Does subsidised temporary employment get the unemployed back to work ?

An econometric analysis of two different schemes

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Abstract

Subsidised employment is one tool of active labour market policies to improve the chances of the unemployed to find permanent employment. Using informative individual data coming from various administrative sources we investigate the effects of two different schemes of subsidised temporary employment implemented in Switzerland. One scheme operates as a non-profit employment programme (EP), whereas the other is a subsidy for temporary jobs (TEMP) in firms operating in competitive markets. Using econometric matching methods we find that TEMP is considerably more successful in getting the unemployed back into work than TEMP. We also find that compared to nonparticipation both programmes are unsuccessful for unemployed that find job easily anyway as well for those with short unemployment duration. For unemployed with potentially long unemployment duration and for actual long term unemployed, both programmes may have some positive effect, but the effect of TEMP is much larger.

Keywords

1   Introduction

Subsidised employment is an important tool of labour market policy in many (all?) 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).

Although widely used, there are considerable differences in the design of the subsidy schemes. The most common form is a wage subsidy paid either to the employer or the employee. The subsidy itself my be permanent (conditional on low earnings like the EITC), or it may have a maximum eligibility period. The programmes may subsidise strictly temporary employment or just decrease the initial wage of a job that is supposed to become a permanent one. Furthermore, within the temporary subsidy schemes, there is a difference whether a 'real' job in a firm acting on competitive markets is subsidised, or whether the subsidised job is located in some specialised non-profit operation active in some sheltered part of the economy which does not compete with private firms. The latter are the typical European 'employment programmes'. Finally, there is the issue of the actual direct cost of programme participation to individuals, the unemployment insurance system, and society as whole.

The different programmes may influence the allocation in the labour market in different ways. While in operation they offer temporary employment that otherwise would not be accepted by the unemployed or created by firms because there are no incentives to do so. These negative incentives may be due to unemployment or welfare benefits or minimum wages, respectively. But are the different schemes of temporary subsidised employment successful in reintegrating the unemployed into work faster? Do they differ? What can we learn from these differences?

As far as we know few theoretical research and no comparative empirical research has been devoted to analyse the different effects of different types of employment subsidies on those who receive the subsidy. Cross-country studies face the substantial problem of comparing two programmes under potentially very different labour market conditions. Therefore, it would useful to 'partial out' the effect of local labour market conditions by comparing two different programmes

within the same country. This approach trivially requires that a country really runs two different types of subsidy programmes accessible to the same group of people. 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 demanding task when one concentrates on the more subtle differences between two programmes that may be only small parts of the diverse active labour market policy a particular country runs.

In fact Switzerland can be used to study the differences between two version of employment subsidies that are both operated on a larger scale and both targeted at more or less the same population of unemployed. Both programmes use 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 is a subsidy of temporary jobs in firms operating in a competitive environment. Furthermore, in the Swiss case a large individual data base (coming from various administrative registers) is available that has previously been used for a microeconometric evaluation study of several active labour market policies by Gerfin and Lechner (2000, GL).[1] GL argue that the data is informative enough to control for selection issues. Furthermore, it contains several thousands participants in both programmes.

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 evaluations 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 the other programmes (Carling and Richardson, 2001, and Sianesi, 2001). However, the two studies about Sweden are based on programmes that do not allow to really isolate the difference whether an temporary employment subsidy is allocated to the competitive sector or to the non-competitive sector.[2] At the moment there seems to be no literature on econometric comparisons of two programmes of subsidised temporary employment, probably for the reason that most countries run only one type of those schemes.

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[1] In fact the data used by GL is updated. It contains additional variables and it covers a longer period of time.

[2] The study by Carling and Richardson (2001) focuses basically on training programmes that are either located in some class room or at a real firm (with some duties of real job being part of the training). The study by Sianesi (2001) compares two programmes, one of which is a subsidy for a temporary job, the other one being a programme that is a subsidy for a job supposed to become permanent. However, the latter one is of course prone to have very substantial direct crowding out effects with respect to unsubsidised employment. Those are probably much larger than those for subsidising only temporary jobs.

There are several important questions regarding the previous results by GL that we also try to answer in this paper: Are the results robust with respect to other outcome variables, especially earnings? Are the results robust in the long run? If so, why are subsidised temporary jobs superior to employment programmes? Does this average results hold for all groups of the unemployed, or are there systematic differences?

In this paper we carefully analyse the institutional settings in which both programmes operate. We relate the institutions to some theoretical thoughts about plausible differentiated effects of the programmes assuming that their effects might be due to three different potential channels, namely the addition of human capital, signalling, or stigma effects.

Our empirical findings strongly confirm the strong positive effects of the subsidy for temporary jobs in firms operating in competitive markets (TEMP) as compared to the employment programme (EP) type of the subsidy. We can also dismiss the concern that the positive effects are to due the participants of TEMP taking up inferior, i.e. lower paying jobs. Furthermore, we find that both programmes are much more effective in raising reemployment probabilities for the unemployed having great difficulties in the labour market. However, even for the 'better risks' TEMP seem to have some, albeit small positive effects. It appears that TEMP as well as EP are adding some human capital, although with different effectiveness. However, we cannot rule out that signalling and stigma effects might play important roles in explaining our results as well.

The next section describes the Swiss programmes in some depth. Section 3 rather informally develops our theoretical insights on why these different types of programmes may have different effects. Section 4 as well as Appendix A describe the data and present some descriptive results. Section 5 analyses the expected and observed differences between the participants in both wage subsidy programmes. Section 6 gives a summary of the econometrics used, which is multiple treatment evaluation framework using a 'matching on the propensity score estimator'. Section 7 presents the results and Section 8 concludes. Appendix B contains the results of the estimation of the propensity scores in multinomial probit framework. Appendix C describes the extent of the common support problem and our remedies. Appendix D adds additional results concerning the subgroup heterogeneity of the effects of the programmes.

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 subsidises to foster reintegration of the unemployment into the labour market. Therefore to understand the effects and the composition of the group of participants in these programmes it is necessary to first get an understanding of the institutional environment in which these programmes operate. Then we describe the two programmes.

Swiss unemployment insurance

The basic rules of Swiss unemployment insurance (UI) are as follows: Benefit entitlement lasts for a maximum of two years (conditional on employment history). The entitlement period is split in two parts: the first 30 weeks are unconditional on programme participation, but 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 of the unemployment spell. 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 between 70% and 80% of the insured earnings, depending on socio-demographic characteristics. 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 main differences between b) and c) are that employment programmes take place outside the "regular" labour market (see below) and are subsidised to a larger extent. By contrast a subsidised temporary job must be in a regular for-profit firm.

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[3] More details can be found in Gerfin and Lechner (2000) and in Lalive, van Ours, and Zweimüller (2000).

The cantons are obliged by law to fill a minimum of places in labour market programmes per year. Until January 2000 this minimum was 25'000 places distributed across cantons according to their unemployment rates. By comparison, there were about 190'000 registered job-seekers in 1997 and 140'000 in 1998. An important feature of subsidised temporary jobs is that they are (probably for historical reasons) not part of the official ALMP. Thus, they are not counted towards the minimum of places to be filled per canton in the various programmes of the ALMP.

Employment Programmes (EP)

Employment programmes are offered by both public and private institutions and usually last for six months.[4] There are two different types of programmes: either it is a single position, i.e. a special job in a public (e.g. administration or hospitals) or private institution (e.g. restaurants), 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.[5] 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. Given the placement decision participation is compulsory. Sometimes, informal interviews we conducted at the placement offices strongly suggest that in a considerable number of cases case workers use employment programme 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 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

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

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 latter pays the participant directly. The wage has to be at least the minimum wage set for the region and sector (if there is a collective wage agreement). It may be larger than unemployment benefits, but in practice this appears to be rather an exception. For the placement office there are no direct savings associated with placing an unemployed into an employment programme. In 1998, roughly 17'000 persons participated in an employment programme (about 10% of the registered job seekers).

It is worthwhile noting that the rule and the design of the Swiss employment programmes closely follow the rules of the German employment programmes. Similar programmes are also operated in a couple of 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 programmes that we call subsidised temporary jobs is to encourage job seekers to accept job offers for "unsuitable" jobs (see above) that pay less than their unemployment benefits (scaled up by the replacement ratio) by compensating 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.[6] 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 are related to the wage earned in the temporary job which is below 80% of previous insured earnings (thus combining many such spells would lead to a consistent drop in income). Mean duration of these temporary jobs is roughly 4 months, but there is considerable variation.

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[5] 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.

[6] 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.

Although TEMP is not part of the ALMP, it is its single largest programme. In 1998 roughly 20% 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 minimum wage set for the region, sector and occupation (if there is a collective wage agreement). Given that the wage has to be less than 80% of previous earnings (the unemployment benefit) to be eligible for a subsidised temporary job it is obvious that many jobs in TEMP are below the qualification level of the unemployed.

The OECD (1996) states that TEMP can be a powerful instrument to bring unemployed back to employment. However, there is concern that it 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 in order to have a more flexible work force, or TEMP might be used to avoid the wage levels set in collective wage bargaining agreements. However, at the moment there appears to be no evidence of abuse of TEMP in these respects.

Arrangements like TEMP subsidising jobs (i) within firms competing in the market and (ii) that are explicitly intended not to become permanent employment are not commonly used in 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.

It is important to recognise that the main difference between TEMP and EP are the kind of job and the work experience they generate. Ignoring any potential market distortions and assuming that EP do not produce public goods to a considerable extent, then from the point of view of the tax payer EP are more expensive than subsidised temporary jobs. An interesting question we look at with our data in Sections 4 and 5 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).

