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Does Subsidised Temporary Employment Get the Unemployed Back to Work? An Econometric Analysis of Two Different Schemes

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An Econometric Analysis of Two Different Schemes

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October 2002

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
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ABSTRACT

Does Subsidised Temporary Employment Get the Unemployed Back to Work? An Econometric Analysis of Two Different Schemes*

Subsidised employment is an important tool of active labour market policies to improve the chances of the unemployed to find permanent employment. Using informative individual administrative data 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 one 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 EP. We also find that compared to nonparticipation both programmes are ineffective for unemployed who find job easily anyway as well as for those with short unemployment duration. For unemployed with potentially long unemployment duration and for actual long term unemployed, both programmes may have positive effects, but the effect of TEMP is much larger.

JEL Classification: J38, J68

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

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1 Introduction

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

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 may 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 operating in 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. Typical European 'employment programmes' can be subsumed under the latter category. Finally, the actual direct cost of programme participation to individuals, the unemployment insurance system, and society as a whole may also differ substantially.

The different schemes 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 not created by firms because both have no incentives to do so. The reasons could be unemployment and welfare benefits or minimum employment costs (minimum wages, unionised sector, or other restrictions increasing wage costs), 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 and comparative empirical research has been devoted to analyse the different effects of different types of employment subsidies on those who receive the subsidy.[1] 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 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

[1] An exception are experimental studies in the USA which compared wage subsidies payed to the employee and the employer, respectively (Burtless, 1985, Woodbury and Spiegelman, 1987) and two recent studies of the Swedish active labour market policy (Carling and Richardson, 2001, and Sianesi, 2002)

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 usually diverse active labour market policy a particular country runs.

In fact Switzerland can be used to study the differences between two versions of employment subsidies that are both operated on a larger scale and both are targeted at more or less the same population of unemployed. Both programmes use subsidised temporary employment to increase the reemployment chances of their participants. The crucial difference is that one programme operates as a non-profit employment programme, whereas the other one subsidises temporary jobs in firms operating in a competitive environment. Furthermore, in the Swiss case a large and informative individual data base (coming from various administrative registers) is available that was previously used by Gerfin and Lechner (2002, GL hereafter) for a microeconometric evaluation study of several active labour market policies.[2]

Although not of primary interest in their study, GL already note a substantial difference between the effects of the two types of temporary employment subsidies. Recent evaluation studies of the Swedish active labour market policies, for example, also draw the general conclusion that programmes most closely attached to a 'real' job in a competitive environment dominate other programmes (Carling and Richardson, 2001, and Sianesi, 2001). Sweden appears to be the only country running a similar mix of programmes, including employment programmes and temporarily subsidised jobs. The main difference between the subsidised jobs programmes is that these jobs are expected to become permanent in Sweden, while in Switzerland they are expected to be temporary. Obviously, the Swedish programme is much more likely to have substantial direct crowding out effects with respect to unsubsidised employment

There are several other 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? Do these 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 considerations 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.

In the empirical part we investigate the effects of the programmes on subsequent employment chances for and earnings in unsubsidised jobs. Our empirical findings strongly confirm the positive effects of the subsidy for temporary jobs (TEMP) as compared to the employment programme (EP) type of the subsidy. We

[2] Here, the data used by GL is updated. It contains additional variables and it covers a longer period of time.

can also dismiss the concern that the positive effects are due to the participants of TEMP taking up inferior, i.e. lower paying jobs. Furthermore, we find that both programmes are more effective in raising reemployment probabilities for the unemployed having great difficulties in the labour market. However, even for the 'better risks' TEMP seems to have some, albeit smaller 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 effects play an important role in explaining our results as well. In terms of direct costs of the programmes TEMP is much less costly than EP (and not participating in any programme), which adds to the positive assessment of TEMP.

The next section describes the Swiss programmes in some depth. Section 3 rather informally develops our theoretical ideas 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 a 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 a multinomial probit framework. Appendix C describes the extent of the common support problem and our remedies. Appendix D adds results concerning the subgroup heterogeneity of the effects.

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

As already noted subsidised employment can take many forms. Switzerland uses two different types of subsidies to foster reintegration of the unemployment into the labour market. Therefore to understand the effects and the composition of the group of participants in these programmes it is necessary to understand 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 condi-

tional on a previous contribution to the unemployment insurance for at least 6 months within the past two years. After the two year entitlement period expires, receiving a new entitlement period is conditional on being employed for at least 12 months within three years after the end of the previous unemployment spell. The replacement ratio is usually 80% of the insured earnings, depending on socio-demographic characteristics.[3] The maximum monthly benefit is about CHF 7000.

Switzerland runs a substantial and diverse active labour market policy.[4] 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.

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.[5] There are two different types of programmes: either it is a single position, i.e. a special job in a public organisation (e.g. administration or hospitals), or it is a collective programme. Collective programmes are carried out by specialised non-profit organisations. The jobs should be as similar as possible to regular employment, but they should be *extraordinary*, i.e. the organisers of employment programmes should not be in competition with other firms. However, in practice some organising firms may operate on the same market as other private firms with comparable products (e.g. in the repair and restoration sector). Collective employment programmes are regulated by the cantonal unemployment offices in consultation with the employer and the employee organisations.[6] 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.

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

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

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

Unemployed are placed in employment programmes by the labour office. Given the placement decision participation is compulsory. Interviews we conducted at the placement offices strongly suggest that in a considerable number of cases case workers use employment programmes as a test for the willingness to work. While participating in an EP the unemployed has to continue job search and must accept any *suitable* job offer (a job would not be considered *suitable* if it pays less than current unemployment benefits, the working conditions are unacceptable, or if the workplace is too far away from home). Formally, the organiser of the employment programme acts as the employer and the participant as an employee (but the organiser cannot "hire" the employees, they are selected by the placement office). Duration of the programme (usually 6 months), the wage and the social security contributions are regulated in a temporary work contract between the organiser and the worker. In particular, the organiser has to send a monthly payroll account to both the employee and the placement office. The 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 rules and the design of the Swiss employment programmes closely follow the rules of the employment programmes in Germany. 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 by overcompensating the difference with additional payments from the UI system. The income generated by this scheme is larger than unemployment benefits in case of not accepting the temporary job.[7] 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

[6] 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.

[7] 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.

spells would lead to a drop in income). Mean duration of these temporary jobs is roughly 4 months, but there is considerable variation. The wording of the law regulating TEMP is not very specific. Rehiring laid-off workers in TEMP jobs by the same firm is usually not possible. But using TEMP as a subsidised screening device for firms is not ruled out and obviously sometimes endorsed by the placement offices in order to improve job matches. However, TEMP jobs are not explicitly expected to become permanent after the subsidy runs out.[8]

Although TEMP is not part of the ALMP, roughly 20% (1998) of the unemployed participated at some point in TEMP. Bauer, Baumann, and Künzi (1999) report that only about 20% of the jobs in TEMP are arranged by the placement office. Employer and employee have a regular temporary work contract defining the conditions of the job (mainly duration of the contract, wage and contributions to future pensions). The wage cannot be below the 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 to increase the flexibility of their work force, or TEMP might be used to avoid the wage levels set in collective wage bargaining agreements. However, so far there appears to be no evidence of abuse of TEMP in these respects.

Arrangements like TEMP subsidising jobs (i) within firms competing in the market and (ii) that are not expected 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 is the kind of job and the work experience they generate. Ignoring any potential market distortions and assuming that EP does not produce public goods to a considerable extent, then from the point of view of the tax payer EP is more expensive than TEMP. An interesting question we look at with our data is whether these programmes are sys-

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

tematically 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 schemes 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). In the simple competitive model wage subsidies increase employment. The size of the employment effect depends on the supply and demand elasticities, but not on who receives the subsidy. One common result found in many empirical papers (e.g. Burtless, 1985, Woodbury and Spiegelman, 1987, Katz, 1996) is that a wage subsidy paid to the employer does not have positive effects on the employment chances of the target group. Burtless (1985) evaluates a short-lived wage subsidy experiment in Dayton, Ohio, and finds that "job seekers given experimental vouchers identifying them to employers for a generous wage subsidy were significantly less likely to find employment than were job seekers without vouchers" (p. 105). Burtless speculates that the vouchers had a stigmatising effect and provided a screening device for the employers. Similar results are presented by Katz (1996) in an evaluation of the Targeted Jobs Tax Credit. Woodbury and Spiegelman (1987) evaluate an experiment in Illinois. In this experiment one experimental group received a $500 bonus if they found a job within 11 weeks and held that job for at least four months (claimant experiment). Another experimental group consisted of those where the employer received the $500 bonus if he hired an eligible claimant and employed that claimant for at least four months (employer experiment). A main result is that "offering unemployed workers the option of assigning the bonus bonuses to employers who hired them had a far smaller [but still positive] effect on employment than offering the unemployed workers the same bonus directly" (p. 529). There are several reasons why the results differ. First, the target groups in these programmes were different: Burtless and Katz evaluate programmes for disadvantaged workers, whereas the programme analysed by Woodbury and Spiegelman is aimed at unemployed workers. Woodbury and Spiegelman argue that unemployed workers may seem more attractive to potential employers than disadvantaged workers. Second, the required employment duration is relatively short in the Illinois experiment (4 months) which might help in creating positive effects. Evaluations of the largest employee-based wage subsidy in the USA, the Earned Income Tax Credit, unambiguously show positive

effects on labour market participation (see Hotz and Scholz, 2000, for a summary of the empirical evidence).

