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MANPOWER TRAINING PROGRAMS

AND

EMPLOYMENT STABILITY

by

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Manpower training programs and employment stability

Josef Zweimüller  
Rudolf Winter-Ebmer

ABSTRACT

We evaluate Austrian labor market policy focusing on its possible effects upon recurrent unemployment. Without properly considering the selection processes for public training programs, perverse results emerge. Taking the participation decision into account in a bivariate probit setting, Austrian manpower training programs turn out to be a sort of 'catching up' strategy: (i) disadvantaged and less motivated job-seekers are given priority in enrollment into training programs and (ii) participation in such courses improves employment stability significantly.

JEL: J64 J24

## 1. Introduction

Most industrialized countries spend non-negligible amounts on 'active labor market policies', such as direct job creation, employment subsidies and labor market training programs. Among these policy measures manpower training programs (MTPs) are of high importance. In most countries the budgetary funding of MTPs has been increasing, both in absolute and relative terms.<sup>1)</sup>

With the notable exception of the US, there is a remarkable scarcity of evidence on the effect of MTPs in economic literature. Programs in the US are mostly designed to combat poverty by raising incomes of disadvantaged groups. This may explain why most studies focus on the effect of MTPs on wages and/or incomes rather than on the employment history of enrolled individuals.<sup>2)</sup>

In many European countries, on the other hand, MTPs are seen as measures against unemployment problems, in particular the problem of long-term unemployment or unstable employment careers. This is also the case for Austria. Austria's position as a low-unemployment country has eroded and the share of problem groups among the unemployed has increased during the 1980's. MTPs are the most important measure of active labor market policy (OECD, 1988). According to the Austrian Ministry of Labor they are intended to improve subsequent labor market prospects and to avoid future

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1) See OECD Employment Outlook, Sept. 1988, where labor market policies of 22 OECD-countries are surveyed, for policies adopted in the UK., see Carruth, Disney (1989), for the FRG Disney (1989) and for Sweden Johannesson (1988).

2) E.g. Ashenfelter (1978), Ashenfelter and Card (1985) and Bassi (1984) among many others.

unemployment of participants.<sup>3)</sup> Around 5 per cent of the unemployment outflow in 1989 were involved in training programs.<sup>4)</sup> In this paper we will analyze the effect of MTPs on participants' subsequent employment experience.<sup>5)</sup> We use a non-experimental data set, drawn from the Austrian unemployment register to answer this question. This raises some methodological problems which we will discuss in the next section. Section 3 describes the data and shows preliminary evidence without proper consideration of sample selection issues, whereas section 4 presents our main empirical results. We conclude that - taking selective enrollment into account - MTPs have strong positive effects on participant's employment careers in Austria.

## 2. Methodological issues

It is often claimed in the evaluation of programs that non-experimental data are misleading. Objections to this method stem from the fact that training participants are a non-random (self-selected) sample. Comparing a sample of MTP-enrolled (treatments) to a control group (controls) with similar observable characteristics will lead to biased

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3)

For a detailed description as well as targets of the various policy measures see BMfAS (1990).

4)

In absolute numbers, more than 30.000 individuals participated in MTPs in 1989. Total unemployment was 180.000.

5)

There exists only a small number of studies dealing with this issue. Those using micro data include Ham and LaLonde (1990), Card and Sullivan (1988), Gritz (1990) and Kaitz (1979) for the US and Ridder (1986) for the Netherlands. For time-series studies see Haskel and Jackman (1988) for the UK as well as Bellmann and Lehmann (1990) for the FRG.

estimates: it is likely that unobservable characteristics will differ systematically between the two groups. 6)

Whereas traditional non-experimental studies ignore these differences, more recent approaches address the issue of sample selectivity directly. This is done by the application of appropriate methods correcting for sample selection bias. 7) The studies by Main and Shelly (1990) as well as Ackum (1991) apply these methods to study MTP-effects on wages in the UK and in Sweden, respectively. Both estimate a MTP-participation function and include a selectivity correction term in the wage regressions for trainees and controls. This guarantees unbiased estimates of coefficients in wage equations, 8) which are used to assess the effectiveness of training programs.