3   Why and for whom should these programmes work?

The main purpose of this paper is to answer the question if, and if so why the subsidised temporary job programme is found to be superior to the employment programme. As described in Section 2 these two programmes can be seen as different implementations of temporary subsidised employment, both with the clear intention to improve the chances to find (other) permanent employment. To the best of our knowledge there is no other paper that addresses the question why two such different programmes may have different effects. In the following we sketch some hypotheses by borrowing from the literature on wage subsidies, human capital accumulation and signalling.

There is a large literature on the effects of wage subsidies (e.g. Katz, 1996). However, in the present context the standard theory of wage subsidies is not applicable. It is important to recognise that the Swiss subsidised employment programme is for temporary jobs only, intended as stepping stones to permanent employment. Individuals in the subsidised temporary employment programme are still unemployed in the sense of receiving unemployment benefits (in the form of the compensation payment) and of being registered as job seekers. Only after leaving unemployment into non-subsidised employment they are considered as being employed. This is completely different compared to the standard wage subsidy case which is based on a static model and only considers permanent jobs. A dynamic extension of the static model of wage subsidies recently described in Bell, Blundell, and van Reenen (1999) shows 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 due to the programme. Two main points arise from their model.[7] First, it implies a long-run effect of a short-run subsidy. By getting some low skilled workers into temporary jobs their productivity is raised and so their chance of moving out of unemployment is enhanced. Second, the model raises the question why the unemployed do not take up (temporary) jobs as an 'investment' because of these long term gains. The answer could be that there are constraints that prevent some unemployed from finding temporary jobs.

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[7]   In a simplified version of this model there are two periods, three sectors and two types of workers, type I (high productivity) and type II (low productivity). A worker increases his or her probability of acquiring a real increase in productivity with tenure (e.g. learning by doing). Variation in the individual's reservation wage generates lower probabilities of the less productive workers being in employment at any given time. After a period in employment type II workers look identical to skilled worker with probability, p (see Bell et al., 1999, who also only present a verbal description of the model).

In our case the problem is extended to two types of temporary employment, one found to be successful (TEMP) in Gerfin and Lechner (2000), the other unsuccessful (EP). Our question is thus: why would employment programmes and subsidised temporary jobs have different effects on employment chances? One possibility is that they both have an effect on productivity, but generate different human capital. 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. Consequently, these jobs are concentrated in non-competitive sheltered sectors in which there are too few vacancies to absorb the programme participants. By contrast, temporary jobs are concentrated in competitive sectors which have thus stronger labour demand, so the sector-specific human capital generated by the temporary job is valuable for finding a permanent job in the same sector.

An argument against the human capital explanation is the fact that the 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). It is uncertain whether a couple of weeks in a job for which the unemployed is overqualified really enhances his human capital. Furthermore, it might be difficult for a potential employer to observe the human capital effect, given that the pool of possible employees consists of unemployed, many of them long-term unemployed. However, it is possible that the programme an unemployed participated in carries a signal to employers. Suppose a potential employer has to choose between two job applicants with similar characteristics, one coming from an employment programme (A), the other coming from the subsidised temporary job (B). Applicant B, however, looks like he is coming from a temporary "real" job. The employer may use this information to conclude that applicant B is better in the sense of having a closer attachment to the labour market. This effect will be especially strong when the potential permanent job is in the same sector as the subsidised temporary job.

Another possible explanation is the occurrence of stigma effects and the related concept of statistical discrimination. Suppose employment programmes are stigmatised in the sense that there is common belief among employers that participants in employment programmes are on average less productive then their counterparts in subsidised temporary jobs. Hence in a hiring situation they will always choose the participant in subsidised temporary jobs, even if he is not more productive, other factors observable to the employer being equal. If the unemployed know this, the

more productive unemployed self-select themselves into the temporary employment programme, turning the stigma effect into a self-fulfilling expectation. There is some (anecdotal) evidence that employment programmes indeed carry the described stigma. The question then is why not all unemployed try to get a subsidised temporary job. The answer is that there is a limited supply of suitable temporary jobs and, in general, they are not arranged by the placement office. Hence, it is costly to find these jobs.

The literature on signalling and screening effects does not directly address the problem at hand. Two somewhat related papers are Ma and Weiss (1993) and McCormick (1990). Ma and Weiss (1993) show that it may be better to become unemployed than to take up a low-skill job in case of job loss. Taking up a low-skill ("lousy") job may be seen as a bad signal by future employers. A similar argument is made in McCormick (1990). The basic story is that workers possess private information about their abilities which is correlated with employers' evaluations. There is a potential gain to employers from using this information to select among job applicants who are observationally equivalent. Therefore, workers with favourable private information want to signal this to employers. In McCormick's model there is a separating equilibrium in which, after learning about their layoff in the next period, the most productive workers remain employed (by searching on-the-job), the medium productive workers experience an unemployment spell, and the least productive workers experience a longer spell between skilled jobs, spending this time in unskilled work.

The human capital and the signalling hypotheses are not exclusive. Of course, it is possible that the subsidised temporary job at the same time enhances productivity and embodies the positive signal. Hence, it is not possible to derive strict tests of the hypotheses. Nevertheless, we can think of hypotheses about effect heterogeneity for different groups of unemployed that are plausible under some explanations and that not plausible under others. Examining the empirical evidence for these hypothesis may indicate some answers to the question why programmes have different effects.

We 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. In addition, we assume that the benefit of preserving human capital (i.e. the participation in the programme should at least prevent a further depreciation of human capital due to un-

employment) is balanced by the lock-in effect.[8] Consider the expected effects of employment programmes and subsidised temporary employment compared to nonparticipation for unemployed with a short unemployment spell. For those, we would expect the human capital effects of the programme to be negligible.[9] Hence if a programme has any effect with respect to nonparticipation it must be primarily due to a signal. This is a case where the models of McCormick (1990) and Ma and Weiss (1993) are most appropriate in our setting. On the other hand, for the long term unemployed we would expect human capital effects. Under the assumption that the signalling effect is constant, a test for human capital effects would be whether the effects for the long term unemployed are stronger than for the short-term unemployed.

Now consider effect heterogeneity with respect to the level of previous earnings. For unemployed with relatively high previous earnings (and productivity), the interesting comparison is with nonparticipation. This is another case where the models of McCormick (1990) and Ma and Weiss (1993) are directly applicable. Since most subsidised temporary jobs and temporary employment are low pay jobs, unemployed with high previous earnings are overqualified to perform these jobs. In this case the theoretical models imply that it is optimal not to take such a job. It may even bad for them to enter a programme due to the negative effects predicted by the theoretical signalling models described above. Hence we should find no positive effect with respect to nonparticipation, and even negative effects when signalling is important. For those with relatively low earnings (and presumably productivity), on the other hand, the human capital and the signalling effect are not separable. So if we find significant effects for a programme we attribute these to either human capital or to signalling.

A similar argument applies to qualification measured by the case worker's evaluation of the chances to find a job. The main difference is that previous earnings are known to potential employers while the case worker's evaluation are not. So possibly the comparison between the effects for the subgroups defined by these two indicators for qualification may help to separate human capital from signalling effects.

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[8] Programmes can actually harm individual employment chances. Even if the programmes themselves do not change the employment chances directly, an immediate indirect effect of all programmes could be a reduction in job search activities compared to nonparticipants. Furthermore, participants may receive fewer job offers from the labour office.

[9] 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.

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 case worker in 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 usually proposed 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 which 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 of course indicative of the 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 chance 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.

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 case workers in order to fulfil their quota, which is hardly a good incentive). 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 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 Gerfin and Lechner (2000). The first 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 all persons who were 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 these data of about 30'000 observations we have data from 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 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. We are confident that after controlling for this wealth of information there is little unobserved heterogeneity left that is systematically correlated with labour market outcomes and programme participation.

Compared to Gerfin and Lechner (2000) there is one important extension to the data. We now have social security data for the years 1998 and 1999 which allows us to construct additional outcome variables. In the previous study the most important outcome variable used to measure the

effects of the programme was *leaving unemployment towards employment* as recorded in the unemployment registrar. Now, we are able to measure employment by the entries in the social security data. Hence, we can also form variables measuring the quality of employment in terms of earnings and to some extend job duration. This allows us to address the question whether specific programmes, subsidised temporary jobs in particular, lead to employment that may be of lower "quality" than the employment prior to the current unemployment spell. Furthermore, we can also evaluate the effects on earnings per se. Given the new data we can now evaluate the effects up to 24 months after the programmes start. More details on the data can be found in Appendix A and in Gerfin and Lechner (2000).

4.2   The definition of programmes used in the empirical analysis

We differentiate only four groups of ALMP participation statuses to which we allocate all observations in our data. Because 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 Gerfin and Lechner (2000), 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.[10] A major programme is defined as having a duration of at least two weeks. Following the arguments in Gerfin and Lechner (2000) we evaluate only the *first* major programme starting between January and December 1998 (see that paper for details).[11]

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.

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[10] 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.

[11] 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.

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 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 sufficient time left to participate in a programme after December 31, 1997. Furthermore, given our focus 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 18'668 observations. Table 1 displays selected descriptive statistics of some important variables differentiated by programme status.[12] For more descriptive statistics the interested reader is referred to Table A.3 in Appendix A.

