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ABSTRACT

What is the Value Added by Caseworkers?*

We investigate the allocation of unemployed individuals to different sub-programs within Swiss active labour market policy by the caseworkers at local employment offices in Switzerland in 1998. We are particularly interested in whether the caseworkers allocate the unemployed to services in ways that will maximize the program-induced changes in their employment probabilities. Our econometric analysis uses unusually informative data originating from administrative unemployment and social security records. For the estimation we apply matching estimators adapted to the case of multiple programmes. The number of observations in this database is sufficiently high to allow for this nonparametric analysis to be conducted in narrowly defined subgroups. Our results indicate that Swiss caseworkers do not do a very good job of allocating their unemployed clients to the sub-programs so as to maximize their subsequent employment prospects. Our findings suggest one of three possible conclusions. First, case-workers may be trying to solve the problem of allocating the unemployed to maximize their subsequent employment, but may lack the skills or knowledge to do this. Second, caseworkers may have a goal other than efficiency, such as allocating the most expensive services to the least well-off clients, that is not explicit in the law regulating active labour market policies. Third, the distortions of the local decision process could be due to federal authorities imposing strict minimum participation requirements for the various programs at the regional level.

JEL Classification: H00 and J68
Keywords: active labour market policy, caseworkers, statistical profiling, statistical treatment rule and targeting

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Introduction

This paper considers the problem of how best to assign unemployed persons to one of a set of available employment and training programs. Several different methods exist to do this. The most common one consists of having the unemployed person meet with a caseworker. Together, the unemployed person and the caseworker come to an agreement about the services that the person should receive based on the person's interests, the caseworker's evaluation of his or her capabilities and the availability of slots in particular programs in the local area. Caseworker allocation is based on the idea that optimal assignment requires knowledge of the characteristics of the unemployed person, the local labour market and local service providers, combined with the presumed professional expertise of the caseworker.

Three other allocation schemes have also been used in practice. The first scheme consists of random assignment to services, a practice typically confined to experimental evaluations. For example, in the Canadian Self-Sufficiency Project experiment, treated persons were randomly assigned to receive only a wage subsidy or both a wage subsidy and employment and training services.[1] The second scheme consists of deterministic assignment, in which everyone in a particular status gets the same service. For example, everyone on social assistance might be required to receive job search assistance.

The third allocation scheme consists of using statistical treatment rules to assign persons to services (or to any service). This scheme is sometimes called profiling or targeting. It is presently used to assign unemployment insurance claimants in the United States to mandatory employment and training services.[2] It is also being considered for use in combination with

[1] See the description in Michalopoulos et al. (2002).
[2] See, e.g., Manski (2001), Black, Smith, Berger and Noel (2003) or Eberts, O'Leary and Wandner (2002).

caseworker assignment in the form of the Frontline Decision Support System for Workforce Investment Act (WIA) programs in the United States. In its existing implementation in the U.S. unemployment insurance system, the profiling is based on a statistical prediction of each claimant's probability of benefit exhaustion or expected benefit receipt duration. Claimants with higher predicted probabilities of exhaustion (or longer expected durations of benefit receipt) receive the mandatory services while those with lower predicted probabilities do not. As discussed at length in Berger, Black and Smith (2000), this scheme assigns treatment based on the predicted outcome in the absence of treatment, rather than on the predicted impact of the treatment. Assignment on the basis of outcomes rather than of impacts may serve equity goals (such as allocating the least employable among the unemployed to the most intensive services), but does not serve efficiency goals unless outcomes correlate negatively with impacts.

In this paper, we consider the use of statistical treatment rules to assign treatments on the basis of their predicted impacts. In particular, we use data on the Active Labour Market Policies (ALMPs) in place in Switzerland following their unemployment insurance reform in 1996 to examine the relative performance of alternative allocation rules. We employ these Swiss data for four reasons. First, the Swiss ALMPs include a wide variety of different treatments, of which we consider eight here. This variety allows substantial scope for caseworker discretion in treatment assignment. Second, the highly decentralized nature of the Swiss government means that caseworkers typically have substantial discretion to use their professional expertise in assigning persons to services. Third, the rich data available in the Swiss context give credibility to the non-experimental matching methods we use to generate our impact estimates. Finally, the Swiss programs are similar enough in terms of design and services offered to those of other developed countries to make it credible to generalize our findings beyond the Swiss border.

The remainder of the paper develops as follows. In Section 2, we describe the policy environment in Switzerland at the time our data were collected. This includes a detailed description of the available employment and training programs. Section 3 describes the existing caseworker assignment mechanism and the basic patterns of assignment to the various treatments. Section 4 outlines the matching methods used by Gerfin and Lechner (2002) to produce the impact estimates upon which part of our analysis builds. Section 5 considers how well the existing caseworker allocation does at maximizing the mean impact of the employment and training services currently provided. Following on the somewhat negative findings in Section 5, in Section 6 we estimate the mean impacts associated with some alternative allocation rules and find that some of them substantially outperform the caseworkers on this dimension. In Section 7 we make some concluding remarks.

2. The Policy Environment

Switzerland is unique among European countries in its low unemployment rates throughout much of the post-war period. In the 1970s, the Swiss unemployment rate never exceeded one percent, and it did not exceed 1.1 percent in the 1980s. In the 1990s, however, it began to rise to historically high levels, with a peak of 5.2 percent in 1997. These historically high levels of unemployment, though still remarkably low by European standards, prompted the Swiss government to enact a series of unemployment law reforms and active labour market policies in the 1990s.

Under the 1996 unemployment law reform in Switzerland, which is the one in place at the time our data were generated, individuals may be required to participate in employment and training services once they have been unemployed for 150 days (or 30 weeks) out of their two-year benefit entitlement.[3] If they are requested to participate after the deadline and do not comply, then their benefits may be cut off. The data, discussed in more detail in Section 3, indicate that some claimants participate in services before the deadline, while in other cases the deadline appears not to be enforced, perhaps because appropriate services were not immediately available.

Table 1 describes the different employment and training services provided under the Swiss unemployment insurance reform in 1996 (and defines the abbreviations we use to identify them in the remaining tables). There are three general categories: classroom training of various sorts, work experience in public and private sector jobs that are created specifically as part of the active labour market policy, and (partial) wage subsidies for temporary regular jobs in the private sector (where the latter may sometimes, but are not supposed to, substitute for permanent regular jobs). The training courses offered under the Swiss ALMP do not include occupational retraining – only further training within the current occupation. Courses last from one day to six months, but only courses at least two weeks in length are counted in our empirical work. Employment programs typically last six months, although participants are required to continue their job search while participating and to accept appropriate offers. Wages on the employment programs can in principle exceed the UI benefit level, but in practice usually do not. Neither courses nor employment programs count toward further UI eligibility. Temporary wage subsidies are not formally a part of the ALMP, but caseworkers appear to treat them as if they were. We follow the caseworkers in doing so here. Local placement offices arrange only about 20 percent of temporary wage subsidy placements, with the remainder arranged through employers or private temporary employment agencies. The local placement office must confirm placements in the

[3] The two-year entitlement is available to persons who contributed to the UI system in at least six of the past 24 months. After the two-year entitlement has been exhausted, obtaining a new entitlement requires 12 months of

latter category in order for them to receive the subsidy. Time spent employed on a temporary wage subsidy counts toward further UI eligibility.

The general categories of programs offered in Switzerland mirror those available in other developed countries. With the exception of the wage subsidies for temporary jobs, which represent the one unique aspect of the service mix in the Swiss system, Swiss ALMP resembles that in Germany quite strongly. The New Deal for Young People in the United Kingdom also provides classroom training, subsidized employment and work experience, where the last of these corresponds to the New Deal's Voluntary Sector and Environmental Task Force options. The Swiss options also resemble those provided as Employment Benefits and Support Measures to unemployed persons in Canada. They are somewhat less similar to the service structure of the new U.S. Workforce Investment Act program, given the emphasis in the latter on services related to job search, at least as a first step.

3. Data

Our data consist of administrative records on all persons who were registered unemployed in Switzerland as of December 31, 1997. Our analysis sample consists of the subsample of this population that results from imposing a number of exclusion criteria. In particular, we keep only unemployed persons with the following characteristics: age between 25 and 55 (inclusive), not disabled, at least 100 Swiss Francs of past earnings, valid value of mother tongue variable, Swiss citizen or foreigner with annual or permanent work permit, not working at home, not a student, not an apprentice, unemployed less than one year, no program duration longer than 14 days in 1997, no employment program (at all) in 1997, and no program start on January 1, 1998 (such a

employment within the three years after the previous unemployment spell. The usual replacement rate in the Swiss UI system is 0.70 or 0.80, depending on the recipient's family status.

start date implies a continuing program).[4] The analysis sample includes over 19,000 persons, which is large enough to allow us to estimate the impacts of particular service alternatives with standard errors of reasonable size. See Gerfin and Lechner (2002) for more details on the construction of the analysis sample.

We code the first major spell of program participation starting after January 1, 1998, where we define a major spell of participation as one lasting at least 14 days. We code persons not participating in any single program for more than two weeks between January 1, 1998 and January 1, 1999 as non-participants. In order to code time-varying variables for non-participants, we assign each one a random start date drawn from the empirical distribution of start dates among participants. Non-participants whose simulated start date occurs after the end of their unemployment spell are dropped from the sample.[5]

In coding service receipt, we have to deal with the familiar problem that participants often participate in more than one program in a given unemployment spell. As in other countries, these additional programs sometimes represent part of a planned sequence but often represent an endogenous response to a poor match between the claimant and the initial program in which he or she participates. In our data, about 30 percent of those participating in at least one program also participated in another; however, for the majority of these, the second program was of the same type (in the typology shown in Table 1) as the first. In light of these facts, we follow Gerfin and Lechner (2002) by coding persons based on the first program they participate in for more than two weeks during a given unemployment spell.

[4] See Appendix A.2 of Gerfin and Lechner (2002) for even more detail about the sample definition.
[5] See Lechner (1999), Sianesi (2001) and Fredricksson and Johansson (2002) for discussions regarding the temporal alignment of non-participants.

4. The Caseworker Allocation

Currently, Swiss ALMPs rely on caseworkers to assign unemployed persons to employment and training services. In the Swiss system, each caseworker has 75 to 150 persons to work with, and the caseworker has an in-depth interview with each client every month. This represents substantially more in person contact than participants would receive in most other developed countries. It also means that Swiss caseworkers have the opportunity to gain a large amount of information about the claimant's needs and abilities, information that, in principle, they should be able to use in effectively matching claimants to services. Given the large amount of information they possess about their clients, and given the flexibility present in the highly decentralized Swiss system, it could be argued that the performance of Swiss caseworkers in the allocation task should represent an upper bound for caseworkers in other developed countries.

Table 2 presents information on the allocation chosen by the caseworkers. The first column of Table 2 shows the number of sample observations in each service type. It reveals temporary wage subsidies as the most common service, followed by language courses. The predominance of the latter reflects the over-representation of foreigners among the Swiss unemployed. The second column indicates the mean duration of the program for persons receiving each service. In general, employment-related services tend to last longer than classroom-based services. The third and fourth columns indicate the mean days of unemployment prior to the start of services and the fraction of persons for whom the services started prior to the 150-day deadline. The fifth column indicates the mean qualification of persons receiving each service type, with qualifications measured on a scale from one (skilled) to three (unskilled). Perhaps not surprisingly, participants in language courses have the lowest mean level of qualifications, while participants in computer courses have the highest. The opposite pattern holds in the sixth column, which indicates the percentage of foreigners in each service type. The highest percentage is now found for language courses, and the lowest for computer courses.