However, in the present context the standard theory of wage subsidies is not applicable. It is important to recognise that the Swiss subsidies are for temporary jobs only, intended as stepping stones to permanent employment with another employer. Individuals in the subsidised temporary employment programmes are still unemployed in the sense of receiving benefits similar to unemployment benefits and of being registered as job seekers. Only after leaving unemployment into non-subsidised employment they are considered employed. This is different compared to the standard model of wage subsidies which is static and only considers permanent jobs. A dynamic extension of that model 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 in the programme.[9] The model implies a long-run effect of a short-run subsidy. By getting some low skilled workers into temporary jobs their productivities are raised and so are their chances of moving out of unemployment.

In our case the situation is more complicated, because there are two types of temporary employment, one found to be successful (TEMP) in GL, 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 types of human capital. Given the institutional differences it is possible that employment programmes generate human capital that is less valued by potential employers because of different work environment in the sheltered sector compared to competitive firms.

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. Because the subsidised temporary jobs are "real" jobs the employer may use this information to conclude that participants in TEMP are better in the sense of having a closer attachment to the labour market. This effect will be especially strong when the potential perma-

[9] 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 a given probability (see Bell et al., 1999).

nent job is in the same sector as the subsidised temporary job. In order to be a credible signal temporary subsidised jobs must be more costly to find for less productive workers. Since these jobs are limited and usually arranged by the unemployed themselves, hence requiring additional search efforts, this requirement appears to be fulfilled.

Signalling may also occur in terms of stigma effects. Suppose employment programmes are stigmatised in the sense that there is 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 subsidised jobs programme, turning the stigma effect into a self-fulfilling expectation. There is some (anecdotal) evidence that employment programmes indeed carry the described stigma. Of course, in this case the question arises why not all unemployed try to get a subsidised temporary job. The answer is the same as above: finding a subsidised temporary job is more costly for less productive unemployed.

Since firms know that workers hired in the subsidised temporary job programme are unemployed there is also a possible stigma attached to this programme, given the US evidence on employer-based wage subsidies.[10] However, given that the subsidised jobs are strictly temporary, the costs of hiring an unproductive worker are not as high as for permanent jobs. For this reason we do not think that the subsidised temporary jobs carry the described stigma. By contrast, firms may even use this programme as a subsidised screening device.

The literature on signalling 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 in case of job loss it may be better to become unemployed than to take up a low-skill job. Taking up a low-skill ("lousy") job may be seen as a bad signal by future 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.

[10] Of course, the temporary subsidised jobs are employee-based. But the stigma effect found in the US experiments stem from the firms' knowledge about the subsidy, not from the receipt of the subsidy.

These models are not directly applicable to our setting since we concentrate on the pool of unemployed workers who try to find the optimal strategy to find permanent employment. A simple model could be sketched as follows. Assume that there are two types of unemployed, skilled und unskilled, and two time periods, short term (period 1) and long term unemployed (period 2). Assume further that the unskilled will profit from programme participation in both periods (either in terms of human capital accumulation or in terms of aquiring a positive signal) with a given probability, whereas the skilled unemployed only profit in period 2 when some of their skills have depreciated, making them observationally identical to the unskilled. In this setting the unskilled have an incentive to enter the programme as early as possible. The skilled, however, would not profit from programme participation in the first period.[11] Therefore, we would expect a positive effect of programme participation for the unskilled in the first period and no effect for the skilled in the first period. If we do find an effect it must be due to signalling, given the assumptions of the model. For the unemployed in period 2 programme participation should have a positive effect compared to nonparticipation independent on their previous skill level.

In this simple model it is not possible to disentangle human capital and signalling effects. Of course, it is possible that programmes both enhance productivity and embody signals. Hence, it is not possible to derive strict tests for the signalling effect. Signalling may be tested by checking whether the importance of the type of programme is declining as firms obtain more information on workers' productivity (see e.g. Albrecht and van Ours, 2001, for a test along this line). In the present context, it is not clear how a similar test can be derived. Nevertheless, we can think of hypotheses about effect heterogeneity for different groups of unemployed that are plausible under some explanations and not plausible under others. Examining the empirical evidence for these hypotheses may indicate some answers to the question why programmes have different effects.

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 unemployment) is balanced by the lock-in effect.[12] Consider the expected effects of employment programmes and subsidised temporary employment compared to nonparticipation for unemployed with a short unemployment spell. For those, we

[11] There is also a search-theoretic explanation why skilled unemployed do not want to enter programmes early on. For these unemployed the distribution of wage offers is such that the expected wage is larger than the wage received in either programme.

[12] 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.

would expect the human capital effects of the programme to be negligible.[13] Hence if a programme has any effect with respect to nonparticipation it should 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. 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 be 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.[14]

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

[13] 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.

[14] This is also compatible with the argumentation is Albrecht and van Ours (2001). When the productivity of an unemployed is uncertain (e.g. because of long-term unemployment) the programme may carry an important signal. If the firm is more certain about the productivity, the signal becomes less important.

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

How will these considerations affect the composition of the participants in both programmes? In fact, it seems that nobody has an incentive to get into an employment programme (except for case workers in order to fulfil their quota, 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.

For firms the main incentive to offer subsidised temporary jobs is to have subsidised cheap labour input. The programme also offers a way to adjust the work force in the short term. Since the jobs are strictly temporary the risk of getting an unproductive worker is not as important as in the case of permanent jobs. Some firms may also use the programme as a screening device with the intention to hire the best participants afterwards.

4 Data and descriptive statistics

4.1 Data base

Our empirical analysis is based on two matched sources of administrative data that have already been used by GL. 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.

Compared to GL there is an 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 register. Now, we measure employment by the entries in the social security data. Hence, we construct variables measuring the quality of employment in terms of earnings and to some extent job duration. This allows us to address the question whether specific programmes, subsidised temporary jobs in particular, lead to types of employment that may be of lower "quality" than the employment prior to the current unemployment spell. Furthermore, we evaluate the effects on earnings per se. Given the new data we evaluate the effects up to 24 months after the programmes start. More details on the data can be found in Appendix A and in GL.

4.2 The definition of programmes used in the empirical analysis

We differentiate only four groups of ALMP participation states 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 GL, because our earlier study found no systematic differences of the effects of these two similar forms of employment programmes. The third programme category covers participants in subsidised temporary jobs, and the final (comparison) group consists of those who did not participate in any major programme between January and December 1998.[15] A major programme is

[15] 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.

defined as having a duration of at least two weeks. Following the arguments in GL we evaluate only the *first* major programme starting between January and December 1998 (see that paper for details).[16]

For the group of nonparticipants important time varying variables like 'unemployment duration prior to the programme' are not defined. To make meaningful comparisons to those unemployed entering a programme, we use an approach suggested in Lechner (2002b): For each nonparticipant a hypothetical programme starting date is predicted by relevant information available in Dec, 1997. Persons with predicted starting date later than their actual exit date from unemployment are excluded from the data set.

4.3 The sample

We apply a series of sample selection rules to the data. Full details are given in Appendix A.1. The most important selection criteria are that we consider only individuals unemployed (without any other part-time job) on Dec 31, 1997 with an unemployment spell at that time of less than 12 months who have not participated in any major programme in 1997 and who are between 25 and 55 years old. The reasons for these selection criteria are that given the two-year entitlement period we want to make sure that there is 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 18'354 observations. Table 1 displays selected descriptive statistics of some important variables differentiated by programme status.[17] For more descriptive statistics the interested reader is referred to Table A.3 in Appendix A.

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 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 subsidised job, but lower for participants in employment programmes. Somewhat surprisingly is the relatively high employ-

[16] 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.

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

ment 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.

