave for employment (of 'good' quality) immediately after she is informed about future participation. We must emphasise that whenever a person participates in any of the programmes she is considered as registered unemployed (and not employed) in the definition of all outcome variables.

For programmes starting in January 1998 we measure outcome variables for 23 months (2/1998-12/1999) or 12 months for the accumulated measures. However, since the evaluated programmes may start between Jan. 1998 and Dec. 1998, only 12 months of nonaccumulated outcomes are observable for everybody. Since a large share of the programmes started in the first quarter of 1998, for most observations we measure the effects for at least 18 months. When interpreting the result we should also keep in mind that the economy came out of the 1997 recession fairly quickly in 1998 and particularly in 1999 leading to a drop in the unemployment rate from 5.0% in December 1997 (share of unemployment spells longer than 1 year: 33%) to 2.5% in December 1999 (21%).

¹⁵ It is possible to remain registered when the eligibility period is over, but of course there will be no more unemployment benefits.

6.2 Mean effects of programmes for their participants

Table 3 shows the means of the outcomes in the various groups, the estimated counterfactual expectations and pair-wise comparisons between the subsidy programmes and between the programmes and nonparticipation. For the sake of brevity COURSES are omitted because their effects are not central to this paper. Furthermore, we concentrate on three outcome variables: employment during at least 3 consecutive months generating on average more than 90% of previous monthly earnings, average monthly earnings, and the number of additional unemployment over the next 12 months. For the first two outcomes we present results 3, 9, 15 and 21 months after the begin of the programme, for the third outcome that is based on accumulating effects over the 12 months ahead, we present effects 1 and 6 months after the start. Column (3) and (4) give the exact sample sizes (after imposing common support) available at each point of (process) time. Note that sample sizes fall after month 12 (the last month observed is month 24, programme participation starts between month 1 and 12). Thus, the population changes somewhat after month 12 in the sense that, for example, the estimate for month 23 is entirely based on individuals entering the programme in January 1998. Therefore, the precision of the estimates falls as well, which is reflected in the estimated standard errors increasing after month 12.

Columns (5) and (8) show the mean outcomes for the participants in programme one (5) as well as the mean outcomes for participants in programme zero (8). Column (6) shows the estimated mean counterfactual outcome of treatment one for population zero. Column (7) shows the respective estimated mean counterfactual outcome of treatment zero for population one. In general, all variables are increasing because more and more of the unemployed find jobs the longer the duration of unemployment (and only a few return to unemployment), which is not surprising given the economic upswing during 1998 and 1999. Nevertheless the pattern of the increase varies considerably between different treatments and different populations. The variation in earnings (coded as zero if the individual is not employed in the first labour market) is largely driven by the variation in employment status.

The comparison of column (5) to column (6) and of column (8) to column (7) reveals the magnitude of the selection bias corrected for by the estimation procedure. It is interesting to note that the selection bias would be largest in the comparison of EP and NONP and relatively modest in the comparisons involving TEMP. The comparisons clearly show that the unemployed in EP have the worst labour market perspectives in all potential states. From these estimates the estimated mean effects of two different states for participants in treatment 1 and 0 can be directly computed. These estimates and the corresponding asymptotic standard errors are given in columns (9) to (12). Columns (13) and (14) show the effects for the joint population of participants (TEMP, EP, courses) and the nonparticipants. When bold, effects are significant at the 1% level, when in italics they are significant at the 5% level.

Table 3: Estimates of effects

Out- come	Month after begin	Sample size		$E(Y^1 $	$E(Y^1 $	$E(Y^0 $	$E(Y^0 $	$\hat{\theta}_N^{1,0}$	Std.	$-\hat{\theta}_N^{0,1}$	Std.	$\hat{\rho}_N^{1,0}$	Std.
		1	0	$ S = 1)$	$ S = 0)$	$ S = 1)$	$ S = 0)$		err.		err.		err.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Subsidised temporary job (1) compared to employment programme (0)													
EWEL	3	5182	2085	17	15	9	8	8	1.2	8	1.3	7	1.1
in %	9	5182	2085	29	26	20	18	9	1.7	7	1.6	7	1.5
	15	4846	1889	42	39	33	30	9	2.1	9	1.9	7	1.8
	21	2925	778	45	43	38	34	7	3.2	8	2.9	7	2.6
EARN	3			1154	921	580	415	574	56	506	65	552	52
in	9			1832	1609	1213	1022	618	81	587	78	528	72
CHF	15			2491	2217	2073	1808	418	96	409	89	331	84
	21			2716	2405	2421	2179	295	148	226	135	188	123
UE in	1-12	5182	2085	5.8	6.0	7.4	7.5	-1.6	0.2	-1.5	0.2	-1.7	0.1
months	6-17	4505	1640	3.9	3.9	4.8	4.8	-0.9	0.2	-0.9	0.2	-1.1	0.2
Subsidised temporary job (1) compared to nonparticipation (0)													
EWEL	3	5182	5225	17	15	19	19	-2	1.1	-4	1.1	-2	0.9
in %	9	5182	5225	29	27	27	25	2	1.3	2	1.2	3	1.1
	15	4846	5097	42	40	34	33	8	1.4	7	1.4	8	1.2
	21	2925	3921	45	43	38	35	7	1.7	7	1.6	8	1.5
EARN	3			1154	1048	1244	1163	-90	59	-116	54	-58	49
in	9			1832	1799	1734	1633	98	67	166	61	193	55
CHF	15			2491	2348	2148	2055	343	73	293	66	359	60
	21			2716	2601	2411	2211	305	85	389	79	359	73
UE in	1-12	5182	5173*	5.8	5.9	4.7	5.1	1.1	0.1	0.8	0.1	0.9	0.1
months	6-17	4505	4987	3.9	3.8	2.8	3.0	1.1	0.1	0.8	0.1	0.9	0.1
Employment programme (1) compared to nonparticipation (0)													
EWEL	3	2085	5225	8	8	13	19	-6	1.5	-10	1.4	-9	1.1
in %	9	2085	5225	18	22	20	25	-1	1.7	-4	1.9	-4	1.5
	15	1889	5097	30	32	27	33	3	2.0	-1	2.3	1	1.8
	21	778	3921	34	37	29	35	6	2.7	1	3.0	1	2.6
EARN	3			415	528	796	1163	-381	76	-635	63	-610	54
in	9			1022	1273	1231	1633	-209	91	-359	91	-334	74
CHF	15			1808	2014	1621	2055	187	103	-40	106	28	87
	21			2179	2436	1849	2211	330	137	225	143	170	122
UE in	1-12	2085	5173	7.5	7.7	5.3	5.1	2.2	0.2	2.6	0.2	2.6	0.1
months	6-17	1640	4987	4.8	5.0	3.0	3.0	1.8	0.2	2.0	0.2	2.0	0.2

Note: EWEL: Employed for at least 3 months with average earnings of more than 90% of previous earnings. EARN: Monthly gross earnings in employment with minimum duration of 3 months. UE: Months of registered unemployment in 12 months period. Results are based on matched samples (see Table 3). **Bold** numbers indicate significance at the 1% level (2-sided test), numbers in *italics* indicate significance at the 5% level. Results for COURSES are available on request. * 52 nonparticipants have a simulated starting date of Jan 1999.