The final column in Table 2 gives employment rates as of March 1999. The highest employment rate corresponds to temporary wage subsidies and the lowest to private employment programs. Of course, these employment rates reflect a combination of non-random assignment to services based on employment-related characteristics such as level of qualifications and the impact of the services themselves on the probability of unemployment.

We draw three main lessons from Table 2. First, Swiss caseworkers are making use of the flexibility available to them to assign unemployed persons in large numbers to all of the treatment types we consider here. Second, the caseworkers do not allocate persons at random with respect to their observed characteristics. Mean unemployment durations, mean qualifications and percent foreign all differ among the service types. Assuming that most services have only modest impacts (consistent with the survey in Heckman, LaLonde and Smith, 1999, and with our own estimates presented in the next section), there are also strong differences in mean employment chances in the absence of treatment across treatment types as well. Third, the caseworker allocation shows evidence of systematic, reasonable patterns. It makes sense to assign foreigners to language courses and the most qualified among the unemployed to computer courses, which are presumably among the most challenging courses offered.

5. Econometric Strategy

Our analysis builds in part on the non-experimental impact estimates for the different service alternatives presented in Gerfin and Lechner (2002). Readers interested in a complete account of the econometric strategy employed to generate the estimates should refer to that paper.

Here we present a shorter, less technical discussion that gives the basics regarding where our estimates come from.

Let $S \in \{0,...,M\}$ denote one of the nine service alternatives, where we define $S = 0$ as non-participation and note that $M = 8$. The evaluation problem arises because we observe each unemployed person in only one of the nine possible states, and so only observe one of the associated nine outcomes, $Y_0,...,Y_M$.

We require estimates of three different parameters of interest in our investigation. The first of these are estimates of the impact of treatment on the treated, given by:

$$\theta_{m,l} = E(Y_m - Y_l \mid S = m) = E(Y_m \mid S = m) - E(Y_l \mid S = m). \tag{1}$$

The second of these are estimated average treatment effects, given by:

$$\gamma_{m,l} = E(Y_m - Y_l) = E(Y_m) - E(Y_l). \tag{2}$$

The third of these are estimated expected outcome levels in each service alternative for unemployed individuals with a particular value of observed covariates X:

$$E(Y_m \mid X = x) \text{ for } m = 0,...,M \text{ and } \forall x \in \chi. \tag{3}$$

To identify these three parameters of interest, we follow Lechner (2001a) and Imbens (2000) and adopt the following multi-treatment version of the conditional independence assumption (CIA):

$$Y_0, Y_1,...,Y_M \perp S \mid X = x \quad \forall x \in \chi. \tag{4}$$

This assumption states that the potential outcomes associated with each service alternative (including non-participation) are independent (denoted by "$\perp$") of the service alternative choice conditional on some set of observed covariates *X*. This "data hungry" assumption becomes plausible in our context because of the availability of exceptionally rich data on both unemployed individuals and their local economic and programmatic environments. Given our rich data, we argue that we can condition on all of the important factors that affect both the choice of service alternative and labour market outcomes.

In order to compare unemployed individuals with a given set of values $X = x$ in two different service alternatives, we require that there be a non-zero probability of each service for each possible value of *X*. Formally, we assume that

$$0 < \Pr(S = m \mid X = x) \quad \text{for } m = 0,\ldots,M \text{ and } \forall x \in \chi .$$

This is the so-called common support condition. In practice, there are two separate conditions, one in the population and one in the sample. Because Gerfin and Lechner (2001) show that only a small fraction of the sample gets dropped due to imposition of the support condition, and because we will switch to a parametric model in Section 7, we do not impose the support condition in our analysis here. See Lechner (2001b) and, e.g., Smith and Todd (2003) for further discussions of the support issue.

In addition to the CIA, we also assume that the outcomes for each person, $Y_{0i},\ldots,Y_{Mi}$, do not depend on the distribution of the population among the different service alternatives. Put differently, we assume the absence of spillovers or general equilibrium effects. The formal name for this assumption in the literature is the Stable Unit Treatment Value Assumption, or SUTVA. It is common to all partial equilibrium analyses, including those using matching methods. This is a strong assumption in our context. Assigning all of the unemployed to, say, vocational training, would raise the quantity of labour with certain skills, and thereby likely depress its price, relative to a situation in which only a modest fraction of the unemployed receive such training. This is one reason, the other being the practical difficulties (supply constraints) associated with rapid changes in the distribution of service types, that our analyses consider allocations that do and do not impose the current distribution of service types as a constraint.

The multi-treatment CIA justifies using a matching estimator to estimate the parameters of interest in (1) and (2) (and (3), although we do not do so here). As is well known, matching directly on X leads to the so-called "curse of dimensionality". Following Rosenbaum and Rubin (1983), as generalized for the multi-treatment context by Lechner (2001a) and Imbens (2000), we use balancing scores for our matching estimates. The balancing score combines marginal probabilities of each service alternative conditional on X estimated in a multinomial probit with a short vector of Xs to which we want to assign greater weight than they implicitly receive by being included (as they are) in the estimated probabilities.[6] Gerfin and Lechner (2002) describe the multinomial probit estimation in greater detail. The Mahalanobis distance serves as the distance metric for single nearest neighbour matching with replacement.

As discussed in Gerfin and Lechner (2002), an important issue that arises in implementing the matching estimator concerns how to compute the estimated standard errors. The usual way to construct standard errors for estimates based on matching is by bootstrapping. In this context, estimation of the multinomial probit takes long enough that obtaining sufficient bootstrap replications becomes infeasible. Lechner (2002a) suggests an estimator of the asymptotic standard errors for the treatment on the treated ($\theta_{m,l}$) and average treatment effect ($\gamma_{m,l}$) parameters. His estimator assumes that the variance component resulting from the estimation of the probabilities themselves in the first step multinomial probit is sufficiently small that it can safely be ignored. The comparison presented in Lechner (2002b) between these approximate standard errors and bootstrap standard errors utilizing the same data we utilize for this paper finds

[6] The set of Xs included on their own in the balancing score includes native language not a Swiss language, sex, the calendar date of program start, and the duration of the unemployment spell prior to program start.

only a small difference between the two. Thus, where we report standard errors, they rely on the procedure outlined in Lechner (2002a).

Table 3 presents various quantiles of the distributions of marginal probabilities that result from the multinomial probit. The table yields some interesting findings. First, there are very few extremely high probabilities. The highest value of the 99th percentile is 65.1 percent for non-participation, while the lowest is 0.1 percent for further vocational training. Second, our model produces a substantial amount of differentiation for all nine of the service alternatives. The variables included in the model clearly do predict participation, not just in some cases, but in all cases. Finally, the distributions reflect the underlying unconditional probabilities. The distributions for services received by only a small fraction of the population are clearly stochastically dominated by those for services (or no service, in the case of non-participation) received by a larger fraction of the population.

6. Does the Caseworker Allocation Maximize Employment Rates?

In this section, we utilize the non-experimental impact estimates from the multi-treatment matching procedure to examine how well the caseworker allocation does at maximizing the ex post employment rate of the Swiss unemployed in our sample. Put differently, and putting aside both cost considerations and longer-term impacts for the moment, we consider whether the caseworker allocation serves the goal of efficiency in service allocation.

We begin with Table 4, which presents estimates of the impact of treatment on the treated, $\theta_{m,l}$. The outcome variable is employment status 365 days after the start of the program. For the participants in each treatment, the estimates in Table 4 indicate which treatment (including possibly the one they received or no treatment at all) our estimates indicate would have yielded the highest post-program employment rate. To see how this works, consider the first row of Table 4, labelled "NONP", for non-participation. The shaded value of 41.3 indicates that the observed employment rate for the non-participants one year after their simulated start date is 41.3 percent. The remaining entries in the first row indicate the estimated *difference* in employment rates that the non-participants would have experienced had they received the service in the corresponding column. Thus, we estimate that the non-participants would have had an employment rate of 31.4 (= 41.3 – 9.9) had they undertaken basic courses. Overall, our analysis indicates that the non-participants would have achieved a higher employment rate than they actually did in only two of the eight services: "other training" and temporary wage subsidy. The value of 7.3 for the temporary wage subsidy is highlighted to indicate that it is the alternative yielding the highest employment rate in the row. Similarly, the value of -9.9 for basic courses appears in italics to indicate that this alternative yields the lowest estimated employment rate for the individuals in the non-participant row. The lower panel of Table 4 presents estimated standard errors for the estimates in the upper panel.

What general conclusions emerge from Table 4? In every row, and thus for the individuals assigned to each of the nine services we examine, some other service would yield a higher estimated employment rate. Indeed, our estimates suggest that if maximizing post-program employment rates were the goal, then everyone should have received either "other training" or a temporary wage subsidy. Perhaps surprisingly, our estimates suggest that those who actually received either one of these two services would have had a higher probability of employment, had they received the other! In most cases, the implied difference in employment rates between the service assignment with the highest employment rate and the employment rate corresponding to the service actually received exceeds 10 percentage points; in two of the remaining three cases, it exceeds five percentage points.

Things are not as bad as they could be, however. In only one case – basic courses – is the estimated employment rate lowest for the service actually received. Basic courses have the lowest estimated employment rate for individuals receiving all but two of the available services. In every case other than basic services, the observed employment rate for the service actually received lies more or less in the middle of the distribution of estimated employment rates associated with the other services. Taken as a whole, the evidence in Table 4 suggests that caseworkers do neither very well nor very poorly at allocating workers to services relative to the goal of maximizing their post-program employment rate.

Having established in Table 4 that caseworkers do not appear to allocate the unemployed to alternative services in a way that maximizes their post-program employment rate overall, we set a somewhat lower standard in Table 5. In Table 5, we ask whether the individuals with a very high probability (in the top quintile in our sample) of being assigned to each particular alternative achieve the highest estimated post-program employment rate in that service. The idea here is that caseworkers seem to agree about what to do with individuals with sets of characteristics that lead them to have very high probabilities of assignment to particular services. This agreement suggests that it is for these individuals that caseworkers believe they have the best knowledge of the correct alternative. Table 5 aims to evaluate that knowledge.

Table 5 has the same format as Table 4, with observed employment rates on the diagonal of the top panel, treatment on the treated impact estimates in the remaining cells of the top panel, and estimated standard errors for the elements of the top panel presented in the bottom panel. Thus, we see that for those whose probabilities of non-participating lie in the upper quintile in our sample, the observed employment rate is 33.4. Comparing this value to the corresponding element in Table 4, we learn that persons with high probabilities of being non-participants have lower employment rates than all those who actually do not participate. We estimate that individuals with high probabilities of being non-participants would have had substantially higher employment probabilities (49.5 = 33.4 + 16.1) if they had received temporary wage subsidies. At the same time, we estimate that they would have had much lower employment probabilities (21.8 = 33.4 – 11.6), had they received basic courses.