Our approach is similar in spirit, but differs from these studies in one important respect. Rather than looking at wages after MTP-participation, our aim is to evaluate training effects on subsequent employment experience. This makes things more complicated since we have to deal with the time component.

Basically, there are two variables which seem to be obvious candidates for modelling potential training effects: (i)

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6)

See e.g. LaLonde (1986) who shows that empirical results obtained from experimental data differ strongly from those obtained from traditional non-experimental methods.

7)

For a discussion of these methods and their application to program evaluation see Heckman and Robb (1985) and Heckman, Hotz and Dabos (1987).

8)

This is the familiar two-step procedure, initially proposed by Heckman (1979).

re-employment probabilities (i.e. unemployment durations after MTP-enrollment) which should reveal short-term effects of training; and (ii) the stability of employment following MTP-participation, thus revealing longer-term effects of training.

Unfortunately, the comparison of re-employment probabilities between treatments and controls, as discussed in Ham and LaLonde (1990), turns out to be very problematic. This is the case even if there are no systematic differences in unobservable variables between the two groups, so that sample selectivity problems can be ignored. Consider two identical groups, which only differ in their MTP-enrollment status and assume that there is negative duration dependence in unemployment spells. A comparison of hazard rates of treatments (calculated from 'fresh' spells after the end of training participation) to the hazards of controls (calculated from spells in progress) will then bias the estimates in favour of positive MTP-effects. We will observe shorter unemployment spells for treatments even if there is no training effect: simply as a consequence of state dependence. <sup>9)</sup>

Concentrating on the second possible effect of training seems to be more fruitful. Instead of using duration models and analyzing the duration of subsequent employment spells, we adopt a different method. We define subsequent employment experience as a dichotomous variable. A work history is

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9)

Ham and LaLonde (1990) point out that the exclusion of spells in progress does not solve the problem. It instead raises the issue of selection bias. They also present evidence from the National Supported Work experiment: mean unemployment duration for treatments is lower than post-baseline durations of all controls but higher if spells in progress are eliminated for the control group.

assumed to be 'unstable' if the individual rejoins the unemployment register within a given 'risk period', and is considered to be 'stable' otherwise. The main advantage of this procedure is that it enables us to deal with the sample selection issue in a convenient way. Our problem is reduced to the joint determination of two (0,1) events: training participation and employment stability.

It is worth noting that the structure of our model is very similar to the studies of Main and Shelly (1990) as well as Ackum (1991) mentioned above. They are interested in the joint determination of MTP-participation and wages <sup>10)</sup>. However, since their focused dependent variable (wages) is continuous, they are able to apply the standard two-stage procedure proposed by Heckman (1979). In our case the variable of interest (employment stability) is dichotomous, giving rise to different econometric techniques. We shall return to this issue below.

### 3. Data and preliminary results

The data we use concern a representative 2%-sample of Austrian unemployed males <sup>11)</sup> in the years 1986/87 who left the register in 1986 either directly after a training episode or after an unemployment spell. As we are interested in employment stability we restrict the population to Austrian citizens below age 52 to exclude disturbing

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10)

Main and Shelly (1990) look also at employment probabilities of trainees, yet they don't take selective participation into account.

11)

Similar results for females may be attained in Winter-Ebmer, Zweimüller, 1991.