The estimated effects show that TEMP is the superior programme. About 15 months after the begin of the programme we find a more or less stable and significant positive employment effect of participating in TEMP of about 7-9% points compared to EP and NONP. There does not appear to be too much variation of this effect between different populations defined by treatment status. Similarly, there is an average

earnings gain after 15 months of about 300-400 CHF. Comparing both programmes to nonparticipation reveals a particular shape: negative effects appear in the beginning that eventually get positive and significant. In the medium run it seems that both programmes increase the employment probabilities for their participants by about 6-7% points. However, even for the participants in EP it would have been more beneficial to enter TEMP instead. This view is confirmed when considering the accumulated effects: Participating in TEMP instead of EP reduces registered unemployment by about one month per year. For reasons already discussed in section 6.1, in comparison to nonparticipation both programmes increase unemployment benefit duration by about 1 (TEMP) to 2 months (EP) per year in the time immediately after the start of the programme.

Before returning to the dynamic shape of the effects in more detail, it is instructive to get an idea about the magnitude of earnings. If we assume that those not working would receive the mean earnings of those working, we are able to compute counterfactual earnings for the employed in all states by dividing the earnings displayed in Table 3 by the employment probability. Earnings computed that way (Table 4) suggest that the effects presented in Table 3 are mainly driven by employment effects. For example, for participants in TEMP the difference between potential earnings in TEMP and in EP is CHF 121 (treatment effect 15 months after programme start). Based on the different definition of the earnings variable (coded 0 for nonemployed), the corresponding treatment effect in Table 3 is 418, because it also includes an effect on employment. However, these numbers have to be interpreted with care because the assumption used to compute them is not very convincing: there may be considerable selection going on (of another type than the one already corrected for) due to different groups of unemployed entering employment at different times for different treatments. Furthermore, the estimates may be unreliable particularly for the smaller samples in the second year because dividing one estimated quantity by another small estimated quantity (between 0 and 1) may result in very imprecise estimates.

Table 4: Average potential earnings for those who would be employed

Potential outcome Population		TEMP TEMP	TEMP EP	TEMP NONP	EP TEMP	EP EP	EP NONP	NONP TEMP	NONP EP	NONP NONP
Outcome	month after begin	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
EARN	3	3596	3320	3453	3472	3069	3579	3707	3385	3738
in	9	3574	3309	3540	3534	3230	3755	3759	3266	3768
CHF	15	3729	3418	3611	3608	3275	3627	3703	3185	3735
	21	3781	3421	3674	3675	3317	3733	3791	3320	3751

Note: Estimated mean earnings divided by estimated employment probability for respective population.

Although Table 3 already indicated the time shape of the effects, the following figures summarise the dynamics of the effects by showing their development over time after the start of the programme on a monthly base (if significant at the 5% level). Note again that the sample sizes decrease after 12 months. The sample is probably large enough to estimate the effects for about 21 months after the start of a programme with sufficient precision.

Figure 1: Dynamics of average effects for participants in TEMP compared to EP, COURSES, and NONP after the start of the programme

Fig. 1a: Employment with duration ≥ 3 months

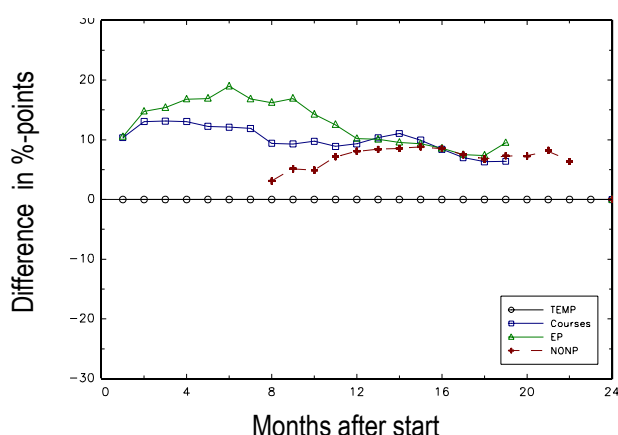


Fig. 1b: Searching for a job

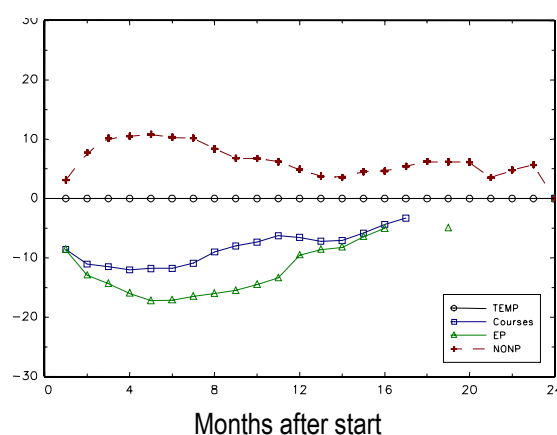


Fig. 1c: Earnings in employment ≥ 3 months

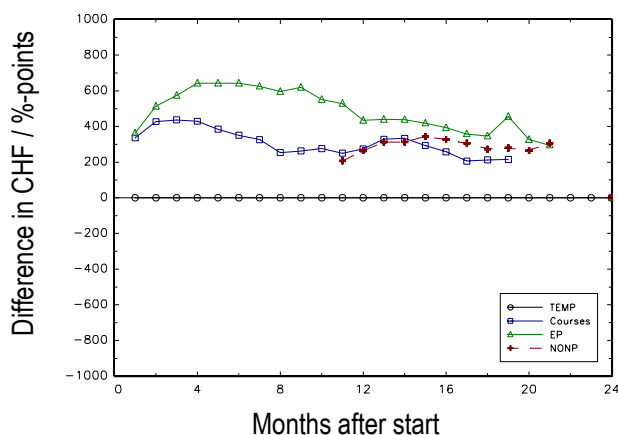
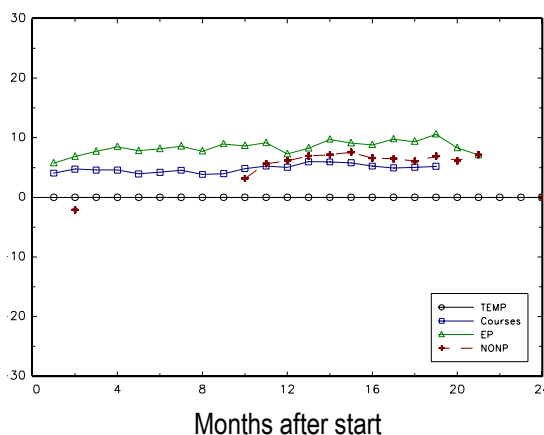


Fig. 1d: Employment without earnings loss



Note: NONP: Nonparticipation; EP Employment programme; TEMP: Subsidised temporary job. Start dates for nonparticipants are simulated. Only estimated effects that are significant at the 5% level (two-sided test) are reported.