Overall, we find that in no case are those with a high probability of receiving a particular service estimated to have their highest probability of employment in that service. At the same time, in only one case do those with a high probability of receiving a particular service have their lowest estimated probability of employment in that service. Overall, the story parallels that in Table 4, and indicates that even when case workers generally agree regarding what service someone should receive based on their observable characteristics, they do not do a very good job of assigning them to services that will maximize their post-program employment rate.

Finally, Table 6 presents a third way of looking at the current allocation of the Swiss unemployed to alternative services in our data. The values in the table consist of the difference in the corresponding values in Tables 5 and 4. Basically, the question addressed here is, do the caseworkers do a *better* job of allocating the persons with a high probability of allocation to a particular service than they do in general. Put differently, while Table 5 addresses the absolute quality of the allocation for those with a high probability of allocation to a particular service, Table 6 addresses the relative quality of the allocation. Note that we leave the diagonal elements in Table 6 empty; these values combine differences in baseline outcomes with differences in assignment quality, and so do not have a clear interpretation.

Evidence of relatively good performance at allocating individuals with high probabilities of assignment to a particular service consists of negative estimates of the off-diagonal entries in Table 6. A simple vote count shows negative estimates that 24 of the 72 elements of Table 6. This pattern suggests that the caseworkers do not do a better job of assigning persons with high probabilities of receiving particular services than they do in general.

Taken together, the findings in Tables 4, 5 and 6 clearly indicate that caseworkers either do not seek to maximize post-program employment rates when they assign the unemployed to alternative services, or else they do try to do so but do not do a very good job of it. These findings suggest the value of looking at alternative allocation schemes based on econometric estimates of the employment probability associated with each alternative for each person, conditional on observed characteristics. Such econometric allocation schemes hold the promise of higher average post-program employment rates among Swiss ALMP participants.

7. Alternative Allocation Rules

Having established in Section 6 that Swiss caseworkers are not doing an especially good job of allocating their unemployed clients so as to maximize their estimated post-program employment rates, in this section we consider how a variety of alternative allocation mechanisms perform relative to this same standard.

Consideration of these alternative participation rules requires the estimation of person-specific employment probabilities associated with each of the nine service alternatives (including non-participation). The matching estimator described in Section 5 does not estimate such person-specific probabilities with sufficient precision. As a result, in this section we proceed in a more parametric manner. In particular, we estimate a binary probit model with employment in day 365 as the dependent variable for each of the nine subsamples defined by the observed alternative. As conditioning variables in the probits we include the marginal probabilities of each treatment from the multinomial probit model of treatment choice, as well as indices from the multinomial probit model (to increase the flexibility of the functional form), along with sex, a Swiss language dummy variable, and duration of unemployment up to the participation date. The specification has been tested against omitted variables and functional misspecification using standard score tests. We also performed specification tests against heteroscedasticity, information matrix tests, and a normality test.[7] These probits allow construction of the conditional probability of employment for each sample member in each treatment; it is these conditional probabilities that we employ in what follows.

Five caveats apply to our findings on alternative allocation rules in this section. First, as in our earlier analyses, we continue to assume no scale effects, so that if we allocate, say, all of the unemployed to temporary wage subsidies, this does not affect the validity of our estimates. Because this represents a fairly strong assumption, we also consider allocation schemes that reallocate the unemployed among the various alternative services while keeping the proportion of the unemployed assigned to each alternative the same as what we actually observe. Second, we do not have information on direct costs for the different services, so our results rely on estimates of gross rather than net impacts. Our estimates do (partly) capture differences in indirect cost savings among alternative services due to reductions in the amount of time spent collecting unemployment insurance benefits. Third, because we condition on functions of X, rather than on X itself, in our employment probits, our results understate the ability of the econometric assignment models. Fourth, in contrast to the third caveat, because we take the maxima and

[7] Lack of omitted variables, conditional homoscedasticity and normality of the probit latent error terms are tested using conventional specification tests (Bera, Jarque, and Lee, 1984, Davidson and MacKinnon, 1984, and White,

minima of sets of estimated values to determine assignments with no consideration of the variance of these estimates, we overstate somewhat the performance of the econometric assignment models. That is, sampling variation will lead us to over-state the improvement associated with assignment rules based on the best or worst predicted outcomes or impacts.

Fifth, our outcome variable measures employment on one specific day – the day 365 days after the start of the program. If the different service alternatives imply different times paths of employment probabilities, then our one-day measure may provide a biased guide to the discounted present value of the time spent employed associated with each service (and, likewise, to the discounted present value of earnings which would represent the object of interest in North American active labour market policy). In light of these caveats, we view our estimates not as definitive statements of expected gains, but rather as suggestive of the improvements that could be achieved by supplementing or replacing caseworker judgement with econometric forecasts in the allocation of unemployed persons to services.

Table 7A presents the employment rates associated with alternative allocations of the unemployed workers in our data to the nine available services (including non-participation) we consider. The table includes employment rates for both the full sample of the unemployed, and for that sub-sample (about 60 percent) who report as their native language one of the three primary Swiss national languages (German, French or Italian).[8] This separate analysis allows us to determine whether caseworkers do better with the unemployed immigrants who make up the non-Swiss language group.

1982). The information matrix tests statistics (IMT) are computed using the second version suggested in Orme (1988), which appears to have good small sample properties.

[8] The fourth official Swiss language, Romansch, is spoken by only a tiny fraction of the population.

The first two rows of Table 7A present the estimated employment rate given random assignment of the unemployed to the nine service alternatives in their existing proportions, and the observed overall mean employment rate associated with the caseworker allocation. These two rows provide a succinct summary of the evidence in Tables 4, 5 and 6. They show that for both the full sample and the Swiss language sample, the caseworkers do just a bit worse in their allocation than random assignment would do.

The next nine rows present the estimated employment rates associated with assigning everyone to each of the nine service alternatives in turn. These allocations have the advantage of greatly simplifying the allocation decision, which presumably would save on program administration costs. For five of the service alternatives, assigning everyone to that alternative leads to a lower estimated employment rate than either the current caseworker allocation or random assignment to services in the existing proportions. In contrast, in the remaining four cases – non-participation, vocational training, other training, and temporary wage subsidies – assigning everyone to the service dominates both the caseworker allocation and random assignment in terms of our post-program employment rate outcome. The non-participation case holds special interest, as it represents simply getting rid of the active labour market policy. It requires zero direct costs, but still dominates all of the one-service-for-all alternatives other than other training and temporary wage subsidies. This finding is consistent, of course, with the general finding in the literature that most active labour market policies do not work very well; see, e.g., the survey in Heckman, LaLonde and Smith (1999).

In the next four rows we consider allocations that maximize and minimize the predicted employment rate. These allocations (like the ones that assign all of the unemployed to one particular service) relax the constraint imposed by the existing service proportions. The first of the four allocations assigns each person to that one of the nine alternatives for which he or she has the highest predicted employment probability. The resulting mean post-program employment rates of 55.5 overall and 61.9 for the Swiss language sub-sample represent large increases over those implied by either random assignment in the existing service proportions or the observed caseworker allocation. The implied distributions of the unemployed among the various services for this allocation and for the other three allocations in this group appear in Table 7B. The allocation that maximizes the predicted employment rate assigns far more of the unemployed to vocational training, other training and temporary wage subsidies than does the observed caseworker allocation, and far fewer to non-participation, basic courses and language courses.

The second of the four allocations resembles the first, only it rules out non-participation as an alternative (and also drops the non-participants from the sample). Not surprisingly, given that the first allocation assigned only 1.6 percent of the unemployed to non-participation, ruling out this option makes little difference to the resulting estimated overall post-program employment rate. These two allocations capture the spirit of the Canadian Service Outcomes and Measurement System (SOMS) described in Colpitts (2002) and the American Frontline Decision Support System (FDSS) described in Eberts, O'Leary and DeRango (2002). These systems sought (in the case of SOMS) or seek (in the case of FDSS) to promote efficiency in allocation through the assignment of individuals based on predicted impacts.

The next pair of allocations turns the previous pair on its head by assigning individuals to that alternative for which they have the lowest predicted probability of employment, with or without non-participation included in the set of available options (and non-participants in the sample). These allocations provide worst-case estimates. We find that allocating services so as to minimize the post-program employment rate leads to overall rates of 25.7 percent with non-participation as an option and 26.7 percent without non-participation as an option. These figures are far below (over 10 percentage points) the employment rates resulting from either the observed caseworker allocation or random assignment with existing service proportions. This large difference reinforces our conclusion from Tables 4, 5 and 6: while the caseworkers are not maximizing post-program employment rates, they are not minimizing them either. Relative to the observed caseworker allocation, the allocation that minimizes the estimated employment rate assigns more of the unemployed to temporary employment in the public (especially) and private sectors, and to language training. It assigns almost no one to temporary wage subsidies.

The final six assignment schemes in Table 7A impose "supply constraints" at either the national (in the first three rows in this group) or regional (in the second three rows) level. By supply constraints, we just mean that we force the allocation to adopt the observed distribution of services either for the country as a whole or separately for unemployed workers in each region. The cantons included in each region for this purpose appear in the notes to Table 7A. The point of imposing these constraints on the allocations we consider is realism; in many cases, there may be no way, particularly in the short to medium term, to substantially increase the number of slots in computer courses, or to substantially increase the number of temporary wage subsidies which, after all, require a willing employer. By considering both cases of unlimited flexibility (with no supply constraints) and no flexibility (where we impose the existing distribution of services) we bracket the true situation, which involves some limited amount of flexibility, and more flexibility in the amounts of some services than others.

The supply constraints raise the problem that who gets assigned to what now depends on the order in which we consider the unemployed persons in our data. Those who get assigned first will get their preferred service alternative, but those who get assigned later may find that all the slots for their preferred service have already been filled. We deal with this issue by utilizing the following two schemes to order the sample:

1. *"Effect-based" ordering*: First we put our sample in a random order. We then calculate for each sample member the estimated mean impact on the probability of post-program employment, relative to non-participation, associated with each service alternative, where some (or all) of these estimated impacts may be negative. We then sort the sample members by the difference between the most positive (or least negative) impact and the second most positive (or least negative) impact. Assignment to services then proceeds in order by this difference, until one service becomes full. At that point, we reset the estimated impact for the service with no remaining slots to a very large negative number (for purposes of the allocation), and the unassigned observations are re-sorted. Allocation then proceeds based on the resorted order until a second service becomes full, and so on.

2. "*Need-based" ordering*: First we estimate the probability of employment conditional on non-participation for each sample member. Next we sort the sample based on this probability. Then we assign services in order starting with the lowest value of this probability, which we take as a measure of need. That is, we equate need with having a low predicted probability of employment in the absence of participation, which is similar in spirit to the allocation mechanism used by the Worker Profiling and Reemployment Services system in the United States. This system assigns mandatory employment and training services to new Unemployment Insurance benefit recipients with high probabilities of benefit exhaustion or long predicted spells of benefit receipt. See the related chapters in Eberts, O'Leary and Wandner (2002) for details.

Separate from the ordering scheme is the choice of which service alternative to assign to each person when they come up. We consider two alternatives here: (1) assignment to the alternative with the largest predicted employment rate; and (2) assignment to the alternative with the smallest predicted employment rate. The first represents a best-case assignment that maximizes, given the available estimates and subject to the indicated supply constraints, the efficiency of service allocation. The second is a worst-case scenario, from an efficiency standpoint, again given the available estimates and subject to the supply constraints.