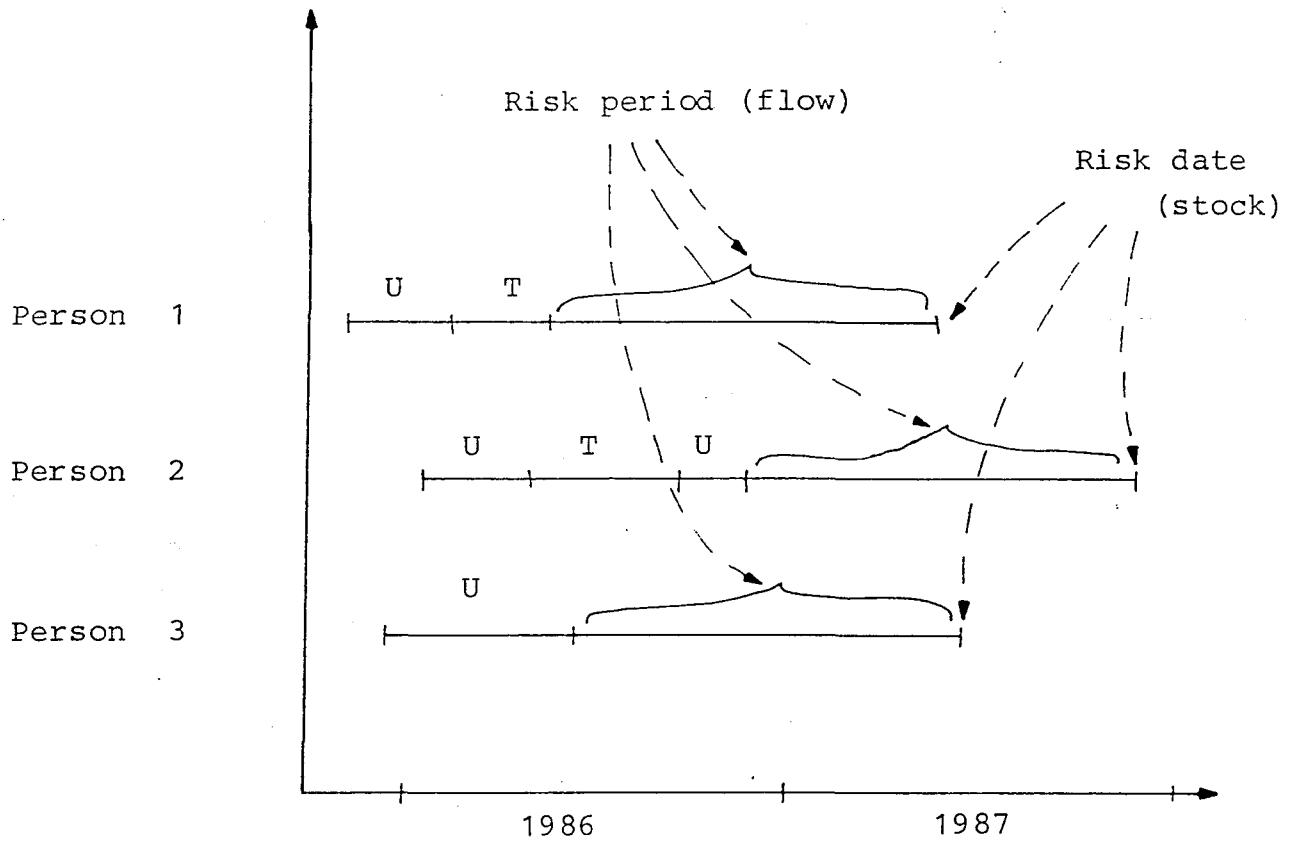


Figure 1: Definition of repeat unemployment  
(U unemployment spell, T training)

influences from early retirement schemes and foreign workers' legislation.

To evaluate training effects we look at the occurrence of repeat unemployment spells within a period of 12 months beginning with the day after the 1986-spell in order to take an equal 'risk period' into consideration. Strategies and opportunities of workers may be different: some may find a new job during course participation, others may remain unemployed after completion of the program. However, this new unemployment episode should not be counted as a repeat spell, rather as a logical continuation of the '86 spell, interrupted by a training program. For this purpose we merge an eventual training episode with unemployment spells directly before and after this period (individuals 1 and 2 in figure 1). In order to model previous labor market history consistently we take into account a span of three years going back in time from the first day of this extended 1986 spell. 12)

Out of our sample of 3537 unemployment leavers 4.8% received manpower training in the course of this episode. Aggregate figures reject any positive training impact on employment stability: 59.8% of trainees suffered a repeat spell compared to only 58.5% of controls.