Figures 1 and 2 display the estimates of the effects of TEMP (compared to the other states) for participants in TEMP (Figure 1) as well as the effects of EP for the participants in EP (Figure 2) for 4 different outcome variables. A line above zero indicates that TEMP has a positive employment effect, or a positive effect on the probability to be registered as searching, respectively, relative to the programme associated

with that particular line. Only effects significant at the 5% level are displayed. Note that the effects displayed in Figures 1 and 2 are not symmetric because the effects are estimated for different treatment populations.

First consider the effects of TEMP for TEMP participants (Figure 1) for the various outcome variables. The share of unemployed finding a *job with a duration of at least three months* (Fig. 1a) reveals that TEMP dominates the two other programmes. In the beginning the effect with respect to EP is above 15% points but declines subsequently and stabilises around 5 to 7% points after 15 months. This shape may be related to differences in the duration of the programmes. As expected there is no initial significant effect compared to nonparticipation but the effect becomes significantly positive after 9 months. Again the effect seems to stabilise at about 5 to 7% points.

The alternative outcome measure *searching for a job* (and registered with the labour office; Fig. 1b) appears to draw a more negative picture in the sense that relative to nonparticipation TEMP increases the probability of being registered as job seeker. It is particularly worrying that after the initial negative effect due to lock-in, it remains negative throughout. At first sight one might conclude that this finding is caused by the particular feature of the Swiss unemployment insurance system: participation in TEMP increases the entitlement period for unemployment benefits. However, a similar picture can be found in Figure 1b, where we present the effect of EP relative to NONP. From this we conjecture that the entitlement prolongation cannot be the whole story. The most plausible explanation is that nonparticipants have a significantly larger probability of leaving the labour force (see also Table A.3 in Appendix A).

The dynamics of the *earnings* variable more or less mirror the dynamics of the employment variable. The remaining variable combines earnings and employment information. The continuous positive effects of TEMP measured by the indicator *earnings without employment loss* indicates that the quality of jobs generated by TEMP is not worse than for any other programme. Note, however, that the effect with respect to EP initially is much smaller compared to Figure 1a, where we do not take account of previous earnings. If we compare the effects of TEMP with respect to EP in Figures 1a and 1d they appear to be converging. This suggests that part of the early success of TEMP according to Figure 1a could be due to relatively bad and short-lived jobs.

Combined with the result for the outcome *searching for a job* these findings suggest that participants in TEMP fall into two groups: those who find stable employment and those who switch between regular (but short-lived) employment, unemployment and participation in TEMP. For this latter group there is the possibility of a TEMP career, but given that unemployment benefits fall at least every second year this cannot be a sustainable strategy to follow. The data do not allow to analyse this possibility. From a policy perspective it is an important finding that a programme which is subsidising below qualification jobs does not necessarily lead to unemployed subsequently searching and accepting low quality jobs.

Figure 2: Dynamics of average effects for participants in EP compared to TEMP, COURSES, and NONP after the start of the programme

Fig. 2a: Employment with duration ≥ 3 months

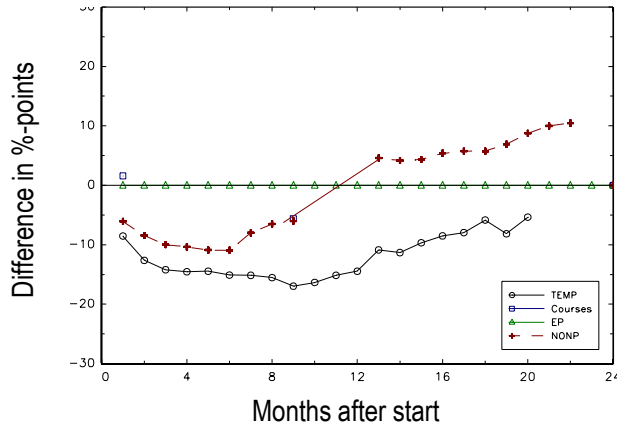


Fig. 2b: Searching for a job

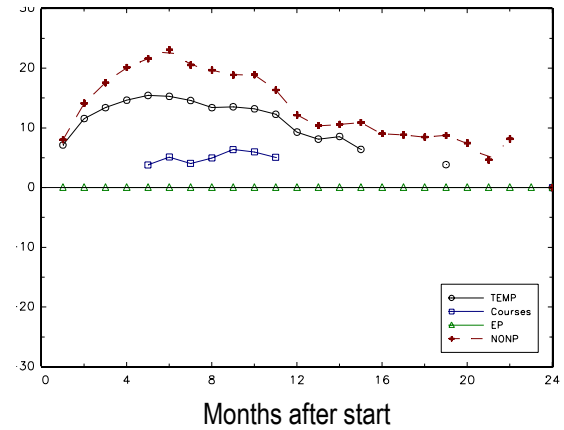


Fig. 2c: Earnings in employment ≥ 3 months

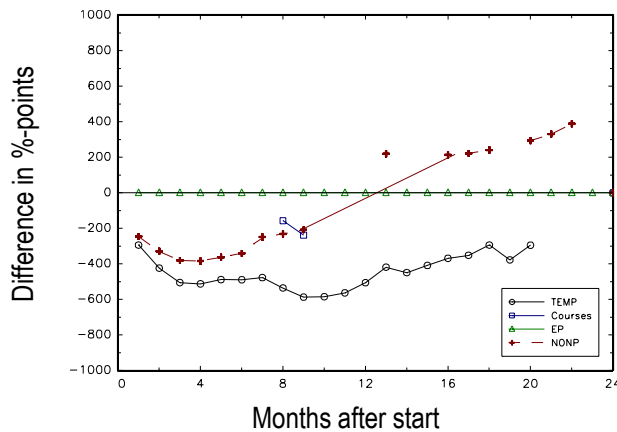
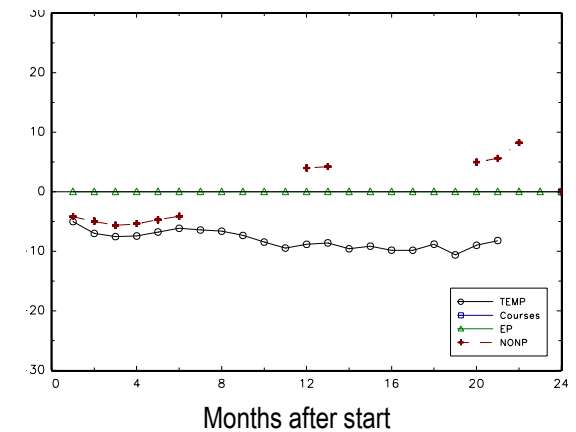


Fig. 2d: Employment without earnings loss



Note: See note below Figure 1.

