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Active Labor Market Policies in Poland: Human Capital Enhancement, Stigmatization or Benefit Churning?[1]

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Abstract

This paper provides micro-econometric evidence on the effectiveness of Active Labor Market Policies (ALMP) in Poland. We sketch the theoretical framework of matching estimators as a substitute for randomization in labor market programs. Using retrospective data from the 18th wave of the Polish Labor Force Survey we implement a conditional difference-in-differences matching estimator of treatment effects. Treatment and control groups are matched over individual observable characteristics and pre-treatment labor market histories to minimize bias from unobserved heterogeneity. We also require that observations on controls are from the same regional labor market and from an identical phase of the transition cycle. Considering as the outcome a multinomial variable of labor market status, our first important finding suggests that training of men and women has a positive effect on the employment probability. For men public works and intervention works have negative treatment effects, while participation in intervention works does not affect women's employment probabilities. We attribute the negative treatment effects for men to benefit churning rather than to stigmatization of intervention and public works participants.

Journal of Economic Literature Classification Numbers: C41. J68.

Non-technical summary

Since the beginning of the transition to a market economy the Polish government has applied a wide menu of Active Labor Market Policies (ALMP) to combat unemployment and long-term unemployment. In terms of expenditures three programs have been of particular importance: training, "intervention works" and public works. Training programs are meant to solve skill mismatch in the Polish labor market. Workers with redundant or no skills are trained in those occupations where there exists a strong demand by entrepreneurs in the expanding sectors of the economy. "Intervention works" is a program that in essence gives wage or jobs subsidies in the amount of the level of unemployment benefit. These wage subsidies are given to firms in the private or public sector if they hire an unemployed person and are larger the longer this person is kept on in the firm. Public work jobs are jobs directly created by the government, in particular by the municipalities, targeted mainly but not exclusively at the long-term unemployed. Many of these jobs are in construction and cleaning of public buildings, parks etc., i.e. they have a low skills content. In principle, though, both intervention works and public works have been conceived to enhance or maintain the human capital of participants.

This study tries to evaluate these three ALMP measures for the period 1992-1996 at the level of the individual. To fully understand the context of this evaluation it is important to be aware of the incentive structures that form part of these measures. Since the beginning of 1992 unemployment compensation is limited to one year. If persons are unemployed for more than one year they have to rely on social assistance which is not always paid or paid in the form of material help. On the other hand, workers are entitled to unemployment benefit if they have worked at least 180 days in the preceding year. Participating in a jobs program like intervention works or public works for at least 6 months entitled a person in the period under study to another round of 12 months of benefit payment.

A rigorous evaluation of Polish ALMP at the individual level has until recently not been possible because of a lack of appropriate data. Using a special supplement from the August 1996 wave of the Polish Labor Force Survey that includes a retrospective part it is possible in this paper to perform such an evaluation. The key question we ask is whether after participating in an ALMP program a person finds himself or herself better positioned in the labor market than if he or she had not taken part in the program. We do this by comparing employment and unemployment rates of persons who have undergone treatment (the ALMP program) with the corresponding rates of persons who were not subjected to the treatment ("controls"). We employ an innovative procedure to match persons who have participated in the program and controls. To ensure that our results reflect the true impact of the program, we compare participants and non-participants who have the same observable characteristics and an identical labor market history before the treatment took place. Also, we only compare persons who find themselves in an identical phase of the transition cycle. By doing the latter we avoid that our results are influenced by differences in the macroeconomic environment and by insisting on identical pre-treatment histories differences in unobservable characteristics of the participants and the controls are minimized.

How effective are Polish ALMP programs? Training and re-training is the ALMP measure that performs well. Our estimates suggest that the post-treatment employment rates of both female and male participants are higher than they would have been had these individuals not participated in the program. So, this ALMP measure clearly seems to improve the efficiency of the Polish labor market and more resources should be dedicated to this program in future. In contrast, the Polish employment programs seem to be burdened by major distortions. Despite their intention to enhance or rebuild the human capital of the unemployed, we do not find any overall treatment effects for women who participate in intervention works, but find strong negative overall treatment effects on the employment rate of men who take part in intervention and public works. These negative effects suggest two explanations. First, participating in intervention works or public works might carry a stigma. Employers will not hire such persons as they perceive them as low productivity workers. A competing second explanation is "benefit churning". Polish employment programs might often be the intermediate stage between two spells of unemployment benefit receipt. We cite some numbers on this "recycling" of unemployment compensation recipients which takes place above all via intervention works. These numbers strengthen our conviction that while stigmatization might have some role to play, benefit churning explains most of the negative overall treatment effects of these programs. Out of "social considerations" officials in local labor offices deem males as heads of households particularly worthy of prolonged income support from the state. On our evidence, a reform of the Polish employment programs seems to be needed that eliminates the distortions arising from interactions between the unemployment compensation system and these programs.

1. BACKGROUND

The transition to a market economy had serious repercussions in the Polish labor market. Most significantly open unemployment rose from virtually zero to a peak of around 16% at the end of 1993, declining slightly and hovering since then around 13%. Like in most transition countries unemployment in Poland can be characterized as a "stagnant pool" (Boeri, 1994). This stagnancy came about because of very low outflow rates from unemployment and led to unemployment persistence and a large share of long-term unemployment. Individuals with unfavorable demographic and skill characteristics, i.e. workers who are old and have obsolete skills, whilst being an important element of the total stock of unemployment dominate long-term unemployment (Góra and Schmidt, 1998 and Lehmann, 1998).

Given this context, Active Labor Market Policies (ALMP) might, in principle, have an important role to play in combating unemployment in general and long-term unemployment in particular. Further training and re-training measures might help to solve skill mismatch, while subsidized employment in private and public firms and direct public job creation could be useful instruments in the re-building of human capital of some of the long-term unemployed. Measures of this kind are then meant to boost outflow rates from unemployment, in particular from long-term unemployment, thus raising labor turnover and improving the performance of the labor market. This rationale for labor market intervention by the government was developed in mature market economies. Whether it can be easily carried over into the context of a labor market in transition is an important and contentious issue that we do not further pursue here. This paper has a more modest aim and focuses on the evaluation of the three most important Polish ALMP, i.e. publicly financed further training and re-training, intervention works[2] and public works.

Evaluation of ALMP in a labor market in transition has its own difficulties. In most transition countries rules for the assignment of ALMP measures and for the monitoring of the unemployed are either not well developed or not strongly enforced, leading often to large unforeseen distortions. Another difficulty is the absence of a stationary environment in which the evaluation takes place. In addition, the quality of the data used for the evaluation is often quite poor. These difficulties seem to be mirrored in the literature that exists on the evaluation of Polish ALMP. Góra et al. (1996) and Góra and Schmidt (1998) look at the rather loose application of assignment and monitoring rules in Poland and show some of the distortions arising from this. Most of the studies that have tried to econometrically evaluate Polish ALMP have certainly been plagued by data problems. These problems are mainly responsible for the not entirely convincing model specifications that underlie the impact analysis of Polish ALMP that have been undertaken in the past (cf. e.g. Puhani and Steiner, 1997 and O'Leary, 1998).

In our analysis we use data from the supplement to the August 1996 wave of the Polish Labor Force Survey (PLFS). This supplemental data set has a detailed retrospective part on monthly labor market histories over the period from January 1992 to August 1996 which can be linked to the August 1996 PLFS quarterly wave. This linked data set allows separate assessment of the three ALMP measures and lends itself to an evaluation procedure that matches controls to the treated individuals, both conditional on pre-treatment histories as well as on observable characteristics. Recent developments in the econometric evaluation literature suggest that this kind of matching seems to perform well in controlling for unobserved heterogeneity and self-

[2] "Intervention works" basically entails wage subsidies to boost employment of the unemployed in private or public

selection (Heckman et al., 1997, 1998). Parallel to our study Puhani (1998) employs a similar but nonetheless distinct variant of this matching approach.