To model recurrent unemployment let  $y_i^*$  be an index for the risk of unemployment repetition, which is assumed to be positive if the individual joined the unemployment register again within a period of one year and 0 or negative if not. Assume that  $y_i^*$  is determined by

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12)

Those who did not participate in the labor force the whole observation period - school leavers - were eliminated.

$$(1) \quad y_i^* = \alpha d_i + \beta' x_i + \epsilon_i$$

$d_i$  is a dummy variable, taking the value 1 if the individual was enrolled in vocational training, 0 otherwise.  $x_i$  is a vector of individual characteristics and labor market variables. If  $\epsilon_i$ , the error term, follows the standard normal distribution, the coefficients of interest,  $\alpha$  and  $\beta$ , can be estimated by the standard probit model.

This procedure leaves out the length of a repeat unemployment spell: any repeat spell is weighed equally regardless if it lasts five days or one year. To control for spell length we define a second success indicator of the programs: We count a repeat spell, if the individual is unemployed on the day exactly one year after the '86 spell (stock measure, see figure 1). In contrast to the flow measure, unemployed with longer spell durations face higher risks of being counted as repeat cases using the stock measure. Using this definition, 30.2% of trainees experienced a repeat spell compared to 23.4% of controls.

To control for population heterogeneity, several personal characteristics like age, marital status and education are included in the regressions. In addition, local and occupational labor market conditions are accounted for by U/V relations; past employment career of the job-seeker is modelled by the tenure of the last job and the last unemployment spell as well as the unemployment record in the preceding three years. Inclusion of a replacement ratio was prohibited because of a lack of wage data. The usual procedure of computing it should also take into account the

Table 1: Recurrent unemployment\*

	Flow	Stock
Training	-0.027 (0.2)	0.102 (0.9)
Age	0.004 (1.6)	0.008 (3.1)
Hard-to-place	-0.110 (1.8)	-0.004 (0.1)
Married with children	0.063 (1.2)	0.056 (1.1)
Schooling (middle)		
primary	-0.013 (0.3)	0.095 (1.9)
higher	-0.416 (3.7)	-0.224 (1.8)
Seasonal occupation	0.562 (11.)	0.199 (4.1)
U/V Relation		
occupational	0.015 (2.6)	0.017 (2.8)
regional	-0.002 (0.3)	0.006 (1.0)
City size		
100.000-500.000	-0.448 (6.4)	-0.274 (3.5)
> 500.000	-0.261 (4.0)	-0.044 (0.6)
Tenure last job (year)	-0.039 (5.3)	-0.026 (3.1)
Unemployment duration 1986 (years)	0.013 (1.3)	-0.002 (0.2)
Unemployment dur. 1986 squared	-0.0004(1.6)	0.0002(0.6)
Log (cumulated unemp. duration		
3 year preceding)	0.054 (5.7)	0.037 (3.8)
Constant	-0.258 (2.5)	-1.234 (11.)
Log L	-2175.7	-2008.5
Log L restr.	-2401.4	-2080.7
LR-Test	451.3	144.3
N	3537	3537

\* Dependent variable: Flow: probability of repeat unemployment within one year, stock: probability of repeat unemployment at the reference day, t-values in brackets.

earnings effect of training, which again poses tricky problems.<sup>13)</sup>

A look at table 1 draws a pessimistic picture of the efficiency of these policies. The coefficient of the training dummy is insignificant and, in one case, takes the wrong sign. However, to conclude that training programs were ineffective, one has to assume that the training selection process had no relation to the probability of recurrent unemployment - an assumption we should test for.

#### 4. Correcting for selective enrollment into training

##### 4.1 A bivariate probit approach

The selection problem between trainees and controls arises because the trainees are not randomly chosen from the population. In our case the actual enrollment procedure concerns three different stages: the availability of training programs at a certain point in time, the eligibility of the individual and the decision of the unemployed to participate. Estimation of an enrollment equation can only be based on a reduced form of these three influences.