The previous comparison between EP and TEMP for TEMP participants is confirmed when considering the population of participants in EP (Figure 2). An interesting feature appears with respect to the comparison of EP with NONP. At least in the longer run there is some evidence for a positive effect of EP after about 14 months. Although these effects are not significant throughout in Figure 1d, the fact that some of them exist for all outcome variables (with the exception of *searching for a job*) does indicate that EP may have some positive medium run effect compared to nonparticipation.¹⁶

In summary, the results presented above confirm that on average TEMP is superior to the other programmes as well as to nonparticipation. This was already found by GL to some extent, but they could not take into account any earnings information, and they had to rely on a time horizon of no more than 15

¹⁶ These effects have not been detected by Gerfin and Lechner (2002) because their observation period ended in March 1999.

months. However, this average analysis does not give any answers to the question why TEMP might work better than EP. Therefore, the following section compares effects across subgroups of unemployed.

6.3 *Heterogeneity of the effects*

In Section 3 we developed a number of hypotheses about effect heterogeneity. The empirical evaluation of these hypotheses may indicate some answers to the question why TEMP is superior.

Heterogeneity with respect to unemployment duration

Assuming that any human capital generated by the programmes is a replacement of lost human capital due to ongoing unemployment implies that the effects of both programmes (compared to nonparticipation) should be larger for individuals whose unemployment spell is already fairly long when they enter the programme. Table 5 shows the effects of TEMP and EP for unemployed with less than 180 days of unemployment before entering the programmes (left panel) as well as for unemployed with more than 270 days of unemployment (right panel). The results are compatible with our hypothesis in the sense that the effects of both programmes with respect to nonparticipation are significantly larger for the subpopulation with longer previous unemployment.¹⁷ For this subpopulation EP is clearly superior to NONP 10 months after programme start, and not significantly different in the first 9 months, when the lock-in effect works against EP. This result indicates that for the long-term unemployed doing nothing is not a good strategy. This is strongly confirmed by the large positive effects of TEMP with respect to NONP for this subpopulation.

For the short-term unemployed, we do not expect human capital effects, but relatively strong negative lock-in effects compared to nonparticipation. We only find significant negative effects of EP with respect to NONP in the period between 4 and 9 months after programme start. This result is most likely due to lock-in effects. Given that there are no significant negative effects after 9 months casts some doubts on the stigma story regarding EP. TEMP, on the other hand, even has positive effects after 9 months suggesting that TEMP has positive signalling effects that cancel the negative lock-in effect in the first months after programme start. However, it is also possible that getting a foot back into working life early on may be helpful in finding permanent jobs. Observing a positive effect of TEMP compared to EP is also what would be expected under the signalling and the improved job match explanations, given the assumption that for these unemployed the programmes do not generate human capital. Note that there are significant

¹⁷ All effects presented in this subsection are based on average treatment effects, because the theory suggest that the results should hold for the same populations. Conditioning in addition on treatment status might in fact invalidate the comparisons. Note also that the identity $\gamma^{1,2} = \gamma^{1,0} - \gamma^{2,0}$ must hold.

positive effects after 9 and 15 months, i.e. they should not be due to the different duration of the programmes.

Table 5: Estimates of effects (by unemployment duration) with “employment without earnings loss” as outcome

Month after begin	$E(Y^1 \mid S = 1)$	$E(Y^0 \mid S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 \mid S = 1)$	$E(Y^0 \mid S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$
Short duration (less than 180 days)				Long duration (more than 270 days)		
TEMP (1) compared to EP (0)						
3	15	14	(1)	11	7	3
9	26	16	6	26	18	7
15	39	27	7	35	27	8
21	43	33	(6)	36	32	(3)

TEMP (1) compared to NONP (0)						
3	15	17	(-1)	11	7	3
9	26	22	(2)	26	14	10
15	39	29	6	35	21	15
21	43	31	8	36	23	12
EP (1) compared to NONP (0)						
3	14	17	(-3)	7	7	(0)
9	16	22	-4	18	14	(3)
15	27	29	(-1)	27	21	7
21	33	31	(2)	32	23	9

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes after correcting for common support for UE < 180: TEMP / courses / EP / NONP: 2328 / 2226 / 454 / 2293. Sample sizes UE > 270: 1769 / 1794 / 1150 / 1804.

Heterogeneity with respect to qualification

In Section 3 we argued that (relatively) skilled unemployed should not enter either temporary employment programme because they are overqualified for the typical job in any of these programmes. Hence for these unemployed we should not find positive effects of the programmes with respect to nonparticipation. Probably the best summary measures of human capital available in our data are previous earnings and the chances to find a job. It turns out that there are no systematic differences between the results based on these two measures.

Therefore, we concentrate on skill measured by the chance to find a job which is shown in Table 6.¹⁸ The left panel refers to the unemployed with good a priori chances. The results with respect to nonparticipation support our hypothesis to some extent. There is no systematic effect for either programme (except a large negative early effect for EP, probably due to lock-in). But we do not find a negative effect of TEMP,

¹⁸ The figures for effect heterogeneity by previous earnings are contained in Appendix D.

which would be predicted by the signalling model. Also for this group of unemployed TEMP is clearly superior to EP at most points in time.

For unemployed with bad a priori chances (right panel of Table 6) a completely different picture arises for the comparison of TEMP with nonparticipation. For these unemployed TEMP is better than nonparticipation throughout the evaluation period. On the other hand, EP has a negative effect in the first seven months compared to nonparticipation, which probably is a lock-in effect again, and no significant effect afterwards. This result may be due to the relatively small sample size of EP participants with low employability. When we measure skills by previous earnings we find significant positive effects of EP compared to nonparticipation after eleven months (see Table D.4 in the Appendix). The positive effect of TEMP with respect to EP is of the same magnitude as for the high-skilled group, but significant throughout.

Table 6: Estimates of effects (by employability) with “employment without earnings loss” as outcome

Month after begin	$E(Y^1 \mid S = 1)$	$E(Y^0 \mid S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 \mid S = 1)$	$E(Y^0 \mid S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$
High employability				Low employability		
TEMP (1) compared to EP (0)						
3	19	9	12	13	4	7
9	31	22	7	22	11	8
15	44	35	8	33	21	12
21	47	37	(6)	36	26	10

TEMP (1) compared to NONP (0)						
3	19	24	-4	13	9	(0)
9	31	31	(1)	22	15	5
15	44	41	(4)	33	18	13
21	47	41	(2)	36	21	13

EP (1) compared to NONP (0)						
3	9	24	-16	4	9	-7
9	22	31	-8	11	15	(-3)
15	35	41	(-4)	21	18	(1)
21	37	41	(-4)	26	21	(3)

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes after correcting for common support TEMP / courses / EP / NONP for high employability: 1480 / 1158 / 471 / 1286. Sample sizes TEMP / courses / EP / NONP for low employability: 650 / 898 / 480 / 883.