The next section presents the theory underlying our matching approach. Section 3 gives a brief account of the three ALMP measures evaluated. Section 4 discusses the labor market histories in the August 1996 supplement and the matching algorithm that we applied while section 5 looks at the empirical results. Matching estimates of treatment effects on the employment and unemployment rates as well as on employment retention and job accession rates are presented for the three ALMP measures, taking into account two regional taxonomies and two variants of matching analyzing short- and medium-term treatment effects, respectively. Section 6 concludes.

2. APPLICATION OF MATCHING METHODS

Two problems affect the evaluation of measures of Active Labor Market Policy in transition economies. The first is the usual evaluation problem that characterizes all non-experimental analyses of interventions. Since counterfactual outcomes under no intervention cannot be observed for individuals receiving the intervention, one has to find an appropriate group of controls that, together with some identifying assumptions, facilitates the construction of the desired counterfactual. Matching estimators have recently received a lot of attention in the econometric literature as one serious alternative non-experimental evaluation approach (cf. Heckman et al., 1997, 1998, Angrist, 1998). The second problem stems from transition itself. Interventions administered at different points of the transition cycle may have very distinct

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firms.

effects and non-experimental controls observed at a different time period may not be appropriate. Since the interventions in our data are widely dispersed over the observation period, a matching approach that very stringently enforces the same temporal structure across intervention group and control group is a particularly promising evaluation strategy. This is the approach chosen in our application.

Our empirical work is in the spirit of Card and Sullivan (1988) who analyze the effect of the CETA training program on employment status by using conditional difference-in-differences matching estimators that match over labor market histories. We extend their analysis by considering a richer variable of labor force status (employment, unemployment and out-of-the-labor-force), by matching over observable individual characteristics and by adapting the analysis to the temporal structure of our data. All of their trainees received the treatment within a single year; however, they did not consider the timing and the duration of the treatment any further. In contrast, we establish the exact beginning and the duration of an intervention, and match controls accordingly.

The formal development of matching techniques, in particular the role of exclusion restrictions and of time-persistent individual heterogeneity has been discussed recently by Heckman et al. (1997 and 1998) who explicitly derive a non-parametric conditional difference-in-differences estimator of treatment effects. Another recent application of matching methods can be found in Lechner (1997) who analyzes the effects of training in the East German labor market.

The PLFS data provide information on labor force status at the individual level, together with information on individual and household characteristics. In the empirical analysis, the three ALMP interventions under scrutiny, *training*, *intervention works*, and *public works*, are considered separately. Thus, for purposes of the formal exposition, we only need to consider a single intervention. Furthermore, in our matching approach we will explicitly require that individuals who receive treatment are matched with individuals from the identical set of observed pre-treatment and post-treatment months. Any reference to the time period is therefore omitted from the exposition as well.

Denote the state associated with receiving the intervention with "*1*", and the state associated with not receiving the intervention with "*0*". Assume that there are N_1 individuals in the intervention sample, with indices $i \in I_1$, and N_0 individuals in the sample of potential controls, with indices $i \in I_0$. Receiving the intervention is indicated by the individual indicator variable D_i (*1* = yes, *0* = no). Denote the potential labor market outcomes in post-treatment quarter q ($q = 1, 2, \ldots, Q$) by Y_{qi}^1 if individual i received treatment, and Y_{qi}^0 if individual i did not receive treatment. These outcomes are defined as multinomials with three possible realizations ("*0*" = out-of-the-labor-force, "*1*" = employed, "2" = unemployed). While only one of these two outcome variables can actually be observed for each individual i, this being denoted Y_{qi}, the definition of potential outcomes allows for the formal construction of the unobservable counterfactual outcome $Y_{qi}^0 \mid D_i = 1$.

For purposes of evaluating the impact of the intervention, the post-intervention labor market success of each individual i will be summarized by the individual's average employment and unemployment rates, taken over the Q quarters following the intervention. Using indicator function $\mathbf{1}(.)$, these outcomes are $\frac{1}{Q}\sum_q \mathbf{1}(Y_{qi} = 1)$ for employment rates and $\frac{1}{Q}\sum_q \mathbf{1}(Y_{qi} = 2)$ for unemployment rates, respectively. These formulations extend those of Card and Sullivan (1988) from a binomial to a multinomial setting.

Using the indicator of intervention status D_i and the index $k \in \{1,2\}$ observed outcomes for individual i could be written as

$$\tfrac{1}{Q}\sum_q \mathbf{1}(Y_{ai}=k) \;=\; \tfrac{1}{Q}(D_i\sum_q \mathbf{1}(Y_{qi}^1=k)+(1-D_i)\sum_q \mathbf{1}(Y_{qi}^0=k)) \quad , \tag{1}$$

and the impact of the intervention on the average labor market status of individual i could be expressed as

$$\Delta_{ki}=\tfrac{1}{Q}(\sum_q \mathbf{1}(Y_{qi}^1=k)-\sum_q \mathbf{1}(Y_{qi}^0=k)) \tag{2}$$

for average employment rates $(k = 1)$ and for average unemployment rates $(k = 2)$.

Unfortunately, we can never observe Y_{qi}^1 and Y_{qi}^0 simultaneously for a given individual, and neither the joint distribution of the two outcomes across the intervention sample. Instead, we have to focus on evaluation parameters for which we can construct counterfactuals by invoking appropriate identification assumptions. Our interest here is on the mean effects of treatment on the treated,

$$E(\Delta_{ki}|X_i,h_i,D_i=1) \;=\; E(\tfrac{1}{Q}(\sum_q \mathbf{1}(Y_{qi}^1=k)-\sum_u \mathbf{1}(Y_{qi}^0=k))|X_i,h_i,D_i=1) \quad , \tag{3}$$

that is the mean of the average employment and unemployment rates, respectively, over the population of the treated, conditional on observable individual characteristics X_i and previous labor market history h_i which is captured by a sequence of labor market states in the four quarters preceding the intervention. This slightly extends the discussion of Heckman et al. 1997 to a pair of evaluation parameters. Conditioning on previous labor market history was advocated by Card and Sullivan (1988) and by Heckman et al. (1997, 1998), accounting for the panel nature of their data.

More specifically, we will concentrate on average treatment effects over the joint support S of X and h given $D=1$,

$$M_k = \frac{\int_S E(\Delta_k | X, h, D=1)\, dF(X, h | D=1)}{\int_S dF(X, h | D=1)} \quad . \tag{4}$$

In the absence of observations on the labor market status Y_{qi}^0 that recipients of the intervention would have realized had they not received the intervention, one needs to invoke appropriate identification assumptions in the construction of estimates for M_1 and M_2. The objective is to replace those expected values whose sample counterparts are unobservable by expected values whose counterparts can be constructed from sample data. In randomized experiments, if several conditions regarding the timing of the randomization, the process of sample attrition and the impact of randomization itself on individual behavior are met (cf. Heckman 1996, Heckman et al. 1997), the counterfactual expected values under no intervention can be estimated for intervention recipients by the mean values of the outcome for randomized-out would-be recipients. Non-experimental methods instead use data on non-recipient control groups to estimate the required counterfactuals.

The principal idea of matching is to assign to (preferably) all individuals i in the intervention sample as matching partners one or more individuals from the non-experimental control sample who are similar in terms of their observed individual characteristics (cf. Heckman et al. 1997). Within each matched set of individuals, one can then estimate the impact of the intervention on individual i by the difference over sample means, and one can construct an estimate of the overall impact by an average over these individual estimates. Matching estimators thereby approximate the virtues of randomization mainly by balancing the distribution of observed attributes across treatment and control groups, both by ensuring a common region of support for individuals in the intervention sample and their matched controls and by re-weighting the distribution over the common region of support.