Now return to the final six assignment schemes in Table 7A. The first three represent assignment to the service with the largest gross impact with effect-based ordering, assignment to the service with the smallest gross impact with effect-based ordering and assignment to the service with the largest gross impact with need-based ordering, all with supply constraints imposed at the national level. The next three assignments are the same but with the supply constraints imposed at the regional level.

These six assignments provide several useful lessons. First, comparing the constrained and unconstrained allocations based on gross impacts for the full sample, we see that imposing the national supply constraints makes a large difference, by reducing the estimated post-program employment rate from 55.5 to 49.3. In contrast, imposing the supply constraints at the regional rather than the national level leads to only a small further reduction from 49.3 to 47.2. Thus, supply constraints matter, and without further information about just how elastic the supply of subsidized jobs and training slots, the data leave us with a fairly wide range of potential employment rates associated with service assignment based on estimated impacts.

Second, comparing the estimates based on assignment to the largest and smallest gross impacts (with effect-based ordering) shows that imposing the supply constraints moderates the difference in estimated employment rates between these best and worst cases, relative to that found for the unconstrained case. In addition to the decrease in the employment rate associated with allocation based on the largest predicted impacts, the employment rate associated with allocation based on the smallest predicted impacts increases from 25.7 to 37.0 for the full sample when we impose the supply constraints. The supply constraints strongly limit the number of unemployed allocated to either relatively effective or relatively ineffective services.

Third, the way in which we order the respondents makes very little difference. For the full sample, switching from effect-based ordering to need-based ordering lowers the estimated post-program employment rate from 49.3 to 47.8 with the national supply constraints and from 48.4 to 47.2 for the regional supply constraints. In this case, adding an equity dimension to the allocation has only a small cost.

In Table 8 we consider the same allocations as in Table 7A, but with the estimated employment rates broken down into subgroups based on regional characteristics. The first two columns present results for urban and rural regions as defined by the size of the town the regional employment office (RAV) is located in. The third, fourth and fifth columns present estimates separately for Type I, Type II and Type III RAVs, as defined Atag Ernst and Young Consulting (1999). These types relate to estimated inflow and outflow rates from unemployment for each office, conditional on local economic conditions. Type I RAVs have low inflow rates and high outflow rates, Type II RAVs have high inflow rates and high outflow rates and Type III RAVs have high inflow rates and low outflow rates. There are no cantons with low inflow rates and low outflow rates. The final two columns break the cantons down by whether their primary language is German, or French or Italian.

The patterns observed for the full sample, and for the Swiss language sample, largely carry over for all of the subgroups in Table 8. We note two additional findings of interest. First, the difference between the employment rates implied by the observed caseworker assignment and by random assignment remains remarkably stable for the various subgroups. It varies between 1.2 (for rural RAVs and Type I RAVs) and 0.0 (for Type II) RAVs. Caseworkers do not appear to vary very much on a geographic basis in their ability to allocate the unemployed. Second, the gains from moving from caseworker allocation to unconstrained (or constrained) allocation based on estimated impacts appears noticeably larger for Type I RAVs, and for primarily German-speaking cantons. We do not have a clear explanation for this pattern.

8. Conclusions

Most active labour market policies in the developed world feature a variety of different employment and training services. With a few notable exceptions, such as the WPRS system for the unemployed in the U.S.[9], individuals seeking help in the labour market get allocated to these services with the assistance of caseworkers.

In this paper, we show, using recent data on the Swiss unemployed, that caseworkers do about as well at allocating clients to services as random assignment to services (in their existing proportions) when performance consists of employment rates measured one year after the start of the program. By examining allocations based on assigning each person to that service with the largest, or smallest gross impact (relative to non-participation), we show that things could either be much better, or much worse. Taking our estimates for the full sample without supply constraints, we estimate that assigning individuals to the service with the largest impact would raise post-program employment rates by 14.0 percentage points. At the other end, deliberately assigning the unemployed to the service with the lowest predicted impact reduces the estimated employment rate for the same group by 15.8 percentage points. Thus, caseworkers do not add much value, but they do not subtract much either, in their role as service allocators.

[9] Even WPRS represents only a partial example. The system uses a statistical treatment rule to assign the requirement of mandatory employment and training services to a subset of those collecting unemployment insurance, but among those required to receive services, caseworkers help to guide service assignment.

Our findings may seem surprising, particularly to those who have interacted with caseworkers confident of their abilities. Despite this, our findings generally comport with the (very) small literature that has examined related questions. The analysis in Frölich (2001) corresponds most closely to the one in this paper. Frölich (2001) applies statistical treatment rules to non-experimental data on Swedish rehabilitation programs and finds large gains relative to caseworker assignment.

Plesca and Smith (2000) examine caseworker decisions regarding program participation. In this context, rather than assigning participants to particular services within a program, caseworkers decide who gets any service, rather than none, from among a pool of applicants. Plesca and Smith (2000), utilizing the experimental data from the U.S. National Job Training Partnership Act Study, find that caseworkers do a bit better on this dimension. They estimate that the gain in employment rates from replacing them with a statistical treatment rule based on predicted impacts amounts to a few percentage points.

Bell and Orr (2002) report on a study that asked caseworkers which applicants they thought would benefit most from the AFDC Homemaker-Home Health Aide program, which trained welfare mothers to become home health aides. This information was collected prior to the random assignment of applicants. By interacting the experimental treatment indicator with the caseworkers' ratings of potential benefits in the impact analysis, they show that caseworkers have, essentially, no idea who will benefit more or less from the program. This suggests, in turn, that their choices regarding participation are unlikely to do as well as those of a statistical treatment rule based on predicted impacts.[10]

[10] Dehejia (1999) compares a statistical treatment rule for assigning welfare mothers to participate or not in the California Greater Avenues to Independence (GAIN) program to either having everyone participate or having no one participate. Consistent with our evidence in Tables 7A and 8, he finds that a statistical treatment rule based on predicted impacts dominates all-or-nothing assignments into or out of treatment. O'Leary, Decker and

What evidence exists, including the evidence presented in this paper, does not make a strong case for the abilities of caseworkers at assigning individuals to services within ALMPs. Should the governments fire their caseworkers and replace them with statistical treatment rules? While the evidence presented here (and elsewhere) is suggestive, some important considerations remain unresolved.

Consider the Swiss context examined here. Swiss caseworkers perform a number of functions in addition to service allocation. These include monitoring the unemployed and encouraging them to look for work or training, networking with employers to develop opportunities for subsidized temporary jobs, keeping abreast of local training opportunities and so on. Our results do not pertain to these other functions, which caseworkers may perform either well or poorly.

In addition, as we have already noted, our analysis has some limitations that flow out of the data we use. First, we lack the cost data necessary to examine allocations based on net rather than gross impacts. Second, our dependent variable consists of employment at a particular point in time, rather than discounted sums of future earnings. Because some treatments may have a different path of labour market benefits (or harms) over time, an outcome variable based on one specific day may not rank the alternatives correctly for some individuals in some cases. Third, our impact estimates rely, of necessity, on non-experimental data. While the methods we employ have credibility in our context due to the wealth of covariate information available on the individual unemployed and their local economic environments, data in which individuals were randomly assigned to services would make our analysis even more compelling.

Wandner (2002) provide a similar analysis in the context of bonus payments to individuals collecting unemployment insurance who find work early in their spells.

Finally, a decision about how to organize the assignment of the unemployed to services requires a full comparison of the benefits *and the costs* of the alternative methods under consideration. In this paper, we have compared observed mean outcomes under caseworker allocation to estimated mean outcomes under various statistical treatment rules. Caseworkers cost money, but so do statistical treatment rules. In particular, the latter require data collection, analysis, programming and so on. These are not cheap. At one point in the late 1990s, the State of Kentucky shut down its WPRS profiling system because it was cheaper to serve all of its Unemployment Insurance claimants than to serve only some and pay the University of Kentucky to operate the profiling system.

The findings here, in addition to their important implications for the question of how best to organize active labour market policy, also raise several broader questions, which we note here but whose resolution awaits future work. First, why do caseworkers think they do a good job of allocating individuals to services when in fact they do not? Second, could a system of feedback be developed that would allow them to update their beliefs and to learn to do better? Third, could some improved system of initial training allow the caseworkers to do better? Fourth, would a combination of caseworkers and guidance from statistical treatment rules dominate either mechanism on its own? The Frontline Decision Support System under development in the United States represents just such a hybrid. Finally, from a political economy standpoint, who benefits when caseworkers fail to maximize the (economic) efficiency of their allocation? Does the failure of casework allocation that we document represent special interests at work, human errors of design, or the outcome of a compromise between many competing policy goals?

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Table 1: Descriptions of programmes

COURSES	Courses must be necessary and adequate with the goal of improving individual employment chances; duration varies between one day and several months; here a minimum duration of two weeks is required
BASIC COURSES (BAC)	Short courses teach some basic skills not necessarily related to a particular occupation (*basic programme, courses to promote self-esteem and personality, courses for acquiring basic skills*)
LANGUAGE COURSES (LAC)	Language courses
COMPUTER COURSES (COC)	General computer courses, specific computer courses
FURTHER VOCATIONAL TRAINING (FVT)	Business and trade training (up to the level of a vocational degree), business and trade training (above the level of a vocational degree), manufacturing and technical training (up to the level of a vocational degree), and manufacturing and technical training (above the level of a vocational degree)
OTHER COURSES (OTC)	Practice firms, practical courses for the young unemployed, courses for jobs in the tourism sector, courses for jobs in the health care sector, and other courses
EMPLOYMENT PROGRAMMES	Goal: Work practise. These jobs should be as similar as possible to regular employment, but they should be *extraordinary*, i.e. employment programmes should not be in competition with other firms. Both public and private institutions offer employment programmes. During an employment programme the unemployed has to continue his job search and must accept any suitable job offer. Employment programmes usually last for six months.
PUBLIC (TE-PU)	Employment programmes within the public sector
PRIVATE (TE-PR)	Employment programmes within the private sector
TEMPORARY WAGE SUBSIDY (TEMP)	The objective of a TEMPORARY WAGE SUBSIDY is to encourage job seekers to accept job offers that pay less than their unemployment benefit by making up the difference with additional payments. The total income generated by this scheme is larger than the unemployment benefit. The TEMPORARY WAGE SUBSIDY scheme does not officially belong to the ALMP but there is compelling evidence that the placement offices intentionally use the subsidies as an active labour market policy instrument.

Note: We consider only treatments of at least two weeks in duration. (NONP) denotes non-participation.