Overall, the results concerning subgroup heterogeneity mostly correspond to the hypotheses presented in section 3. However, we do not find support for the signalling model of Ma and Weiss (1990) and the stigma effects related to EP. We consistently find that for low-skilled and longer-term unemployed participation in a programme is better than doing nothing, but in each case TEMP is better than EP. We cannot attribute the positive effects to single explanations, but the results suggest that positive signals and improved job matching are important.

Appendix D contains further investigations into subgroup heterogeneity. We find that separating the sample along the line of sectors (of previous occupation) with high and low TEMP participation probabilities does not reveal any effect heterogeneity (table D.1). The same holds true for effect heterogeneity between women and men. Finally, considering effect heterogeneity between skilled (50% of the unemployed, due to the specific definition of the skill variable in these data) and low skilled does not reveal major differences. However, the latter result probably merely reflect that this measure of skill level (unskilled, semiskilled, skilled) is too crude. This suspicion already appeared for the estimation of the participation probabilities. In these estimations the skill level is not significant (see Section 5 and Appendix B).

We also performed another estimation on a subsample aged 35 and older (see left panel in table D.1). We want to check if the results would change rather dramatically. This would be an indication of either effect heterogeneity or selection problems, because for this group we typically control for 10 years for employment history, whereas many of the group of younger unemployment enter the labour market later so that we cannot yet observe such a long history. Nevertheless, the results appear to be very similar to those presented in the main section of the paper.

7 Conclusion

Subsidised temporary employment is one of the tools of active labour market policies to improve the chances of the unemployed to find permanent employment. Using large and informative individual data coming from various administrative sources we investigate the effects of two different schemes of subsidised temporary employment in Switzerland. Concentrating on one country has the advantage of holding the economic environment constant. The crucial difference between the two schemes is that one operates as a non-profit employment programme, whereas the other one is a subsidy for temporary jobs in firms operating in a competitive market. We are interested in how successful these programmes are in speeding up the reintegration of the unemployed into work.

We find that from the point of view of the unemployed the subsidies for temporary jobs in 'normal' firms subject to market pressure (TEMP) are superior to the 'classical' employment programmes (EP) operating in areas without much private competition. About a year and a half after the begin of the programme, TEMP generates additional employment of about 9%-points for its participants compared to EP. Even for the participants in EP, TEMP would have generated about 9%-points additional employment if they had participated in TEMP instead. Compared to EP TEMP saves about one month of UE benefits in the first year after the start of the programmes. We generally find the programmes much less successful for unemployed that may be characterised as the 'good-risks', whereas they tend to increase the reemployment probabilities of 'bad risks'. However, in almost all cases TEMP is more effective than EP. Finally, we can,

to some extent, dismiss the concerns that the gains in the chances to find a job are due to 'low-quality' jobs taken up by TEMP participants. However, there is some evidence that some of the short-term success of TEMP is due to a group of participants that appear to be switching between unemployment, TEMP participation, and short-lived regular employment.

While searching for possible explanations for our findings, the fairly systematic heterogeneity of effects between different groups of unemployed indicates evidence for positive signalling effects of TEMP (based on the results for the short-term unemployed). Furthermore, there is also evidence for positive human capital effects for both EP and TEMP (based on the results for low-skill unemployed). However, the evidence in favour of or against one of the competing hypotheses is not based on strict tests, but is rather based on compatibility of the estimated effect heterogeneity with our knowledge about programme allocation processes as well as theoretic considerations about possible effects.

One obvious policy conclusion is that sending people with good a priori chances on the labour market into these programmes is not cost-effective. Furthermore, it appears to be a bad idea of using these subsidy programmes in the beginning of an unemployment spell. However, even for these groups TEMP seems to be superior to EP. In fact we find no subpopulations for which EP is systematically superior to TEMP. Finally, the unemployment insurance legislation in effect in 1998 requires that cantons fulfil a regional quota of programme places they have to fill (towards which EP counts, but TEMP participation does not). Failure to meet this requirement would have financial consequences for the cantons by reducing federal payments to the cantonal unemployment office. This may have had fairly counterproductive effects, especially given the recovery of the labour market that lead to a diminished pool of unemployed that had to be allocated to the various programmes of the ALMP.

With respect to cost-effectiveness it is obvious that the direct costs of TEMP are lower than those of EP and of nonparticipation. Given that TEMP is also the most effective programme this suggests a clear-cut policy recommendation in favour of TEMP. However, this conclusion has to be taken with some caution, because it ignores the potential indirect costs of both schemes. There are concerns that programmes like TEMP may lead to substitution of unsubsidised labour through subsidised labour, i.e. they may create unemployment. We cannot address this question with our data, but it seems unlikely that this effect is large for a temporary programme like TEMP. The set-up of the employment programmes should prevent crowding out unsubsidised firms. Again, we cannot deal with this problem with our data. Obviously, future research should be directed towards this issue. Extending the sample in the future would allow to dig deeper into these issues without having to rely on extrapolations coming from parametric functional assumptions on the outcome equation.

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Appendix A: Data

The population of interest are prime age individuals who are registered as unemployed in December 31, 1997 and have some previous work experience. For these individuals we use the following data bases of the *unemployment insurance system*:

- *AVAM database*: data generated in the regional placement offices;
personal characteristics, information about last job, duration of job search, ...
- *ASAL database*: data from the unemployment benefit payment system;
all payments and information about programme participation.

Table A.1: Sample Selection Rules

	Subsidised temporary job	Courses	Employment programme	Non- participation
Initial number of observations	7'799	7'305	2'805	12'524
<i>Personal characteristics:</i>				
- not disabled				
- previous earnings > SFr. 1000				
- 24 < age < 56				
- information about mother tongue is not missing				
- Swiss or yearly or permanent working permit				
- no seasonal workers, no home workers, no students				
- unemployed and not working part-time Dec., 31, '97				
- less than 1 year unemployed	-300	-1'434	-519	-2'426
remaining observations	6'499	5'871	2'286	10'098
<i>Programme experience criteria:</i>				
- no programme with duration > 14 days in 1997				
- no employment programme in 1997				
- programme does not start on January 1, 1998 (continuing programme)	-1'134	-450	-179	-789
remaining observations	5'365	5'421	2'107	9'309
Nonparticipants being not unemployed at their simulated starting date of programme				-3'848
final sample (18'354 obs.)	5'365	5'421	2'107	5'461

Note: The full sample selection process is documented in GL. Here we display only the steps that use (slightly) different selection rules.

For a randomly chosen subsample (30'454 observations; about 20% of the population of interest) we have *social security data* with earnings based monthly information over the past 10 years. This appendix explains the selection rules for the sample used (Table A.1), gives the definition of the outcome variables (Table A.2), and presents descriptive statistics of explanatory (Table A.4) as well as outcome variables (Table A.3 and Figure A.1). More detailed information about the data sources are available in GL.