The trainee-control differences, which are most difficult to handle, are mainly motivation and immediate pre-training labor-market position and earnings. American studies often assume that motivation of trainees is superior. This can be deducted from the behavior of 'no-shows' - persons who were eligible for a program and did not show up or dropped out within the first few days; a group often preferred as a

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13) There is some evidence (Winter-Ebmer, Zweimüller, 1992) that the replacement ratio is not important in the determination of unemployment entry in Austria.

control group (Cooley, et al, 1979). On the other hand, if program administrators are paid or evaluated according to the post-program earnings and employment record of their clients, they have an incentive to 'cream' (Bassi, 1984, p.37), i.e. to choose the best among the eligible.

Trainees have typically experienced a decline in their earnings, both absolutely and relative to any comparison group selected, in the period immediately prior to treatment (Ashenfelter, Card, 1985, p.648); the same applies to employment ratios (Card, Sullivan, 1988, p.501). Problems may especially arise if these 'dips' are the consequence of negative transitory components whereas permanent components<sup>14)</sup> may be larger than those of the controls (Cooley et al, 1979, p. 124). Modelling enrollment should take these considerations into account.

An appropriate statistical method for correcting the selection problem is the bivariate probit model consisting of two equations, one of which is identical to equation (1) explaining repeat unemployment, the other determines the outcome of training enrollment. The model can be written as follows (Maddala, 1983, p.122):

$$\begin{aligned} (1') \quad y_i^* &= \alpha d_i + \beta' x_i + \epsilon_i & y_i^* > 0 & \text{ iff } y_i = 1 \\ & & y_i^* \leq 0 & \text{ iff } y_i = 0 \\ (2) \quad d_i^* &= \delta' z_i + u_i & d_i^* > 0 & \text{ iff } d_i = 1 \\ & & d_i^* \leq 0 & \text{ iff } d_i = 0 \end{aligned}$$

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14) This applies in particular to earnings, but may also be relevant to volatile labor demand.

$d_i^*$  is an unobservable index measuring the propensity to participate in a training program,  $z_i$  a vector of variables causing enrollment and  $u_i$  is an error term following the standard normal distribution. The joint distribution of  $u_i$  and  $\epsilon_i$  is the bivariate standard normal, with cumulated density  $\Phi(0,0;1,1;r)$ , where  $r$  denotes the correlation coefficient between  $u_i$  and  $\epsilon_i$ .

This model is appropriate if one wants to analyze the influence of the occurrence of a certain state (i.e. training participation,  $d_i$ ) as opposed to the influence of the intention to choose that state ( $d_i^*$ ).

Equations (1') and (2) are estimated by maximizing the following likelihood function with respect to the coefficients  $\alpha$ ,  $\beta'$ ,  $\delta'$  and  $r$ .

$$(3) \quad \mathcal{L}(\alpha, \beta', \delta', r) =$$

$$\sum_{i=1}^N [y_i d_i \ln \Phi(Y_i, D_i, r) + y_i (1-d_i) \ln \Phi(Y_i, -D_i, -r) +$$

$$+ (1-y_i) d_i \ln \Phi(-Y_i, D_i, -r) + (1-y_i) (1-d_i) \ln \Phi(-Y_i, -D_i, r)]$$

where  $Y_i = -(\alpha d_i + \beta' x_i)$  and  $D_i = -(\delta' z_i)$ , respectively.

#### 4.2. Identification

Identification of the model is of crucial importance. In general, identification is accomplished, if at least one regressor in equation (2) can be excluded from equation (1') or vice versa.<sup>15)</sup> The training equation includes all variables, which also appear in the unemployment equation; with two exceptions. First, rather than viewing the current state of the labor market (measured as U/V-relations for regions and occupations in the unemployment equation) as the important determinant of training selection, we will use long-run labor-market conditions as the main variable.