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

Group		Obs.	Chances to find a job		Qualification	Unemployment duration before programme	Earnings before unemployment	Earnings Dec 1999 (if employed)	Employed 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)	5365	23	13	1.74	222	3970	3629	66
Courses		5421	18	18	1.73	229	3960	3685	61
Employment programmes	(EP)	2107	18	24	1.87	303	3660	3127	59
Nonparticipation	(NONP)	5461	22	18	1.73	218	3950	3697	53

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

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 comparison 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 programmes 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. The low skilled, measured by a previous job position, chances to find a job and 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 low skilled may have problems finding them and after some time the case workers allocate them to employment programmes.

There is obvious regional and occupational heterogeneity in the selection process. The federal structure of Switzerland gives the cantons a considerable degree of autonomy. They may put different emphasis on the various programmes in their local implementation of the national ALMP.

Table 2: Estimated coefficients of a binary probit model (employment programme versus subsidised temporary job)

Variable	Coeff.	Variable	Coeff.
Age in years / 10	**.17**	Average earnings in CHF 1993 - 1992 /1000	.00
Female	**-.28**	Average earnings in CHF 1997 - 1996 /1000	**-.10**
Marital status single	**.20**	Nondecreasing earnings path before 1997	-.12
Female and married	.01	Current unemployment spell is first spell	**.18**
Female and mother tongue Italian	**-.38**	Positive number of sanction days without benefit payment (current spell)	.05
Mother tongue not German/French/Italian	*.15*	Duration of UE spell at beginning of programme (days / 100)	.02
German mother tongue in 'German language' canton	.01	Duration of UE at beginning of progr. < 3 months	**-.39**
French mother tongue in 'French language' canton	-.04	< 6 months	**-.23**
Italian mother tongue in 'Italian language' canton	**.48**	Unemployment benefits/1000 in 1995	**.010**
Nationality: Foreign with permanent permit	**-.18**	in 1996	-.006
Foreign with yearly permit	-0.08	in 1997	.004
Foreign Languages: Other Swiss language	.04	Number of UE spells 1997-1993	.02
English, Spanish, Portuguese	-.01	1992-1988	**.069**
Job position very low	**.17**	Month of entry into social security system	.18
Qualification level: skilled (highest)	.02	Immigrant 1992 or later	.07
unskilled (lowest)	.02	Number of employment spells 1988-1997	**-.027**
Chances to find a job : no information	**-.21**	Self-employment 1992 or later	-.04
(reference category: medium) very easy	-.11	Out of labour force 1992 or later	.09
easy	*-.10*	Months without social security data 1988-1997	**.01**
difficult	**.21**	Months out of labour force 1988-1997	**.0095**
special case	**.34**	Subsidised temporary job before July 97	-.17
Looking for a part time job	-.01	Begin of programme in 1998 (month)	**.045**
Unemployment-status: full-time UE	**.23**	Size of town (previous employment) > 200.000	**.27**
Desired = previous occupation, 2-digit level	-.04	Region of placement office in rural area	.01
Previous occupation: construction	-.10	Region (reference category: Zurich) West	**.47**
textiles	**.42**	Eastern	**.26**
architecture, engineer	*.29*	Central	**.55**
restaurants	**-.***15*	South-west	**.14**
entrepreneurs, senior officials, justice	.09	North-west	.13
office and computer	**.15**	Ticino	.16
retail trade	-.03	Additional regional effects by canton: Geneva	**-.45**
science	.26	Solothurn	-.16
Previous sector: public services	*.14*	St. Gall	**-.59**
Industry unemployment rate in %, 1/98	*-.13*	% of people against stricter unemployment insurance law (national referendum in 1997; disaggregated at local level)	-.03
Monthly earnings in last job in CHF /1000	**-.069**		

Note: Binary probit model estimated on the subsample of participants in TEMP and EP. Dependent variable is participation in EP. *N = 7472*. Value of log-likelihood function: - 3809.
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 concerning unemployment duration is the timing of the selection into non-participants 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 unem-

ployed. Indeed we find that for short unemployment durations the probability of subsidised temporary jobs is lower compared to nonparticipation.[18] The results concerning skills are not clear cut depending on the various possible measures of the skill level.

6 Econometrics

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

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

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

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

6.2 Identification

ATE's and ATET's are generally not identified so that additional (plausible) assumptions are needed. We already made clear above that the data is so rich that it seems plausible that we can observe all important

[18] These results are available on request.

factors that jointly influence labour market outcomes and the process selecting people into the four different states. Therefore, we assume that treatment participation and treatment outcome is independent conditional on a set of (observable) attributes (conditional independence assumption, CIA). Imbens (2000) and Lechner (2001) consider identification 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):

$$Y^0, Y^1, \ldots, Y^M \coprod S | X = x, \forall x \in \chi. \qquad (3)$$

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. $0 < P(S = m \mid X = x)$, $\forall m = 0,\ldots,3$, $\forall x \in \chi$).

Equation (3) postulates that conditional on the observable attributes there remains no systematic selection on unobservables. In other words there are no exogenous variables left out that are both correlated with potential outcomes and the participation decision. Candidates for such unobservables include variables like motivation, ability, and personal appearance. Our unusually informative data allows us to capture the major effects of these unobservables. For example, motivation can be measured by sanctions imposed by the placement office as well as by the employment history in the past ten years. Unobserved ability is captured by past earnings, and specific labour-related problems can be measured by past employment profiles (repeated movement between labour market states). Of particular importance is the variable "chances to find a job", which is a subjective judgement by the placement officer. This judgement is based on interviews and the impressions the placement officer receives through these interviews in the beginning of the unemployment spell. This variable should capture characteristics like motivation and personal appearance that are usually unobservable. After controlling for this wealth of information there should be little unobserved heterogeneity left that is systematically correlated with labour market outcomes and programme participation. For detailed arguments about identification the reader is referred to GL.

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 conditional on the marginal probabilities of the states (conditional on *X*), denoted by $[P^0(X), P^1(X), P^2(X), P^3(X)]$.[20] Based on this insight Lechner (2001,

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

[20] 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 (2001).

2002a, b) proposes and applies different matching estimators for that problem. Here we use the version implemented in the paper by GL which is described in Table 3.

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

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 estimates of treatment effects using the results of Step 4 by means in matched samples.

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

A third remark about the matching algorithm 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 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

[21] This condition is also called the 'common-support requirement'. Note that if we would only be interested in pairwise 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.

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.[22] Furthermore, a programme that be generally considered a success because it increases individual employment probabilities may have an ambiguous effect on this variable. Of course it should reduce the number of job seekers by bringing them into job instead of staying unemployed. But on the other hand it may encourage them not to leave the labour force but continue search, thus increasing the number of job-seekers.

Therefore, we compute a more reliable outcome variable for programme success 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 (continuously employed for at least 3 months, earnings at least 90% of earnings in the previous job). All employment indicators that are also based on earnings information impose that employment has a minimum duration of 3 months. Furthermore, as a crude proxy for individual productivity we include gross earnings coded as zero when an individual is not employed in the regular labour market. We also compute the months of unemployment within the next 12 months to get a measure on how many months of unemployment programme participation may save within a year. Tables A.2 and A.3 in Appendix A describe the outcome variables more precisely and shows descriptive statistics.

We measure the effects of a programme in the month after the programme started (with simulated beginning dates for nonparticipants, see Section 4.2. In case an individual is known to be informed about programme participation prior to the month of the actual start of the programme we use this month of information as start date, because a programme may have an effect on individual behaviour from the very mo-

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

ment when an individual knows that she will participate. Furthermore, focusing on the begin of the programme rules out that programmes appear to be successful, just because they keep their participants busy by making them stay in the programme. We consider a programme to be most successful if everybody would leave for employment (of 'good' quality) immediately after she is informed about future participation. We must emphasise that whenever a person participates in any of the programmes she is consider as registered unemployed (and not employed) in the definition of all outcome variables.

For programmes starting in January 1998 we measure outcome variables for 23 months (2/1998-12/1999) or 12 months for the accumulated measures. However, since the evaluated programmes may start between Jan. 1998 and Dec. 1998, only 12 months of nonaccumulated outcomes are observable for everybody. 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 (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 non-participation. 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 three 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), average monthly earnings, and the number of additional unemployment over the next 12 months. For the first two outcomes we present results 3, 9, 15 and 21 months after the begin of the programme, for the third outcome that is based on accumulating effects over the 12 months ahead, we present effects 1 and 6 months after the start. Column (3) and (4) give the exact sample sizes (after imposing common support) available at each point of (process) time. Note that sample sizes fall after month 12 (the last month observed is month 24, programme participation starts between month 1 and 12). Thus, the population changes somewhat after month 12 in the sense that, for example, the estimate for month 23 is entirely based on individuals entering the programme in January 1998. Therefore, the precision of the estimates falls as well, which is reflected in the estimated standard errors increasing after month 12.