As Heckman et al. (1997) point out, the strong assumptions traditionally invoked in the matching literature, conditional independence of the labor market status Y_{qi}^0 and of the treatment indicator D_i, given individual observable characteristics X_i, are not necessary to ensure identification of the mean effects of treatment on the treated. Instead, weaker mean independence assumptions are all that is needed for our matching estimators to identify the desired evaluation parameters,

$$E(\mathbf{1}(Y_{qi}^0=k)\,|\,X_i,h_i,D_i=1) \quad = \quad E(\mathbf{1}(Y_{qi}^0=k)\,|\,X_i,h_i,D_i=0) \quad . \tag{5}$$

That is, given the observable individual characteristics X_i and previous labor market history h_i that together form the basis for the individual matches, the fact that an individual received the intervention is assumed not to carry further information on the distribution of his or her no-intervention outcome.

In our empirical analysis, we use a variant of a nearest-neighbor matching estimator that implicitly performs a conditional difference-in-differences comparison between individuals in the intervention sample and their matched controls. For any treatment history h for which at least one match could be found, we estimate the impact of the intervention by

$$\hat{M}_{kh} = \frac{1}{N_{1h}} \sum_{i \in I_{1h}} \left[\frac{1}{Q} \sum_q 1(Y_{qi}^1 = k) - \sum_{j \in I_{0h} | X_j \in C(X_i)} \frac{1}{n_{i0}} \left(\frac{1}{Q} \sum_q 1(Y_{qj}^0 = k) \right) \right] , \tag{6}$$

where N_{1h} is the number of individuals with history h who receive the intervention ($N_1 = \sum_h N_{1h}$), I_{1h} is the set of indices for these individuals, $C(X_i)$ is the appropriate neighborhood of individual i's characteristics X_i, and n_{i0} is the number of controls with history h who are falling within this neighborhood ($N_0 = \sum_i n_{i0}$), with the set of indices for control-individuals with history h being I_{0h}. The variance of this expression is then estimated as a function of the estimated probabilities from the underlying multinomial models[3].

The overall effect of the intervention is estimated in a last step by calculating a weighted average over the history-specific intervention effects,

$$\hat{M}_k = \sum_h \left[\frac{N_{1h}}{\sum_h N_{1h}} \hat{M}_{kh} \right] , \tag{7}$$

[3] Whenever feasible we based the estimation on unrestricted multinomial models.

using the treatment group sample fractions as weights. The variance is derived as the corresponding weighted average of the history-specific variances.

The main impact of the intervention on labor market outcomes might arise from a positive one-shot effect at the end of the treatment period. One would like to know, however, whether workers who received the intervention were more successful in holding on to employment after the first post-intervention quarter than workers in the control group (cf. Card and Sullivan 1988). Hence, define the *job retention rate* as the probability of holding on to a job until post-intervention quarter Q conditional on being employed in the first quarter after treatment. The intervention effect is then

$$\begin{aligned} E(r_i|X_i,h_i,D_i=1) = E(&\mathbf{1}(Y_{2i}^1=1\wedge\ldots\wedge Y_{Qi}^1=1) \\ &-\mathbf{1}(Y_{2i}^0=1\wedge\ldots\wedge Y_{Qi}^0=1)|Y_{1i}=1,X_i,h_i,D_i=1), \end{aligned} \tag{8}$$

conditional on observable individual characteristics X_i and previous labor market history h_i. The average impact r over the joint support S of X and h given $D=1$ is then defined similarly to the intervention effect in equation (4).

For pre-intervention history h we estimate the impact of the intervention on retention rates as

$$\hat{r}_h = \frac{\sum_{i\in I_{1h}} \mathbf{1}(Y_{1i}^1=1\wedge Y_{2i}^1=1\wedge\ldots\wedge Y_{Qi}^1=1)}{\sum_{i\in I_{1h}} \mathbf{1}(Y_{1i}^1=1)} - \frac{\sum_{j\in I_{0h}} \mathbf{1}(Y_{1j}^0=1\wedge Y_{2j}^0=1\wedge\ldots\wedge Y_{Qj}^0=1)}{\sum_{j\in I_{0h}} \mathbf{1}(Y_{1j}^0=1)}. \tag{9}$$

The variance of this expression is calculated using the delta method. The overall effect of the intervention, $\hat{r}$, is estimated as in equation (7) by calculating a weighted average of the history-specific effects, deriving the variance of this weighted average accordingly.

By raising their rates of access to a new job, the intervention might also exert a positive influence on workers who were unemployed at the end of the treatment period. Hence we define the *job accession rate* as the probability of starting a new job and holding on to it until post-intervention quarter Q conditional on being unemployed in the first quarter after treatment. The intervention effect is then

$$\begin{aligned} &E(a_i \mid X_i, h_i, D_i = 1) = \\ &\quad E\big(\textstyle\sum_r \mathbf{1}(Y^1_{1i} = 2 \wedge \ldots \wedge Y^1_{r-1,i} = 2 \wedge Y^1_{ri} = 1 \wedge \ldots \wedge Y^1_{Qi} = 1) \\ &\quad - \textstyle\sum_r \mathbf{1}(Y^0_{1i} = 2 \wedge \ldots \wedge Y^0_{r-1,i} = 2 \wedge Y^0_{ri} = 1 \wedge \ldots \wedge Y^0_{Qi} = 1) \,\big|\, Y_{1i} = 2, X_i, h_i, D_i = 1\big), \end{aligned} \tag{10}$$

conditional on observable individual characteristics X_i and previous labor market history h_i, where r is the first quarter of being employed, $1 < r \leq Q$. The average impact over the joint support S of X and h given $D_i=1$ is then defined accordingly (see equation (4)).

For pre-intervention history h we estimate the impact of the intervention on accession rates as

$$\begin{aligned} \hat{a}_h = &\frac{\sum_{i \in I_{1h}} \sum_r \mathbf{1}(Y^1_{1i} = 2 \wedge \ldots \wedge Y^1_{r-1,i} = 2 \wedge Y^1_{ri} = 1 \wedge \ldots \wedge Y^1_{Qi} = 1)}{\sum_{i \in I_{1h}} \mathbf{1}(Y^1_{1i} = 2)} - \\ &\frac{\sum_{j \in I_{0h}} \sum_r \mathbf{1}(Y^0_{1j} = 2 \wedge \ldots \wedge Y^0_{r-1,j} = 2 \wedge Y^0_{rj} = 1 \wedge \ldots \wedge Y^{\upsilon}_{Qj} = 1)}{\sum_{j \in I_{oh}} \mathbf{1}(Y^0_{1j} = 2)}. \end{aligned} \tag{11}$$

The variance of this expression is again calculated using the delta method. As for the other intervention effects, the overall effect of the intervention, $\hat{a}$, and its variance are estimated by calculating the appropriate weighted averages of the history-specific effects and their variances.

3. ALMP MEASURES IN POLAND

The ALMP measures that we analyze, training, intervention works and public works, have been described at length in Lehmann (1998), Puhani and Steiner (1997) and Góra et al. (1996) for example. We, therefore, only briefly discuss these measures here. Besides presenting the evolution of expenditures on these measures during the period of interest (1992-1996) we concentrate on those institutional aspects of the design and implementation of the programs that are central in the context of this paper.

Expenditures on labor market policies have only slightly risen over the period 1992-96 as can be seen in Table 1. Apart from 1992 when expenditures on ALMP amounted to only 5% of PLMP, the ratio of expenditures on the types of programs has been about 1 : 8 throughout the period. In an international comparison of Visegrad countries, Poland is roughly in line with Hungary that also spends predominantly on PLMP, but spends relatively less than the Czech Republic or the Slovak Republic. In relation to western OECD economies with similar unemployment levels, Poland spends little on ALMP and has low inflow rates into ALMP schemes. Of the three programs analyzed intervention works and public works have received the bulk of funds, while we see a monotonic decline of the relative fraction of expenditures going to training.

< Table 1 about here >

The main objective of *training* and *re-training* courses is to solve skill mismatch. By increasing the human capital of the unemployed in skills that employers in the expanding sectors want, the chances of the unemployed to enter a regular job are meant to increase and bottlenecks in the supply of certain skilled workers are meant to be eliminated. Popular courses are in the fields of data processing, accounting and secretarial work, as well as in tailoring and welding. The length of the courses is relatively short, in our sample the mean length being 2.6 months in the case of male and 2.5 months in the case of female trainees. The courses are organized by the local labor offices (LLOs) or by private agencies, which are then paid by the LLOs, or take place directly in firms. Trainees receive 115 percent of the amount of unemployment benefit, part of which has to be repaid if they do not complete the course.