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

Group		Obs.	Chances to find a job		Qualifi-cation	Unem-ployment duration before programme	Earnings before unemploy-ment	Earnings Dec 1999 (if emplo-yed)	Employ-ed Dec 1999
			good or very good	difficult or special case					
		(persons)	(share in %)		(mean)	(mean days)	(mean per month in CHF)	(mean in CHF)	(share in %)
Subsidised temporary job	(TEMP)	5614	23	13	1.73	220	3950	3635	68
Courses		5462	18	18	1.73	228	3960	3685	64
Employment programmes	(EP)	2128	18	24	1.86	301	3660	3134	64
Nonparticipation	(NONP)	5464	22	18	1.74	217	3920	3624	56

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

The descriptive statistics in Table 1 conform with our expectations. The participants in employment programmes are clearly the least skilled, measured by the chances to find a job, qualification, and previous earnings. For the other three programmes there are hardly any differences in the skills of the participants, with the exception of the chances to find a job which are favourable

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[12] Compared to Gerfin and Lechner (2000) 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%.

for the participants of the subsidised temporary job programme. Unemployment spell duration at the time of programme start is three or four 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. Earnings in December 1999 are almost identical for nonparticipants and participants in courses and temporary wage subsidies, but lower for participants in employment programmes. Somewhat surprisingly is the relatively high employment share of participants in EP, which is higher than for nonparticipants. Of course, these figures for the outcome variables cannot be interpreted as the causal effects of the programmes.

5 Different participants in the two subsidised temporary employment programmes

Our main interest is the comparison of employment programmes and subsidised temporary jobs. Therefore, we focus on the determinants of this particular selection in this section. We are interested how the results conform with our expectations derived from the above discussions. Table 2 displays the estimation results of a binary probit for the choice between employment programme and subsidised temporary jobs, estimated on the subsample of participants in either programme. The results for the multinomial probit estimation on the full sample can be found in Appendix B.

Our previous arguments are based on two central attributes: skill level and unemployment duration. For both we find the expected selection into the programmes. The unskilled, measured by a low previous job position, low chances to find a job and low monthly earnings before unemployment, have a significantly larger probability to enter employment programmes. The probability to get a subsidised temporary job is significantly higher when the unemployment spell duration is short. This is in accordance with the hypothesis that there is a limited supply of temporary jobs. The unskilled may have problems finding them and after some time the case workers allocate them to employment programmes.

There is an obvious heterogeneity in the selection process according to the previous occupation. There is also strong regional heterogeneity in the selection process. Due to the federal structure of Swiss policy making, the cantons have a considerable degree of autonomy and put different emphasis on the various programmes in their local implementation of the national ALMP.

Table 2: Estimated coefficients of a binary employment programme versus subsidised temporary job

Variable	Coeff.		Coeff.
Age in years / 10	.17	Industry unemployment rate in %, 1/98	*-.13*
Female	**-.28**	Monthly earnings in last job in CHF /1000	**-.072**
Marital status single	**.20**	Average earnings in CHF 1993 / 1992 /1000	.00
Female and married	.01	Average earnings in CHF 1997 / 1996 / 1000	**-.099**
Female and mother tongue Italian	**-.38**	Nondecreasing earnings path before 1997	-.12
Mother tongue not German/French/Italian	*.14*	Current unemployment spell is first spell	**.16**
German mother tongue in 'German language' canton	.00	Positive number of sanction days without benefit payment (current spell)	.05
French mother tongue in 'French language' canton	-.00	Duration of UE spell at beginning of programme (days / 1000)	.14
Italian mother tongue in 'Italian language' canton	**.48**	Duration of UE at beginning of progr. < 3 months	**-.39**
Nationality: Foreign with permanent permit	**-.17**	< 6 months	**-.24**
Foreign with yearly permit	-.06	Unemployment benefits/1000 in 1995	**.010**
Foreign Languages: Other Swiss language	.04	in 1996	*-.0064*
English, Spanish, Portuguese	-.01	in 1997	.004
Job position very low	**.17**	Number of UE spells 1997-1993	.01
Qualification level: skilled (highest)	.02	1992-1988	**.072**
unskilled (lowest)	.03	Month of entry into social security system	.18
Chances to find a job : no information	**-.21**	Immigrant 1992 or later	.06
(reference category: medium) very easy	-.10	Number of employment spells 1988-1997	**-.027**
easy	*-.11*	Self-employment 1992 or later	-.04
difficult	**.21**	Out of labour force 1992 or later	.08
special case	**.33**	Months out of labour force 1988-1997	**.0095**
Looking for a part time job	-.00	Subsidised temporary job before July 97	-.16
Unemployment-status: full-time UE	**.21**	Begin of programme in 1998 (month)	**.047**
in part time employment	**-.53**	Size of town (previous employment) > 200.000	**.28**
Desired = previous occupation, 2-digit level	-.05	Region of placement office in rural area	.00
Previous occupation: construction	-.09	Region (reference category: Zurich) West	**.51**
textiles	**.41**	Eastern	**.25**
architecture, engineer	**.27**	Central	**.55**
restaurants	**-.15**	South-west	**.20**
entrepreneurs, senior officials, justice	.08	North-west	.13
office and computer	**.16**	Ticino	.15
retail trade	-.02	Additional regional effects by canton: Geneva	**-.44**
science	.27	Solothurn	*-.18*
Previous sector: public services	*.16*	St. Gall	**-.60**

Note: Binary probit model estimated on the subsample of participants in TEMP and EP. Dependent variable is participation in EP. *N = 7742*. Value of log-likelihood function: - 3870.
    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.

Another interesting question with respect to unemployment duration is the timing in the selection into nonparticipants and temporary jobs. We have argued that it may be preferable not to accept a temporary job while unemployment duration is short. Subsidised temporary jobs are usually below the skills of the unemployed, and having a temporary job too early may be a negative signal, particularly for skilled unemployed. Indeed we find that for short unemployment durations the

probability of Subsidised temporary jobs is significantly lower compared to nonparticipation (see Appendix B, Table B.1). The results concerning skills are not as clear cut. When measuring skills by previous position (*management etc*), qualification, and previous earnings (*above CHF 5000*) the results correspond to our expectation that highly skilled unemployed have higher probabilities of nonparticipation in any programme. But measuring skills by chances to find a job indicates a higher probability of nonparticipation for the unemployed with low chances to find a job.

6   Econometrics

The prototypical model of the microeconometric evaluation literature with multiple treatments is the following: An individual chooses between several states, like participation in a training pro-gramme or non-participation in such a programme. The potential participant in a programme will get an hypothetical outcome (e.g. earnings) in both states. This model is based on the binary potential outcome (the Roy (1951) - Rubin (1974) model) extended by Imbens (2000) and Lechner (2001) to multiple, mutually exclusive states. Here, we consider outcomes of four differ-ent states denoted by $\{Y^0, Y^1, Y^2, Y^3\}$. The different *states* will to be called *treatments* in the fol-lowing to stick to the terminology of that literature. For any individual, only one component of $\{Y^0, Y^1, Y^2, Y^3\}$ is observable. Participation in a particular treatment m is indicated by the realisa-tion 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:

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

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

$\gamma_0^{m,l}$ denotes the expected (average) effect of treatment m relative to treatment l for a participant drawn randomly from the population.[13] Note that both average treatment effects are symmetric in the sense that $\gamma_0^{m,l} = -\gamma_0^{l,m}$. $\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of participants in treatment m only. Note that if the participants in treatments m and l

differ in a way that is related to the distribution of *X*, and if the treatment effects vary with *X*, then $\theta_0^{m,l} \neq -\theta_0^{l,m}$, i.e. the treatment effects on the treated are not symmetric.

6.2   Identification

The average causal treatment effect is generally not identified so that additional (plausible) as-sumptions are needed. We already made clear above that the data is so rich, that it seems plausi-ble that we can observe all important factors that jointly influence labour market outcomes and the process selecting people into the four different states.[14] Therefore, we assume that treatment participation and treatment outcome is independent conditional on a set of (observable) attributes (conditional independence assumption, CIA). Imbens (2000) and Lechner (2001) consider identi-fication under CIA in the model with multiple treatments. CIA defined to be valid in a subspace χ of the attribute space is formalised in expression (3):

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

This assumption requires the researcher to observe all characteristics that jointly influence the outcomes as well as the selection into the treatments. In addition it is required that all individuals in that subspace could participate in all states (i.e. $0 < P(S = m \,|\, X = x)$, $\forall m = 0, ..., 3$, $\forall x \in \chi$).

Equation (3) postulates that conditional on the observable attributes there remains no systematic selection on unobservables. In other word there are no exogenous variables left out that are both correlated with potential outcomes and the participation decision. What would be such candidates causing selection bias in our application? Candidates for these unobservables include variables like motivation, ability and personal appearance. We believe that our unusually informative data allows us to capture the 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

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[13] 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*.

judgement by the placement officer. This judgement is based on interviews and the impressions the placement officer receives through these interviews. We believe that this variable captures characteristics like motivation and personal appearance that are usually unobservable. We are confident that after controlling for this wealth of information there is little unobserved heterogeneity left that is systematically correlated with labour market outcomes and programme participation. For detailed arguments about identification the reader is referred to Gerfin and Lechner (2000).

6.3   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 on the marginal probabilities of the states conditional on *X*, denoted by $[P^0(X), P^1(X), P^2(X), P^3(X)]$.[15] Based on this insight Lechner (2001, 2002a, b) proposes and applies different matching estimators for that problem. Here we use the version implemented in the paper by Gerfin and Lechner (2000). The exact matching algorithm used is given in Table 3.

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[14] Here we use the same setting as in Gerfin and Lechner (2000), but with an extended sample. In Gerfin and Lechner (2000) we argue extensively that CIA is plausible in our setting. For reasons of brevity we refer the interested reader to that paper.

[15] In fact 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 (2000).