Columns (5) and (8) show the mean 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 perspectives in all potential states. From these estimates the estimated mean effects of two different states for participants in treatment 1 and 0 can be directly 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.

The estimated effects confirm the previous findings in GL 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 7-9%-points compared to EP and NONP. There does not appear to be too much variation of this effect between different populations defined by treatment status. Similarly, there is an average earnings gain after 15 months of about 300-400 CHF. 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 6-7% points. However, even for the population of participants in employment programmes it would have been more beneficial to enter TEMP instead. This view is confirmed when considering the accumulated effects: Participating in TEMP instead of EP reduces registered unemployment by about one month per year. For reasons already discussed in section 7.1 (it will also be discussed again below), in comparison to nonparticipation both programmes increase unemployment benefit duration by about 1 (TEMP) to 2 months (EP) per year in the time immediately after the start of the programme.

Table 4: Estimates of effects

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

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

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 3500. However, these num-

bers 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 would be employed

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

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

Although Table 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 4 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 populations for which the effects are defined differ.

First consider the effects of TEMP for TEMP participants (Figure 1) for the various outcome variables. The share of unemployed finding a *job with a duration of at least three months* (Fig. 1a) reveals that TEMP dominates the two other programmes. In the beginning the effect is 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 no initial significant effect compared to nonparticipation but the effect becomes significantly positive after 9 months. Again it seems to stabilise at a difference in employment rates of about 5 to 7% points.

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

Fig. 1a: Employment with duration ≥ 3 months

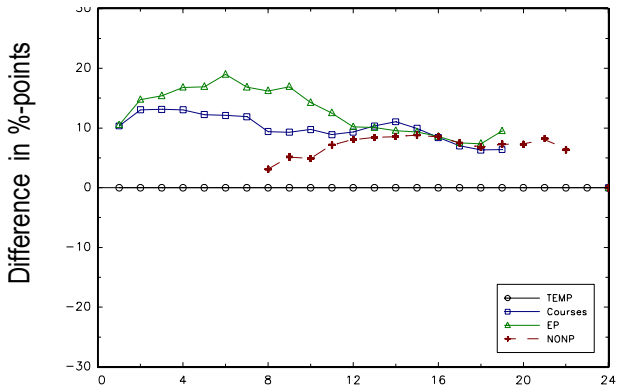


Fig. 1b: Searching for a job

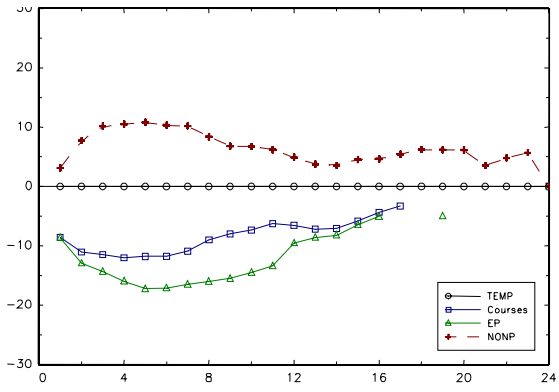


Fig. 1c: Earnings in employment ≥ 3 months

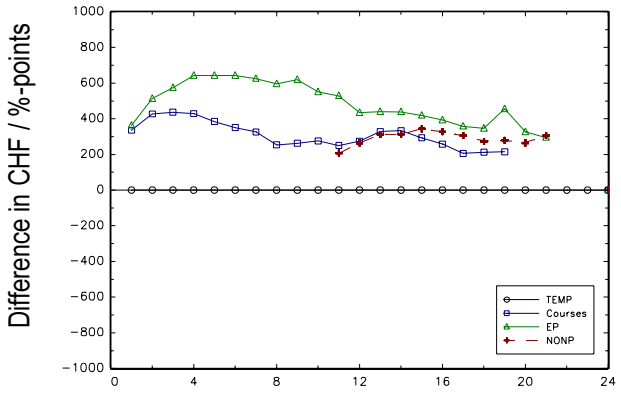
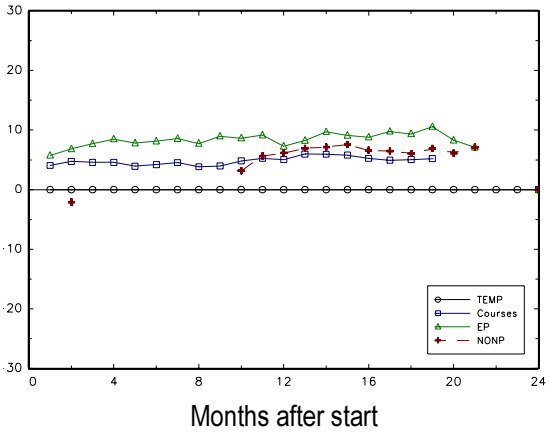


Fig. 1d: Employment without earnings loss



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

An alternative outcome measure called *searching for a job* (and registered with the labour office; Fig. 1b) does reveal the same dynamic patterns but appears to draw a much more negative picture in the sense that TEMP is always dominated by nonparticipation. We conjecture that this effect results from a particular feature of the Swiss unemployment insurance system already mentioned: Participation in TEMP increases the entitlement period for unemployment benefits and / or keeps participants in the labour force. Therefore, it has been argued above that this variable is not an appropriate measure for the success of the programmes in the labour market.

The dynamics of the *earnings* variable more or less mirror the dynamics of the employment variable. Both remaining variables combine earnings and employment information. The continuous positive 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. Note, however, that the effect with

respect to EP is much smaller compared to Figure 1.a, where we do not condition on previous earnings. Combined with the result for the outcome *searching for a job* these findings suggest that participants in TEMP fall into two groups: those who find stable employment and those who 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. The data do not allow to analyse this possibility. From a policy perspective it is an important finding that a programme (TEMP) which is subsidising below qualification jobs does not necessarily lead to unemployed subsequently searching and accepting low quality jobs.

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

Fig. 2a: Employment with duration ≥ 3 months

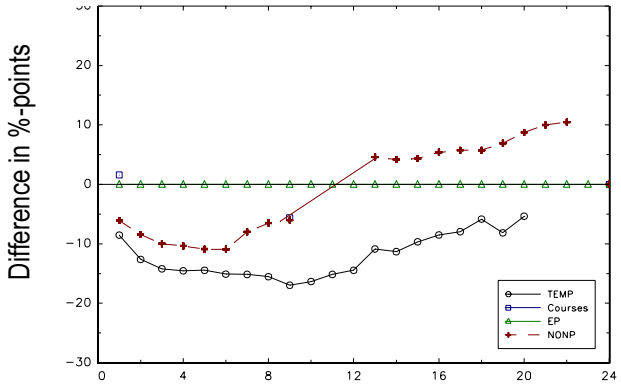


Fig. 2b: Searching for a job

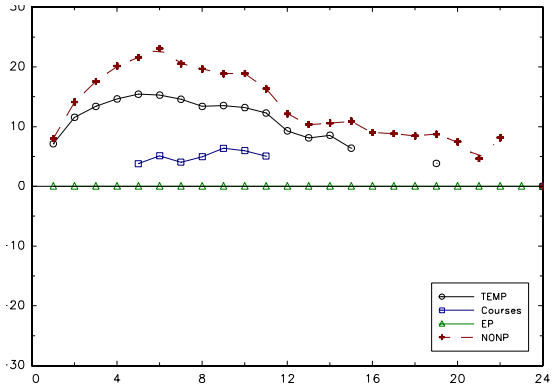


Fig. 2c: Earnings in employment ≥ 3 months

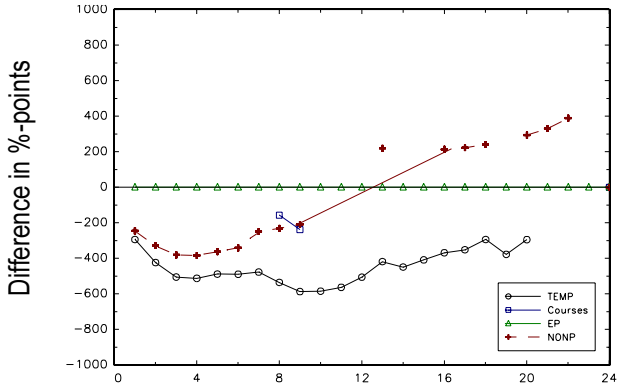
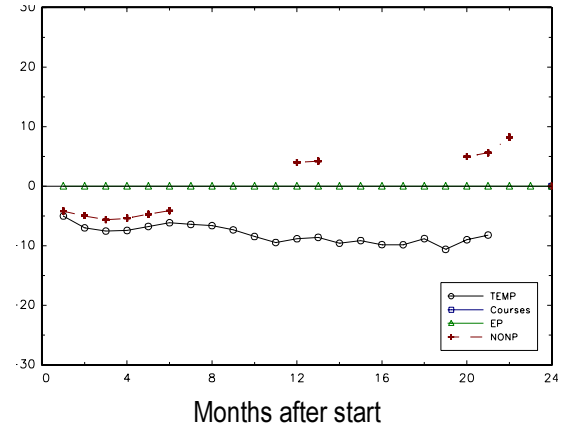


Fig. 2d: Employment without earnings loss



Note: See note below Figure 1.