Table 2: Number of observations and selected characteristics of different groups

Group		obs.	duration of programme	unemployment before...		qualifi-cation	foreign	employ-ed March 1999
		(persons)	(mean days)	(mean days)	(share of duration < 150 days)	(mean)	(share in %)	(share in %)
NONPARTICIPATION	(NONP)	6918	0	240		1.8	47	39
BASIC COURSES	(BAC)	1491	46	236	36	1.8	45	32
LANGUAGE COURSES	(LAC)	1719	71	225	36	2.2	72	29
COMPUTER COURSES	(COC)	1394	36	214	40	1.3	22	44
FURTHER VOCATIONAL TRAINING	(FVT)	424	74	231	35	1.6	38	42
OTHER TRAINING COURSES	(OTC)	497	94	263	23	1.8	43	42
EMPLOYMENT PROGRAMMES (PUBLIC)	(EP-PU)	1124	153	302	18	1.7	41	28
EMPLOYMENT PROGRAMMES (PRIVAT)	(EP-PR)	1349	142	299	18	2.0	51	25
TEMPORARY WAGE SUBSIDY	(TEMP)	4390	114	228	35	1.7	46	48

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

Table 3: Descriptive statistics of marginal probabilities

		Quantiles						
Group		1	5	25	median	75	95	99
NONPARTICIPATION	(NONP)	13.0	18.2	27.5	35.2	43.4	55.8	65.1
BASIC COURSES	(BAC)	0.9	1.6	4.4	7.4	10.3	15.3	20.3
LANGUAGE COURSES	(LAC)	0.1	0.5	2.2	5.0	12.2	29.9	45.0
COMPUTER COURSES	(COC)	0.2	0.6	1.9	5.0	10.7	20.6	27.5
FURTHER VOCATIONAL TRAINING	(FVT)	0.1	0.2	0.7	1.5	2.8	5.9	10.8
OTHER TRAINING COURSES	(OTC)	0.2	0.3	0.5	1.7	3.2	7.0	13.1
EMPLOYMENT PROGRAMMES (PUBLIC)	(EP-PU)	0.1	0.3	1.7	4.0	8.2	16.9	24.3
EMPLOYMENT PROGRAMMES (PRIVAT)	(EP-PR)	0.2	0.5	2.1	4.7	9.6	21.1	31.5
TEMPORARY WAGE SUBSIDY	(TEMP)	5.7	9.1	15.8	21.6	28.1	40.0	52.5

Note: Probabilities in %.

Table 4: Average potential employment rates one year after the programme starts in %-points relative to observed state

Programme (l) ------------------- Population (m)	Non-part.	basic courses	language courses	computer courses	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
					$EY^l \mid S = m$				
NONP	41.3	*-9.9*	-2.8	-3.1	-3.4	4.0	-8.8	-8.9	**7.3**
BAC	9.3	35.7	4.4	0.2	11.2	**17.7**	3.3	0.9	14.2
LAC	9.4	-3.0	31.1	2.6	12.4	**17.2**	*-11.4*	2.2	16.7
COC	6.8	*-12.0*	-0.7	45.6	6.4	**16.5**	0.0	-1.6	9.7
FVT	-3.0	-12.2	4.5	-2.0	44.7	8.4	*-12.6*	-11.8	**10.7**
OTC	-4.0	*-11.4*	-4.6	-7.4	-9.5	44.2	-8.9	-10.0	**8.3**
TE-PU	6.8	*-4.0*	3.8	5.2	10.3	13.1	32.9	5.8	**13.7**
TE-PR	7.3	-0.7	-1.4	2.0	-0.4	15.1	*-1.1*	30.9	**19.5**
TEMP	-5.7	*-16.6*	-9.1	-9.7	-2.8	**4.6**	-16.7	-12.5	51.2
All (levels)	42.9	*32.2*	38.8	38.6	42.3	50.0	33.2	35.4	**50.0**
					standard errors of estimated levels				
NONP	0.7	2.7	2.5	2.9	4.3	4.2	3.3	3.0	1.4
BAC	1.8	1.5	3.0	3.3	4.6	4.6	3.9	3.4	2.1
LAC	2.2	3.3	1.3	7.7	9.1	6.2	5.4	4.7	2.6
COC	1.9	2.9	3.2	1.6	4.1	5.2	4.4	4.2	2.2
FVT	3.1	3.8	3.9	3.5	2.9	5.7	4.8	4.4	3.2
OTC	3.1	3.7	4.4	4.7	6.9	2.9	4.3	4.3	3.3
TE-PU	2.4	3.7	4.0	4.2	5.6	5.4	2.1	3.6	2.6
TE-PR	2.3	3.2	3.8	4.7	6.1	5.9	3.9	1.9	2.6
TEMP	1.3	2.6	3.3	3.1	4.6	4.8	3.5	3.2	1.0
All	0.8	2.1	2.2	2.6	3.8	3.8	2.9	2.6	1.2

Note: The employment rate is measured at day 365 after the start of the programme. Results are based on matched samples. The value for the treatment with largest expected outcome for the particular population appears in **bold** and the one with the smallest expected outcome appears in *italics*.

Table 5: Average employment rates one year after the programme starts in %-points for population most likely to participate in specific programme

Programme (l) ------------------- Population (m)	Nonpart.	BAC	LAC	COC	FVT	OT	EP public	EP private	TEMP	treated obs.
					$E[Y^l \mid S=m, P(S=m \mid x) > a^m]$					
NONP	33.4	*-11.6*	1.1	-7.9	-0.4	-6.6	-1.1	-9.6	**16.1**	
BAC	10.5	37.5	6.2	-2.6	-0.9	**16.8**	8.2	*-9.0*	10.6	
LAC	9.2	-0.9	25.8	3.6	19.6	**24.4**	*-12.8*	8.9	22.3	
COC	8.2	*-7.2*	3.9	48.3	6.8	**23.2**	2.0	-2.6	14.0	
FVT	5.1	-11.2	5.6	-7.9	45.8	**14.7**	*-18.6*	-14.5	12.0	
OTC	4.3	-1.8	5.2	*-11.6*	-6.1	37.1	8.1	-4.5	**10.5**	
TE-PU	4.3	8.9	8.9	10.3	4.4	**31.4**	*29.4*	4.8	11.8	
TE-PR	8.3	1.7	*-1.9*	5.6	-1.4	**31.4**	-2.8	22.9	28.9	
TEMP	-3.3	-14.0	-5.6	-7.5	9.0	**11.5**	*-18.4*	-1.3	52.2	
					standard error of estimate					
NONP	1.2	6.1	4.5	5.3	9.1	7.3	6.4	5.7	2.9	1659
BAC	3.2	2.6	5.7	6.0	9.1	8.3	7.3	6.1	3.9	344
LAC	3.0	4.1	1.5	11.3	13.1	9.3	7.3	6.9	3.6	802
COC	2.7	4.4	4.7	2.1	6.0	8.0	7.4	7.8	3.1	551
FVT	4.3	5.5	6.1	5.0	3.9	8.3	8.3	7.5	4.5	166
OTC	4.5	6.2	9.0	8.8	12.8	4.2	8.3	8.2	5.5	132
TE-PU	4.2	7.9	9.7	9.3	13.1	17.0	3.6	7.1	5.6	163
TE-PR	3.9	6.4	6.6	10.0	9.5	11.8	7.3	2.8	5.3	227
TEMP	2.7	6.1	9.6	7.3	9.8	13.4	7.6	8.1	1.8	808

Note: The employment rate is measured at day 365 after the start of the programme. Results are based on matched samples. The value for the treatment with largest expected outcome for the particular population appears in **bold** and the one with the smallest expected outcome appears in *italics*. The population in the table is defined as individuals above the 80th quantile (in the full population) of the distribution of the marginal probability for being in that particular treatment. Therefore, the sample in each row is only 20% of the sample used for the estimates presented in Table 4.

Table 6: Differences between those people most likely to be allocated to specific programme and actual allocation

Programme (l) ------------------- Population (m)	Nonpart.	BAC	LAC	COC	FVT	OT	EP public	EP private	TEMP
NONP	-	-1.7	3.9	-4.8	3.0	-10.6	7.7	-0.7	8.8
BAC	1.2	-	1.8	-2.8	-12.1	-0.9	4.9	-9.9	-3.6
LAC	-0.2	2.1	-	1.0	7.2	7.2	-1.4	6.7	5.6
COC	1.4	4.8	4.6	-	0.4	6.7	2.0	-1.0	4.3
FVT	8.1	1.0	1.1	-5.9	-	6.3	-6.0	-2.7	1.3
OTC	8.3	9.6	9.8	-4.2	3.4	-	17.0	5.5	2.2
TE-PU	-2.5	12.9	5.1	5.1	-5.9	18.3	-	-1.0	-1.9
TE-PR	1.0	2.4	-0.5	3.6	-1.0	16.3	-1.7	-	9.4
TEMP	2.4	2.6	3.5	2.2	11.8	6.9	-1.7	11.2	-

Note: Entries in Table 5 minus entries in Table 4.

Table 7A: Allocation of participants using different assignment rules

		All		Native languages 'Swiss'	
Assignment		Mean	Std. error	Mean	Std. error
Random assignment in existing treatment proportions		42.2		46.9	
Case worker assignment		41.5		46.1	
Assignment of everyone to	NONP	42.9	0.8	46.4	1.1
	BAC	*32.2*	2.1	36.9	2.6
	LAC	38.8	2.2	46.9	3.4
	COC	38.6	2.6	42.4	2.3
	FVT	42.3	3.8	47.6	4.1
	OTC	50.0	3.8	54.2	4.6
	TE-PU	33.2	2.9	42.6	3.5
	TE-PR	35.4	2.6	41.1	3.4
	TEMP	50.0	1.2	53.4	1.4
Assignment to treatment based on largest predicted gross impact for each person		55.5		61.9	
Assignment to treatment based on largest predicted gross impact for each person without nontreatment		57.2		63.3	
Assignment to treatment based on smallest predicted gross impact for each person		25.7		30.3	
Assignment to treatment based on smallest predicted gross impact for each person without nontreatment		26.7		31.0	
Assignment to treatment based on largest predicted gross impact for each person imposing national supply constraint – Effect based		49.3		54.8	
Assignment to treatment based on smallest predicted gross impact for each person imposing national supply constraint – Effect based		37.0		40.2	
Assignment to treatment based on largest predicted gross impact for each person imposing national supply constraint – Need based		47.8		53.6	
Assignment to treatment based on largest predicted gross impact for each person imposing regional supply constraint - – Effect based		48.4		54.0	
Assignment to treatment based on smallest predicted gross impact for each person imposing regional supply constraint – Effect based		37.5		40.6	
Assignment to treatment based on largest predicted gross impact for each person imposing regional supply constraint – Need based		47.2		52.7	

Note: The seven regions used to define the regional supply constraints are defined as follows: (SG, AI, AR, TH, GR, GL, SH), (LUZ SZ, UR, OW, NW, ZU), (BE, FR, JU, SO, NB), (WT, WS, GE), (BS, BL, AA), TE, ZR. "Swiss" languages are defined for the current study as German, French and Italian.