There are good reasons both from the point of view of the unemployed as well as from program administration for this to be the case. Availability of courses should be the result of planning in advance, taking into account the long-term state of the labor market. It is also rational from the point of view of the unemployed to join a training program if he expects labor market problems to be permanent rather than transitory. This is why we used 'industry employment growth in past five years' as well as 'projected employment in district in 1991' as the relevant labor market variables in the training equation.

Second, it is the unemployment duration of the recent unemployment spell which enters both equations differently. This arises from two reasons. On the one hand, it is a matter of definition: past unemployment duration before training is not equal to unemployment duration before leaving the unemployment register, if the individual spent some time unemployed after finishing the program.

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15) If the error terms in equations (1) and (2') are uncorrelated we can estimate two univariate probits, and the model is also easily identifiable.



On the other hand, we expect training selection to depend discontinuously on elapsed duration. Although there are no official eligibility criteria for training enrollment, civil servants at the labor office usually proceed as follows: the first task is job placement <sup>16)</sup>. This can also be accomplished by employment subsidies. Qualifying measures can only then be considered. Unemployment duration should therefore play a role. Since civil servants commonly check up on their clients at regular intervals, we included three dummy variables.

Concerning the remaining variables in the training equation, care has been taken to include control variables in both equations in the same way. To overcome 'dips' in pre-training experience mentioned above, past employment and unemployment records in the three years preceding are taken into account. In addition, matching of program demand and supply should be facilitated in a 'thick market' (city size). As motivation and certain special qualifications of participants are partly observed by the program administrator but unobservable for the analyst, they should be picked up by the error structure, i.e. the coefficient of correlation.

#### 4.3 Results

From an econometric point of view, the results of table 2 should be compared with two independent probit equations, the coefficient of correlation  $r$  being the important parameter. Both wald and likelihood-ratio test imply that the hypothesis  $r=0$  can be rejected at the 5% level. After correcting for observable characteristics, the positive

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16) See Ebmer (1990) for an investigation of placement strategies of the Austrian labor exchange.

Table 2: Recurrent unemployment corrected for training enrollment

	Flow	Stock
<u>Training equation</u>		
Age	-0.007 (1.3)	-0.007 (1.3)
Hard to place	0.124 (1.3)	0.125 (1.3)
Married with children	-0.094 (1.0)	-0.077 (0.8)
Schooling (middle)		
primary	-0.100 (1.2)	-0.099 (1.2)
higher	0.220 (1.3)	0.225 (1.3)
Projected employment		
in district for 1991	-0.016 (2.0)	-0.014 (1.7)
Industry employment growth		
past 5 years	-0.897 (1.5)	-0.868 (1.5)
Seasonal occupation	-0.189 (2.1)	-0.172 (1.9)
City size		
100.000-500.000	0.214 (1.9)	0.229 (2.1)
> 500.000	-0.338 (2.5)	-0.314 (2.4)
Tenure last job (years)	0.0009(0.1)	0.002 (0.2)
Log (unemp. duration before		
training, days)	-0.352 (7.9)	-0.357 (8.1)
duration 3-6 months	0.183 (1.6)	0.201 (1.8)
duration 6-12 months	0.665 (4.1)	0.666 (4.1)
duration > 12 months	1.380 (5.5)	1.413 (5.9)
Log (Cumulated unemp. duration		
3 years preceding)	0.042 (2.3)	0.041 (2.2)
Constant	0.602 (1.0)	0.590 (0.9)