The previous comparison between EP and TEMP for TEMP participants is confirmed when considering the population of participants in EP (Figure 2). An interesting feature appears with respect to the comparison of EP with NONP. At least in the longer run there is some evidence for a positive effect of EP after

about 14 months. Although these effects are not significant 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.[23]

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 GL to some extent, but they could not take into account any earnings information, and they had to rely on a time horizon of no more than 15 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.[24]

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.[25]

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 negative effects of employment programmes appear compared to NONP for the short-term unemployed might indeed point

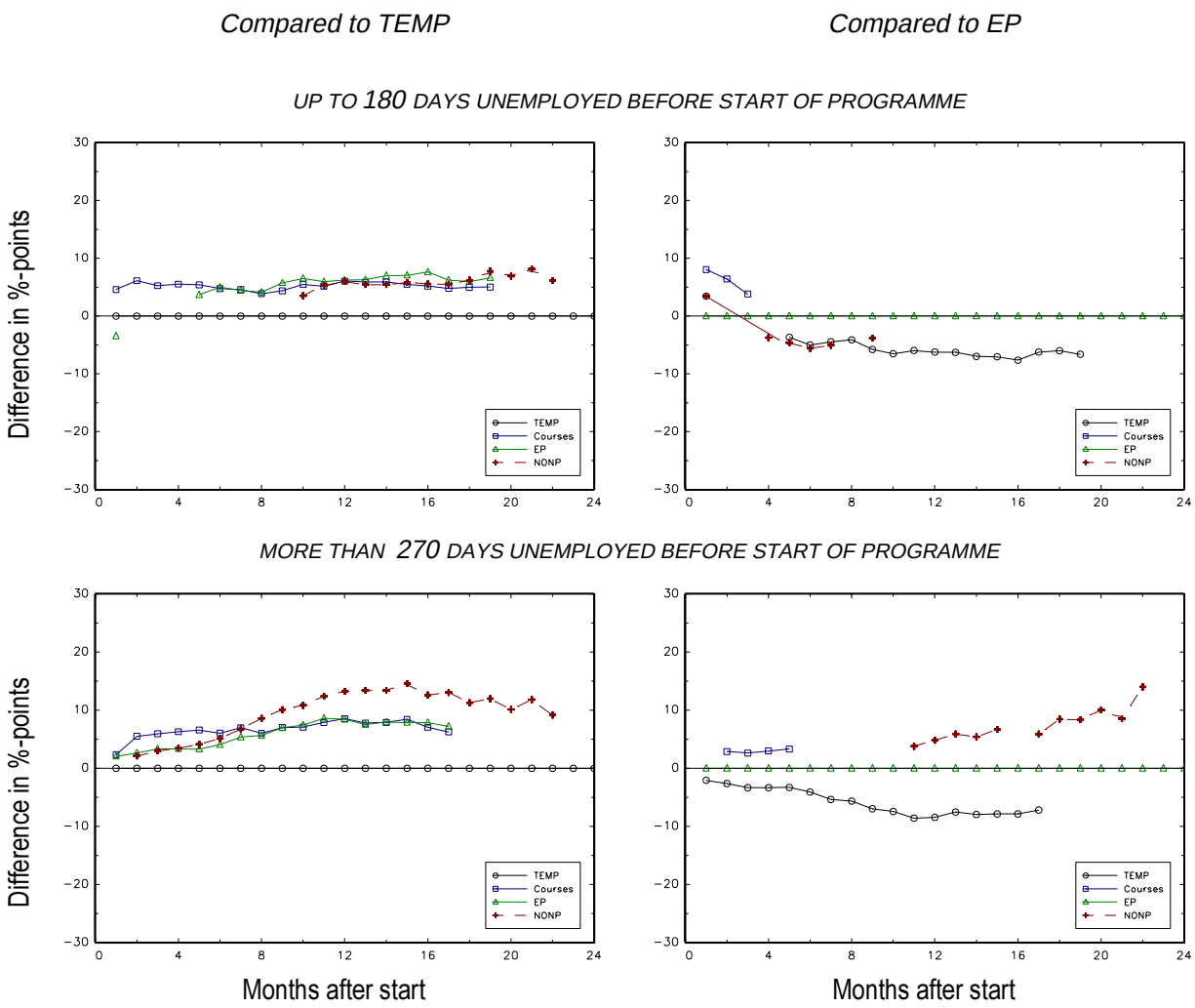
[23] These effects have not been detected by GL because their observation period ended in March 1999.

[24] 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.

[25] 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.

to some stigma effects. However, it could 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 prior to programme*



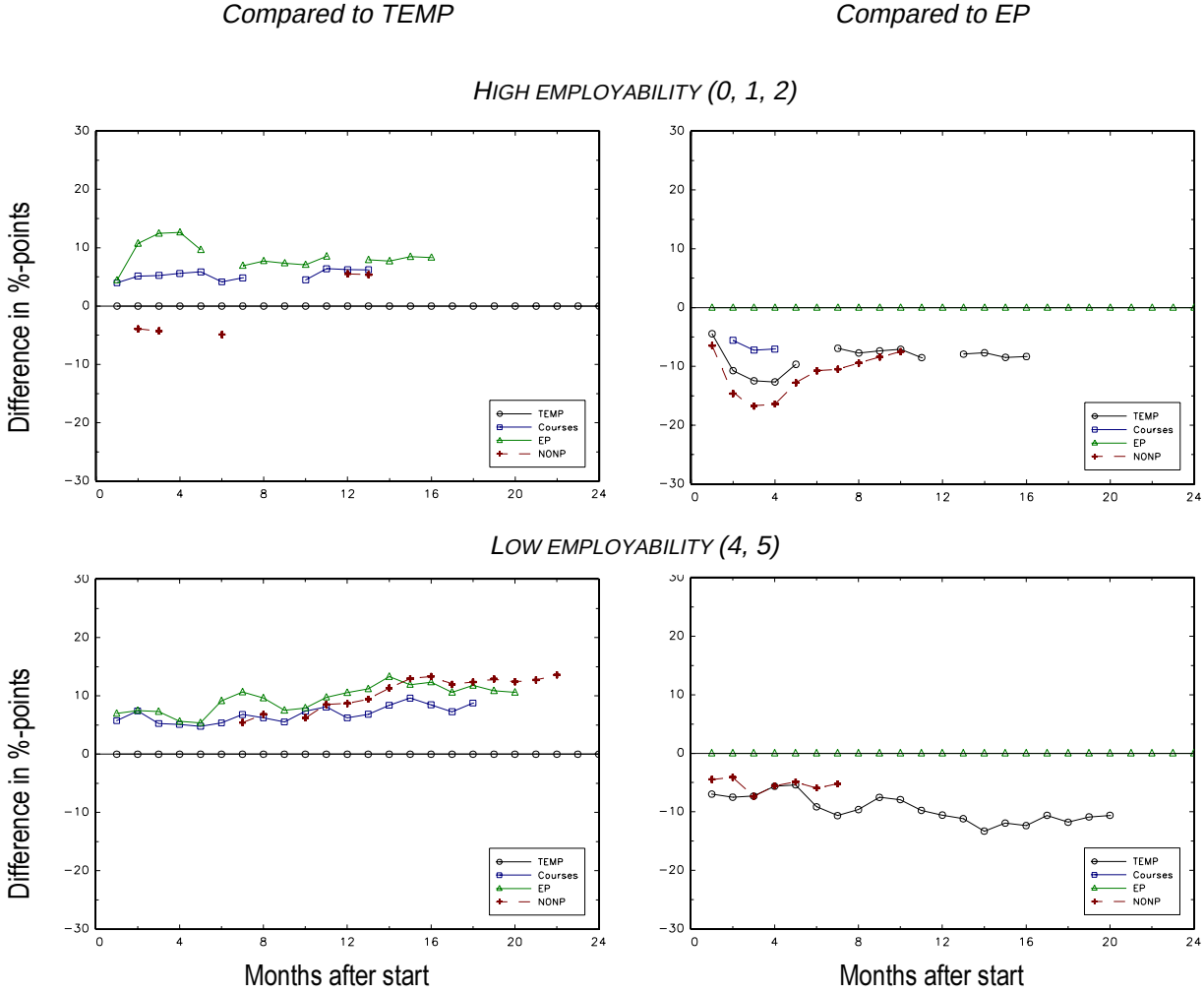
Note: Sample sizes after correcting for common support for UE < 180: TEMP / courses / EP / NONP: 2328 / 2226 / 454 / 2293. Sample sizes UE > 270: 1769 / 1794 / 1150 / 1804. See note below Figure 1.

