Discussion Paper No. 98-25

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Effects of Active Labour Market Programmes in Poland

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September 1998

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Non-Technical Summary

Active labour market policies are supposed to raise the re-employment chances of (long-term) unemployed people. To achieve this aim, Poland like other countries has adopted different types of programmes. *Training* courses either re-qualify their participants or upgrade skills in an old occupation. Typical courses are computing, accounting, foreign languages, but also crafts like tailoring or welding. Normally, these courses take place off the job. A conceptually rather different type of active labour market policies are job subsidies, called *intervention works* in Poland. Here the idea is to establish a contact between the unemployed person and an employer in order to remove potential prejudices that employers might have against the (long-term) unemployed. In the Polish case, the unemployment pool is relatively stagnant with low outflow rates even in comparison to other transition economies. Hence, from a conceptual point view, such job subsidies may be a good way to increase the turnover of the unemployment pool. *Public works* are direct job creation programmes which are not only targeted at very hard to employ people, but also at certain regions with underdeveloped infrastructure. Typical jobs are in construction or cleaning. The programmes of *training*, *intervention works*, and *public works* are the most important ones in Poland and are analysed in this paper.

When estimating the re-employment effects of active labour market programmes in Poland one has to take into account that programme participants are different from the ordinary non-participant unemployment population. This means that simple comparisons of re-employment rates between participants and non-participants after the programme should not be interpreted as causal effects of the programme. To obtain a valid comparison that controls for the different personal characteristics the various programmes are targeted at, econometric estimation methods are used.

We show that training programmes increase the re-employment chances of the unemployed. Intervention and public works programmes, on the other hand, have a negative employment effect in the medium run. Comparable results have been found in research on similar programmes for Hungary and eastern Germany. We have three explanations for this finding that are not mutually exclusive.

The *first* explanation is conceptual. As lack of qualification is most often key to unemployment, training may work better because it addresses the very cause of the problem. The *second* explanation is institutional. In our observation period (1992-1996) participation in intervention or public works renewed the entitlement period to unemployment benefits to another full term of 12 months. As a consequence, a lot of people 'cycled' between the works programmes and unemployment benefits. The fact that around 25 percent of works programmes participants are from the (public) administration (as opposed to only 2 (!) percent of the unemployed) is consistent with the existence of some comradeship amongst public administrators which results in a misuse of works programmes as a means to prolong unemployment benefits. The *third* explanation refers to stigma effects of works programmes. If employers have a bad opinion of works programmes, they will see participation in such a programme as a negative signal on a person's productivity.

Policy conclusions that may follow are to increase the expenditure share of training in active labour market programmes and to separate unemployment benefit entitlement from participation in an active programme. In addition, one may want to think about re-employment premia paid to the unemployed (*i.e.* not to the employer) to avoid stigma effects.

Abstract

We estimate the employment effects of training, intervention works (subsidised employment), and public works programmes in Poland. The analysis is based on retrospective monthly calendar information on the labour force state and active labour market programme (ALMP) participation between January 1992 and August 1996. The data are obtained from the Polish Labour Force Survey of August 1996 and its Supplement on Labour Market Policies.

Because there is no general agreement on the appropriate evaluation methodology when working with non-experimental data, we use two widely applied approaches to identify causal effects. First, non-parametric estimates of the programme effects are obtained on the basis of matched samples. Second, we use traditional econometric modelling in the form of duration models with unobserved individual heterogeneity.

We find that training improves the employment opportunities of both men and women, whereas intervention and public works do not: intervention works prolong unemployment for both genders as do public works for men. The number of observations on women in public works is too small to make a statistically safe statement.

In general, all ALMP effects are larger in absolute size for men than for women.

1 Introduction

In Poland as in all other central European transition countries except the Czech Republic, the transition process was accompanied by the appearance of open mass unemployment. From 1990 to 1993, the registered unemployment rate rose from virtually zero to 16.4 percent. Thereafter it declined, although it still stood at around 10.2 percent in the last quarter of 1997.

Active labour market programmes (ALMPs) have been implemented in Poland right from the beginning of the transition process. Because it is *ceteris paribus* more costly to treat an unemployed person with an active labour market programme like training or a job subsidy than to just hand out unemployment benefits, it is important to know whether these programmes are value for money. This study contributes to this question by estimating the employment effects of training, subsidised jobs (called *intervention works* in Poland) and direct job creation (*public works*) programmes in Poland at the individual (microeconomic) level.

Previous microeconometric studies on Polish ALMPs by Puhani and Steiner (1996; 1997) and Puhani (1996) find no effect of training on employment, but negative effects of works programmes (Puhani and Steiner). It has to be kept in mind, though, that the quality of the data used for evaluation purposes has been a serious problem in these studies, as the timing of unemployment and ALMP participation could not be identified from the Polish Labour Force Survey Supplement of August 1994, which has been the only available data source with which microeconometric ALMP evaluation could be attempted. We have therefore proposed to ascertain retrospective monthly calendar information on employment, unemployment, and ALMP programme participation in the August 1996 Supplement to the Polish Labour Force Survey (Steiner, Puhani, and Kwiatkowski, 1995). Such data is now available and, to our knowledge, we are able to present the first microeconometric evaluation of the re-employment effects of ALMP programmes in Poland that use this data.

The paper is structured as follows. Section 2 gives a brief overview of Poland's main ALMPs. Because there is no agreement on the best estimation method when working with non-experimental data (Ashenfelder and Card, 1985; LaLonde, 1986), we shortly present two widely used approaches to the identification of causal effects in Section 3. Section 4 is the empirical part of the paper where we exhibit the main estimation results. For more detail on these results, the reader is referred to Puhani (1998). Section 5 concludes.