Repeat unemployment equation\*

	Flow	Stock
Training	-0.768 (2.0)	-0.680 (2.0)
Age	0.004 (1.4)	0.008 (2.8)
Hard-to-place	-0.099 (1.7)	-0.009 (0.2)
Married with children	0.052 (1.0)	0.043 (0.8)
Schooling (middle)		
primary	-0.019 (0.4)	0.085 (1.7)
higher	-0.394 (3.5)	-0.201 (1.5)
Seasonal occupation	0.540 (11.)	0.176 (3.5)
U/V Relation		
occupational	0.015 (2.6)	0.016 (2.7)
regional	-0.001 (0.2)	0.007 (1.2)
City size		
100.000-500.000	-0.426 (6.0)	-0.247 (3.1)
> 500.000	-0.270 (4.2)	-0.058 (0.8)
Tenure last job	-0.038 (5.3)	-0.024 (3.1)
Unemployment duration 1986 (years)	0.009 (1.0)	-0.006 (0.6)
Unemployment dur. 1986 squared	-0.0004(1.4)	0.0003(1.0)
Log (cumulated unemp. duration 3 years preceding)	0.056 (5.9)	0.040 (4.1)
Constant	-0.208 (2.0)	-1.162 (10.)
r	0.367 (2.0)	0.398 (2.2)
Log L	2797.2	2630.6
Log L restr.	3082.2	2762.5
LR-Test	570.0	263.8
N	3537	3537

\* See notes table 1

coefficient of correlation suggests that people with unfavorable expected employment careers will be more frequently selected into training. There is no evidence for a strategy of choosing the best among potential participants but, rather on the contrary, for a support of disadvantaged or less-motivated persons by program administrators. This corresponds to the guiding-principles of Austrian labor market policy (BMfAS, 1990), stressing the promotion of problem groups <sup>17)</sup>.

Taking enrollment in courses into consideration does not change the parameters of the repeat unemployment equations (compare tables 1 and 2) except one: the training dummy. The coefficients change dramatically, indicating a high and significant improvement in employment stability for both definitions of repeat unemployment.

As Heckman, Robb (1985, p.161) point out, these coefficients may give answers to two distinct questions. The evaluator of manpower programs wants to know above all whether participants have experienced perceptible improvements in their labor-market position. Training enrollment lowers repeat unemployment risk of a typical trainee from 84.4% to 57.7% (Cols. 1 and 2 in Tab. 3. Computations refer to the flow definition of repeat unemployment only). This remarkable reduction emphasizes the efficiency of such policies.

The second question extends the problem to the whole population. What would be the impact of training on a randomly chosen job-seeker, irrespective of his enrollment

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17)

Different results concerning motivation of trainees can be found in Allen et al (1991), where - contrary to this approach - only the demand for training is studied.

Table 3: Repeat unemployment probabilities of different types of sample members

	Treatments		All		Controls
	No <sup>I)</sup>	Yes <sup>II)</sup>	No <sup>III)</sup>	Yes <sup>IV)</sup>	No <sup>V)</sup>
Training					
Reference <sup>VI)</sup>	84.4	57.7	56.0	26.9	54.9
20 years older	87.3	62.7	59.0	29.4	58.2
higher education	69.8	38.4	40.4	15.0	38.6
favorable career. <sup>VII)</sup>	66.8	35.2	36.8	13.5	35.1
unfavorable career <sup>VIII)</sup>	86.8	58.7	65.2	35.3	62.0

I)  $\Phi[-B'x_i, -\delta'z_i; r]/F(\delta'z_i)$ .  $F()$  denotes the univariate cumulative density of the standard normal distribution.

II)  $\Phi[-(\alpha+B'x_i), -\delta'z_i; r]/F(\delta'z_i)$ .

III)  $F(B'x_i)$

IV)  $F(\alpha+B'x_i)$

V)  $\Phi[-B'x_i, \delta'z_i; -r]/[1-F(\delta'z_i)]$

VI) For the reference person all characteristics were taken at means (continuous variables) and modes (Dummies) resp., using Col. 1, Tab.2.

VII) No previous unemployment spells, tenure of last job 10 years, actual spell 30 days.

VIII) Tenure of last job one month, actual unemployment spell 2 years, cumulated duration past three years 2 years.

status?<sup>18)</sup> Our results predict a reduction in recurrent unemployment probability from 56% to 26.9%. (Cols 3 and 4) These calculations may serve as a benchmark for the implementation of any qualification measures not addressed to special problem groups.