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 programmes. Hence for these unemployed we should not find positive effects of the programmes with respect to nonparticipation. Probably the best summary measures of human capital available in our data are previous earnings and the

chances to find a job. It turns out that there no systematic differences between the results based on these two measures.

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



Note: Sample sizes after correcting for common support TEMP / courses / EP / NONP for high employability: 1480 / 1158 / 471 / 1286. Sample sizes TEMP / courses / EP / NONP for low employability: 650 / 898 / 480 / 883. See note below Figure 1.

Therefore, we concentrate on skill measured by the chance to find a job which is shown in Figure 4.[26] The upper panel refers to the unemployed with good a priori chances. The results with respect to nonparticipation support our hypothesis. There is no systematic effect with respect to nonparticipation for either programme (except a large negative early effect for EP). Also for this group TEMP is clearly superior to EP at most points in time. For unemployed with a bad priori chances (lower panel in Figure 4) a completely different picture arises for the comparison of TEMP with nonparticipation. For these unemployed finding

[26] The figures for effect heterogeneity by previous earnings are contained in Appendix D.

a subsidised temporary job is better than nonparticipation throughout the evaluation period. On the other hand, EP has a negative effect in the first seven months compared to nonparticipation, which probably is a lock-in effect, and no significant effect afterwards. The positive effect of TEMP with respect to EP is of the same magnitude as for the high-skilled group, but significant throughout.

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.

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, due to the specific definition of the skill variable in these data) and low skilled does not reveal major differences. However, the latter result probably merely reflect that this measure of skill level (unskilled, semiskilled, skilled) is too crude. This suspicion already appeared for the estimation of the participation probabilities. In these estimations the skill level is not significant (see Section 5 and Appendix B).

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

8 Conclusion

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

We find that from the point of view of the unemployed the subsidies for temporary jobs in 'normal' firms subject to market pressure (TEMP) are superior to the 'classical' employment programmes (EP) operating in areas without much private competition. About a year and a half after the begin of the programme,

TEMP generates on average 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 about 9%-points additional employment if they had participated in TEMP instead of EP. In terms of unemployment benefit duration, TEMP saves about one month per year of UE benefits immediately after the programme. We generally find the programmes much less successful for unemployed that may be characterised as the 'good-risks', whereas they tend to increase the reemployment probabilities of 'bad risks'. However, in almost all cases TEMP is more effective than EP. Finally, we 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 leads us to the conclusion 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). 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 market a priori into these programmes is not cost-effective. Furthermore, it appears to be a bad idea of using these subsidy programmes in the beginning of an unemployment spell. However, even for these groups TEMP seems to be superior to EP. In fact we find no subpopulations for which EP is systematically superior to TEMP. Finally, the unemployment insurance legislation in effect in 1998 requires that cantons fulfil a regional quota of programme places they have to fill (towards which EP counts, but TEMP participation does not). Failure to meet this requirement would have financial consequences for the cantons by reducing federal payments to the cantonal unemployment office. This may have had fairly counterproductive effects, especially given the recovery of the labour market that lead to a diminished pool of unemployed that had to be allocated to the various programmes of the ALMP.

With respect to cost-effectiveness it is obvious that the direct costs of TEMP are lower than those of EP and of nonparticipation. Given that TEMP is also the most effective programme this suggests a clear-cut policy recommendation in favour of TEMP. However, this conclusion have to be taken with some caution, because it ignores the potential indirect costs of both schemes. There are concerns that programmes like TEMP may lead to substitution of unsubsidised labour through subsidised labour, i.e. they may create unemployment. We cannot address this question with our data, but it seems unlikely that this effect is large for a temporary programme like TEMP. The set-up of the employment programmes should prevent crowding out unsubsidised firms. Again, we cannot deal with this problem with our data. Obviously, fu-

ture research should be directed towards this issue. In addition, if the success of TEMP is partly due to positive signalling effects the policy recommendation cannot be to extend the TEMP programme because this would distort the signal. Extending the sample in the future would allow to dig dipper into these issues without having to rely on extrapolations coming from parametric functional assumptions on the outcome equation.

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Appendix A: Data

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

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

Table A.1: Sample Selection Rules

	Subsidised temporary job	Courses	Employment programme	Non-participation
Initial number of observations	**7'799**	**7'305**	**2'805**	**12'524**
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 - no seasonal workers, no home workers, no students - unemployed and not working part-time Dec., 31, '97 - less than 1 year unemployed	*-300*	*-1'434*	*-519*	*-2'426*
remaining observations	**6'499**	**5'871**	**2'286**	**10'098**
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)	*-1'134*	*-450*	*-179*	*-789*
remaining observations	**5'365**	**5'421**	**2'107**	**9'309**
Nonparticipants being not unemployed at their simulated starting date of programme				*-3'848*
final sample (18'354 obs.)	**5'365**	**5'421**	**2'107**	**5'461**

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

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

Table A.2: Description of outcome variables

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	*Months of unemployment*	Months of registered unemployment in 12 months period

Table A.3: Descriptive statistics

Variable		Subsidised temporary job	Courses	Employ-ment pro-gramme	Nonpar-ticipation
	number of observations	*5'365*	*5'421*	*2'107*	*5'461*
		Mean / share in %			
Age in years		37.3	37.8	38.7	37.5
Female		41	47	38	43
Marital status single		28	26	29	27
Female and married		23	29	22	27
Female and mother tongue not German/French/Italian		13	19	15	15
Female and mother tongue Italian		4	4	3	4
Mother tongue German		36	34	32	33
Mother tongue French		19	19	18	20
Mother tongue Italian		11	9	11	11
Mother tongue not German/French/Italian		34	38	39	36
German mother tongue and living in 'German language' canton		33	32	30	29
French mother tongue and living in 'French language' canton		18	17	16	19
Italian mother tongue and living in 'Italian language' canton		4	2	6	4
Nationality*:*	Foreign with permanent permit	31	28	29	31
	Foreign with yearly permit	15	18	18	16
	Swiss	54	55	53	53
Foreign Languages:	Other Swiss language	64	65	66	64
	English, Spanish, Portuguese	13	16	10	16
Job position	very low	37	38	47	36
	high (management, etc.)	5	7	3	7
Qualification level:	skilled (highest)	55	56	48	56
	semiskilled	17	15	17	15
	unskilled (lowest)	28	29	35	29
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	2	2	4	2
Looking for ... job	part time	11	13	10	13
Unemployment-status	full-time	85	84	86	83
	part time	13	14	12	15
Desired = previous occupation, 2-digit level		75	71	70	75
Previous occupation:	construction	10	5	8	8
	textiles	1	2	2	1
	architecture, engineer	1	3	2	2
	transportation	4	2	5	4
	restaurants	17	15	14	17
	entrepreneurs, senior officials, justice	2	4	2	4
	office and computer	12	18	13	15
	retail trade	7	10	7	8
	science	1	2	1	3
	news and communication	1	1	1	1

Table A.3 to be continued.

Table A.3 continued – 1

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

Table A.3 to be continued

Table A.3 continued - 2

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

Appendix B Estimates of the multinomial probit model

Table B.1 shows the estimation results of a multinomial probit model (MNP) using simulated maximum likelihood with the GHK simulator.[27] Although being fully parametric, the MNP is a flexible version of a discrete choice model, because it does not require the **I**ndependence of **I**rrelevant **A**lternatives assumption to hold.

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

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

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

Table B.1 to be continued.

Table B.1 continued

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

Note: Simulated maximum likelihood estimates using the GHK simulator (200 draws in simulator for each observation and choice equation). Coefficients of the category TEMP are normalised to zero. All equations include a constant. Inference is based on the outer product of the gradient estimate of the covariance matrix of the coefficients ignoring simulation error. *N = 18354*. Value of log-likelihood function: - 22109.2.
Bold numbers indicate significance at the 1% level (2-sided test), numbers in *italics* relate to the 5% level.
If not stated otherwise, all information in the variables relates to the last day in December 1997.

The variables that are used in the MNP are selected by a preliminary specification search based on binary probits (each relative to the reference category SUBSIDISED TEMPORARY JOB) and score tests against

omitted variables. Entries for variables excluded from a particular choice equation show a 0 for the coefficient and '-' for the standard error. The final specification contains a varying number of mainly discrete variables that cover groups of attributes related to personal characteristics, valuations of individual skill and chances on the labour market as assessed by the placement office, previous and desired future occupations, and information related to the current and previous unemployment spell, and past employment and earnings.