2 Active Labour Market Policy in Poland

The main active labour market programmes (ALMPs) that currently exist in Poland are training or retraining for the employed and unemployed, subsidies to employment in terms of wage subsidies (called *intervention works* in Poland) or loans to enterprises, public works, and start-up loans to support self-employment.

The Act on Employment and the Act on Group Layoffs which took effect at the beginning of 1990 established the Labour Fund as the main financial source for the funding of labour market policies. About one third of the Labour Fund's resources is drawn from employer's contributions. These are equal to 3 percent of the product wage. The other two thirds of the fund are drawn directly from the state budget. So far, employees do not have to pay directly into the fund.

The development of Labour Fund expenditures is described in Table 1.

Table 1: The Development of Labour Fund Expenditures 1990-1996

	1990	1991	1992	1993	1994	1995	1996
Share of PLMP exp. in GDP	0.34	1.38	1.71	1.72	1.72	1.93	1.75
Share of ALMP exp. in GDP	0.21	0.12	0.09	0.23	0.27	0.26	0.21
Policy activism	38.1	8.0	5.0	11.8	13.6	11.9	10.7
Share of training in ALMP exp.	1.3	9.5	18.0	12.7	12.7	9.19	13.5
Share of IW in ALMP exp.	17.6	47.8	43.7	38.5	40.7	44.8	40.1
Share of PW in ALMP exp.	0.0	0.0	16.2	33.7	36.8	36.5	34.6
Share of loans in ALMP exp.	81.0	42.7	22.1	15.1	9.7	9.6	13.5

Notes: policy activism is defined as the percentage share of ALMP (active labour market policy) expenditure in total Labour Fund expenditure; loans include both start-up loans and loans to enterprises. PLMP: passive labour market policy (unemployment benefits); IW: intervention works; PW: public works; exp.: expenditure.
Source: Polish Ministry of Labour and Social Policy.

It is shown that policy activism defined as the share of ALMP expenditures in total Labour Fund expenditures was as high as 38 percent right at the beginning of the transition period, but declined to 5 percent in 1992. After 1992, there was a rise in the activity rate again to between 11 and 14 percent. Although policy activism as just defined was never as high again as in 1990, the share of expenditures on ALMPs in GDP surpassed the level of 1990 as soon as 1993. What happened was that from 1990 to 1992, Polish unemployment skyrocketed from 0 to almost 14 percent so that PLMP expenditures crowded out ALMP expenditures and reduced policy activism. Nevertheless, as can be seen from Table 1, policy makers renewed their commitment to active labour market policies in the subsequent period.

However, Table 1 also shows that the policy mix has changed dramatically between various labour market policies. In 1990, loans (mainly start-up loans) were the main tool of active labour market policy, but already in 1991, interventions works had increased their expenditure share above the one of start-up loans. Loans were then reduced to a tenth of total expenditures with the largest funds going to intervention and public works. As we will evaluate training, intervention and public works in this study as the main ALMPs in Poland, short descriptions of these programmes are given in the following.

Training and Retraining

The main aim of the Polish training programmes is to increase the employment chances of the unemployed. However, training courses may also be granted to workers who are in danger of losing their jobs due to lack of skills. There are training courses that take place in classrooms and are organised by the labour offices themselves. Others are delivered by private agencies which are paid for by the local labour offices. Or, as a third alternative, training can take place within firms (O'Leary, 1997a). The training courses usually last from three to six months. Prominent courses taught are using computers, accounting, secretarial work, working as a salesperson, and some craft trades, such as tailoring or welding. Whilst on a training course, an allowance is paid out to the participants which is equal to 115 percent of the amount of unemployment benefit. In order to make sure that the training participants take the programme seriously, they have to repay the costs of the course if it is not completed. There are not only incentives on the demand side of training, but also on the supply side. Partnership firms get a reduction on income tax since 1992 if they organise training courses in regions declared to be threatened by structural unemployment. This reduction is equal to six or nine times the minimum wage, depending on the length of the course (Kwiatkowski, Kubiak, and Kucharski, 1997).

Intervention Works (Job Subsidies)

As training courses, intervention works programmes seek to facilitate the re-integration of unemployed persons into the world of work. Intervention works participants can in principle choose to work in any type of job. The local labour office then pays out a subsidy to the employer. The employment subsidy can be paid out for 6 to 12 months. For 6 months' work contracts, the local labour office pays a subsidy equal to the unemployment benefit. The social security contributions are also covered by the labour office. If the employer decides to employ the intervention works participant for 12 months, the labour office pays out an amount equal to the minimum wage every second month, which is about 15 percent higher than the unemployment benefit. In addition, the social security contributions are covered by the labour office every second month. A further incentive to the

employer is the possibility of receiving a grant of 150 percent of the average wage in the economy if the employer continues to employ the programme participant after the intervention works programme (Kwiatkowski, Kubiak, and Kucharski, 1997). The concept of intervention works programmes is that they allow the employer to gather information on the productivity of the unemployed worker very cheaply. This productivity might be higher than initially perceived by the allegedly prejudiced employer. In addition, the worker has the potential to acquire firm-specific human capital during his or her intervention works period so that he or she can raise his or her value to the employer. A rather different incentive to the unemployed worker to join intervention works is the fact that, up to the 1st of January 1997, the completion of an intervention works course led to a complete renewal of the 12 months' entitlement period to unemployment benefits.