Table 3: 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	Estimate the respective (counterfactual) expectations of the outcome variables. For a given value of *m* and *l* the following steps are performed: a) Choose one observation in the subsample defined by participation in *m* and delete it from that pool. b) Find an observation in the subsample of participants in *l* that is as close as possible to the one chosen in step a) in terms of $[\hat{P}_N^m(x), \hat{P}_N^l(x)]$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again. c) Repeat a) and b) until no participant in *m* is left. d) Using the matched comparison group formed in c), compute the respective conditional expectation by the sample mean. Note that the same observations may appear more than once in that group.
Step 4	Repeat Step 3 for all combinations of *m* and *l*.
Step 5	Compute the estimate of the treatment effects using the results of Step 4 by means in matched samples.

Note: Lechner (2001) suggests an estimator of the asymptotic standard errors for $\hat{\gamma}_N^{m,l}$ and $\hat{\theta}_N^{m,l}$ based on the approximation that the estimation of the probabilities in Step 1 can be ignored.

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.[16] Otherwise the estimator will give biased results (see Heckman, Ichimura, Smith, Todd, 1998). In total the common step criteria discarded only about 3.5% of the observations (see Appendix C for details).

As a third remark with respect to the matching algorithm outlined in Table 3 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

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[16] 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.

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.

7   Empirical estimates of the effects

7.1   *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 available to develop indicators that proxy these objectives. One outcome variable (*seeking a job)* is based on the information whether somebody is registered with the labour office as job seeker. The problem with this otherwise 'natural' outcome variable is that subsidised temporary jobs can increase the maximum duration of receipt of unemployment benefits. 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.[17] Therefore, we compute a more reliable outcome variable called *employed* from the social security data by using information whether there are payments from employment that can be related to a particular month. In addition we create a couple of variables that measure the quality of employment by various means (employment for at least 3 months, monthly earnings larger than CHF 3000, earnings larger than cost of living given the number of dependent member in the household and region, earnings at least 90% of earnings in the previous job). The employment indicators including earnings information all impose that employment has a minimum duration of 3 months. Furthermore, as a crude proxy for productivity that is available to the economy, we include gross earnings from employment, coded as zero when an individual is not employed. Tables A.2 and A.3 in Appendix A describe the outcome variables more precisely and show some descriptive results.

We start to measure the effects of a programme in the month after the programme started (with simulated beginning dates for nonparticipants, see Section 4.b). In case an individual is informed

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[17] However, insured earnings drop to the lower level determined by the earnings received in the temporary jobs.

about having to attend a programme prior to the month of the actual start we use this month as start date, because it seems plausible that a programme already has an effect on individual behaviour when an individual knows that she will participate. Furthermore, focusing on the begin of the programme rules out that programmes appear to be successful, 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 it for employment (of 'good' quality) immediately after it started.

For programmes starting in January 1998 we measure outcome variables for 23 months (2/1998-12/1999). However, since the evaluated programmes may start between Jan. 1998 and Dec. 1998, only 12 months are observable for everybody. However, a large share of the programmes started in the first quarter of 1998, so 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 with the economy wide unemployment rate falling from 5.0% in December 1997 (share of unemployment spells longer than 1 year: 33%) to 2.5% in December 1999 (share of unemployment spells longer than 1 year: 21%).

7.2   Mean effects of programmes for their participants

Table 4 shows the mean of the outcomes in the various groups, the estimated counterfactual expectations as well as pair-wise comparisons between the subsidy programmes as well as the programmes and nonparticipation. For the sake of brevity COURSES are omitted from this table because their effects are not central to this paper. Furthermore, in the table we concentrate on only two outcome variables:, employment during at least 3 consecutive months generating on average more than 90% of previous monthly earnings (a proxy for a somewhat stable job not much inferior to the job before unemployment); and average monthly earnings. For these outcomes we present results 3, 9, 15 and 21 months after the begin of the programme. 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 outcome 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 column (8) to column (7) reveals the magnitude of the selection bias corrected by the estimation procedure. These comparisons confirm that the unemployed in the employment programmes have the worst labour market perspective 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 deduced. 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 4: Estimates of effects

Out-come	Month after begin	Sample size		$E(Y^1\|$ $S=1)$	$E(Y^1\|$ $S=0)$	$E(Y^0\|$ $S=1)$	$E(Y^0\|$ $S=0)$	$\hat{\theta}_N^{1,0}$	Std. err.	$-\hat{\theta}_N^{0,1}$	Std. err.	$\hat{\gamma}_N^{1,0}$	Std. err.
		1	0										
(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	5353	2110	23	19	12	11	**11**	1.4	**8**	1.5	**9**	1.3
in %	9	5353	2110	36	31	25	20	**11**	1.8	**11**	1.8	**10**	1.6
	15	5018	1911	47	40	41	35	**6**	2.2	**5**	1.9	*5*	1.9
	21	3066	789	51	47	46	42	5	3.2	4	2.8	*4*	2.6
EARN	3			1160	925	620	542	**540**	57	**385**	65	**479**	53
in	9			1833	1617	1283	1040	**550**	80	**577**	77	**525**	71
CHF	15			2483	2135	2103	1810	**380**	96	**325**	88	**327**	83
	21			2705	2328	2379	2196	*326*	154	172	132	219	120
Subsidised temporary job (1) compared to nonparticipation (0)													
EWEL	3	5353	5230	23	24	27	22	**-4**	1.2	2	1.2	-2	1.0
in %	9	5353	5230	36	33	34	29	2	1.3	4	1.3	**5**	1.2
	15	5018	5114	47	42	41	36	**6**	1.4	**6**	1.4	**8**	1.2
	21	3066	3946	51	46	46	39	**5**	1.7	**7**	1.7	**8**	1.5
EARN	3			1160	1090	1257	1160	-97	41	-70	54	-14	34
in	9			1833	1796	1764	1573	69	65	**196**	60	**207**	54
CHF	15			2483	2293	2211	1996	**272**	70	**297**	64	**352**	58
	21			2705	2521	2530	2186	*175*	82	**335**	78	**335**	71
Employment programme (1) compared to nonparticipation (0)													
EWEL	3	2110	5230	11	12	15	22	-4	1.7	**-10**	1.5	**-10**	1.3
in %	9	2110	5230	20	25	23	29	-3	1.9	-4	1.9	**-6**	1.6
	15	1911	5114	35	40	29	36	**6**	2.1	4	2.4	3	1.9
	21	789	3946	42	45	35	39	7	2.8	6	3.0	4	2.6
EARN	3			542	586	780	1160	**-238**	79	**-574**	64	**-572**	55
in	9			1040	1303	1142	1573	-102	91	**-270**	90	**-318**	73
CHF	15			1810	1963	1595	1996	*215*	102	-33	105	25	85
	21			2196	2447	1856	2187	*340*	137	258	139	115	120

Note: EWEL: Employed for at least 3 months with average earnings more than 90% of previous earnings. EARN: Monthly gross earnings in employment with a minimum duration of 3 months.
Results are based on matched samples. **Bold** numbers indicate significance at the 1% level (2-sided test), numbers in *italics* indicate significance at the 5% level. Standard errors are computed according to the asymptotic approximation given in Lechner (2001). The results for COURSES are available on request from the authors.

The estimated effects confirm the previous findings in Gerfin and Lechner (2000) 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 plus 6%-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 CHF. The comparison of 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 % points. However, even for the population of participants in employment programmes it would have been more beneficial to enter TEMP instead.

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. Earnings computed that way (Table 5) indicate that the mean earnings level is roughly CHF 5000. 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 that the one already corrected for) due to different groups of unemployment 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 an another small estimated quantity (between 0 and 1) may result in estimates that are not precise enough.

Table 5: Average potential earnings for those who are employed

| | Potential outcome | TEMP | TEMP | TEMP | EP | EP | EP | NONP | NONP | NONP |
| | Population | TEMP | EP | NONP | TEMP | EP | NONP | TEMP | EP | NONP |
Out-come	month after begin	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
EARN	3	5043	4868	4541	5166	4927	4883	4655	5200	5272
in	9	5091	5216	5442	5132	5200	5212	5188	4965	5424
CHF	15	5282	5337	5459	5129	5171	4907	5392	5500	5544
	21	5303	4953	5480	5179	5228	5437	5500	5302	5606

Note: Estimated mean earnings divided by estimated employment probability.

Although Table 4 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.

Figures 1 and Figures 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 six different outcome variables. A line above zero indicates that TEMP has a positive employment effect relative to the programme associated with that particular line. Only effects significant at the 5% level are displayed. Note that the results are not symmetric across these figures because the population for which the effect is defined differs.

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* reveals that TEMP dominates the two other programmes. In the beginning the effect is even above 10%-points but declines subsequently and stabilises around 5 to 7% points after 15 months. This shape may be related to difference in the duration of the programmes. As expected there is an initial negative effect compared to nonparticipation but it disappears after about half a year and becomes significantly positive after 9 months. Again the effect seems to stabilise at a difference in employment rates of about 5 to 7% points.

An alternative outcome measure called *searching for a job* (and registered with the labour office) does reveal the same dynamic patterns but appears to draw a much more negative picture in the sense that TEMP does not dominate any of the other programmes after 16 months and is always dominated by nonparticipation. We conjecture that is because of particular feature of the Swiss unemployment insurance system already mentioned: Participation in TEMP increases the entitlement period for unemployment benefits. Therefore, this variable is not an appropriate measure for the success of the programmes in the labour market, at least with respect to TEMP. However, this result indicates that TEMP has an unwanted effect generated by the possibility to prolong the entitlement period.

The dynamics in the *earnings* variable more or less mirror the dynamics in the employment variable. The remaining three variables combine earnings and employment information. The continuous increase of the effects of TEMP measured by the indicator *earnings without employment loss* strongly suggests that the quality of jobs generated by TEMP is not worse than for any other programme. Combined with the result for the outcome *seeking a Job* these findings suggest that participants in TEMP fall into to groups: those who find stable employment and those who remain unemployed and probably continue to participate 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. At the moment the data do not allow to analyse this possibility. From a policy perspective it is an important finding that a programme subsidising

below qualification jobs does not necessarily lead to unemployed subsequently searching and accepting low quality jobs.