In practice, some restrictions on the covariance matrix of the 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.2: Estimated covariance and correlation matrices of the error terms in the multinomial probit

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

Note: [a] 3 Cholesky factors are estimated to ensure that the covariance of the errors remains positive definite. t-values refer to the test whether the corresponding Cholesky factor is zero.

Appendix C: Common support

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

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

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

Note: Estimated probabilities in %.

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

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

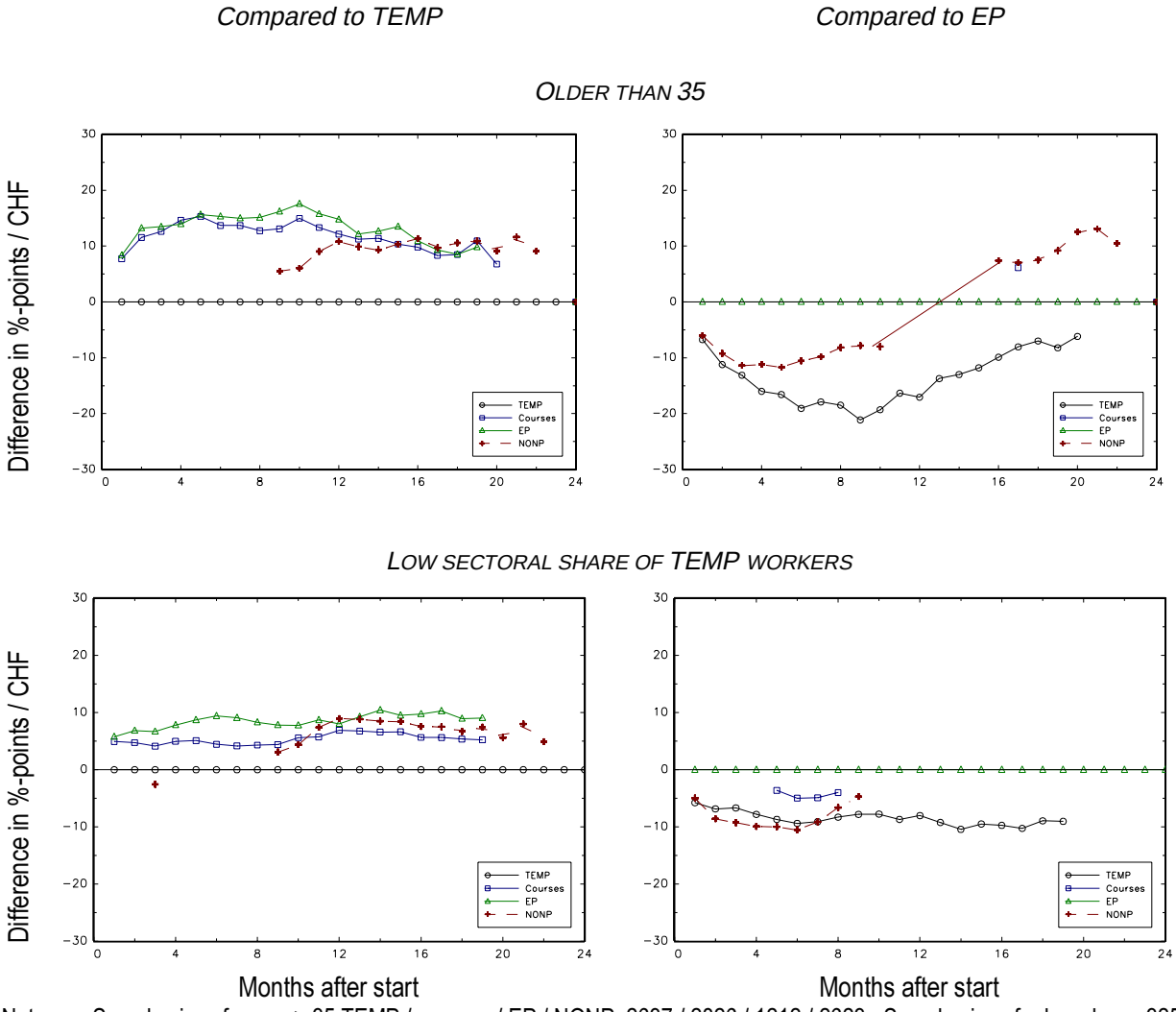
Note: The total number of observations decreases due to the enforcement of the common support requirement from 18354 to 17786 (-3.1%).

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

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

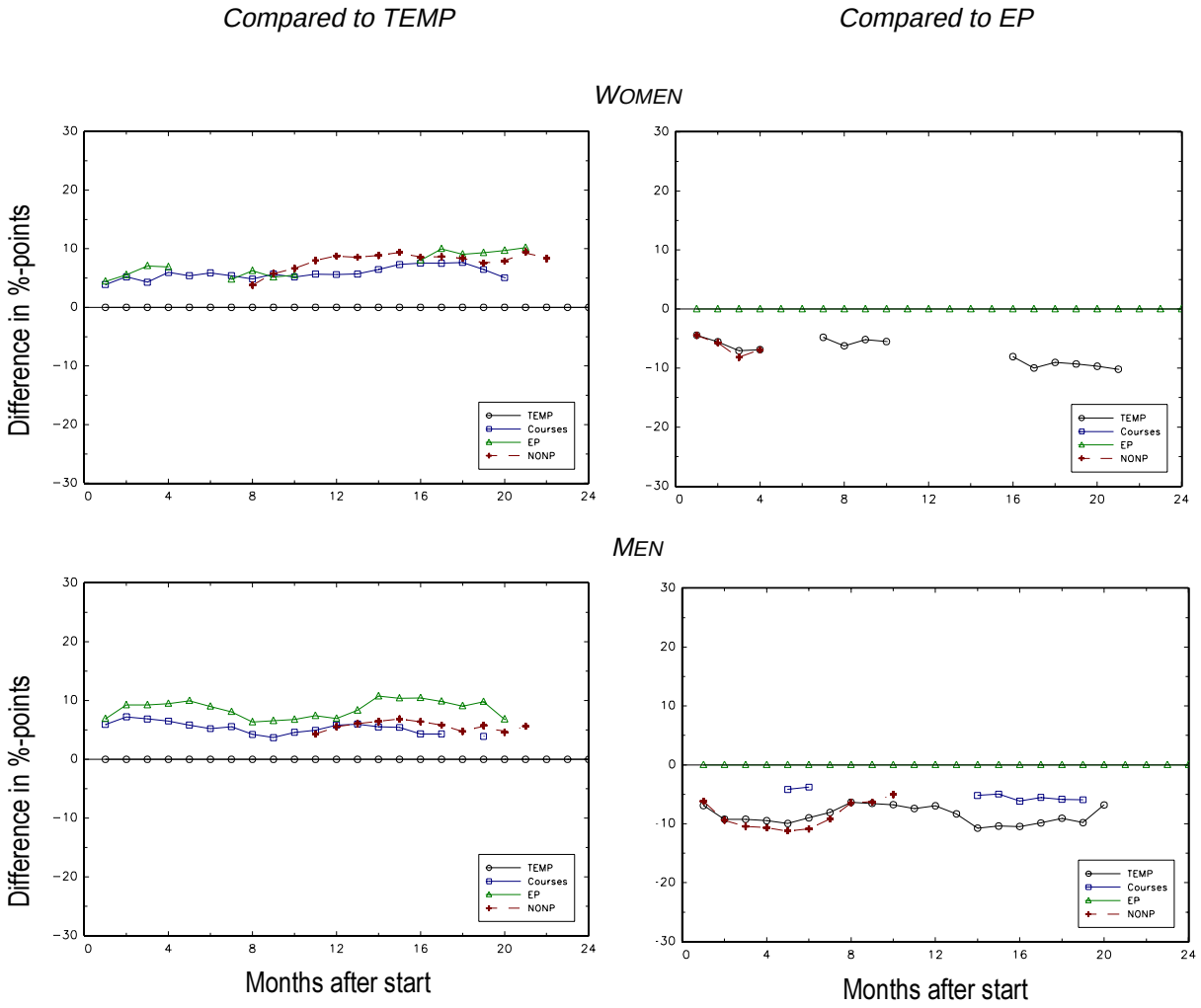
Appendix D: Additional results concerning subgroup heterogeneity

Figure D.1: Dynamics of average effects (ATE) for employment without earnings loss – *Low sectoral share of TEMP workers compared to unemployed and age*