Intervention works (wage subsidies) have two major goals. First, by hiring an unemployed person on a subsidized job he or she can enhance or regain human capital that might enable him or her to subsequently enter a regular job. Secondly, entrepreneurs can learn about the productivity of a worker without paying him or her a full wage. Incentives to the firm are structured in such a way that ensures the longest possible employment relationship. The longer a previously unemployed worker is kept in an intervention works slot the higher the cumulative subsidy going to the firm will be. Workers have an incentive to hold on to such a subsidized job for at least 6 months as, in the period under study, an employment relationship of this length entitled workers to another round of 12 months benefit receipt. The modal length of intervention works jobs in our sample is 6 months for men (63%) and women (48%), with very few jobs below this duration. So, most participants in intervention works qualify in principle for another round of benefit payment.

Public works are directly created public jobs that are mainly but not exclusively targeted at the long-term unemployed. Like intervention works they are meant to enhance the human capital of participants, many of the jobs offered are, however, of a very low skill nature. The focus of these public works is the amelioration of the environment and the improvement of local infrastructure. Public works are organized by the LLOs in cooperation with municipal authorities. The incentive structures facing employers and workers are similar to the ones in connection with intervention works. The cumulative subsidy is larger the longer a previous unemployed is kept in a directly created public job, while such a worker qualifies for another round of benefit receipt if he or she remains in the job for at least 6 months. Not surprisingly the modal length of public works jobs is 6 months (53%).

Since the end of 1991 unemployment benefits have been limited to 12 months and been paid as a flat rate amounting to slightly below the minimum wage. Unemployed persons who exhaust their benefits have to rely on social assistance which is often, however, either only sporadically paid or paid out in the form of material help (Góra and Schmidt, 1998). So, in many cases the only route to prolonged income support at a decent level is involvement in an ALMP measure, which entitles the unemployed to a further 12 months of benefit payment.

4. THE DATA: LABOR MARKET HISTORIES AND THE MATCHING ALGORITHM

The Polish Labor Force Survey (PLFS) is a quarterly survey, which was started in May 1992 and which has been structured as a rotating panel since its fifth wave (May 1993). Supplements on labor market policies were introduced in August 1994 and in August 1996. These supplements make it possible to generate a database for the evaluation of labor market policies. This paper focuses on the 18th wave of the PLFS, taken in August 1996, in connection with its complementing supplement. Since we are interested in the effectiveness of ALMP measures offered by Local Labor Offices (LLOs) to the unemployed or to the previously unemployed, we use a sub-set of the full PLFS data set generated from this wave. For the construction of treatment and control groups we select those respondents who were registered at least once as unemployed between January 1992 and August 1996.

The supplement to the 18th wave includes individual labor market histories containing information on an individual's labor market state in every single month from January 1992 to August 1996. Table 2 shows the various labor market states in these histories. Not all these labor market states are mutually exclusive, as e.g. training and registered unemployment, intervention works and employment are logical double entries. We re-coded such double entries very carefully to ensure consistency of the data.[4]

< Table 2 about here >

We created sub-samples of ALMP participants, choosing those who had been offered participation in training, intervention works or public works by their LLO and who had accepted the offer. The sample sizes for these sub-samples are 241, 532 and 93 respectively. We then generated a corresponding sub-sample of potential controls by selecting all those who at least once had been registered as unemployed since January 1992 and excluding all individuals in the ALMP sub-samples. This control sample has a size of 7784 records.

As was discussed formally in some detail in section 2, we match participants and controls not only across certain observable characteristics, but also across their pre-treatment history. Since we want to use as many treatment and control cases as possible we apply a "moving window" to the data as shown in Figure 1.[5] Given that an individual participated in a labor market program at a particular point in time for a particular number of months, we require a control to have an identical pre-treatment labor market history at the same point in time. Also, we compare employment outcomes for exactly the sequence of months that started when the participant's program spell ended. While non-participants might have an advantage insofar as their job search activities are not restricted during the participants' program spells, evaluation of an ALMP measure at the individual level should take this situation into account. For example, the impact of a training measure for an unemployed will consist of two countervailing effects with respect to employment. On the one hand, during the training spell the unemployed will not be able to engage in job search as vigorously as his/her not-participating colleague, i.e. *ceteris paribus* participation lowers the probability of finding employment after the end of the program. On the other hand, training is meant to increase the participant's human capital and should, therefore, *ceteris paribus* increase the probability of finding employment after the end of the

[4] A detailed account of the transformation and re-coding of the data can be found in Kluve (1998). Exploiting the panel nature of the data, the author also shows in this study that recall error is a minor problem.

[5] Figure 1 and what follows focus only on training. The same matching procedures apply for intervention works and public works.

program. In this study we are interested in the *net* overall impact that arises out of these two countervailing effects.

< Figure 1 about here >

In matching across individual pre-program histories as well as in analyzing the post-treatment outcomes we are mainly interested in the labor market outcomes "employed" (="1"), "unemployed" (="2"), and "out-of-the-labor-force" (="0"). These realizations, "0", "1" and "2", are recorded on a quarterly basis, where quarters are those three-month-intervals either ending in the month immediately preceding training or beginning in the month immediately succeeding training. If those three months that form a quarter do not contain identical entries, the interval is assigned the value of the event appearing twice, e.g. "212" constitutes a "2". If the interval reads "021", it constitutes a "1", since being in a "successful" labor market state, i.e. being employed, during one month, constitutes a corresponding "successful" quarter.

Extending conventional matching to a dynamic setting implies that an individual trainee's record has to meet the following requirements:

(a) An entry "training" must exist for at least 1 month in the 56 months observed.

(b) The record must have a complete 12-month pre-training history.

(c) The record must have a complete 9-month post-training history (i.e. $Q=3$).

(d) The beginning and the end of the training spell must be defined.

(e) Demographic information that we use in the matching algorithm must be complete.

As far as (a) is concerned we retain those observations with exactly one training spell and we select that training spell of those few individuals with multiple spells that is the longest and that has a complete pre- and post-training history. Requirements (b) and (c) imply that very early (1992) or very recent (1996) spells of training cannot be part of our analysis. The information stored in (d) is crucial for our dynamic matching algorithm and essential for the control of macroeconomic effects while the demographic information (e) includes the following categorical variables: gender (male/female), marital status (married/not married), education (high = university; low = primary school or below; medium = all other), and age. Furthermore, following Heckman et al. (1997) who emphasize the need to control for local labor market conditions, we perform the matching algorithm for two different regional taxonomies. Taxonomy 1 considers a regional dummy distinguishing Warsaw/not Warsaw taking account of a more dynamic labor market in the capital, while taxonomy 2 accounts for an exact regional match across all 49 voivodships. Theory predicts that taxonomy 2 will yield the least biased estimators. We will see, though, that matching conditional on exact voivodship matches reduces the number of participants who find matching partners in the control sample.

All those observations meeting the above-described requirements with respect to characteristics and, after the appropriate transformation from months into quarters, with respect to 4 pre-training and 3 post-training quarterly labor market outcomes constitute the data set of trainees for our analysis.

Matching proceeds then as follows. We match participants with all those controls who satisfy our requirements in terms of observable attributes and identical pre-treatment history.[6] In this, the 4-quarter pre-treatment history has to be identical for program participant and corresponding control group member, while the control needs to have a complete 9-month-history starting with the first month of the trainee's 9-month post-training history. Also, the control has to be identical in the following categorical variables: gender, marital status, education, and region.

[6] We apply a procedure of sampling with replacement, since we allow for an observation in the comparison group sample to control for more than one trainee, if he or she meets the necessary requirements. Such a constellation, however, rarely occurs.