Finally, when considering the two outcome variables that take only jobs into account that pay either more than CHF 3000 or a pay above some level of earnings necessary to cover basic needs (depending on location, family size, etc.), the results for the employment variable are confirmed to a large extent.[18] In conclusion, the results strongly suggest that from the point of view of its participants TEMP is on average highly successful in improving their position in the labour market.

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[18] The level of CHF 3000 is motivated by a debate about minimum wages in Switzerland.

*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
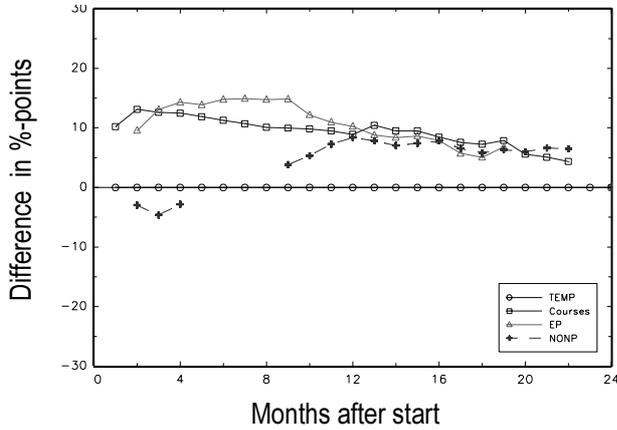
Fig. 1b: Searching for a job
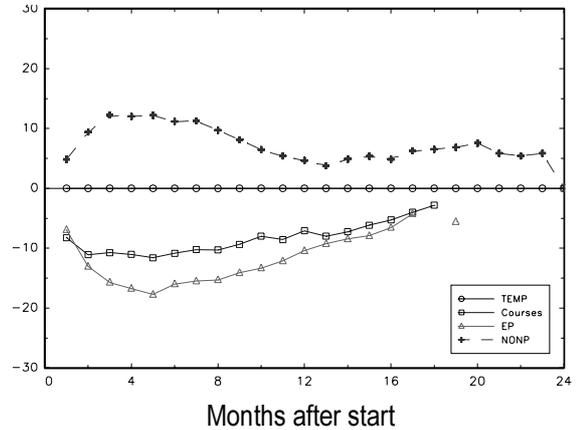
Fig. 1c: Earnings in employment > 3 months
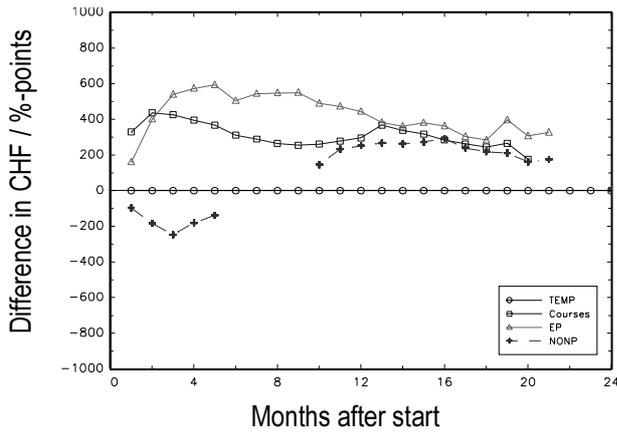
Fig. 1d: Employment without earnings loss
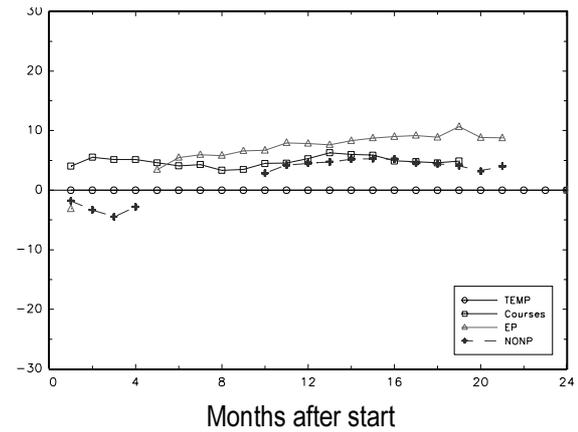
Fig. 1e: Earnings > CHF 3000
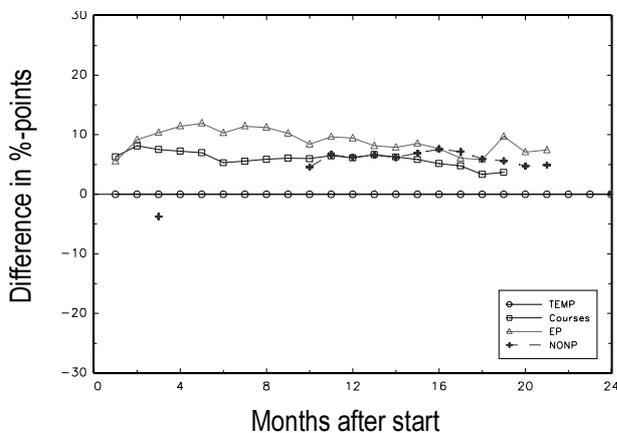
Fig. 1f: Earnings above equivalence earnings
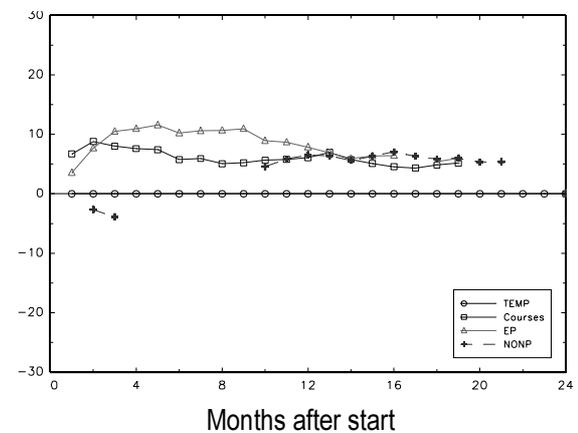
Note: NONP: Nonparticipation; EP Employment programme; TEMP: Subsidised temporary job. Start dates for nonparticipants
        are simulated. Only estimated effects that are significant at the 5% level are reported.

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The previous comparison between EP and TEMP for TEMP participants seems also to be confirmed when considering the population of participants in EP (Figure 2). One difference appears to be that in the medium run EP may not be so harmful on average for its own participants as it was found to be for the participants in TEMP.

An interesting feature appears with respect to the comparison of EP with NONP. It seems that at least in the longer run there is some evidence for a positive effect of EP after about a year. Although these effects are not significant all the time, the fact that some of them exist for all outcome variables (with the exception of the variable *searching for a job*) does indicate that EP may have some positive medium run effect compared to nonparticipation. Another feature when comparing EP to nonparticipation is that there is a positive effect of EP in the very first months of the programme. Informal interviews we conducted with various representatives of the labour offices suggest that this effect may be due to the threat of having to 'work' in EP instead of just receiving unemployment benefits (remember that the start of the programme is coded as the month in which the individual gets notified about compulsory participation in the respective programme). This threat may lead some unemployed to deregister as well as to increased job search in order to avoid actual participation in the programme.

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

Fig. 2a: Employment with duration > 3 months
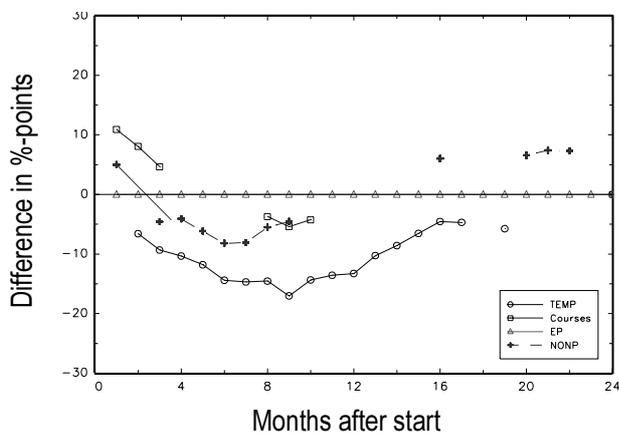
Fig. 2b: Searching for a job
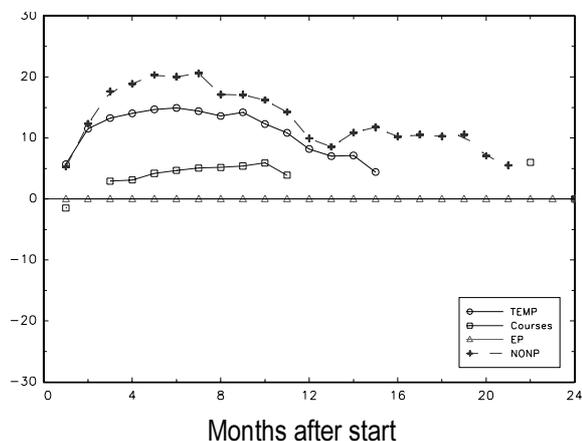