Public Works

Public works are direct job creation programmes which are both targeted on the long-term unemployed and on certain regions to develop local infrastructure. The organisation of public works programmes is carried out by local labour offices in co-operation with the municipalities in question. Typical public works jobs are in construction or cleaning. As in the case of intervention works, the duration of the programmes lies between 6 and 12 months. The incentives for the organisation carrying out the public works programmes are similar to the ones for intervention works. For 6 months' contracts, the wage subsidy equals 75 percent of the average wage in the economy. For 12 months' contracts, a subsidy amounting to the average wage is paid out every second month. In both cases, the labour office pays the social security contribution monthly or every second month, respectively (Kwiatkowski, Kubiak, and Kucharski, 1997). In regions that are declared to be threatened by structural unemployment, the organiser of the public works programme may receive further subsidies for non-labour inputs. This subsidy may not exceed 25 percent of the total sum of wage subsidies, though. As in the case of intervention works, the completion of a public works course led to a complete renewal of the 12 months' entitlement period to unemployment benefits until the 1st of January 1997.

In our empirical analysis, we evaluate Polish active labour market programmes for the period 1992 to 1996. At that time, the new labour market institutions had already been operating for at least two years and gathered some experience. It has to be kept in mind, though, that during this period unemployment was very high in Poland. On the one hand, such a situation is not a good precondition for trying to re-integrate unemployed people into the labour market. On the other hand, times of high unemployment are exactly those when ALMPs are most needed. Our observation period is thus well suited to put active labour market programmes to the test.

3 Methodological Issues of Microeconometric ALMP Evaluation

The fundamental problem of estimating causal effects of labour market programmes is that the outcome variable for an individual is never observed in both states of the world: treatment (participation in the ALMP programme) and non-treatment (non-participation). To illustrate this point, let the outcome of a training programme one tries to evaluate be the re-employment opportunity of an unemployed person. For an unemployed person who has participated in a training programme, the causal effect of that participation is the difference in his or her re-employment opportunity with training to his or her re-employment opportunity without training. Of course, his or her re-employment opportunity without training is counterfactual and therefore unobservable. Similarly, for a person who has not participated in a training course, the outcome had the person participated is counterfactual. For this reason, the causal effect of a programme can never be observed. In this sense, the problem of estimating the causal effects of labour market programmes is a missing data problem (Rosenbaum and Rubin, 1983).

More formally, we can define the treatment effect τ for person i as:

$$\tau_i = Y_{i1} - Y_{i0}$$

where i is an index running over the individuals in the population. Y_{i1} is the hypothetical value of the outcome variable (say unemployed or not) when person i gets treatment (*e.g.* a training course), and Y_{i0} is the hypothetical value of the outcome variable when the same person does not receive treatment. Each person has in theory both a Y_{i1} and a Y_{i0}. However, for programme participants only Y_{i1} is observable, and Y_{i0} is counterfactual. On the other hand, for non-participants only Y_{i0} is observable, and Y_{i1} is counterfactual.

3.1 The Statistical Matching Approach to the Selection Problem in Non-Experimental Studies

It is the purpose of the statistical matching method to obtain unbiased estimates of τ when working with non-experimental data. Statistical matching means to explicitly select (match) one or more control observations to each treatment observation such that both persons are as similar as possible in terms of observed characteristics. Of course, in the non-experimental context, treatment assignment $D = 1$ is not random. However, treatment assignment may be random given a set of covariates $\mathbf{z}$ (Rubin, 1977). More formally, this reads

$$\Pr \left(c_i = 1 \middle| \mathbf{z} = \mathbf{z}_i \right) = \Pr \left(c_i = 1 \middle| \mathbf{z} = \mathbf{z}_j \right) \quad \forall \mathbf{z}_i = \mathbf{z}_j$$

Rosenbaum and Rubin (1983) have shown that the dimensionality of the matching problem can be reduced to one by matching not on $\mathbf{z}$, but on

$$\Pr \left(c = 1 \middle| \mathbf{z} \right)$$

i.e. the probability of receiving treatment conditional on the characteristics $\mathbf{z}$. In case the programme participation probability is equal for two individuals, these two people can be regarded as taking part in a 'sub-experiment': if one of these two people has participated in a programme and the other one not, there is no *a priori* reason that the distributions of the outcome variables (Y_1, Y_0) should differ between the treatment and comparison persons. In other words, the probability to receive treatment *balances* the distributions of the outcomes (Y_1, Y_0) for treatment and comparison persons. It is therefore called a *balancing score* (Rosenbaum and Rubin, 1983). The principle of matching is to find for each treatment person a comparison person with equal balancing score. This way one simulates 'sub-experiments' for each treatment person. The difference in the average outcome between the treatment and matched comparison samples is therefore an unbiased estimate of the treatment effect.

The matching approach is in essence a model-free method of evaluation that stands in the statistical tradition. The traditional approach of econometricians is to first build an economic model of the outcome variable of interest and then specify on the basis of this model a relationship that can be estimated empirically. This procedure will be described in the following section.

3.2 Evaluation of ALMPs by Duration Models

As just outlined, the identification problem for causal effects arises from the fact that programme participants and non-participants may differ in other aspects besides treatment. The matching approach presented in Section 3.1 attempts to solve this problem by selecting for each member of the treatment group one person of the non-treatment group who is as similar as possible to the treatment group member in terms of observed characteristics. Likewise, regression-type methods control for differences in observed characteristics by including them as regressors in an econometric model of the outcome variable (*e.g.* employment). In addition to estimating the causal effect of a programme, the traditional econometric approach builds a whole model of the outcome variable. Programme participation is then only one of many determinants of the outcome, and may be implemented by way of a dummy variable, which takes on the value *one* if participation has occurred and *zero* if not.