If these requirements are met, we choose controls with the minimum distance in years of age. Most, i.e. 97.6 per cent of the matched controls, do not deviate more than 5 years from their treatment group matching partners, but we do allow for a maximum distance of 20 years. This procedure, which has the most stringent matching requirements compared with other possible matching algorithms we employed, yields sufficiently large treatment and matched control samples. Due to the stringent matching requirements this procedure also generated the most robust results.

Using $Q=3$ post-treatment quarters focuses on the short-term treatment effects. To estimate medium-term treatment effects we apply a second matching procedure ("second match") with the same stringent requirements, but where we compare labor market outcomes for $Q=6$ post-treatment quarters.

5. RESULTS

Basic demographic characteristics and labor market outcomes are described for the full sample and the various sub-groups that are used in the analysis in Tables 3 and 4. *Potential controls* are all those who between 1992 and 1996 were unemployed at least once but did not participate during this period in any of the three ALMP. Looking at men (Table 3), the demographic characteristics indicate that relative to the full subsets of trainees, intervention and public works participants (columns 3 – 5 in the upper panel) the group of potential controls (column 2 in the upper panel) is slightly younger than trainees and intervention works participants, but on average even more than four years younger than public works participants.

Marital status is quite similar across the three participant groups. but for trainees the marriage rate is 7 percentage points higher than for controls.

The difference in educational attainment across treatment groups is striking: The fraction of trainees with non-compulsory education[7] is slightly higher than that among potential controls, but substantially lower among intervention works and public works participants. So, on this measure, unemployed individuals are targeted for training who have slightly more human capital than the average unemployed while for intervention works and public works we see individuals targeted with significantly less human capital than the average unemployed. The differences in observable characteristics are on the whole maintained as we move from the full subset of treatment groups to the smaller subsets of treatment groups that are used for the two types of matches (columns 1 – 6 of lower panel).

< Table 3 about here >

One particularly interesting labor market outcome is the employment rate, which is shown here for August 1992 and August 1996. Recall that our matching algorithm requires the pre-treatment history to be 4 quarters long and the post-treatment history to have a length of at least three quarters. Hence, the two employment rate estimates shown are free from any bias arising from the participation in an ALMP scheme.

Inspection of these employment rates shows large variations across the various treatment groups and over time. Trainees have much higher employment rates than individuals who participate in other schemes. It is also noteworthy, that whilst the employment rate of the full sub-sample of trainees has risen by 3 percentage points from August 1992 to August 1996, during the same period it has fallen by 13 and 22 percentage points for the full sub-samples of

[7] Compulsory education is defined here as primary school attainment or less.

intervention works and public works participants, respectively. A naive way to evaluate the three Polish ALMP measures would consist in constructing a difference-in-differences estimator based on these differences and the difference between the August 1996 and August 1992 employment rates of the set of potential controls. Such a rough difference-in-differences estimator would indicate that training raises the probability of employment for men by 4 percentage points, while intervention works and public works lower this probability by 12 and 21 percentage points respectively. Such a crude approach would, however, tell us little about the true impact of these programs at the individual level.

Women hardly participate in the public works program; Table 4 and all subsequent tables therefore only report the involvement of women in training and in intervention works. Regarding employment outcomes the same pattern of discrepancies in observable characteristics arises across the various sub-samples for women as for men. Inspection of Tables 3 and 4 generates additional interesting information regarding differences between men and women. In Poland women have a higher incidence of unemployment than men which is reflected in the larger female pool of those in this 18th wave of the PLFS who have been unemployed at least once between 1992 and 1996. Women tend to be represented more substantially in training schemes, whereas men dominate intervention works.

The higher educational quality of female participants in training courses, but also in intervention works is striking. For example, if we take the full sub-sample of treatment groups, in training as well as in intervention works women have fractions of program participants with non-compulsory education that are 13 percentage points higher than the corresponding fractions of male participants.

< Table 4 about here >

Employment rates by contrast are substantially lower for women than for men. This is due to higher female unemployment rates as well as lower female participation rates during the period under study. Finally, our naive difference-in-differences estimator would establish that for women training and intervention works raise the probability of employment by about 17 percentage points and 6 percentage points respectively.

As discussed in the previous sections, treatment effects are estimated as the average differences within matches of employment and unemployment rates during either three or six consecutive quarters after the intervention. The matching estimators are conditioned on observable individual characteristics, identical pre-treatment histories and on the regional labor market. The first estimator looks at short run effects of treatment, while with the second estimator we attempt to pick up more persistent, medium-term effects of the treatment.

< Table 5 about here >

< Table 6 about here >

Tables 5 and 6 illustrate for men and women, respectively, how the weighted total treatment effect of an ALMP measure on employment and unemployment rates is constructed from the detailed results for specific labor market histories. First, for each pre-treatment history a history-specific treatment effect is calculated as a simple average of the differences in the post-treatment rates of individual treatments and their matched controls. In this calculation, these individual differences are constructed as the differences between the outcome for the treated individual and the average outcome over all his or her matching partners.

Second, the history-specific estimates are condensed into an estimate of the overall effect. This overall effect is the weighted average of the history-specific estimates, where the weights are given by the fractions of the participant sample. As regional taxonomy 2 is more disaggregated, the number of histories of individuals in the treatment group and of matching partners declines. However, the difference is not severe for the individuals in the treatment group, so the precision of the estimate of the total effect suffers only slightly as we reduce bias when moving from the aggregate to the more sophisticated regional taxonomy.

As can be seen in Tables 5 and 6, the vast majority of participants in intervention works was unemployed at least for one quarter during the year preceding the beginning of the treatment spell, and roughly 75 percent of participants were unemployed throughout the year preceding the beginning of their spell on intervention works. However, a small number of participants who were offered a slot on the scheme by their LLO comes from inactivity or from employment. As we define any measure offered by the LLOs as an ALMP measure we include these histories in our calculations. The dominance of the unemployment state in the pre-treatment histories also characterizes the other ALMP schemes.

Even with a casual glance at Tables 5 and 6 one notes the widely differing treatment effects of the individual matches conditioned on pre-treatment histories. Not only do they have very different magnitudes, but also the estimated treatment effect often changes its sign as we go from one history to another. We also see that most individuals in the treatment group are concentrated in a few histories. In previous work (Kluve et al., 1998) we checked the sensitivity of the estimates of the overall treatment effects to the presence of these important histories. To this purpose we removed one of these important histories at a time and re-estimated the overall treatment effects. The results of this procedure indicate that the total treatment effect estimates are quite robust: in the case of significant estimates removing an important history typically affects the magnitude of the overall estimates, while leaving the sign unchanged.

The overall treatment effects of the three ALMP programs on employment and unemployment rates are displayed in Tables 7 and 8, where each total treatment effect estimate is constructed as shown in tables 5 and 6. Let us first concentrate on Table 7, where we present short-term effects.

< Table 7 about here >

If matching is conditioned on the first regional taxonomy, no discernible effect of training on employment and unemployment rates can be observed. When local labor market conditions are seemingly better controlled for, we find a statistically significant positive overall treatment effect of training on the employment rate. This positive effect can still be established when the estimation is done separately for men and women, although the separate treatment effects for each gender are less well defined than the overall effect. Also, while training raises the average employment rate by some 15 percentage points for male trainees and by 13 percentage points for female participants, training measures have a negative, but statistically not significant impact on the unemployment rates of participants. So, the higher employment rates could result from training measures preventing workers from flowing out of the labor force rather than from lowering unemployment rates among active workers.

Irrespective of the regional taxonomy, according to our estimates intervention works have a large negative and statistically significant effect on the employment rate of men. This negative overall treatment effect of approximately minus 24 percentage points has its counterpart in a positive and significant overall treatment effect on the unemployment rate, which is, in absolute value, of the same magnitude. It is noteworthy, though, that women's employment and unemployment rates do not seem to be affected by the participation in this program. For men, the overall treatment effects of public works display a similar pattern as the effects of intervention work. Public works seem to depress the employment rate of participants and raise their unemployment rate, even if the magnitude of these effects is somewhat smaller.