Fig. 2c: Earnings in employment > 3 months
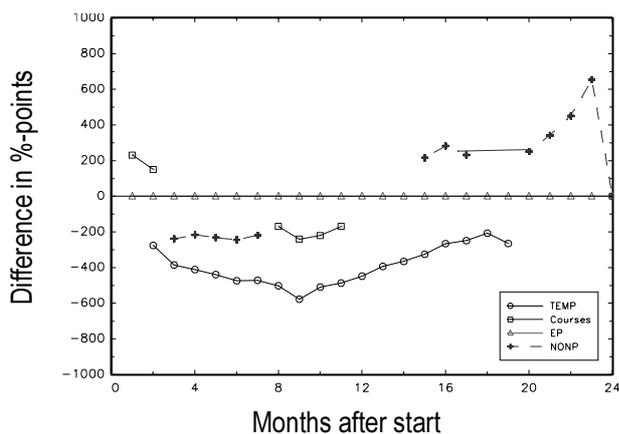
Fig. 2d: Employment without earnings loss
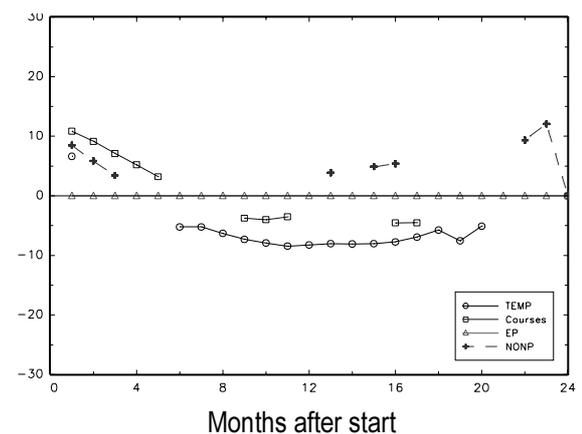

Fig. 2e: Earnings > CHF 3000
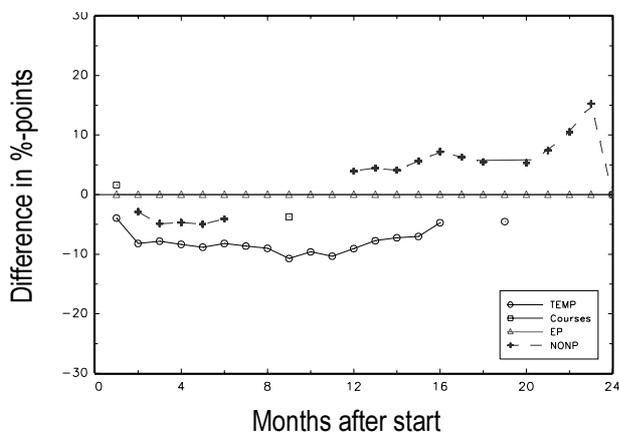
Fig. 2f: Earnings above equivalence earnings
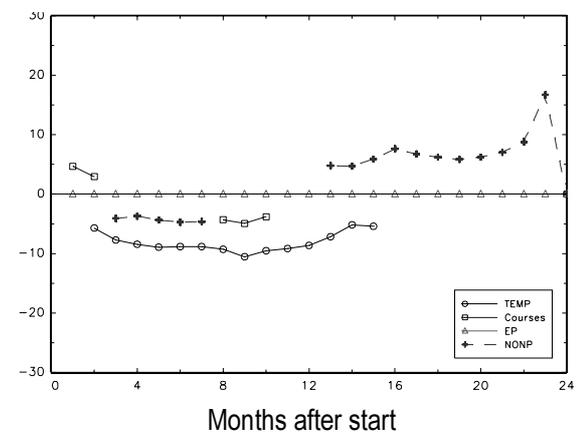
Note: See note below Figure 1.

In summary, the results presented above confirm that on average TEMP is superior to the other programmes as well as to nonparticipation. This finding already appeared in Gerfin and Lechner (2000) 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 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.

7.3 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. The hypotheses are based on the assumption that any human capital generated by the programmes is a replacement of lost human capital due to ongoing unemployment.[19]

Heterogeneity with respect to unemployment duration

One implication of this assumption is 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. Therefore, Figure 3 shows the effects of TEMP and EP for unemployed with less than 180 days of unemployment before entering the programmes (upper panel) as well as for unemployed with more than 270 days of unemployment. The results are compatible with our hypothesis in the sense that the effects of both programmes compared to nonparticipation are significantly larger for the subpopulation with longer previous unemployment.[20]

Furthermore, observing a positive effect of TEMP compared to EP for those who enter the programmes early is exactly what is expected under the signalling explanation, given the assumption that for these unemployed the programmes do not generate human capital. On the other hand, the fact that relatively large negative effects of employment programmes appear compared to NONP for the short-term unemployed might indeed point to significant stigma effects. However, it could

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[19] As already noted we also implicitly assume that the effects of lost job offers and of stopping the depreciation of human capital while in the programme more or less cancels.

[20] 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.

also be that the loss of time for job search in EP compared to NONP is not made up by a positive signal or a positive human capital effect.

Figure 3: Dynamics of average effects (ATE) for employment without earnings loss – *UE duration*

Compared to TEMP   *Compared to EP*

UP TO 180 DAYS UNEMPLOYED BEFORE START OF PROGRAMME

MORE THAN 270 DAYS UNEMPLOYED BEFORE START OF PROGRAMME
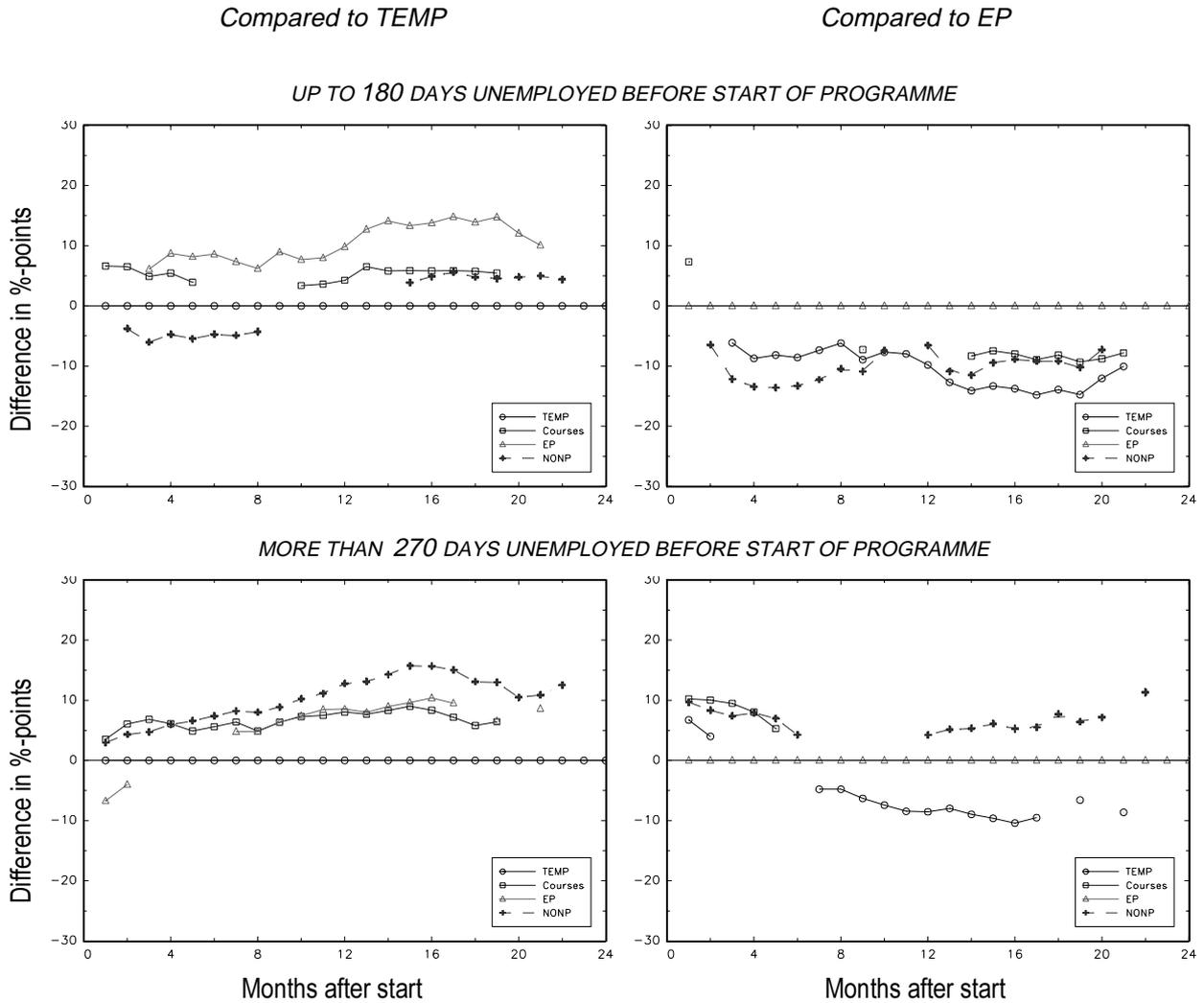
Note: Sample sizes TEMP / courses / EP / NONP for UE < 270: 3710 / 3585 / 923 / 3623. Sample sizes TEMP / courses / EP / NONP for UE > 270: 1904 / 1877 / 1205 / 1841. See note below Figure 1.

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 pro-grammes. 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 hardly any differences between the results based on these two measures. Hence, we concentrate on skill measured by the chance to find a job which is shown in Figure 4.[21] The upper panel refers to the skilled unemployed. The results with respect to nonparticipation strongly support our hypothesis. There is no effect with respect to nonparticipation for either programme (except a negative effect for EP for some months), but also no effect between the two programmes of interest. For low skill unemployed (lower panel in Figure 4) a completely different picture arises for the comparison of TEMP with nonparticipation. For these unemployed finding a subsidised temporary job is better than nonparticipation almost throughout the evaluation period. On the other end, participation in EP does not increase employment chances (nor does it decrease them).

Summarising, the results of this section appear to support our hypotheses about the effects of the programmes with respect to unemployment duration and skill level of the unemployed.

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[21] The figures for effect heterogeneity by previous earnings are available on request.