The problem with this approach is that ALMP participation may be an endogenous variable so that coefficient estimates of the programme participation variable will be biased. However, Heckman and Hotz (1989) demonstrate that if all variables that drive programme participation are observed (*selection on observables*), the endogeneity problem can be addressed by including all these variables in the outcome equation.

However, there might also be *selection on unobservables*. This is equivalent to saying that some of the variables in the participation (selection) equation are unobservable, *i.e.* there is some unobserved individual heterogeneity in the selection equation. As the existence of any remaining unobserved individual heterogeneity can be tested, one can use a test on unobserved individual heterogeneity as a test for *selection on unobservables*. If no unobserved individual heterogeneity is found, then one can reject *selection on unobservables*. In the presence of unobserved individual heterogeneity, *selection on unobservables* cannot be rejected, which means that programme effect estimates may be biased, although they need not necessarily be biased. Details on the test for unobserved individual heterogeneity which will be applied in this study are described in Section 4.3.2, below.

4 Microeconometric ALMP Evaluation: Empirical Analysis for Poland

In this section, we estimate the effects of training, intervention, and public works programmes on the re-employment chances of the unemployed. As unemployment is *the* indicator of being disadvantaged in the Polish labour market, we will exclusively focus on re-employment effects and not consider any possible effects on wages. Section 4.1 describes the data source as well as our methodology to create unemployment spells from it. The empirical evaluations of Polish ALMPs are then presented in Sections 4.2 and 4.3. The two estimation methods used will be mean comparisons on the basis of matched samples on the one hand (*cf.* Lechner, 1995; 1996a; 1996b; O'Leary, 1997b; and Fitzenberger and Prey, 1998), and duration model analysis on the other (*cf.* Pannenberg, 1995; 1996; Puhani, 1996; Kraus, Puhani, and Steiner, 1997; 1998; and Staat, 1997).

4.1 Data

The best currently available data source which allows an evaluation of ALMPs on the individual level in Poland is the Polish Labour Force Survey (PLFS) together with its Supplement on Active Labour Market Policies of August 1996. There has already been a supplementary survey on ALMPs in August 1994, yet the

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information on the timing and duration of ALMPs is very sparse there and not compatible with the August 1996 survey (*cf.* Puhani and Steiner, 1996; 1997; Puhani, 1996; Kwiatkowski, Kubiak, and Kucharski, 1997). Therefore, we only use data from the August 1996 survey. The main part of the PLFS has information on socio-economic variables like age, gender, education, occupation, and industry at the time of interview or the last time of employment. Unlike the 1994 survey, the August 1996 supplement gives *retrospective monthly calendar information on a person's labour force state* from January 1992 to August 1996. This information is asked for in the following way:

Figure 1:   Retrospective Information on the Labour Force State in the Polish Labour Force Survey Supplement of August 1996

In which months of the year 1992 (1993/4/5/6) were you	01	02	03	04	05	06	07	08	09	10	11	12
Working for money	x										x	x
Registered as unemployed		x	x	x	x				x	x		
Participating in your last public training						x	x	x				
Participating in intervention works												
Participating in public works												

Note: In this example, the person has been employed (working for money), unemployed, and in a training programme in 1992. The person has obviously not participated in any intervention or public works programmes in 1992.

Source: Polish Labour Force Survey August 1996 Supplement Questionnaire; available from the Central Statistical Office (GUS) of Poland.
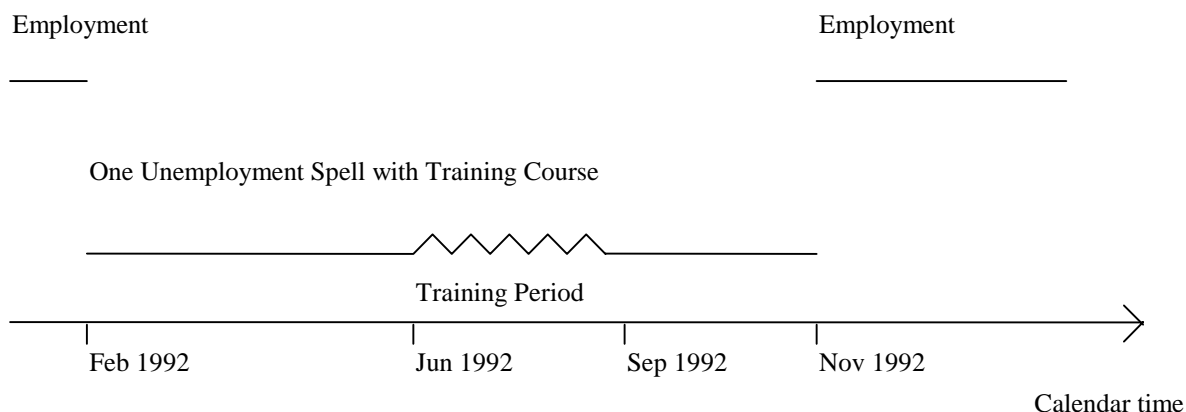

It is thus possible to observe the labour market history of each individual between January 1992 and August 1996. Employment, registered unemployment, and ALMP participation spells can hence be retrieved. However, whereas all intervention and public works spells between 1992 and 1996 can be identified from the survey, only the last publicly-financed training course is asked for. Therefore, we do not know whether somebody has been in public training more than once.