< Table 8 about here >

Turning to Table 8, we find that in the medium term the employment rate of women is raised by 17 percentage points through participation in a training measure, while for men the overall treatment effect amounts to only 10 percentage points and is also not well defined. As in the short term the unemployment rates of both men and women are not affected by training in the medium term. On the other hand, males who participate in intervention works apparently have a more negative labor market experience even in the medium term. The overall treatment effect estimates are, however, in absolute value, some 7 percentage points lower than the effects in the short-term. A final interesting result from Table 8 is the large positive and statistically significant overall treatment effect of public works on the unemployment rate if we use the second regional taxonomy. The number of cases in the treatment and control groups is very low, though, and this result needs to be interpreted with some caution.

In summary, from an efficiency point of view training appears to be an ALMP program that performs well in Poland. Both men and women raise their chances of being employed in the short-term if they participate in this program, while women in particular benefit also in the medium term. Previous microeconometric work did not find such a beneficial treatment effect (cf. Puhani and Steiner, 1997). In contrast, both subsidized employment (intervention works) and direct public employment (public works) are highly inefficient when targeted at men. Whilst these measures are meant to raise the human capital of participants and thus *ceteris paribus* raise their employment rate, our estimates imply that they do exactly the opposite in the Polish case. It is certainly ironic, that the very ALMP program that seems to improve the performance of unemployed individuals in the Polish labor market, training, has experienced sharp expenditure cuts in recent years, while this has not been the case for the apparently ineffective intervention works and public works.

Additional information about the effects of active labor market programs is provided by employment retention and job accession rates. Given that an individual is employed in the first quarter following treatment, we ask whether the ALMP has raised the probability that he or she will hold on to the job in the short-term or in the medium term (*job retention*). Given that the first quarter after treatment was spent in unemployment, we analyze the conditional probability of starting a new job and holding on to it (*job accession*). Since the employment stock at a particular point in time is crucially affected by job retention and job accession, looking at the overall treatment effects on these two rates might also help us better understand what lies behind the overall treatment effects on the employment rate. Given our definitions of retention and accession rates, the number of individuals that we match might be too small for serious statistical analysis. In the case of public works this is precisely what happens and we do not analyze this ALMP measure when estimating the treatment effects on retention and accession rates.

< Table 9 about here >

< Table 10 about here >

The short-term positive effect on the employment rate of both men and women that we found in the case of training and the short-term negative effect on the employment rate of men in the case of intervention works are reflected in the results reported in Table 9. The increased employment rate of male trainees appears to be supported by an improved retention rate, while the employment rate of female training participants apparently benefits from better access to jobs. Intervention works, on the other hand, significantly depress the job accession rate of men. If male participants find themselves unemployed in the first quarter after the end of the program, they have a much lower probability of flowing to a regular job than if they had not participated.

The medium-term estimates of the overall effects on retention and accession rates shown in Table 10 are in general less well defined. There are some interesting results, nevertheless. The treatment effect of training on the accession rate of women is still positive and large, though not significant at conventional levels. Most noteworthy is the large negative effect of training on the retention rate of women when the second regional taxonomy is used. This effect might be worrying as it seems to imply that the type of training courses women are offered makes it difficult for them to hold on to a job for more than a year. In contrast, there is a positive treatment effect of intervention works on the retention rate of women in the medium term. So, if women are retained after the treatment spell ended, which is one of the effects intended by the program, they have a greater probability of holding on to a job for at least 18 months than if they had not taken part in such a program

Polish training measures seem to enhance the human capital of unemployed workers of either gender and thus improve their chances to find employment in regular jobs. In contrast, for men, participation in intervention and public works lowers the likelihood of finding regular work. One reason often given in the literature is that participation in such employment programs carries a stigma. Because of asymmetric information employers do not know the productivity of new workers some of whom they might hire from the pool of the unemployed. Prospective employers might then perceive participants in such employment programs as low productivity workers or workers with tenuous labor market attachment.

While such stigmatization might affect some workers in Poland, it cannot fully explain our results. If stigmatization were the full story, why do women placed on intervention works apparently escape this stigmatization? A competing explanation of the negative treatment effects of intervention and public works for men could be benefit churning. There is widespread anecdotal evidence that officials in Polish LLOs place some of the unemployed into these schemes so that they re-qualify for benefit payment. In Table 11 we try to provide some evidence for this type of interaction of ALMP programs and unemployment compensation.

The second row of Table 11 shows those participants in the various programs who, prior to treatment, were unemployed continuously for at least six months and who also received benefits for at least one month. The second to last row of the table gives the number of participants who right after treatment were receiving benefits continuously for nine months. An individual who appears in both of these rows might be thought of as someone engaging in benefit churning. There is a circular flow that takes an individual from a long unemployment spell with some benefit payment through an ALMP program and then immediately after its termination back to another unemployment spell with continuous benefit payment for at least 9 months. Churning rates thus understood turn out to be 0 percent for trainees[8], 60 percent and 42 percent for male participants of intervention works and public works respectively, and 37 percent for female participants of intervention works. While these back-of-the-envelope calculations are based on small numbers, the large fractions of male "benefit churners" still make the point convincing that benefit churning apparently contributes to the poor performance of the intervention works and public work programs. As income support for those on long unemployment spells is rather poorly developed in Poland (Góra and Schmidt, 1998), officials of LLOs seem to consider males, often heads of households, particularly worthy to receive prolonged income support.

< Table 11 about here >

6. CONCLUSIONS

In this study we implement a conditional difference-in-differences matching estimator to evaluate, at the micro level, the effectiveness of three ALMP measures in Poland: training and re-training, subsidized employment ("intervention works") and direct public employment ("public works"). Our approach is insofar innovative as we use a "moving window" on the data to account for a changing macroeconomic environment. Most importantly, we match simultaneously on observable characteristics and pre-treatment histories and thus ensure that selection bias and bias due to unobserved heterogeneity are minimized. Individual treatment effects are estimated by estimating the difference in employment and unemployment rates of those subsets of treatment and control groups that have an identical pre-treatment history. Employment and unemployment rates are averaged over 3 post-treatment quarters to characterize short-term effects, while averaging over 6 post-treatment quarters is the basis for analyzing the impact of these policies in the medium term. The overall treatment effect of an ALMP measure is then the weighted sum of the individual effects, where the weights are the fractions of the treatment group belonging to each pre-treatment history.

How effective are Polish ALMP programs? Training and re-training is the ALMP measure that performs well from an efficiency point of view. Our estimates suggest that the short-term post-treatment employment rates of both female and male participants are higher than they would have been had these individuals not participated in the program. Key ingredients of

[8] Participation in a training scheme normally does not imply an employment relationship, i.e. does not carry with it a

these results are higher employment retention rates in the case of men and higher job accession rates for female trainees. In the medium-term we see a statistically significant positive treatment effect only on the female employment rate; this rate, averaged over 6 post-treatment quarters, is raised through training participation by an estimated 17 percentage points. These beneficial effects of the Polish training and re-training program which could not be found in previous econometric work are in line with Puhani's (1998) findings who uses the same data set. So, this ALMP measure clearly seems to improve the efficiency of the Polish labor market and more resources should be dedicated to this program in future.

In contrast, the Polish employment programs seem to be burdened by major distortions. Despite their intention to enhance or rebuild the human capital of the unemployed, we do not find any overall treatment effects for women who participate in intervention works, but find strong negative overall treatment effects on the employment rate of men who take part in intervention and public works. These negative effects are somewhat reduced from minus 24 percentage points to minus 17 percentage points as we move from the short-term to the medium-term perspective. Our estimates also show that we get corresponding positive overall treatment effects on the male unemployment rate that are, in absolute value, of the same magnitude as the negative treatment effects on the employment rate.

Combining this information with the evidence of a sharply depressed job accession rate for male participants of intervention works leads us to believe that Polish employment programs are often the intermediate stage between two spells of unemployment benefit receipt. We cite some numbers on this "recycling" of unemployment compensation recipients which takes place above all via intervention works. These numbers strengthen our conviction that while

new entitlement of benefit receipt.

stigmatization might have some role to play, benefit churning explains most of the negative overall treatment effects of these programs. Out of "social considerations" officials in LLOs deem males as heads of households particularly worthy of prolonged income support from the state. On our evidence, a reform of the Polish employment programs seems to be needed that eliminates the distortions arising from interactions between the unemployment compensation system and these programs.