Figure 4: Dynamics of average effects (ATE) for employment without earnings loss – *Employ-ability*

Compared to TEMP   Compared to EP

HIGH EMPLOYABILITY (0, 1, 2)

LOW EMPLOYABILITY (4, 5)
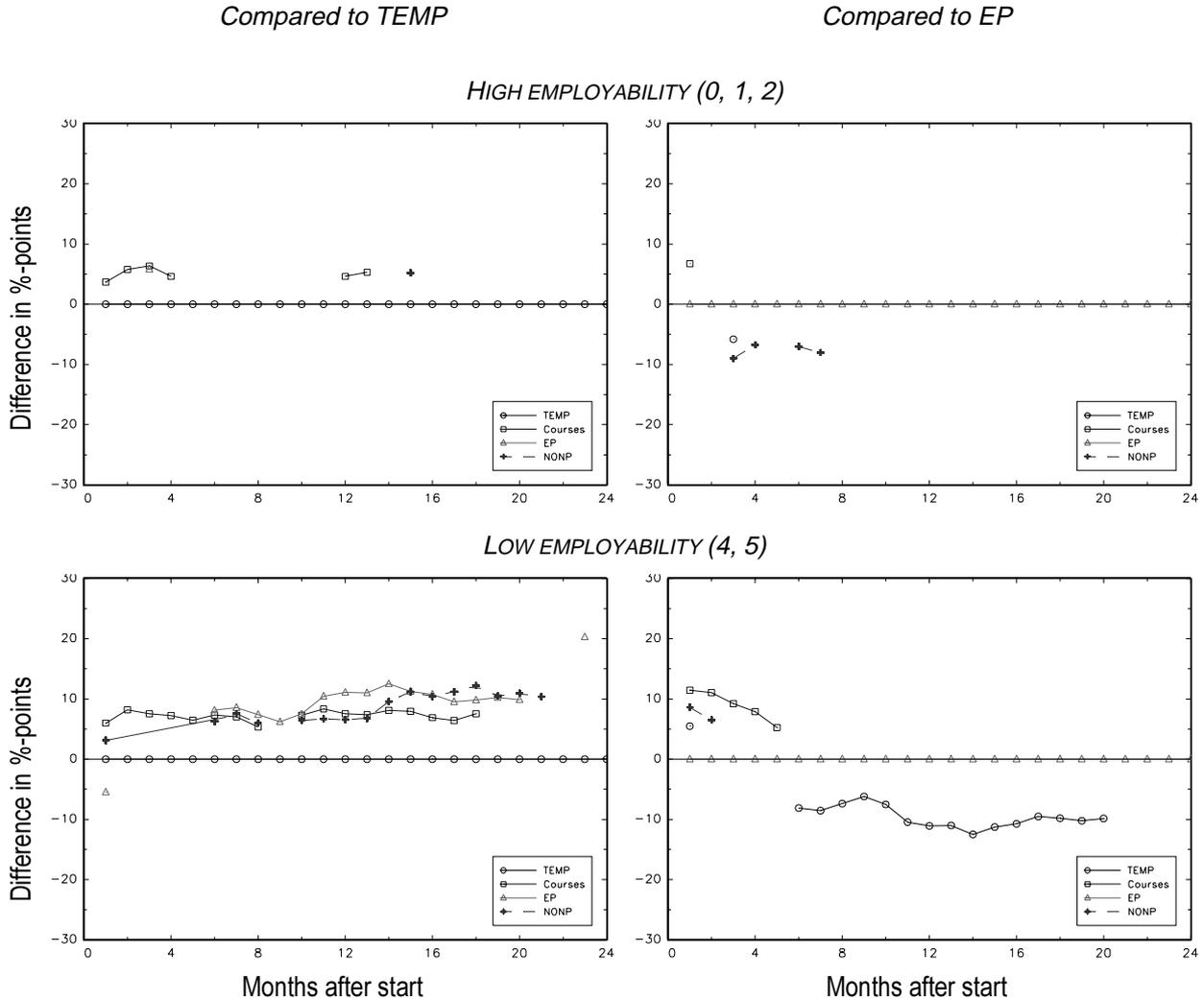
Note: Sample sizes TEMP / courses / EP / NONP for high employability: 1744 / 1271 / 491 / 1499. Sample sizes TEMP / courses / EP / NONP for low employability: 723 / 1011 / 506 / 999. See note below Figure 1.

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. The same holds true for effect heterogeneity between women and men. Finally, considering effect heterogeneity between skilled (50% of the unemployed) 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).

8   Conclusion

Subsidised employment is one tool 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 allows us to hold constant the economic environment. 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 programme 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. The effects are particularly strong for those who might be characterised as 'bad risks' in the labour market. About a year and a half after the begin of the programme, on average TEMP generates additional employment for the unemployed of about 9%-points for its participants compared to EP. Even for the participants in EP, TEMP would have generated on average of about 6%-points additional employment if they had participated in TEMP instead of EP. Although on average the gains generated by the programmes (if any) compared to nonparticipation (NONP) are tiny, we find large gains compared to NONP for the long term unemployed (TEMP: + 13%; EP: + 8%). Combining all evidence for different groups of unemployed we generally find the programmes much less successful (if at all) 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 also dismiss any concerns that the gains in the chances to find a job are due to 'low-quality' jobs taken up by TEMP participants.

While searching for possible explanations for our findings, the fairly systematic heterogeneity of effects between different groups of unemployed let us conclude that there is some 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), but again this effect appears to be much stronger for TEMP. However, the reader should be aware that the evidence in favour or against one or the other competing hypothesis 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 economic considerations about possible effects.

One obvious policy conclusion is that sending people who have good chances on the labour anyway in these programmes is probably a waste of money. Furthermore, it also 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 superior to TEMP. Finally, the unemployment insurance legislation requires that cantons fulfil a regional quota of programme places they have to supply (of which EP is a part, but not TEMP). 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.

However, all these conclusion have to be taken with some caution, because they focus on the unemployed, and thus ignore that there are indirect costs of both schemes. The more immediate costs concern alternative uses of Swiss taxpayers' money:[22] In this respect, TEMP is apparently cheaper for the budget and thus again preferable. However, 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 at this issue. Furthermore, again extending the sample in the future would allow to dig dipper into the issue of effect heterogeneity without having to rely on extrapolations coming from parametric functional assumptions on the outcome equation.

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[22] We do not have in mind subsidising bankrupt airlines.

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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.

For a randomly chosen subsample (30'433 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 Gerfin and Lechner (2000).

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**
Personal characteristics:				
- not disabled				
- previous earnings > SFr. 1000				
- 24 < age < 56				
- information about mother tongue is not missing				
- Swiss or yearly or permanent working permit				
- seasonal workers, home workers, students deleted				
- not unemployed on December 31, 1997				
- no more than 1 year unemployed				
remaining observations	**6'499**	**5'871**	**2'286**	**10'098**
Programme experience criteria:				
- no programme with duration > 14 days in 1997				
- no employment programme in 1997				
- programme does not start on January 1, 1998 (continuing programme)				
remaining observations	**5'614**	**5'462**	**2'128**	**9'441**
Nonparticipants being not unemployed at their simulated starting date of programme				
final sample (18'668 obs.)	**5'614**	**5'462**	**2'128**	**5'464**

Note: The full sample selection process is documented in Gerfin and Lechner (2000). Here we display only the steps that use (slightly) different selection rules.

Table A.2: Description of outcome variables

a	*Employment with duration > 3 months*	Person has an earnings spell of at least 3 successive months (dummy variable)
b	*Searching for a job*	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)
c	*Earnings in employment > 3 months*	Smoothed monthly earnings in CHF in an employment spell with duration of at least 3 months
d	*Employment without earnings loss*	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)
e	*Earnings > 3000 CHF*	Person earns more than 3000 CHF during at least 3 successive months. 3000 CHF is a minimum wage claim of the unions in Switzerland (dummy variable)
f	*Earnings above equivalence earnings*	We compute an equivalence income for each person which depends on the number of dependent persons in the household and the region where she/he lives. This variable indicates earnings in a spell of at least 3 months are above 90% of this equivalence income (dummy variable)