Table 7B: Allocation of participants to treatments when assignment rules allow a deviation from the observed shares

Assignment	NONP	BAC	LAC	COC	FVT	OTC	TE-PU	TE-PR	TEMP
	All								
Observed shares	38.8	7.8	10.1	8.1	2.4	2.3	4.1	4.9	21.5
Assignment to treatment based on largest predicted gross impact for each person	1.6	0.02	1.6	6.3	25.2	27.0	2.2	3.1	33.0
Assignment to treatment based on largest predicted gross impact for each person without nontreatment		0.02	1.7	6.4	25.4	27.4	2.2	3.1	33.8
Assignment to treatment based on smallest predicted gross impact for each person	0.9	10.1	16.2	0.7	5.97	3.28	48.2	14.4	0.1
Assignment to treatment based on smallest predicted gross impact for each person without nontreatment		10.2	16.5	0.7	6.2	3.4	48.3	14.5	0.1
Native languages 'Swiss'									
Assignment to treatment based on largest predicted gross impact for each person	1.1	0.01	6.7	3.5	24.5	31.2	2.9	2.0	27.8
Assignment to treatment based on largest predicted gross impact for each person without nontreatment		0.02	7.0	3.1	25.8	33.3	3.0	2.3	25.6
Assignment to treatment based on smallest predicted gross impact for each person	0.5	22.9	5.0	3.0	17.3	7.3	25.0	18.9	0.2
Assignment to treatment based on smallest predicted gross impact for each person without nontreatment		22.9	5.7	2.9	17.2	5.7	25.9	19.5	0.2

Table 8: Allocation of participants using different assignment rules - Heterogeneity

Assignment		RAV I Rural	RAV I Urban	RAV II Type I	RAV II Type II	RAV II Type III	Region (by language) Ger	Region (by language) F, I
Random assignment in existing treatment proportions		43.6	41.1	45.4	41.1	38.8	45.2	37.9
Case worker assignment		42.4	40.5	44.2	41.1	38.1	44.9	36.8
Assignment of everyone to	NONP	44.7	41.1	47.0	41.8	38.6	46.6	38.4
	BAC	34.5	37.6	35.4	35.1	32.1	37.7	28.2
	LAC	37.3	34.4	37.4	39.3	32.5	35.5	38.5
	COC	42.5	42.1	43.4	38.1	41.6	44.5	36.1
	FVT	46.7	37.2	61.3	37.9	35.6	58.7	28.4
	OTC	44.0	48.2	52.1	42.1	39.2	56.2	33.8
	TE-PU	31.0	33.0	41.8	22.5	25.3	36.9	23.9
	TE-PR	42.6	25.8	43.1	27.2	32.1	39.9	31.4
	TEMP	50.1	49.6	50.0	51.2	48.6	52.7	45.6
Assignment to treatment based on largest predicted gross impact		60.4	61.1	72.5	61.1	62.7	67.4	54.7
Assignment to treatment based on largest predicted gross impact without nontreatment		61.8	62.2	72.3	61.5	65.3	68.0	55.4
Assignment to treatment based on smallest predicted gross impact		23.8	15.5	25.8	14.1	13.7	28.3	14.7
Assignment to treatment based on smallest predicted gross impact without nontreatment		24.2	16.2	26.1	13.9	14.7	28.4	14.8
Assignment to treatment based on largest predicted gross impact imposing national supply constraint – Effect based		51.6	53.1	55.9	52.5	52.6	52.7	49.0
Assignment to treatment based on smallest predicted gross impact imposing national supply constraint – Effect based		33.3	36.0	35.8	32.3	29.1	38.5	30.2
Assignment to treatment based on largest predicted gross impact imposing national supply constraint – Need based		49.7	51.9	54.2	49.6	49.6	50.8	46.1
Assignment to treatment based on largest predicted gross impact imposing regional supply constraint - – Effect based		50.3	52.0	55.2	51.7	50.6	52.0	48.2
Assignment to treatment based on smallest predicted gross impact imposing regional supply constraint – Effect based		33.9	36.4	36.2	32.3	29.9	39.0	30.5
Assignment to treatment based on largest predicted gross impact for each person imposing regional supply constraint – Need based		48.3	50.6	53.7	48.8	48.2	50.2	45.5

Note: Types I to III relate to a classification by Atag Ernst and Young Consulting (1999).
Supply constraints imposed as observed in the specific subgroup considered.

Appendix A: Data

Table A.1: Descriptive Statistics

Variable	Non-part.	basic courses	lan-guage courses	compu-ter courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Number of observations	6918	1491	1719	1394	424	497	1124	1349	4390
					Days, Years, Swiss Francs				
Current Unemployment Spell									
Begin of first programme [a)]	87[b)]	95	76	80	84	107	135	135	100
Duration of first programme	0	46	71	36	74	94	153	142	114
Duration of current unemployment spell at begin of programme	240	236	225	214	231	263	302	299	228
Remaining time of benefit entitlement at start of programme	339	381	411	410	401	352	336	339	343
Duration of current unempl., 31.12.97	153	141	149	134	146	156	167	164	128
Remain. days of „passive regime“, 31.12.97	50	52	53	59	52	43	31	31	52
Unemployment benefit	125.3	125.2	125.2	124.3	120.3	128.3	124.8	125.0	123.9
Age in years	38.0	38.4	37.0	38.3	38.1	37.3	39.2	38.5	37.5
					Proportions in %				
Younger than 30	24	23	26	23	23	27	21	22	25
Older than 50	11	11	7	13	9	10	14	11	9
Female	43	46	55	46	33	55	37	39	42
Number of persons to support	2.21	2.26	2.28	2.22	2.22	2.31	2.23	2.23	2.23
At least one person to support	61	62	63	63	62	65	62	63	62
Mother tongue									
German	30	39	9	49	38	33	37	30	35
French	21	15	13	28	29	21	19	17	19
Italian	12	8	12	8	7	8	9	13	12
Not German/French/Italian	37	38	66	15	25	38	36	41	34
Language spoken in canton of residence	51	52	21	76	67	54	54	50	54
G/F/I, but not canton language	11	10	13	10	8	8	10	10	12
Foreign Languages									
Other Swiss language	64	63	79	54	59	67	64	69	65
English, Spanish, Portugese	14	13	10	26	20	15	12	9	12
Other languages	2	1	2	2	2	1	1	1	1
Marital Status									
Single	26	27	13	35	29	27	30	28	27
Married	61	59	77	47	58	57	55	58	59
Widowed	1	1	1	1	0	1	1	1	1
Divorced	13	13	8	17	13	15	14	13	13
Nationality									
Swiss	53	55	28	78	62	57	59	49	54
Foreign with permanent permit	32	30	39	17	28	23	26	32	31
Foreign with yearly permit	15	15	33	5	11	20	15	19	15

Table A.1 to be continued

Table A.1 continued

Variable	Non-part.	basic courses	language courses	computer courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Qualification									
Skilled	53	54	34	80	63	51	54	43	54
Semi-skilled	16	15	17	9	18	18	18	16	17
Unskilled	31	31	49	11	19	31	28	41	29
Chances to find a job									
No Information	6	5	5	5	4	4	6	4	8
Very easy	6	4	4	5	5	6	3	5	6
Easy	14	13	10	19	14	14	13	12	17
Medium	53	57	54	58	62	57	56	52	56
Difficult	17	18	25	11	13	15	18	22	12
Special case	4	3	2	1	2	4	4	5	2
Mobility									
Not mobile	12	4	8	8	6	11	5	5	8
Daily commuter	83	91	88	85	88	84	90	89	88
Mobile within Switzerland or abroad	5	5	4	7	5	5	5	5	5
Looking for ..... job									
Full-time	34	39	43	34	40	35	37	35	38
Part-time	16	14	12	18	11	16	12	12	12
No information	49	47	45	49	49	49	51	52	50
Unemployment-status									
Full-time	78	81	83	77	85	81	84	81	81
Part-time	18	16	14	18	11	16	13	15	13
In part-time employment	2	1	1	1	1	1	1	2	4
Other	2	2	2	3	3	1	2	2	3
Monthly earnings in last job									
Less than 1000	2	2	3	4	3	1	2	2	3
Between 1000 and 2000	11	11	13	12	14	10	11	10	11
Between 2000 and 3000	25	24	21	22	25	25	24	25	24
Between 3000 and 4000	27	27	29	27	27	27	27	28	28
Between 4000 and 5000	20	20	17	17	17	17	19	19	20
Between 5000 and 6000	8	9	9	9	6	11	9	7	8
More than 6000	8	7	8	9	8	9	7	8	7
Duration of unemployment spell at beginning of programme									
Less than 90 days	18	19	17	18	15	10	6	7	19
Less than 180Tage	42	44	44	48	44	34	23	24	44
Less than 270 days	60	62	66	70	65	58	42	44	63
Less than 365 days	78	80	84	85	81	76	65	66	81
More than 365 days	22	20	16	15	19	24	35	34	19
Job position									
Self-employed	1	0	1	2	1	0	1	1	0
High (management, etc.)	6	7	3	9	10	4	4	3	5
Medium	56	52	39	73	60	55	52	46	58
Low	37	41	58	16	28	41	44	51	37

Table A.1 to be continued

Table A.1 continued

Variable	Non-part.	basic courses	language courses	computer courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Previous occupation									
Agriculture	2	1	2	1	0	1	2	2	2
Mining	0	0	0	0	0	0	0	0	0
Food, tobacco	1	1	1	1	0	1	1	1	1
Textiles	1	1	4	1	0	0	1	2	1
Wood and paper	1	1	1	1	1	0	2	2	1
Chemical	0	0	0	0	0	0	0	0	0
Metals	7	8	5	6	12	4	6	9	8
Watches, jewelry	0	0	0	0	0	0	0	1	0
Health care	3	3	2	3	2	5	3	2	3
Architecture, engineers	1	2	2	5	5	1	2	1	2
Construction	8	6	7	3	7	4	9	8	10
Transportation	4	3	1	2	2	3	4	5	4
Restaurants	16	14	19	8	7	32	14	15	17
Printing	1	1	0	1	1	1	1	1	1
Minerals	0	0	0	0	0	1	0	0	0
Entrepreneurs, senior officials, justice	4	4	3	5	5	3	2	2	2
Painting, technical drawing	5	5	5	6	8	2	7	5	8
Office and computer	14	15	13	28	19	17	14	11	12
Retail trade	9	11	5	13	15	7	7	6	7
Security, cleaning, clerical, social work	5	5	9	2	2	5	5	5	5
Science	2	1	1	3	2	1	2	1	1
Artist	2	1	1	2	1	1	1	2	2
Education	2	1	1	2	1	2	3	2	3
News and communication	1	1	1	2	1	0	1	1	1
Body care	1	1	0	0	0	1	0	0	1
Other	8	12	13	5	7	6	11	16	8
Correspondence between desired and previous job									
2-digit	73	72	70	74	69	67	68	69	75
3-digit	68	66	65	66	62	60	62	63	69
Previous industry sector									
Agriculture	2	1	1	1	0	1	2	2	2
Mining, energy, water	0	0	0	0	0	0	0	0	0
Construction	13	10	11	7	14	6	14	11	17
Public services	11	9	9	9	8	10	10	10	6
Other services	5	4	4	6	4	6	6	7	5
Health care	4	3	3	4	3	5	4	3	4
Research and development	0	0	0	1	0	1	1	0	0
Education	2	2	1	2	2	2	3	2	2
Banking, insurance	3	3	2	6	5	4	2	1	2
Real estate	1	1	1	1	1	1	1	1	1
Consulting	11	11	11	16	12	7	10	11	12
Transportation	3	3	2	4	4	3	3	3	3
News and communication	0	0	0	1	0	1	0	1	0
Trade	15	17	15	19	17	13	15	15	13