In case a person ticks more than one labour force state, we classify the person to be in the state mentioned in a lower line of Figure 1. To give an example, if somebody reports to be employed and to be in an intervention works programme, we classify that person to be in an intervention works programme. This makes sense, as an employee in an intervention works programme cannot be treated as regularly employed for our purposes. A problem arises when somebody states that he or she is both employed and unemployed. In this case, we classify that person to be unemployed on the assumption that he or she is working for less than half the minimum wage, which is officially allowed in Poland whilst being registered as

unemployed and receiving benefits (Kwiatkowski, 1995; Góra and Schmidt, 1997). Unfortunately, we do not have any further information on the type of employment to check whether that person broke the rules and was in fact regularly employed. It is also quite likely that many persons who report both employment and unemployment as labour force states for a month have in fact been in both of these states during that month. By classifying those people as unemployed we also identify short unemployment spells which last less than a month. As it occurs in less than one percent of the cases that a person reports to be both employed and unemployed, these people do not seem to embody a major problem for the empirical analysis.

The focus of this evaluation is on people who join an active labour market programme out of the state of unemployment, because the main purpose of ALMPs is to reduce the re-employability of the unemployed. Therefore, persons who receive treatment (participate in a programme) without a preceding unemployment spell are not included in the sample. As a consequence, all persons that have never been registered as unemployed cannot act as valid comparison persons and are therefore also excluded from the sample. When creating unemployment spells, the state of being in an ALMP programme is also treated as unemployment. To take the example of Figure 1, if a person becomes unemployed in February 1992, gets into a publicly-financed training scheme in June 1992, then becomes unemployed again after the scheme in September 1992 only to find a job in November 1992, we treat the whole period from February 1992 up to October 1992 as one unemployment spell. Figure 2 gives a graphical presentation.

Figure 2:   The Definition of the Unemployment Spell Corresponding to Figure 1

Employment   Employment

One Unemployment Spell with Training Course

Training Period

Feb 1992   Jun 1992   Sep 1992   Nov 1992

Calendar time

The only labour force states which terminate an unemployment spell are employment and non-participation in the labour market. A person is classified as not participating in the labour market if he or she is not in any of the labour force states mentioned in Figure 1.

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Table 2 has the number of the ALMP participants and comparison spells in our sample. As some persons experience more than one spell during the observation period, we report the number of persons and the number of spells separately. Left-censored observations do not allow modelling of process time (Blossfeld and Rohwer, 1995; Chapter 2). As the statistical treatment of left-censored observations is generally not straightforward (Hamerle, 1991), we follow the standard approach in the empirical literature and do not include these observations in the sample. The loss of observations through excluding left-censored spells lies between 20 and 30 percent. Right-censored observations will be included in the statistical analyses of the following two sections. However, because one does not know the time when the spell ends nor the state into which exit occurs, the information content of right-censored spells is much smaller than the one of completed spells.

The share of right-censored spells varies between 19 percent for men in training and 71 for women in public works. The small number of observations for women in public works makes a serious statistical analysis impossible. Nevertheless, we will also carry out an evaluation for this group in the hope that at least some tentative evidence can be obtained. Unfortunately, the number of completed spells for men in public works is also very small.

However, a very important and positive characteristic of the sample, especially for the matching approach, is the large number of comparison spells. The larger the pool of comparison spells, the more likely it is to find good matches for the treatment spells. Depending on the programme, we have 26 to 78 times as many comparison as treatment persons (not considering women in public works with 286 times as many comparison as treatment persons). The corresponding figures are lower in other studies which use the matching approach where treatment and comparison persons are drawn from the same data source. Lechner (1995; 1996a) has around 10 times more comparison than treatment persons. In Lechner (1996b), O'Leary (1997b), and Hujer, Maurer, and Wellner (1997) these ratios are around 4, 1.3, and 3.2, respectively. Our sample thus provides very good *a priori* conditions for applying the matching approach, which will be presented in the following section.

Table 2: Selection of the ALMP Participants Samples

Selection criterion	Men	Women	Total
Training			
Former or Current Participants	97	151	248
Age between 16 and 65	97	150	247
Persons / spells where unemployment precedes ALMP	67 / 67	115 / 115	182 / 182
Persons / spells not left censored	**52 / 52**	**88 / 88**	**140 / 140**
Not right-censored	42 / 42	52 / 52	94 / 94
Intervention Works			
Former or Current Participants	313	269	582
Age between 16 and 65	313	268	581
Persons / spells where unemployment precedes ALMP	267 / 269	217 / 219	484 / 488
Persons / spells not left censored	**193 / 193**	**154 / 155**	**347 / 348**
Not right-censored	74 / 74	68 / 68	142 / 142
Public Works			
Former or Current Participants	82	26	108
Age between 16 and 65	82	26	108
Persons / spells where unemployment precedes ALMP	70 / 72	20 / 20	90 / 92
Persons / spells not left censored	**49 / 50**	**14 / 14** (!)	**63 / 64**
Not right-censored	18 / 18 (!)	4 / 4 (!)	22 / 22
Non-participants (comparisons)			
Unemployed persons / spells aged between 16 and 65	3684 / 4727	4169 / 5068	7853 / 9795
Unemployed persons / spells not left censored	**3160 / 3922**	**3422 / 4010**	**6582 / 7932**
Not right-censored	2165 / 2612	2041/ 2301	4206 / 4913

Note: The bold lines refer to the sample for the empirical analyses below; non-participants have not participated in any ALMP programme.
Source: Polish Labour Force Survey; own calculations.

4.2 Evaluation of ALMPs by Way of the Statistical Matching Approach

4.2.1 Implementation Strategy

To build an estimator of the treatment effect on the treated $\tau|_{T=1}$ by matching on the programme participation probability $\Pr(T = 1|\mathbf{z}_i)$, the first step is to estimate this probability, which is unknown. To this end, a probit equation for participation in each programme will be estimated by maximum likelihood (similarly, Lechner, 1995; 1996a; 1996b; and Almus *et al.* estimate probit, Dehejia and Wahba, 1995a; 1995b; and Heckman, Ichimura, and Todd, 1997 estimate logit models).