Table A.3: Descriptive statistics

Variable		Subsidised temporary job	Courses	Employ-ment pro-gramme	Nonpar-ticipation
	number of observations	*5614*	*5449*	*2128*	*5464*
		Mean / share in %			
Age in years		37.4	37.9	38.7	37.7
Female		42	47	38	43
Marital status single		28	26	29	26
Female and married		23	29	22	27
Female and mother tongue not German/French/Italian		13	19	15	15
Female and mother tongue Italian		5	4	3	4
Mother tongue German		36	34	32	32
Mother tongue French		19	16	18	20
Mother tongue Italian		12	9	11	11
Mother tongue not German/French/Italian		34	38	39	37
German mother tongue and living in 'German language' canton		33	32	30	29
French mother tongue and living in 'French language' canton		17	17	17	19
Italian mother tongue and living in 'Italian language' canton		4	2	6	4
Nationality*:*	Foreign with permanent permit	30	28	29	31
	Foreign with yearly permit	15	17	18	15
	Swiss	55	55	54	53
Foreign Languages:	Other Swiss language	64	66	66	64
	English, Spanish, Portuguese	13	16	10	15
Job position	very low	36	38	47	36
	high (management, etc.)	5	7	3	7
Qualification level:	skilled (highest)	55	56	49	55
	semiskilled	16	15	17	15
	unskilled (lowest)	28	29	35	30
Chances to find a job	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	1	2	4	2
Looking for ... job	part time	12	13	10	14
Unemployment-status	full-time	81	83	85	82
	part time	12	14	12	15
	in part employment	4	1	1	2
Desired = previous occupation, 2-digit level		76	71	70	75
Previous occupation:	construction	10	15	8	8
	textiles	1	2	2	1
	architecture, engineer	2	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	19	13	15
	retail trade	7	10	7	9
	science	1	2	1	2
	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	17	10	12	14
	public services	6	8	10	9
	education	2	1	2	2
	industry unemployment rate in %, 1/98	6.7	6.3	6.3	6.6
Monthly earnings in last job in CHF		3952	3958	3655	3915
Monthly earnings in last job > 5000 CHF		20	22	14	21
Average earnings in CHF 1993 / 1992		3040	2990	2750	3040
Average earnings in CHF 1997 / 1996		2820	2680	2250	2520
Nondecreasing earnings path before 1997		11	11	8	8
Current unemployment spell is first spell		55	65	64	58
Positive number of sanction days without benefit payment (current spell)		22	25	25	28
Duration of unemployment spell at beginning of programme		220	228	301	217
Unemployment benefits in 1995		3240	3260	4370	3450
in 1996		3360	2610	3160	4240
in 1997		11010	12810	13750	14450
Number of UE spells 1997-1993		0.33	0.33	0.43	0.37
1992-1988		1.24	0.93	1.21	1.19
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		3.56	3.07	3.52	3.55
Mean duration of previous unemployment spells in months		5.20	6.14	6.19	5.88
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)		1.77	1.84	2.08	1.84
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		6	8	6	5
Subsidised temporary job before July 97		3	1	1	1
Begin of programme in 1998 (month)		3.72	3.38	5.02	2.77
Size of town (previous employment) > 200.000		6	10	8	8
Region of placement office (reference categories: large city) rural		20	18	18	15
middle town		42	39	46	37
Region (reference category: Zurich) West		28	22	22	31
Eastern		10	11	7	9
Central		5	8	7	5
South-west		21	17	29	15
North-west		11	14	9	9
Ticino		8	4	9	10
Additional cantons Aargau		5	8	4	4
Basel-City		3	4	3	4
Geneva		7	6	4	12
Graubünden		1	2	1	1
Neuenburg		3	1	6	3
Solothurn		4	3	5	2
St. Gall		5	5	2	5
Waadt		13	13	12	14
Zug		1	1	1	1

Table A.3 to be continued

Table A.3 continued - 2

Variable		Subsidised temporary job	Courses	Employment programme	Nonparticipation
		Mean / share in %			
Employment with duration > 3 months	1998/04	21	9	9	22
	1998/08	41	25	21	40
	1998/12	42	32	25	37
	1999/04	58	46	41	50
	1999/08	70	58	54	57
	1999/12	66	61	59	53
Searching for a job	1998/04	80	91	95	76
	1998/08	58	73	83	50
	1998/12	53	63	74	46
	1999/04	38	47	56	33
	1999/08	23	30	35	21
	1999/12	24	21	23	20
Earnings in employment > 3 months	1998/04	743	352	248	824
	1998/08	1483	973	642	1497
	1998/12	1511	1259	804	1416
	1999/04	2161	1807	1323	1862
	1999/08	2605	2189	1788	2097
	1999/12	2398	2247	1848	1921
Employment without fall in earnings	1998/04	11	6	7	14
	1998/08	23	16	15	25
	1998/12	23	20	16	22
	1999/04	36	30	24	30
	1999/08	43	35	30	34
	1999/12	40	35	29	30
Earnings > CHF 3000	1998/04	14	6	4	15
	1998/08	28	18	11	27
	1998/12	29	23	15	26
	1999/04	40	32	23	32
	1999/08	48	38	33	36
	1999/12	44	39	32	32
Earnings above equivalence earnings	1998/04	15	7	5	16
	1998/08	29	18	13	28
	1998/12	29	23	16	25
	1999/04	41	33	26	33
	1999/08	49	40	35	37
	1999/12	45	40	35	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.[23] Although being fully parametric, the MNP is a

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

flexible version of a discrete choice model, because it does not require the **I**ndependence of **I**r-relevant **A**lternatives assumption to hold.

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

Variable			Courses	Employment programme	Non-participation
Age in years / 10			**.06**	**.22**	.04
Female			**-.14**	**-.40**	**-.33**
Marital status single			**.10**	**.26**	*.096*
Female and married			.07	.015	**.24**
Female and mother tongue not German/French/Italian			**.17**	0	0
Female and mother tongue Italian			0	**-.43**	0
Mother tongue not German/French/Italian			.04	*.16*	*.11*
German mother tongue and living in 'German language' canton			**-.29**	.07	-.04
French mother tongue and living in 'French language' canton			**.15**	-.03	-.11
Italian mother tongue and living in 'Italian language' canton			.01	*.42*	-*.29*
Nationality*:*	Foreign with permanent permit		**-.10**	-.17	.03
	Foreign with yearly permit		-.03	-.05	.01
Foreign Languages:	Other Swiss language		*.070*	-.00	-.10
	English, Spanish, Portuguese		**.19**	-.09	-.00
Job position	very low		.03	**.21**	.03
	high (management, etc.)		0	0	**.25**
Qualification level:	skilled (highest)		.05	.00	*.12*
	unskilled (lowest)		.05	.02	*.091*
Chances to find a job (reference category: medium):		no information	**-.14**	**-.29**	-.05
		very easy	-.03	-.15	.11
		easy	**-.11**	*-.12*	-.03
		difficult	**.13**	**.21**	**.20**
		special case	*.16*	**.42**	**.46**
Looking for ... job		part time	.08	-.01	**.23**
Unemployment-status (reference category: part-time):		full-time	*.11*	**.27**	.03
		in part employment	**-.90**	**-.79**	**-.87**
Desired = previous occupation, 2-digit level			**-.08**	-.04	.02
Previous occupation:	construction		*-.10*	-.04	.08
	textiles		**.29**	*.42*	*.27*
	architecture, engineer		**.44**	.19	-.12
	transportation		**-.22**	0	0
	restaurants		-.00	-.17	.08
	entrepreneurs, senior officials, justice		**.36**	.05	**.40**
	office and computer		**.41**	**.19**	**.17**
	retail trade		**.33**	-.05	**.17**
	science		*.23*	*.36*	**.48**
	news and communication		**.43**	0	0
Previous sector:	construction		**-.15**	0	0
	public services		.07	**.20**	.07
	education		**-.25**	0	0
	industry unemployment rate in %, 1/98		-.07	*-.15*	-.08
Monthly earnings in last job in CHF /1000			.00	**-.11**	**-.11**
Monthly earnings in last job > 5000 CHF			0	0	**.14**
Average earnings in CHF	1993 / 1992 /1000		.00	.01	**.029**
Average earnings in CHF	1997 / 1996		-.02	**-.098**	-.02
Nondecreasing earnings path before 1997			*-.086*	-.14	-.18

Table B.1 to be continued.

Table B.1 continued

Variable			Courses	Employment programme	Non-participation
Current unemployment spell is first spell			**.13**	**.20**	.05
Positive number of sanction days without benefit payment (current spell)			.02	*.09*	**.22**
Duration of unemployment spell at beginning of programme			**-.60**	.29	.98
Duration of unemployment spell at beginning of programme		< 3 months	**-.25**	**-.40**	**.18**
		< 6 months	*.079*	**-.32**	*.11*
Unemployment benefits	in 1995		**.0064**	**.011**	-.00
	in 1996		**-.0074**	-.006	**.010**
	in 1997		**.0040**	**.008**	**.013**
Number of UE spells	1997-1993		**-.038**	*.041*	**.066**
	1992-1988		**.075**	*.082*	-.01
Month of entry into social security system			**.44**	.21	.15
Immigrant 1992 or later			.08	.09	-.04
Number of employment spells 1988-1997			**-.03**	-.014	.01
Mean duration of unemployment spells in months			**.76**	0	0
Self-employment 1992 or later			*.11*	-.02	**.25**
Out of labour force 1992 or later			**.12**	.09	*.12*
Months out of labour force '88-'97 (minimum duration of 6 months for each spell(			.00	*.011*	-.00
Months out of labour force (month of entry – months UE – months employed)			*.0033*	0	**.0039**
Previous programme participation : Sum of short programs 7-12/1997			**-.18**	0	0
Subsidised temporary job before July 97			*.095*	*-.30*	**-.44**
Begin of programme in 1998 (month)			*-.016*	**.061**	**-.15**
Size of town (previous employment) > 200.000			**.12**	**.30**	*.16*
Region of placement office (reference categories: large city)		rural	**-.12**	-.02	**-.14**
		middle town	0	0	**-.12**
Region (reference category: Zurich)	West		**-.13**	**.69**	*-.16*
	Eastern		.05	*.27*	*-.16*
	Central		**.45**	**.60**	-.04
	South-west		**-.55**	.31	**-.29**
	North-west		*-.13*	.10	**-.28**
	Ticino		**-.46**	**.47**	**.60**
Additional regional effects by canton	Aargau		**.41**	0	0
	Basel-City		0	0	*.24*
	Geneva		0	**-.58**	**.76**
	Graubünden		**.23**	0	0
	Neuenburg		0	0	**.49**
	Solothurn		0	-.21	**-.31**
	St. Gall		0	**-.76**	0
	Waadt		.27	0	**.45**
	Zug		-.79	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 = 18668*. Value of log-likelihood function: - 22347.8.
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.

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

	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	-	.8	4.4	-.4	-.1	-.1	-.4
Courses			1.6	-	-.3	-	-.1	-
EP					1.1	-	.03	-
Nonparticipation							1.0	-
Correlation matrix [a)] x 100								
TEMP	100		62		-34		-10	
Courses			100		-21		-6	
EP					100		-3	
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 and D are available on request from the authors