Table A.1 to be continued

Table A.1 continued

Variable	Non-part.	basic courses	lan-guage courses	compu-ter courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Restaurants, catering	15	13	17	8	6	27	11	13	16
Repairs	2	1	1	2	1	1	1	1	2
Food, tobacco	1	2	2	1	0	1	1	2	1
Textiles	1	2	2	1	1	1	1	2	1
Wood, furniture	1	1	1	1	2	1	1	1	1
Paper, paper products	0	0	0	0	0	0	0	0	0
Printing	1	2	1	3	1	2	1	1	1
Leather	0	0	0	0	0	0	0	0	0
Chemical	1	2	1	1	1	1	1	1	1
Non-ferrous minerals	1	1	1	0	1	1	1	1	1
Metals	2	3	3	2	6	2	3	4	2
Machinery and equipment	2	3	3	2	6	2	3	2	3
Electrical machinery, optics	2	2	3	2	3	1	2	3	2
Watches, jewelry	1	1	0	1	1	1	1	1	1
Other manufacturing	1	1	1	1	1	0	1	1	1
Industry unemployment rate in %, 1/98	6.6	6.3	6.8	5.7	5.6	7.5	6.2	6.4	6.7
Canton									
Zurich	22	22	27	21	23	18	29	6	18
Berne	8	10	8	9	5	5	14	14	10
Lucerne	3	4	5	5	4	8	3	4	3
Uri	0	0	0	0	0	2	0	0	0
Schwyz	0	3	1	1	0	1	2	1	1
Obwalden	0	0	0	0	0	2	0	0	0
Nidwalden	0	0	0	0	0	1	1	0	0
Glarus	0	0	0	0	0	3	1	0	0
Zug	1	0	2	1	1	1	1	2	1
Freiburg	2	6	4	4	2	2	6	2	3
Solothurn	2	5	2	2	3	1	1	8	4
Basel-City	4	4	3	4	2	2	2	4	3
Basel-Landschaft	2	4	2	3	2	1	1	3	2
Schaffhausen	1	0	1	2	2	0	1	0	1
Appenzell AR	0	0	0	0	0	0	0	1	0
Appenzell IR	0	0	0	0	0	0	0	0	0
St. Gall	4	6	9	4	5	5	2	2	5
Graubünden	2	3	1	2	1	1	0	3	1
Aargau	5	5	9	4	7	6	1	8	5
Thurgau	1	1	3	3	1	2	3	1	2
Ticino	9	2	5	5	4	10	4	12	8
Waadt	14	15	6	17	19	14	10	13	13
Wallis	4	3	3	3	5	4	7	7	8
Neuenburg	4	1	1	2	1	2	7	4	3
Geneva	12	3	8	9	7	10	3	4	7
Jura	1	1	0	1	7	1	2	1	1
Cantonal unemployment rate	5.33	4.72	4.65	5.09	5.19	5.09	5.02	5.16	5.21

Table A.1 to be continued

Table A.1 continued

Variable	Non-part.	basic courses	lan-guage courses	compu-ter courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Canton language									
German	53	70	74	59	56	58	61	58	58
French	38	28	21	36	41	32	35	31	34
Italian	9	2	5	5	4	10	4	12	8
Region									
Eastern	8	12	15	10	9	11	7	7	10
Central	4	7	8	7	6	14	7	7	5
South-west	31	20	16	29	31	28	21	24	28
North-west	10	14	14	10	11	8	4	15	11
West	17	23	16	18	17	10	29	29	20
Size of town where worked before									
<1000	8	7	5	9	8	7	8	9	9
<2000	16	14	11	16	16	15	16	19	18
<5000	32	31	28	31	34	35	33	37	36
<10'000	44	45	42	45	47	47	46	51	49
<20'000	62	62	60	62	65	62	61	68	67
<30'000	67	66	67	68	72	69	66	76	73
<50'000	72	70	71	73	74	72	73	80	77
<100'000	76	76	79	78	80	76	77	84	81
> 100'000.	24	24	21	22	20	24	23	16	19
<200'000 .	92	89	89	93	92	90	88	97	94
> 200'000	8	11	11	7	8	10	12	3	6
Region of placement office									
Large city	47	42	42	45	42	36	44	26	38
Small city	37	36	40	40	42	45	38	52	42
Rural	15	21	17	14	17	18	17	20	19
No information	1	1	1	1	0	0	0	1	1
Long-term unemployment in regional placement office									
Inflow to long-term unemployment [c)]	2.1	2.0	2.0	2.1	2.2	2.1	2.0	2.0	1.9
Outflow from long-term unemployment [d)]	1.1	1.1	1.1	1.2	1.2	1.2	1.0	1.1	1.1
No information	17	21	18	18	14	16	21	18	21
Remaining benefit eligibility									
Less than 6 months	21	16	12	14	13	19	16	17	20
Less than 12 months	47	38	32	32	36	46	49	50	45
Less than 18 months	77	69	66	62	71	79	83	82	72
More than 18 months	17	23	25	28	25	18	12	14	19
Unemployment history									
First spell	60	64	70	67	63	57	64	63	57
Number of spells prior to current spell	0.51	0.45	0.37	0.39	0.47	0.55	0.46	0.46	0.55
Duration of previous spell / 1000	0.08	0.07	0.06	0.07	0.07	0.08	0.09	0.09	0.08

Table A.1 to be continued

Table A.1 continued

Variable	Non-part.	basic courses	language courses	computer courses	vocat. training	other training courses	employment programmes public	employment programmes private	temp. wage subsidy
Sanction days without benefit payment									
Number of sanction days during last unemployment spell	4.4	5.0	4.0	3.6	4.7	5.2	4.2	5.2	3.7
Share in total unemployment spell	0.05	0.06	0.05	0.05	0.05	0.06	0.04	0.07	0.05
Positive number of sanction days (in %)	26	25	25	22	25	28	23	26	22
Previous programme participation									
Sum of short programs between July and December 1997	0.04	0.08	0.07	0.09	0.10	0.08	0.05	0.07	0.06
Participation in training course or employment programme between July and December 97 (less than 14 days)	1	0	1	0	1	1	1	0	0
Employment programme before July 97	1	1	1	1	1	1	2	1	1
Training course before July 97	1	0	1	0	1	1	1	0	0
Temporary wage subsidy before July 97	1	2	1	1	2	1	2	1	3
Employment history from social security data									
Number of months unemployed since entry into social security system	7.6	6.8	5.4	6.4	6.5	6.8	7.9	8.5	6.4
Number of months employed since entry into social security system	85	86	73	91	90	80	86	82	90
Number of months out of labour force since entry into social security syst.	15.4	14.8	14.5	14.0	13.0	15.8	14.8	15.8	12.6
Never unemployed	36	41	49	43	38	39	38	36	40
Month of entry into social security system	12.1	12.6	27.3	8.7	10.6	17.4	11.6	13.5	11.4
Number of employment spells	3.53	3.28	2.87	3.03	3.18	3.23	3.44	3.61	3.51
Number of unemployment spells	1.41	1.26	0.92	1.11	1.24	1.27	1.4	1.51	1.29
Mean duration of employment spell in months	40	43	39	49	46	39	41	37	43
Mean duration of unemployment spell [e] in months	5.9	5.9	6.5	6.3	5.8	5.7	6.2	6.2	5.3
Standard deviation of wages / 1000	0.99	0.93	0.78	1.06	1.04	0.88	0.94	0.87	0.94
Duration of last employment spell	40	43	41	48	46	40	42	38	43
Wage growth during last employment spell	81	180	106	113	25	148	78	62	69
Proportion of time unemployed in %	7	6	6	6	6	7	7	8	6
Proportion of time employed in %	78	79	76	81	81	76	78	76	82

Notes: [a] The start of a programme is measured in days since 1.1.98. [b] Simulated.
[c] Mean number of transition into long-term unemployment relative to total unemployment within regional placement offices. [d] Mean number of transitions to employment relative to total unemployment within regional placement offices
[e] This variable takes a value of zero for persons who have never been unemployed before.

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.[1] Although 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.[2]

The variables included in the MNP are selected by a preliminary specification search based on binary probits (each relative to the reference category NONPARTICIPATION) and score tests against omitted variables. Entries for variables excluded from a particular choice equation show a "0" for the estimated coefficient and "-" for the standard error. Based on this procedure, 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 to past employment and earnings.

In practice, some restrictions on the covariance matrix of the errors terms of the MNP need to be imposed, both because not all elements of the covariance matrix are identified and to avoid excessive numerical instability. Guided by considerations of similarity of options and sample size, we allowed for free correlations between COMPUTER COURSES, FURTHER VOCATIONAL TRAINING, LANGUAGE COURSES and BASIC TRAINING, as well as between EMPLOYMENT PROGRAMME (PUBLIC), EMPLOYMENT PROGRAMME (PRIVATE), and TEMPORARY WAGE SUBSIDY. Furthermore, the variance of the error term related to TEMP is not restricted (for details see Table B.2).

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

2 This section is taken from Gerfin and Lechner (2001).

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

Variable	Basic courses	language courses	computer courses	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
Age in years / 10	***0.06***	0	0	0	0	***0.11***	***0.12***	0
Older than 45	0	**-0.11**	0	0	0	0	0	0
Female	**0.10**	***0.20***	-0.11	**-0.55**	*0.09*	***-0.16***	***-0.20***	***0.15***
Marital status married	0	0	***-0.19***	0	0	***-0.19***	***-0.23***	0
Marital status divorced	0	0	0	0	0	0	0	**0.12**
Number of persons to support	0	**0.03**	0	0	0	0	0	0
Mother tongue								
French	0	***1.13***	0	0	0	0	0	0
Italian	0	***0.74***	0	0	0	0	0	0
Not German/French/Italian	0	***1.18***	***-0.47***	**-0.57**	0	0	0	***-0.31***
GF/I, but not canton language	0	***0.39***	0	*-0.65*	0	0	**-0.13**	**-0.11**
Foreign Languages								
Other Swiss language	0	***0.15***	**0.24**	0	0	**0.08**	***0.12***	***0.14***
English, Spanish, Portuguese	0	***0.27***	***0.42***	0	0	0	0	0
Looking for ... job (reference category: no information)								
Full-time	0	**0.09**	0	0	0	0	0	0
Part-time	0	*-0.13*	0	0	0	0	0	***-0.21***
Unemployment-status (reference category: part-time)								
Full-time	***0.31***	***0.24***	0.09	**0.49**	***0.32***	***0.44***	***0.29***	***0.22***
In part-time employment	0	0	0	0	0	0	0	***1.26***
Nationality (reference category: Swiss)								
Foreign with permanent permit	0	0	***-0.50***	0	***-0.20***	***-0.22***	0	0
Foreign with yearly permit	0	0	***-0.74***	0	*-0.12*	**-0.13**	0.07	0
Monthly earnings in last job (reference category: between 2000 and 6000)								
Less than 2000	0	0	**0.24**	*0.39*	0	0	0	0
More than 6000	**-0.15**	0	0	0	0	0	0	0
Chances to find a job (reference category: medium)								
No information	*-0.13*	-0.09	-0.16	-0.35	-0.19	***-0.25***	***-0.33***	*0.13*
Very easy	0.07	*-0.17*	-0.01	-0.01	-0.03	*-0.16*	***-0.27***	-0.06
Easy	-0.03	***-0.17***	0.11	-0.28	-0.03	***-0.16***	**-0.14**	**0.11**
Difficult	-0.05	**0.12**	**-0.25**	*-0.36*	**-0.16**	*-0.09*	0.02	***-0.34***
Special case	-0.14	**-0.24**	***-0.79***	*-0.93*	-0.08	**-0.20**	-0.04	***-0.87***
Qualification (reference categories: semi-skilled, unskilled)								
Skilled	0	***-0.15***	***0.62***	0	0	0	0	0
Previous industry sector (reference categories: agriculture, mining/energy/water, other services, health care, education, banking/insurance, real estate, transportation, news and communication, trade, repairs, food/tobacco, textiles, wood/furniture, paper/paper products, leather, chemical, non-ferrous minerals, machinery and equipment, electrical machinery/optics, watches/jewelry, other manufacturing)								
Construction	***-0.16***	0	0	0	***-0.31***	0	***-0.36***	0
Public services	0	0	0	0	0	0	0	***-0.33***
Consulting	0	0	***0.32***	0	**-0.17**	0	0	0
Restaurants, catering	0	0	0	0	0	0	***-0.37***	0
Printing	0	0	***0.73***	0	0	0	0	0
Metals	0	0	0	**0.86**	0	0	0	0
Industry unemployment rate in %, 1/98	*-0.08*	-0.06	***-0.32***	**-0.68**	0.03	***-0.18***	0.15	0.05