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Table 1 Distribution of resources between passive and active labor market programs[a]

	1992	1993	1994	1995	1996
Total expenditures[b]	2 282.75	2 370.23	2 527.87	2 796.59	2 825.39
PLMP	1 969.74	1 988.95	2 117.21	2 404.27	2407.97
ALMP	107.48	263.40	323.43	338.41	302.65
of which:	%	%	%	%	%
Intervention Works	43.74	38.54	43.02	41.25	34.39
Public Works	16.20	33.76	36.85	34.36	29.56
Training/Retraining	17.96	12.68	10.46	8.47	6.41
Loans (Self-employment)	14.91	9.03	6.11	5.91	6.84
Other	7.19	5.99	3.56	10.00	22.80
Participant inflows of major ALMP programs in % of labor force					
Intervention Works	0.8	1.2	1.8	2.0	1.5
Public Works	0.2	0.4	0.6	0.7	0.6
Training/Retraining	0.4	0.4	0.5	0.5	0.5

[a] in thousand Polish zlotys.

[b] in 1992 constant prices.

Source: Polish Ministry of Labor and Social Affairs.

Table 2 *Individual labor market history outcomes in August 1996 PLFS supplement*

Code	Outcome
1	Employment
2	Temporarily not in work (for "objective" reasons)
3	Participation in training course
4	Social assistance recipient
5	Taking care of small child
6	Registered unemployed
7	Unemployment benefit recipient
8	Intervention works (wage subsidies)
9	Public works

Source: PLFS supplement August 1996

Table 3 Demographic characteristics and employment rates for full sample, potential controls and treatment group sub-samples – MEN

	Full Sample[a]	Potential Controls[b]	Treatment Groups (Full Sub-sample)[c]		
			Training	IW	PW
Sample Size	3829	3369	94	307	75
Demographic Characteristics:[d]					
1. Average Age	33.4	33.1	35.2	35.5	37.7
2. Fraction Married	0.586	0.581	0.649	0.625	0.613
3. Fraction of Non-Compulsory Education[e]	0.784	0.801	0.830	0.616	0.613
Employment Rates:[f]					
1992	0.686	0.697	0.671	0.504	0.647
1996	0.679	0.684	0.700	0.372	0.423

	Treatment Groups (Suitable for 1st Match)[g]			Treatment Groups (Suitable for 2nd Match)[h]		
	Training	IW	PW	Training	IW	PW
Sample Size	53	164	45	31	102	20
Demographic Characteristics:						
1. Average Age	36.6	36.5	37.2	38.3	36.4	36.7
2. Fraction Married	0.660	0.665	0.556	0.742	0.676	0.650
3. Fraction of Non-Compulsory Education	0.868	0.616	0.689	0.839	0.627	0.550
Employment Rates:						
1992	0.726	0.436	0.651	0.700	0.439	0.650
1996	0.755	0.410	0.556	0.742	0.436	0.650

Footnotes Table 3:

[a] Individuals at least once registered as unemployed since January 1992 (Observations containing histories with less than 15 entries are omitted).

[b] Individuals at least once registered as unemployed since January 1992 who did not participate in an ALMP program.

[c] Individuals who participated in the corresponding ALMP program.

[d] At time of survey, i.e. August 1996.

[e] Excludes all individuals with primary school attainment or less.

[f] Employment rates are calculated for August 1992 and August 1996.

[g] Observations that were used for the short-term matching analysis ($Q=3$).

[h] Observations that were used for the medium-term matching analysis ($Q=6$).

Table 4 Demographic characteristics and employment rates for full sample, potential controls and treatment group sub-samples – WOMEN[a]

	Full Sample	Potential Controls	Treatment Groups (Full Sub-sample) Training	Treatment Groups (Full Sub-sample) IW
Sample Size	4230	3808	147	225
Demographic Characteristics:				
1. Average Age	32.7	32.6	32.6	33.6
2. Fraction Married	0.697	0.698	0.673	0.644
3. Fraction of Non-Compulsory Education	0.812	0.811	0.966	0.747
Employment Rates:				
1992	0.489	0.500	0.381	0.360
1996	0.477	0.472	0.529	0.396

	Treatment Groups (Suitable for 1st Match) Training	Treatment Groups (Suitable for 1st Match) IW	Treatment Groups (Suitable for 2nd Match) Training	Treatment Groups (Suitable for 2nd Match) IW
Sample Size	68	111	42	71
Demographic Characteristics:				
1. Average Age	32.8	36.0	34.2	36.9
2. Fraction Married	0.676	0.694	0.714	0.732
3. Fraction of Non-Compulsory Education	0.956	0.676	0.929	0.690
Employment Rates:				
1992	0.476	0.387	0.550	0.380
1996	0.591	0.417	0.650	0.471

[a] See footnotes of Table 3. Public Works are not reported because of small sample size.

Table 5 Average post-treatment employment rates and treatment effect by pre-treatment labor market history: short-term effects – Intervention Works – MEN

Regional Taxonomy 1

history	treatment group			matched controls				
	N	rate[a]	std.err.	N	rate	std.err.	effect[b]	std.err.
0000	2	0.000	0.000	9	0.208	0.287	-0.208	0.287
0022	1	0.000	0.000	1	0.000	0.000	0.000	0.000
1110	1	0.333	0.000	1	0.000	0.000	0.333	0.000
1111	10	0.900	0.095	184	0.727	0.141	0.173	0.170
1112	4	0.333	0.204	6	0.417	0.247	-0.084	0.320
1122	10	0.167	0.085	21	0.475	0.158	-0.308	0.179
1222	5	0.400	0.219	17	0.678	0.209	-0.278	0.303
2211	2	0.333	0.236	4	0.944	0.162	-0.611	0.286
2212	1	0.000	0.000	1	0.000	0.000	0.000	0.000
2221	1	0.000	0.000	1	1.000	0.000	-1.000	0.000
2222	125	0.107	0.025	496	0.385	0.044	-0.278	0.051
total[c]	162			741			-0.248	0.044

Regional Taxonomy 2

history	treatment group			matched controls				
	N	rate	std.err.	N	rate	std.err.	effect	std.err.
0000	2	0.000	0.000	2	0.500	0.354	-0.500	0.354
1111	10	0.900	0.095	12	0.783	0.130	0.117	0.161
1112	3	0.444	0.240	3	0.222	0.240	0.222	0.339
1122	4	0.083	0.072	4	0.417	0.247	-0.334	0.257
1222	2	0.000	0.000	2	0.833	0.264	-0.833	0.264
2211	2	0.333	0.236	2	0.833	0.264	-0.500	0.354
2222	100	0.127	0.030	108	0.385	0.049	-0.258	0.057
total	123			133			-0.236	0.051

[a] Average employment rate in the 3 post-treatment quarters.

[b] Difference between rates of treatment group and matched control group.

[c] Total effect is the weighted average of the effects for the individual histories using the fractions of the *participants*' sample as weights.