Having obtained an estimate of the programme participation probability $\Pr(T = 1|\mathbf{z}_i)$, one can match to each treatment person a comparison person with the same estimated participation probability $\Phi(\hat{\gamma}'\mathbf{z})$ which acts as the *balancing score*.

Often, it is not possible to find a comparison person with exactly the same estimated balancing score $\Phi(\hat{\gamma}'\mathbf{z})$. In this case, one can define the *nearest neighbour* to the treatment person as that comparison person who has the closest estimated balancing score $\Phi(\hat{\gamma}'\mathbf{z})$. A related method which we will use here has been applied by Lechner (1995; 1996a; 1996b) who uses $\hat{\gamma}'\mathbf{z}$ as the balancing score.

However, we will only use the estimate $\hat{\gamma}'\mathbf{z}$ from the probit model as a *partial* balancing score. The reason is that in order to improve the comparability of treatment and matched comparison persons, we want to match *exactly* on a set of variables, which means we want some variables to take on identical values for treatment and comparison persons. These variables are the *labour force state* before the beginning of the unemployment spell and, most importantly, the *time in unemployment until somebody gets into an ALMP programme*, which is defined as

m_T = *calendar time when ALMP started – calendar time when unemployment started*

In the example of Figure 1 on page 8, m_T is equal to 4 months. In our sample, m_T ranges between 1 to 52 months, whereas the median durations m_T are around 6 months for training and 12 months for intervention and public works programmes.

We choose to condition on m_T exactly, because it is only defined for treatment persons and can therefore not be included in the cross-sectional probit estimation for programme participation. However, when matching a comparison to a treatment person, we require the comparison person to have been unemployed more than m_T months. This way we ensure that the comparison person is also comparable to the

treatment person in terms of process time in unemployment until the treatment person joined the ALMP programme. If this requirement is fulfilled, we match on the estimated *partial balancing score* $\hat{\gamma}$'**z**. The *total balancing score* is $\left(\hat{\gamma}\text{'}\mathbf{z}, m_T, E_1, E_3, E_6\right)$ The variables E_1 to E_6 and the matching algorithm in detail are described in the following.

The Matching Algorithm

*Step 1: For (formerly) unemployed people, a probit model for ALMP participation is estimated. The estimated index $\hat{\gamma}$'**z** from this probit model is in the following used as the <u>partial balancing score</u>.*

Step 2: The observations are split into two pools, a treatment (ALMP participant; $T=1$) and a comparison (non-participant; $T=0$) pool.

Step 3: The person from the treatment pool with the lowest partial balancing score is taken and removed from the treatment pool. The number of months m_T this treatment person has been unemployed until he or she started the ALMP programme is noted. Furthermore, it is noted whether 1, 3, and 6 months before the start of the unemployment spell in which the ALMP programme took place, the treatment person has been in employment or not ($E_1 = 1/0$, $E_3 = 1/0$, $E_6 = 1/0$).

Step 4: All persons are removed from the comparison pool who have not been unemployed for more than m_T months and have not been in the same labour force states (E_1, E_2, and E_6) as the treatment person 1, 3 and 6 months before the start of their unemployment spell.

Step 5: If after Step 4 no-one is left in the comparison pool, Step 4 is undone and then repeated again but without the condition that the labour force status 6 months before unemployment (E_6) is equal to the one of the treatment person. If still no one is left in the comparison pool after the application of this modified Step 4, the condition that the labour force states 3 and 1 months before unemployment (E_3 and E_1) equal those of the treatment person are also dropped one by one. The condition on the number of months in unemployment before treatment m_T is never dropped as this is not necessary with our specific sample.

Step 6: For the treatment person, those persons from the comparison pool selected under Steps 4 and 5 are found who have the partial balancing score with the smallest difference $\left(\hat{\gamma}\text{'}\mathbf{z}_{T=1} - \hat{\gamma}\text{'}\mathbf{z}_{T=0}\right)$ to the partial balancing score of the treatment person. If the number of comparison persons selected this way

exceeds one, one person is randomly drawn from the selected comparison persons. This person is then taken as the matched comparison person and removed from the comparison pool.

Step 7: The removals from the comparison pool undertaken under Steps 4 and 5 are undone.

Step 8: Steps 3 to 7 are repeated until the treatment pool is empty. Then all treatment observations have exactly one partner from the comparison pool matched to them.

The quality of the programme effect estimate rests both on the quality of the programme participation estimate and the ability to find comparison persons with equal participation probabilities as the treatment persons. In order to ensure that the best possible estimate of the programme participation probability $\Pr(T = 1 | \mathbf{z})$ is obtained, the probit model should include all observed variables $\mathbf{z}$ that one may think to influence programme participation. Indeed, probit estimates are inconsistent if any relevant variables are omitted from estimation, even if the omitted variables are uncorrelated with the error term (Greene, 1997; Chapter 19). The estimates of the balancing scores can be found in Puhani (1998). Sample means of treatments, naive and matched comparison groups are given in Table 5, Table 6 and Table 7 in the appendix to this paper. Here we concentrate on the presentation of the evaluation results.