Table A.2: Description of outcome variables

<i>a</i>	<i>Employment with duration > 3 months</i>	Person has an earnings spell of at least 3 successive months (dummy variable)
<i>b</i>	<i>Searching for a job</i>	Person is registered at the regional labor office as searching for a job and doesn't earn more than 3000 CHF during more than 2 successive months at the same time (dummy variable)
<i>c</i>	<i>Earnings in employment > 3 months</i>	Smoothed monthly earnings in CHF in an employment spell with duration of at least 3 months
<i>d</i>	<i>Employment without earnings loss</i>	Person receives earnings during at least 3 successive months and the wage exceeds 90% of the wage earned in the last employment spell (dummy variable)
<i>e</i>	<i>Months of unemployment</i>	Months of registered unemployment in 12 months period

Table A.3: Descriptive statistics

Variable		Subsidised temporary job	Courses	Employ- ment pro- gramme	Nonpar- ticipation
	<i>number of observations</i>	5'365	5'421	2'107	5'461
		Mean / share in %			
Age in years		37.3	37.8	38.7	37.5
Female		41	47	38	43
Marital status single		28	26	29	27
Female and married		23	29	22	27
Female and mother tongue not German/French/Italian		13	19	15	15
Female and mother tongue Italian		4	4	3	4
Mother tongue German		36	34	32	33
Mother tongue French		19	19	18	20
Mother tongue Italian		11	9	11	11
Mother tongue not German/French/Italian		34	38	39	36
German mother tongue and living in 'German language' canton		33	32	30	29
French mother tongue and living in 'French language' canton		18	17	16	19
Italian mother tongue and living in 'Italian language' canton		4	2	6	4
Nationality:	Foreign with permanent permit	31	28	29	31
	Foreign with yearly permit	15	18	18	16
	Swiss	54	55	53	53
Foreign Languages:	Other Swiss language	64	65	66	64
	English, Spanish, Portuguese	13	16	10	16
Job position	very low	37	38	47	36
	high (management, etc.)	5	7	3	7
Qualification level:	skilled (highest)	55	56	48	56
	semiskilled	17	15	17	15
	unskilled (lowest)	28	29	35	29
<i>Chances to find a job</i>	no information	8	5	5	6
	very easy	6	4	5	7
	easy	17	14	13	15
	medium	56	58	53	54
	difficult	11	16	20	16
	special case	2	2	4	2
Looking for ... job	part time	11	13	10	13
Unemployment-status	full-time	85	84	86	83
	part time	13	14	12	15
Desired = previous occupation, 2-digit level		75	71	70	75
Previous occupation:	construction	10	5	8	8
	textiles	1	2	2	1
	architecture, engineer	1	3	2	2
	transportation	4	2	5	4
	restaurants	17	15	14	17
	entrepreneurs, senior officials, justice	2	4	2	4
	office and computer	12	18	13	15
	retail trade	7	10	7	8
	science	1	2	1	3
	news and communication	1	1	1	1

Table A.3 to be continued.

Table A.3 continued – 1

Variable		Subsidised temporary job	Courses	Employ- ment pro- gramme	Nonpar- ticipation
		Mean / share in %			
Previous sector:	construction	18	10	12	14
	public services	6	8	10	10
	education	2	1	2	2
	industry unemployment rate in %, 1/98	6.7	6.3	6.3	6.7
Monthly earnings in last job in CHF		3970	3962	3660	3948
Monthly earnings in last job > 5000 CHF		20	22	14	22
Average earnings in CHF 1993 / 1992		3050	2990	2750	3070
Average earnings in CHF 1997 / 1996		2820	2680	2240	2540
Nondecreasing earnings path before 1997		11	11	8	8
Current unemployment spell is first spell		55	65	64	57
Positive number of sanction days without benefit payment (current spell)		22	25	25	28
Duration of unemployment spell at beginning of programme in days		222	229	303	218
Unemployment benefits	in 1995	3260	3270	4390	3390
	in 1996	3400	2620	3160	4280
	in 1997	11170	12800	13800	14360
Number of UE spells	1997-1993	1.25	0.93	1.21	1.20
	1992-1988	0.33	0.33	0.43	0.36
Month of entry into social security system		11	16	13	12
Immigrant 1992 or later		3	6	5	4
Number of employment spells 1988-1997		3.59	3.07	3.52	3.60
Mean duration of previous unemployment spells in months		5.19	6.14	6.19	5.76
Self-employment 1992 or later		3	4	3	5
Out of labour force 1992 or later		15	20	21	19
Months out of labour force '88-'97 (minimum duration of each spell: 6 months)		1.79	1.84	2.08	1.88
Months out of labour force (month of entry – months UE – months employed)		13	14	15	15
Previous programme participation : sum of short programs 7-12/1997		0.06	0.08	0.06	0.05
Subsidised temporary job before July 97		3	1	1	1
Begin of programme in 1998 (month)		3.79	3.38	5.03	2.82
Size of town (previous employment) > 200.000		6	10	8	8
Region of placement office (reference categories: large city)	rural	19	18	18	16
	middle town	41	39	46	37
Region (reference category: Zurich)	West	21	17	29	16
	Eastern	10	11	7	9
	Central	5	8	7	5
	South-west	28	22	22	30
	North-west	11	14	9	9
Cantons	Ticino	7	4	9	9
	Aargau	5	8	4	4
	Basel-City	3	4	3	3
	Geneva	7	6	4	13
	Graubünden	1	2	1	1
	Neuenburg	3	1	6	3
	Solothurn	4	3	5	2
	St. Gall	5	5	2	4
	Waadt	12	13	12	13
	Zug	1	1	1	1
% of people against stricter unemployment insurance law (national referendum in 1997; disaggregated at local level)		45	47	44	45

Table A.3 to be continued

Table A.3 continued - 2

		Subsidised temporary job	Courses	Employ- ment pro- gramme	Nonpar- ticipation
Variable		Mean / share in %			
Employment with duration > 3 months	1998/04	20	9	4	22
	1998/08	40	24	16	40
	1998/12	41	30	22	38
	1999/04	57	46	41	50
	1999/08	70	58	54	57
	1999/12	66	61	59	53
Searching for a job	1998/04	81	91	96	75
	1998/08	58	73	84	49
	1998/12	54	64	75	46
	1999/04	38	47	56	33
	1999/08	23	30	35	20
	1999/12	24	21	23	19
Earnings in employment > 3 months	1998/04	728	332	128	843
	1998/08	1463	935	514	1549
	1998/12	1485	1204	725	1487
	1999/04	2157	1810	1324	1912
	1999/08	2603	2191	1781	2143
	1999/12	2395	2248	1845	1959
Employment without fall in earnings	1998/04	11	5	2	13
	1998/08	22	14	8	25
	1998/12	23	18	12	22
	1999/04	36	30	24	31
	1999/08	43	35	30	34
	1999/12	39	35	29	30
Months of unemployment	1998/04	6.28	7.79	8.72	5.11
	1998/08	4.61	5.77	6.50	3.51
	1998/12	3.28	3.77	4.03	2.33

Appendix B Estimates of the multinomial probit model

Table B.1 shows the estimation results of a multinomial probit model (MNP) using simulated maximum likelihood with the GHK simulator.¹⁹ Although being fully parametric, the MNP is a flexible version of a discrete choice model, because it does not require the Independence of Irrelevant Alternatives assumption to hold.