Table 6 Average post-treatment employment rates and treatment effect by pre-treatment labor market history: short-term effects – Intervention Works – WOMEN[a]

Regional Taxonomy 1

	treatment group			matched controls				
history	**N**	**rate**	**std.err.**	**N**	**rate**	**std.err.**	**effect**	**std.err.**
0000	4	0.417	0.217	46	0.137	0.172	0.280	0.277
0002	2	0.167	0.118	4	0.083	0.195	0.084	0.228
0022	1	0.667	0.000	1	0.000	0.000	0.667	0.000
1111	8	0.583	0.164	52	0.812	0.138	-0.229	0.214
1112	3	0.667	0.272	9	0.176	0.220	0.491	0.350
1122	7	0.381	0.171	8	0.476	0.189	-0.095	0.255
1222	2	1.000	0.000	4	0.333	0.333	0.667	0.333
2000	1	1.000	0.000	1	0.000	0.000	1.000	0.000
2111	1	1.000	0.000	1	1.000	0.000	0.000	0.000
2211	5	0.400	0.219	9	0.333	0.211	0.067	0.304
2221	2	0.000	0.000	2	0.667	0.333	-0.667	0.333
2222	73	0.247	0.046	643	0.263	0.052	-0.016	0.069
total	109			780			0.010	0.056

Regional Taxonomy 2

	treatment group			matched controls				
history	**N**	**rate**	**std.err.**	**N**	**rate**	**std.err.**	**effect**	**std.err.**
0000	3	0.556	0.240	4	0.333	0.272	0.223	0.363
0002	1	0.000	0.000	1	0.667	0.000	-0.667	0.000
1111	6	0.667	0.192	7	0.639	0.196	0.028	0.274
1112	2	0.500	0.354	3	0.083	0.195	0.417	0.404
1122	2	0.500	0.354	2	0.167	0.264	0.333	0.442
1222	2	1.000	0.000	2	0.833	0.264	0.167	0.264
2000	1	1.000	0.000	1	0.000	0.000	1.000	0.000
2111	1	1.000	0.000	1	1.000	0.000	0.000	0.000
2211	2	0.000	0.000	2	0.500	0.354	-0.500	0.354
2221	1	0.000	0.000	1	0.333	0.000	-0.333	0.000
2222	68	0.265	0.049	83	0.255	0.053	0.010	0.072
total	89			107			0.026	0.062

[a] See footnotes of Table 5.

Table 7 Overall treatment effects on employment and unemployment rates according to treatment, gender and regional taxonomy: SHORT-TERM effects

Training

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	all	118	956	0.005	0.051	0.015	0.052
	men	52	394	-0.041	0.079	0.024	0.080
	women	66	562	0.042	0.065	0.008	0.066
Regional Taxonomy 2	all	87	111	0.138	0.059	-0.092	0.059
	men	36	39	0.148	0.092	-0.139	0.091
	women	51	72	0.130	0.070	-0.058	0.073

Intervention Works

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	all	271	1521	-0.144	0.035	0.160	0.036
	men	162	741	-0.248	0.044	0.252	0.045
	women	109	780	0.010	0.056	0.025	0.057
Regional Taxonomy 2	all	212	240	-0.126	0.040	0.161	0.041
	men	123	133	-0.236	0.051	0.244	0.052
	women	89	107	0.026	0.062	0.045	0.063

Public Works

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	men	45	223	-0.156	0.078	0.159	0.078
Regional Taxonomy 2	men	33	35	-0.131	0.087	0.152	0.088

Table 8 Overall treatment effects on employment and unemployment rates according to treatment, gender and regional taxonomy: MEDIUM-TERM effects

Training

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	all	71	481	0.046	0.059	-0.024	0.059
	men	31	241	-0.010	0.089	-0.013	0.089
	women	40	240	0.090	0.075	-0.032	0.077
Regional Taxonomy 2	all	50	64	0.141	0.070	-0.123	0.071
	men	21	23	0.103	0.110	-0.119	0.108
	women	29	41	0.168	0.084	-0.125	0.089

Intervention Works

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	all	170	871	-0.075	0.039	0.112	0.039
	men	100	423	-0.182	0.050	0.202	0.050
	women	70	448	0.077	0.058	-0.016	0.060
Regional Taxonomy 2	all	128	146	-0.060	0.044	0.122	0.045
	men	73	80	-0.170	0.059	0.176	0.060
	women	55	66	0.086	0.062	0.051	0.067

Public Works

		N treatment	N controls	employment rate effect	employment rate std.err.	unemployment rate effect	unemployment rate std.err.
Regional Taxonomy 1	men	20	102	-0.142	0.094	0.146	0.094
Regional Taxonomy 2	men	13	14	-0.154	0.109	0.218	0.108

Table 9 Estimated retention and accession rate treatment effects according to treatment, gender and regional taxonomy: SHORT-TERM effects

Training

		N treatment	N controls	retention rate effect	retention rate std.err.[a]	N treatment	N controls	accession rate effect	accession rate std.err.
Regional Taxonomy 1	all	107	932	0.022	0.066	109	946	0.125	0.065
	men	47	384	0.082	0.069	45	356	-0.004	0.084
	women	55	541	-0.018	0.095	60	555	0.207	0.068
Regional Taxonomy 2	all	76	99	0.136	0.104	85	109	0.231	0.090
	men	32	35	0.326	0.127	32	35	0.162	0.097
	women	40	59	-0.078	0.171	48	69	0.252	0.069

Intervention Works

		N treatment	N controls	retention rate effect	retention rate std.err.	N treatment	N controls	accession rate effect	accession rate std.err.
Regional Taxonomy 1	all	263	1513	0.076	0.056	265	1515	-0.100	0.025
	men	146	707	0.008	0.102	158	735	-0.121	0.032
	women	105	776	0.114	0.068	98	725	-0.067	0.045
Regional Taxonomy 2	all	204	231	0.109	0.074	209	237	-0.092	0.039
	men	112	122	0.130	0.124	121	131	-0.179	0.059
	women	80	97	0.011	0.098	81	98	0.023	0.051

[a] Standard errors are obtained by the delta method.

Table 10 Estimated retention and accession rate treatment effects according to treatment, gender and regional taxonomy: MEDIUM-TERM effects

Training

		N	N	retention rate		N	N	accession rate	
		treatment	controls	effect	std.err.[a]	treatment	controls	effect	std.err.
Regional Taxonomy 1	all	60	455	-0.027	0.103	63	463	0.091	0.091
	men	27	235	0.021	0.153	29	239	-0.052	0.134
	women	30	216	-0.109	0.130	33	222	0.231	0.098
Regional Taxonomy 2	all	42	56	-0.017	0.163	45	59	0.163	0.143
	men	17	19	0.240	0.251	21	23	0.008	0.211
	women	23	35	-0.345	0.155	23	35	0.259	0.162

Intervention Works

		N	N	retention rate		N	N	accession rate	
		treatment	controls	effect	std.err.	treatment	controls	effect	std.err.
Regional Taxonomy 1	all	162	862	0.187	0.075	161	827	0.007	0.044
	men	91	408	0.105	0.133	96	417	-0.032	0.057
	women	65	442	0.237	0.091	55	371	0.031	0.081
Regional Taxonomy 2	all	116	133	0.177	0.104	123	140	0.044	0.055
	men	67	74	0.154	0.160	70	77	0.099	0.072
	women	46	56	0.194	0.155	45	55	0.046	0.096

[a] Standard errors are obtained by the delta method.

Table 11 Benefit churning - unemployment benefit situation of program participants

	training	intervention works			public works
	all	all	men	women	men
N total	121	275	164	111	45
N conditional[a]	23	130	89	41	19
No. in benefits - 1st month[b]	16	86	66	20	11
No. in benefits - 2nd month	12	93	70	23	12
No. in benefits - 3rd month	11	94	70	24	12
No. in benefits - 4th month	5	92	70	22	11
No. in benefits - 5th month	7	91	68	23	11
No. in benefits - 6th month	4	85	62	23	10
No. in benefits - 7th month	5	83	60	23	11
No. in benefits - 8th month	4	85	61	24	11
No. in benefits - 9th month	2	85	62	23	11
No. in benefits - all 9 months[c]	0	68	53	15	8
No. in benefits - at least 1 month[d]	16	102	76	26	13

[a] No. of program participants in each ALMP sub-sample conditional on a 6-month pre-treatment history with all 6 months unemployed and at least one of these months receiving benefits.

[b] No. of program participants conditional on ([a]) who received benefits in the first month after treatment.

[c] No. of program participants conditional on ([a]) who received benefits for all 9 months succeeding treatment.

[d] No. of program participants conditional on ([a]) who received benefits for at least one of these 9 post-treatment months.

Figure 1 **Matching over identical individual labor market histories applying a "moving window"**