4.2.2 Non-Parametric Programme Evaluation Based on Matched Samples

Whereas it is straightforward to define pre- and post-treatment periods for treatment persons, it is *a priori* unclear how one should set the time scale for the comparisons. Indeed, there is by definition no point in time for the comparison persons when they join an ALMP programme. However, we have matched on the precondition that the comparison person has to have been unemployed more than m_T months, where m_T is the number of months it took the unemployed treatment person to get into the ALMP programme. We can thus define time *zero* for the comparison person at his or her $(m_T + 1)^{th}$ month in unemployment. This definition is very useful, as it guarantees that at time *zero*, the treatment and matched comparison persons have a similar recent unemployment history. The treatment effect starts to work for the treated when they join the ALMP programme. It is an obvious choice to compare the comparison person with the treatment person from process time $(m_T + 1)$ onwards and interpret the difference in unemployment rates after process time $(m_T + 1)$ (defined as time *zero* in Figure 3, Figure 4, and Figure 5 below) as the causal effect of the ALMP programme. Lechner (1995; 1996a; 1996b) and Fitzenberger and Prey (1998) use a similar approach in their papers.

Pre-Treatment Tests

To check whether treatment and comparison groups are really comparable, we compare *pre-treatment outcomes* between treatment and matched comparison persons (Heckman and Hotz, 1989). For a comparison of the sample means of main characteristics, see Puhani (1988). Estimated pre- and post-treatment differences in unemployment rates between treatment and matched comparison groups are plotted in Figure 3, Figure 4, and Figure 5. More formally, the estimated difference between the unemployment rates of treatments and comparisons at time t can be written

$$\Delta \hat{U}R_t = \frac{1}{N_{T=1}} \sum_{\forall i|T=1,\mathbf{z}} Y_{it} - \frac{1}{N_{T=0}} \sum_{\forall i|T=0,\mathbf{z}} Y_{it} = \overline{Y}_{t|T=1,\mathbf{z}} - \overline{Y}_{t|T=0,\mathbf{z}}$$

where Y_{it} is either *zero* (employed) or *one* (unemployed) for person i at time t. If somebody is not in the labour force at time t he or she makes no contribution to the unemployment rate at that time. In order to test whether there are any pre-treatment differences in the unemployment rates of treatments and comparisons, we calculate standard errors for $\Delta \hat{U}R_t$

$$\hat{\sigma}\left(\Delta \hat{U}R_t\right) = \sqrt{\frac{1}{\left(N_{T=1}-1\right)N_{T=1}} \sum_{\forall i|T=1,\mathbf{z}} \left(Y_{it} - \overline{Y}_{t|T=1,\mathbf{z}}\right)^2 + \frac{1}{\left(N_{T=0}-1\right)N_{T=0}} \sum_{\forall i|T=0,\mathbf{z}} \left(Y_{it} - \overline{Y}_{t|T=0,\mathbf{z}}\right)^2}$$

(given random assignment conditional on the covariate $\mathbf{z}$, $\overline{Y}_{t|T=1,\mathbf{z}}$ and $\overline{Y}_{t|T=0,\mathbf{z}}$ are independent random variables). Using the central limit theorem, we approximate the distribution of $\Delta \hat{U}R_t$ by the normal distribution and build a 90 percent confidence interval for $\Delta \hat{U}R_t$ which is given by

$$\Delta \hat{U}R_t \pm 1.645 \times \hat{\sigma}\left(\Delta \hat{U}R_t\right)$$

As Figure 3, Figure 4, and Figure 5 below show, the pre-treatment unemployment rates are in no case significantly different from each other (the results for women in public works are only displayed for comparative purposes in Figure 5). We therefore conclude from this evidence together with the results from comparisons of the means in key characteristics (see Puhani, 1998, for details) that the matching method has worked well in producing an adequate comparison group for the treatment group. Therefore, we interpret the post-treatment differences in unemployment rates of treatment and matched comparison persons as the causal effect of treatment on unemployment. More formally,

$$\hat{\tau}_{t,t>0|T=1} = \Delta \hat{U}R_{t,t>0}$$

is interpreted as the treatment effect on the treated (ALMP participants) t months after the beginning of the ALMP programme.

This approach also accounts for time-invariant unobserved individual heterogeneity if one gives it a difference-in-differences interpretation. The difference-in-differences interpretation generally requires the assumption that

$$E\left(Y_{0it}|T_i=1\right) - E\left(Y_{0it}|T_i=0\right) = E\left(Y_{0it-1}|T_i=1\right) - E\left(Y_{0it-1}|T_i=0\right)$$

where Y_{i1} and Y_{i0} are defined as in Section 3 above. In the matching context, the required assumption is less strong, namely

$$E\left(Y_{0it}|T_i=1,\mathbf{z}\right) - E\left(Y_{0it}|T_i=0,\mathbf{z}\right) = E\left(Y_{0it-1}|T_i=1,\mathbf{z}\right) - E\left(Y_{0it-1}|T_i=0,\mathbf{z}\right)$$

(Heckman, Ichimura, and Todd, 1997). The Heckman and Hotz (1989) pre-treatment test checks whether the right-hand side of this assumption is zero, *i.e.*

$$E\left(Y_{0it-1}|T_i=1,\mathbf{z}\right) - E\left(Y_{0it-1}|T_i=0,\mathbf{z}\right) = 0.$$