Table B.1 to be continued

Table B.1 continued

Variable	Basic courses	language courses	computer courses	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
Job position function (reference category: assistant)								
Self-employed	0	0	0	0	0	0	0	***-0.61***
High (management, etc.)	0	0	*0.28*	0	**-0.24**	***-0.41***	***-0.45***	0
Medium	0	0	***0.40***	0	0	0	***-0.13***	0
Previous occupation (reference categories: mining, wood and paper, chemical, minerals, artist)								
Agriculture	0	-0.18	**-0.79**	*-2.19*	0	0	0	0
Food, Tobacco	0	**-0.46**	0	0	0	0	0	0
Textiles	0	***0.40***	0	0	0	0	0	0
Metals	0	***-0.31***	-0.20	*0.72*	0	***-0.29***	0	0
Health care	0	0	0	0	***0.45***	0	0	0
Architecture, engineer	0	**0.32**	***0.84***	**1.74**	0	0	0	0
Construction	-0.09	***-0.24***	***-0.72***	0	0.00	0	-0.03	0
Transportation	0	***-0.59***	*-0.36*	-0.79	0	0	0	0
Restaurants	0	**-0.12**	0	0	***0.42***	0	0	0
Printing	0	***-0.84***	0	0	0	0	0	***-0.57***
Entrepreneurs, senior officials, justice	0	0	0	0	0	***-0.36***	***-0.38***	***-0.76***
Painting, technical drawing	0	0	0	**0.75**	0	0	0	***0.20***
Office and computer	0	***0.22***	***0.50***	**0.82**	***0.31***	0	0	***-0.25***
Retail trade	**0.15**	0	***0.36***	**1.10**	0	***-0.19***	**-0.17**	***-0.33***
Security, cleaning, clerical, social work	0	0	***-0.63***	0	0	0	0	0
Science	***-0.40***	0	0	0	0	0	0	***-0.49***
Education	0	0	***-0.84***	0	0	0	0	0
News and communication	0	0	***0.87***	0	0	0	0	0
Body care	0	***-1.55***	**-0.99**	0	0	***-1.16***	***-0.96***	0
Other	***0.15***	0	0	0	0	0	***0.21***	0
Desired = previous job, 3-digit	0	0	**-0.14**	*-0.33*	***-0.14***	0	0	0
Additional regional effects by canton								
Berne	***-0.49***	0	0	0	0	0	0	0
Lucerne	0	0	0	0	***-1.08***	0	0	0
Schwyz	***0.99***	0	0	0	***-1.08***	***0.70***	0	0
Glarus	0	0	0	0	***1.52***	0	0	0
Zug	***-1.55***	0	0	0	***-1.99***	0	0	0
Freiburg	***0.51***	0	0	0	0	**0.24**	0	0
Solothurn	0	0	0	0	0	***-0.91***	***0.41***	0
Basel-City	***-0.51***	*-0.28*	0	0	0	0	***-0.56***	*-0.23*
St. Gall	0	0	0	0	0	***-0.61***	***-0.87***	0
Graubünden	0	0	**0.87**	0	0	***-1.45***	0	***-0.46***
Aargau	**-0.25**	***0.55***	***-0.90***	0	0	***-0.41***	0	*-0.20*
Thurgau	0	0	**0.73**	0	0	***0.43***	0	0
Ticino	0.25	**-0.44**	***-1.45***	**-2.17**	0.09	*-0.28*	***1.65***	-0.08
Waadt	0	***-0.53***	0	0	0	***-0.66***	***-1.00***	***-0.51***
Neuenburg	***-0.79***	***-1.15***	***-1.01***	**-1.86**	0	0	0	***-0.50***
Geneva	***-1.20***	***-0.41***	*-0.25*	**-0.70**	0	***-1.47***	***-1.83***	***-0.68***
Jura	**-0.59**	***-0.64***	0	**3.67**	0	0	0	***-0.75***
Cantonal unemployment rate	***-0.28***	-0.01	***0.28***	*0.38*	-0.06	**-0.08**	***-0.28***	-0.02

Table B.1 to be continued

Table B.1 continued

Variable	Basic courses	language courses	computer courses	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
Region (reference category: Zurich)								
Eastern	0.09	***0.31***	*0.28*	0.14	0.03	0.09	***0.64***	***0.38***
Central	*0.19*	***0.41***	***0.88***	*0.61*	***1.76***	**0.20**	***0.81***	0.10
South-west	***0.96***	***-0.44***	***-0.99***	*-0.86*	*0.24*	***0.57***	***2.14***	***0.60***
North-west	***0.40***	-0.12	**0.48**	-0.04	-0.02	***-0.37***	***0.94***	**0.29**
West	***0.69***	0.01	0.03	-0.57	-0.11	***0.37***	***1.01***	***0.34***
Size of town where worked before (reference categories: <100'000, <50'000, <20'000, <10'000)								
>200'000	***0.28***	0	***-0.64***	0	***0.37***	0	0	0
<30'000	0	0	0	0	0	0	0	***0.13***
<5000	0	0	0	0	0	***0.10***	0	0
<2000	*-0.09*	0	*-0.18*	0	0	0	0	0
Region of placement office (reference categories: small city, no information)								
Large city	0	0	0	0	***-0.31***	0	**-0.16**	0
Rural	0	0	***-0.42***	0	0	0	0	0
Long-term unemployment in regional placement office								
Inflow to long-term unemployment	0	0	**4.29**	0	0	0	***2.65***	0
Outflow from long-term unemployment	0	0	***4.81***	0	0	0	***3.93***	0
No information	0	0	***1.59***	0	0	0	***0.99***	0
Sanction days without benefit payment								
Number of sanction days during last unemployment spell	0	***-0.07***	0	0	0	0	0	0
Positive number of sanction days (in %)	0	0.07	***-0.24***	0	0	0	0	***-0.11***
Unemployment history								
First spell	***0.14***	0	***0.23***	0	0	***0.11***	***0.17***	0
Number of spells prior to current spell	0	***-0.17***	0	0	0	0	0	0
Previous programme participation								
Sum of short programs between July and December 1997	**0.15**	0	***0.42***	**0.72**	***0.26***	0	**0.12**	0.08
Employment programme before July 97	0	0	0	0	0	***0.44***	0	0
Temporary wage subsidy before July 97	0	0	0	0	0	0	0	***0.53***
Begin of programme / 100	***0.22***	**0.05**	**0.12**	-0.00	***0.20***	***0.36***	***0.38***	***0.37***
Duration of unemployment spell at beginning of programme								
Duration (days)	-0.02	***-1.20***	**-1.40**	-1.00	-0.19	***-0.88***	**-0.56**	***-2.88***
Less than 90 days	0.03	***-0.15***	***-0.33***	*-0.46*	***-0.27***	***-0.47***	***-0.28***	**-0.14**
Less than 180 days	*0.11*	0	0	0	**-0.15**	***-0.27***	***-0.39***	**-0.17**
Less than 270 days	0	0	0	0	*0.15*	**-0.15**	0	***-0.18***
Less than 365 days	***0.21***	0	0	0	0	0	0	0
Remaining days of "passive regime" on 31.12.97	0	0	**0.30**	0	0	0	0	*-0.12*

Table B.1 to be continued

Table B.1 continued

Variable	Basic courses	language courses	computer courses	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
Employment history from social security data								
Never unemployed	0	0	***0.34***	0	0	0	0	0
Month of entry into social security system	0	***0.84***	0	0	***0.29***	0	0	0
Mean duration of employ. spell in months	0	**0.16**	0	0	0	0	***-0.19***	0
Mean duration of unemploy. spell in months	0	***2.14***	***4.28***	0	0	0	0	0
Standard deviation of wages / 1000	***-0.15***	***-0.15***	**-0.14**	-0.07	***-0.19***	***-0.19***	***-0.25***	***-0.13***
Proportion of time unemployed, in %	-0.13	***-1.62***	**-1.63**	*-1.51*	**-0.58**	**0.45**	**0.62**	***-0.67***
Proportion of time employed, in %	0	0	0	0	0	0	0	***0.79***

Note: Simulated maximum likelihood estimates using the GHK simulator (100 draws in simulator for each observation and choice equation). Coefficients of the category NONPARTICIPATION 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 = *19603*. Value of log-likelihood function: - 31744.08.
Bold numbers indicate significance at the 1% level (2-sided test), numbers in *italics* relate to the 5% level.
If not stated otherwise, all information in the variables relates to the last day in December 1997.

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

	Nonpart.		basic courses		language courses		computer courses		vocat. training		other training		employment programmes public		employment programmes private		temporary wage subsidy	
	Coef	t-val	coef	t-val	coef	t-val	coef	t-val	coef	t-val	coef	t-val	coef	t-val	coef	t-val	coef	t-val
Covariance matrix [a]																		
NONP	1		0	-	0	-	0	-	0	-	0	-	0	-	0	-	0	-
BAC			1		-0.19	0.17	-0.78	0.58	-0.27	1.63	0	-	0	-	0	-	0	-
LAC					1.04		-1.61	0.64	-0.50	1.23	0	-	0	-	0	-	0	-
COC							4.71		-1.44	1.59	0	-	0	-	0	-	0	-
FVT									8.24		0	-	0	-	0	-	0	-
OTC											1		0	-	0	-	0	-
TE-PU													1		0.53	0.22	0.04	0.24
TE-PR															1.28		-0.29	0.25
TEMP																	2.19	1.85
Correlation matrix [a] x 100																		
NONP	100		0		0		0		0		0		0		0		0	
BAC			100		-19		-36		-9.6		0		0		0		0	
LAC					100		-73		-17		0		0		0		0	
COC							100		-23		0		0		0		0	
FVT									100		0		0		0		0	
OTC											100		0		0		0	
TE-PU													100		47		2.9	
TE-PR															100		-17	
TEMP																	100	

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

Table B.3: Correlations of predicted probabilities

	Nonpart.	basic courses	language course	computer course	vocat. training	other training	employment programme public	employment programme private	temporary wage subsidy
NONP	1	-0.33	-0.21	-0.10	-0.09	-0.05	-0.28	-0.31	-0.32
BAC		1	0.03	0.07	0.03	-0.03	0.02	0.00	-0.15
LAC			1	-0.26	-0.17	-0.02	-0.23	-0.16	-0.31
COC				1	0.39	-0.07	-0.13	-0.29	-0.13
FVT					1	-0.09	-0.03	-0.13	-0.12
OTC						1	-0.03	0.02	-0.19
TE-PU							1	0.20	-0.04
TE-PR								1	-0.05
TEMP									1