Apart from these two hypothetical questions discussed by Hekman, Robb (1985), a third comparison emerges quite naturally: the actual outcome of treatments versus controls (cols. 2 and 5). 54.9 % of average non-participants have to expect a repeat spell in contrast to 57.7 % of trainees. This leads us back to the origin where - neither in aggregate figures nor in the preliminary regression results of Tab. 1 - any appreciable differences between treatments and controls could be detected. In particular, no positive training impact was found.

Now we are able to explain this strange observation. Jobseekers with high base risks of re-unemployment are selected with priority into training (see cols. 1 and 5); participating in these courses can be seen as a 'catching-up' process narrowing the risk gap between the respective groups considerably.

It may be interesting to take a look at the impact of covariates both on the probability of training enrollment and of unemployment repetition (Tab.2). Most personal characteristics - with the notable exception of higher education - don't have large effects upon repeat unemployment, whereas variables capturing labor market

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18) If selection into training is purely random the two questions coincide.

conditions like UV-relations, city size and especially seasonal occupation,<sup>19)</sup> do.

The dominant variable in the training equation is the elapsed unemployment period before the training program started. The probability of joining a course is highest for the very short and the long-term unemployed. Individuals may be willing to participate at the beginning of their non-employment spell, whereas leaps may reflect increasing efforts of program administrators to promote training of long-term unemployed. On the other hand, the cumulated past unemployment duration as well as the tenure of the last job prior to 1986 significantly increase the unemployment repetition probability. These findings shed further light on the importance of (un)employment history for subsequent labor market prospects. Necessity and availability of courses modelled by past and future employment outlook turns out to be important.

Taking up the discussion of catching-up behavior we can look at training impacts on different groups. Among these types only trainees with unfavorable employment careers can bridge the gap completely to the controls (Tab. 3, cols. 2 and 5). The reason is that past unemployment plays an important role in enrollment into courses. This policy may be a good strategy to combat stigmatizing effects of past unemployment.<sup>20)</sup>

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19) Seasonal occupations are implicitly defined as such having high (recurrent) seasonal unemployment risk (agriculture, construction and tourism). Regressions showing results without persons having seasonal occupations - which are very similar - may be obtained from the second author upon request.

20) See Winter-Ebmer (1991) for evidence on stigmatizing

## 5. Conclusions

We showed above that clearcut evaluation results of manpower training programs can be obtained using rather standard econometric methods. Appropriate modelling of the training selection rule is of crucial importance. Austrian labor market policy turns out to be a sort of 'catching-up' strategy: i) disadvantaged and less-motivated unemployed are given priority in program enrollment, ii) participation in such courses improves employment stability considerably.

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effects of long-term unemployment upon employers' recruitment behavior.



Appendix: Data description

	Mean	Std.dev. <sup>1)</sup>	Unit of Measurement
Prob. of recurrent unemployment			
Flow	0.59		dummy
Stock	0.28		
Age	30.97	9.3	years
Hard-to-place <sup>2)</sup>	0.17		dummy
Married with children	0.32		dummy
Schooling			dummy
primary	0.42		
intermediate education	0.53		
higher	0.05		
Seasonal <sup>3)</sup> occupation	0.45		dummy
UV-Relation			
occupational <sup>5)</sup>	9.08	4.0	
regional <sup>6)</sup>	7.95	4.4	
Tenure last job	1.66	3.3	years
Unemployment duration 1986	107.50	125.8	days
Prob. of training	0.05		
Industry employment growth			
past 5 years	0.94	2.5	%
Projected employment			
change in district for 1991	- 1.97	5.5	%
City size			
100.000-500.000	0.14		dummy
> 500.000	0.15		
Cumulated past <sup>4)</sup>			
unemployment duration	110.9	158.3	days
Duration before training	104.5	122.3	days

- 1) Only means are reported for dummy variables
- 2) Assessed as such by the employment exchange
- 3) Construction, agriculture, catering trade
- 4) Last 3 years considered
- 5) 33 occupations
- 6) 86 employment districts

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