If this is the case, the difference-in-differences estimator is equal to $\hat{\tau}_{t,t>0|T=1} = \Delta\hat{U}R_{t,t>0}$, which means that simple post-treatment unemployment differences between treatments and comparisons can be interpreted as causal programme effects.
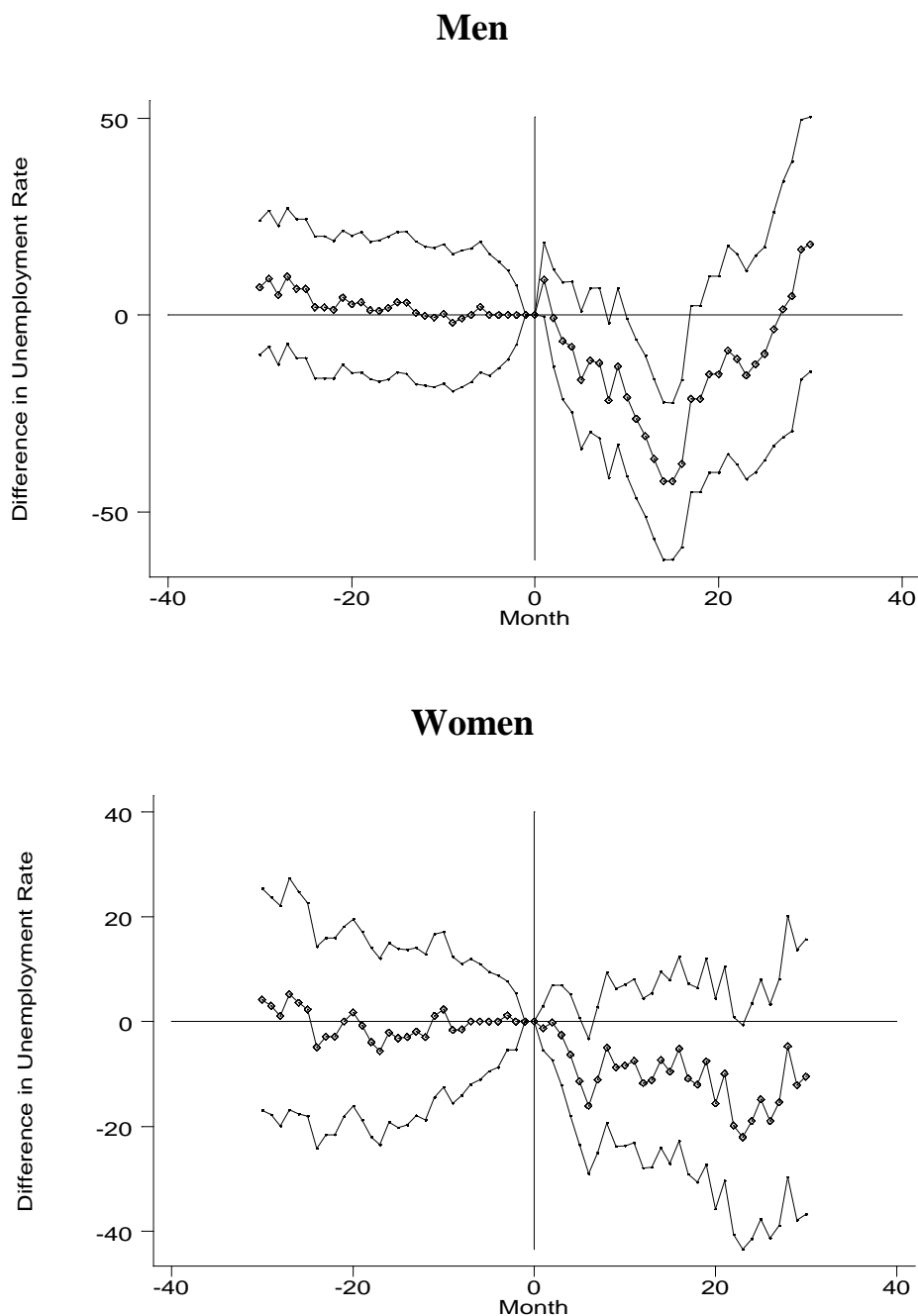

Evaluation Results

The point estimates from Figure 3, Figure 4, and Figure 5 below give a rather clear picture on the effects of the ALMP programmes investigated. Whereas the figures show that training programmes improve the re-employment chances of the trainees, intervention and public works programmes seem to have negative effects on employment opportunities. However, not all of these effects are statistically significant. Because the number of observations (and hence the quality of the approximation through the normal distribution) shrinks the further we move away from time *zero*, we will in the following only interpret the results up to the 20th month after the beginning of the ALMP programme.

Figure 3 shows that the positive effect of training for men is statistically significant at the 10 percent level for most of the post-treatment time period. This is not so for women, where the positive effect of training is only significant for one month. The point estimates, however, suggest that training reduces the unemployment rate of the female programme participants by about 10 percentage points. For men, the estimate is more erratic over time, but a 10-15 percentage point reduction in unemployment seems to be a conservative average number.

For intervention and public works, the estimated effects are generally significant at the 10 percent level (women in public works are the exception). Men after

intervention or public works have unemployment rates about 30 percentage points higher than the comparison group. For women, the unemployment rate after intervention works is about 10 percentage points higher in the first 20 months after the start of the programme. The results for women after public works, which are only presented for comparative purposes and cannot be seriously interpreted, suggest a similar effect as for women in intervention works.

Figure 3: **Difference in Unemployment Rates between Trainees and the Matched Comparison Group (with 90 Percent Confidence Bands)**

Men

Women

Notes: negative (positive) months correspond to the pre- (post-)treatment period;

number of observations N (at month t) for men (T: treatment / C: comparison): N(-30) T/C = 33 / 29; N(-15) T/C = 41 / 39; N(0) T/C = 52 / 52; N(15) T/C = 27 / 28; N(30) T/C = 9 / 13; number of observations for women: N(-30) T/C = 22 / 26; N(-15) T/C = 48 / 47; N(0) T/C = 88 / 88; N(15) T/C = 47 / 42; N(30) T/C = 19 / 19.

Source: Polish Labour Force Survey; own calculations.

Figure 4: **Difference in Unemployment Rates between Intervention Works Participants and the Matched Comparison Group (with 90 Percent Confidence Bands)**

Men