¹⁹ See for example Börsch-Supan, Hajivassiliou (1993) and Geweke, Keane and Runkle (1994).

Table B.1: Estimated coefficients of a multinomial probit model for participation in a programme

Variable	Courses	Employment programme	Non-participation
Age in years / 10	.05	.18	-.001
Female	-.13	-.35	-.28
Marital status single	.09	.23	.08
Female and married	.06	.02	.24
Female and mother tongue not German/French/Italian	.17	0	0
Female and mother tongue Italian	0	-.39	0
Mother tongue not German/French/Italian	.04	.15	.06
German mother tongue and living in 'German language' canton	-.27	.08	-.03
French mother tongue and living in 'French language' canton	.12	-.07	-.14
Italian mother tongue and living in 'Italian language' canton	.02	.38	-.28
Nationality: Foreign with permanent permit	-.11	-.17	.05
Foreign with yearly permit	-.03	-.06	.05
Foreign Languages: Other Swiss language	.07	.02	-.05
English, Spanish, Portuguese	.19	-.06	.08
Job position very low	.03	.20	.06
high (management, etc.)	0	0	.26
Qualification level: skilled (highest)	.05	-.003	.09
unskilled (lowest)	.04	-.007	.04
Chances to find a job (reference category: medium): no information	-.13	-.23	-.01
very easy	-.02	-.14	.17
easy	-.12	-.10	.001
difficult	.13	.20	.19
special case	.15	.39	.49
Looking for ... job part time	.10	-.01	.28
Unemployment-status (reference category: part-time): full-time	.12	.23	.05
Desired = previous occupation, 2-digit level	-.07	-.02	.05
Previous occupation: construction	-.11	-.03	.12
textiles	.27	.42	.33
architecture, engineer	.44	.21	.09
transportation	-.22	0	0
restaurants	.003	-.14	.06
entrepreneurs, senior officials, justice	.36	.08	.34
office and computer	.40	.15	.15
retail trade	.32	-.06	.16
science	.20	.30	.57
news and communication	.47	0	0
Previous sector: construction	-.16	0	0
public services	.06	.16	.13
education	-.30	0	0
industry unemployment rate in %, 1/98	-.07	-.13	-.02
Monthly earnings in last job in CHF /1000	.01	-.09	-.09
Monthly earnings in last job > 5000 CHF	0	0	.14
Average earnings in CHF 1993 - 1992 /1000	.006	0.01	.03
Average earnings in CHF 1997 - 1996 /1000	-.014	-.09	-.02
Nondecreasing earnings path before 1997	-.07	-.12	-.13

Table B.1 to be continued.

Table B.1 continued

Variable	Courses	Employment programme	Non-participation
Current unemployment spell is first spell	.12	.18	.04
Positive number of sanction days without benefit payment (current spell)	.02	.08	.22
Duration of unemployment spell at beginning of programme /100	-.06	.04	.13
Duration of unemployment spell at beginning of programme			
< 3 months	-.24	-.34	.21
< 6 months	-.08	-.28	.13
Unemployment benefits/1000			
in 1995	.006	.009	-.006
in 1996	-.007	-.004	.013
in 1997	.003	.006	.011
Number of UE spells			
1997-1993	-.04	.03	-.01
1992-1988	-.07	.07	.05
Month of entry into social security system	.41	.17	.05
Immigrant 1992 or later	.08	.07	.002
Number of employment spells 1988-1997	-.04	-.01	.02
Mean duration of unemployment spells in months	.86	0	0
Self-employment 1992 or later	.09	-.05	.27
Out of labour force 1992 or later	.11	.08	.12
Months out of labour force '88-'97 (minimum duration of 6 months for each spell)	.003	0	.003
Months out of labour force (month of entry – months UE – months employed)	.003	.01	-.004
Previous programme participation : Sum of short programs 7-12/1997	.11	0	0
Subsidised temporary job before July 97	-.18	.27	0
Begin of programme in 1998 (month)	-.02	.05	-.16
Size of town (previous employment) > 200.000	.10	.23	.16
Region of placement office (reference categories: large city)			
rural	-.12	.02	-.07
middle town	0	0	-.11
% of people against stricter unemployment insurance law (national referendum in 1997; disaggregated at local level)	-.05	-.05	.03
Region (reference category: Zurich)			
West	-.18	.53	-.12
Eastern	.08	.23	-.18
Central	.45	.49	-.16
South-west	-.65	.19	-.23
North-west	-.15	.06	-.30
Ticino	-.49	.43	.65
Additional regional effects by canton			
Aargau	.41	0	0
Basel-City	0	0	.24
Geneva	0	-.48	.86
Graubünden	.20	0	0
Neuenburg	-.75	0	.53
Solothurn	0	.19	-.42
St. Gall	0	-.67	0
Waadt	.29	0	.86
Zug	-.81	0	0

Note: Simulated maximum likelihood estimates using the GHK simulator (200 draws in simulator for each observation and choice equation). Coefficients of the category TEMP 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. $N = 18354$. Value of log-likelihood function: - 22109.2.

Bold numbers indicate significance at the 1% level (2-sided test), numbers in *italics* relate to the 5% level.

If not stated otherwise, all information in the variables relates to the last day in December 1997.

The variables that are used in the MNP are selected by a preliminary specification search based on binary probits (each relative to the reference category SUBSIDISED TEMPORARY JOB) 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. 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 error terms of the MNP 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 SUBSIDISED TEMPORARY JOB and the other three alternatives (for details see Table B.1).

Table B.2: Estimated covariance and correlation matrices of the error terms in the multinomial probit

	Subsidised temporary job		courses		employment programmes		nonparticipation	
	Coef	t-val	coef	t-val	coef	t-val	coef	t-val
Covariance matrix ^{a)}								
TEMP	1.0	-	.95	5.25	-.03	-.11	-.18	-.46
Courses			1.9	-	-.03	-	-.17	-
EP					1.0	-	.01	-
Nonparticipation							1.03	-
Correlation matrix ^{a)} x 100								
TEMP	100		69		-3		-17	
Courses			100		-2		-12	
EP					100		6	
Nonparticipation							100	

Note: ^{a)} 3 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.

Appendix C: Common support

As proposed, discussed and applied in GL and Lechner (2001, 2002a, b) we base the common support requirement on the marginal choice probabilities (conditional on X) for all alternatives. By doing so, we ensure that all effects are estimated for the same underlying support. Table C.1 shows the minima and the maxima of the estimated probabilities. In total the common step criteria discarded about 3.1% of the observations. Table C.2 shows that the largest shares of 'lost observations' appear for NP (4.3%) and TEMP (3.4%). However, overall these numbers appear to be low. Although Table C.3 shows that high-skilled Swiss living in large towns not in the south-west appear to have a lower probability to be rejected by the common support criterion, the overall impact on the estimates must be small due to the small number of rejected cases.