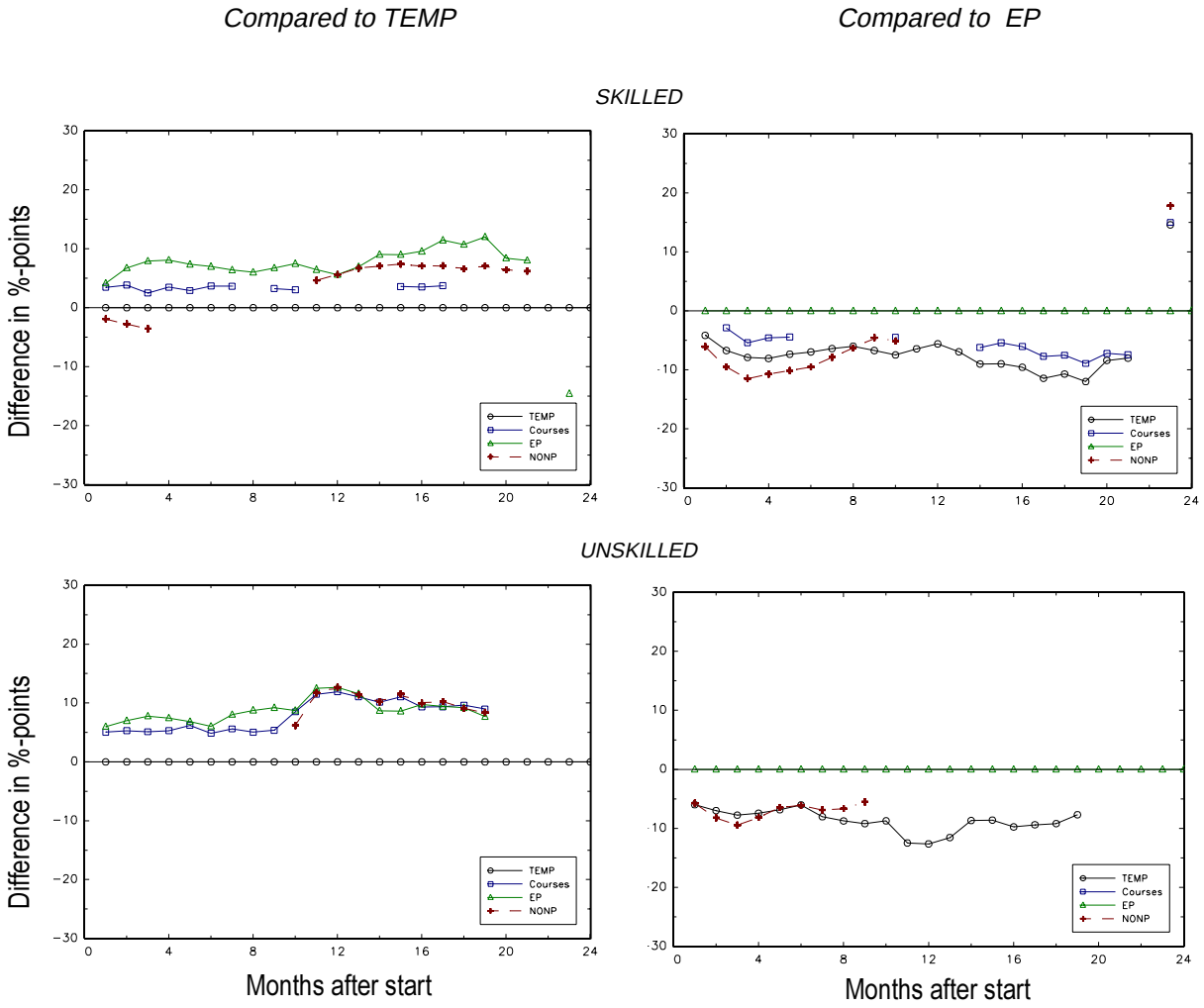
Note: Sample sizes for age > 35 TEMP / courses / EP / NONP: 2637 / 2826 / 1212 / 2623. Sample sizes for low share: 3259 / 3099 / 1244 / 3294. See also note below figure 1.

Figure D.2: Dynamics of average effects (ATE) for employment without earnings loss - *Sex*



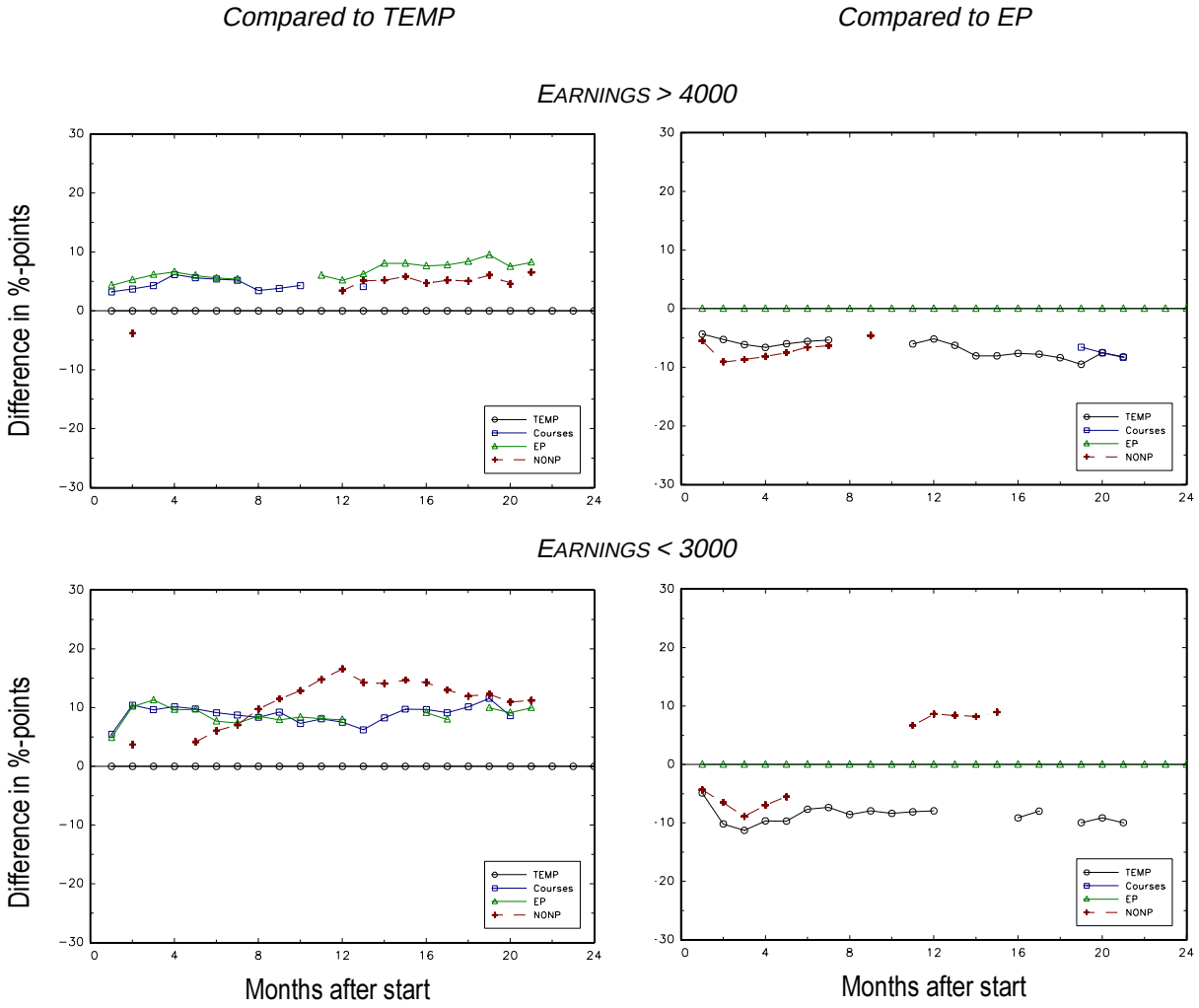
Note: Sample sizes TEMP / courses / EP / NONP for women: 2106 / 2484 / 776 / 2193. Sample sizes TEMP / courses / EP / NONP for men: 2991 / 2719 / 1298 / 2861. See also note below figure 1.

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



Note: Sample sizes TEMP / courses / EP / NONP for skilled: 2782 / 2925 / 1007 / 2808. Sample sizes TEMP / courses / EP / NONP for unskilled: 1462 / 1499 / 697 / 1510. See also note below figure 1.

Figure D.4: Dynamics of average effects (ATE) for employment without earnings loss – *Earnings in last job before UE spell*



Note: Sample sizes TEMP / courses / EP / NONP for earnings > 4000: 2247 / 2134 / 751 / 2089. Sample sizes TEMP / courses / EP / NONP for UE < 3000: 1256 / 1537 / 705 / 1436. See note below Figure 1.

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