Table C.1: Minima and maxima of $[\hat{P}_N^{TEMP}(x), \hat{P}_N^{Courses}(x), \hat{P}_N^{EP}(x), \hat{P}_N^{NP}(x)]$ in subsamples

Subsamples	$\hat{P}^{TEMP}(X)$	$\hat{P}^{Courses}(X)$	$\hat{P}^{EP}(X)$	$\hat{P}^{NP}(X)$
<i>maximum in subsample</i>				
Subsidised temporary job	81	78	60	78
Courses	77	79	66	76
Employment programmes	74	79	69	71
Nonparticipation	74	73	69	91
Minimum of maxima	74	73	60	71
<i>minimum in subsample</i>				
Subsidised temporary job	4.5	2.3	.05	1.1
Courses	2.9	4.5	.12	.96
Employment programmes	3.0	3.5	.70	.22
Nonparticipation	2.6	2.8	.01	2.3
Maximum of minima	4.5	4.5	.70	2.3

Note: Estimated probabilities in %.

Table C.2: Loss of observations due to common support requirement

	Subsidised temporary job	Courses	Employment programme	Nonparticipation
Observations before	5365	5421	2107	5461
Observations after	5182	5294	2085	5225
Percent deleted	3.4	2.3	1.0	4.3

Note: The total number of observations decreases due to the enforcement of the common support requirement from 18354 to 17786 (-3.1%).

Table C.3: Comparison of means of selected variables in the subsample used for matching and the subsample not used for matching because of insufficient overlap

Variable	Observations used for matching	Observations not used for matching
Number of observations	17786	568
<i>shares in %</i>		
<i>Mother tongue</i>		
Not G/I/F	37	32
<i>Chances to find a job</i>		
Special case	2	2
<i>Qualification</i>		
Unskilled	30	18
<i>Previous Occupation</i>		
Metals	7	3
Restaurants	16	16
Office and computer	15	20
Retail trade	8	9
<i>Region</i>		
Eastern	10	16
South-west	26	47
Central	6	3
<i>Region of placement office</i>		
Large city	41	54
<i>Social security data</i>		
Month of entry into social security system	13	11

Appendix D: Additional results concerning subgroup heterogeneity

Table D.1: Dynamics of average effects (ATE) for employment without earnings loss – Low sectoral share of TEMP workers compared to unemployed, age above 35 only

Month after begin	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\rho}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\rho}_N^{1,0} = E(Y^1 - Y^0)$
older than 35 only				Low sectoral share of TEMP workers		
TEMP (1) compared to EP (0)						
3	15	6	7	18	8	6
9	26	15	7	29	18	8
15	39	27	8	43	29	10
21	43	33	(7)	46	36	(5)

TEMP (1) compared to NONP (0)						
3	15	16	(-2)	18	21	-3
9	26	22	(2)	29	26	3
15	39	29	7	43	34	8
21	43	30	10	46	36	8

EP (1) compared to NONP (0)						
3	6	16	-9	8	21	-9
9	15	22	-5	18	26	-5
15	27	29	(-1)	29	34	(-1)
21	33	30	(3)	36	36	(3)

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes TEMP / courses / EP / NONP for older 35 years: 2637 / 2826 / 1212 / 2623. Sample sizes TEMP / courses / EP / NONP for low sectoral share of TEMP workers: 3259 / 3099 / 1244 / 3294.

Table D.2: Dynamics of average effects (ATE) for employment without earnings loss – sex

Month after begin	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$
women				men		
TEMP (1) compared to EP (0)						
3	13	7	7	20	8	9
9	26	21	5	30	17	7
15	37	30	(6)	45	30	10
21	40	31	10	48	37	(6)

TEMP (1) compared to NONP (0)						

3	13	15	(-1)	20	21	(-1)
9	26	21	6	30	28	(0)
15	37	28	9	45	37	7
21	40	28	9	48	40	6
EP (1) compared to NONP (0)						
3	7	15	-8	8	21	-10
9	21	21	(0)	17	28	-6
15	30	28	(4)	30	37	(-4)
21	31	28	(-1)	37	40	(0)

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes TEMP / courses / EP / NONP for women: 2106 / 2484 / 776 / 2193. Sample sizes TEMP / courses / EP / NONP for men: 2991 / 2719 / 1298 / 2861.

Table D.3: Dynamics of average effects (ATE) for employment without earnings loss – skill level

Month after begin	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$
skilled				unskilled		
TEMP (1) compared to EP (0)						
3	17	7	8	17	8	8
9	30	21	7	27	15	9
15	42	31	9	42	30	9
21	46	35	8	42	33	(3)

TEMP (1) compared to NONP (0)						

3	17	19	-4	17	20	(-2)
9	30	28	(2)	27	22	(4)
15	42	35	7	42	33	12
21	46	38	6	42	33	(5)
EP (1) compared to NONP (0)						
3	7	19	-12	8	20	-9
9	21	28	-5	15	22	-6
15	31	35	(-2)	30	33	(3)
21	35	38	(-2)	33	33	(2)

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes TEMP / courses / EP / NONP for skilled: 2782 / 2925 / 1007 / 2808. Sample sizes TEMP / courses / EP / NONP for unskilled: 1462 / 1499 / 697 / 1510.

Table D.4: Estimates of effects (by previous earnings) with “employment without earnings loss” as outcome

Month after begin	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$	$E(Y^1 S = 1)$	$E(Y^0 S = 0)$	$\hat{\gamma}_N^{1,0} = E(Y^1 - Y^0)$
High previous earnings (> CHF 4000)			Low previous earnings (< CHF 3000)			
TEMP (1) compared to EP (0)						
3	17	7	6	17	10	11
9	27	16	(4)	31	24	8
15	40	26	8	42	34	6
21	45	33	8	47	34	10

TEMP (1) compared to NONP (0)						

3	17	19	(-3)	17	16	(2)
9	27	27	(0)	31	20	11
15	40	34	6	42	29	15
21	45	37	7	47	31	11
EP (1) compared to NONP (0)						
3	7	19	-9	10	16	-9
9	16	27	-5	24	20	(4)
15	26	34	(-2)	34	29	9
21	33	37	(-2)	34	31	(1)

Note: Outcome variable is employed for at least 3 months with average earnings more than 90% of previous earnings. Results are based on matched samples. **Bold** numbers indicate significance at the 5% level (2-sided test). Sample sizes TEMP / courses / EP / NONP for earnings > 4000: 2247 / 2134 / 751 / 2089. Sample sizes TEMP / courses / EP / NONP for UE < 3000: 1256 / 1537 / 705 / 1436.