

***Some Econometric Evidence on the Effectiveness of Active  
Labour Market Programmes in East Germany***

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# Some Econometric Evidence on the Effectiveness of Active Labour Market Programmes in East Germany

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**Comments welcome**

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## **Abstract**

In this paper we summarise our previous results on the effectiveness of different kinds of labour market training programmes as well as employment programmes in East Germany after unification. All the studies use the microeconomic evaluation approach and are based on different types of matching estimators. We find some positive earnings effect for on-the-job training and also some positive employment effects for employment programmes. No such effects appear for public sector sponsored (off-the-job) training programmes. Generally, the scope of such analysis is very much hampered by the insufficient quality and quantity of the data available for East Germany. Although in particular the results for public sector sponsored training programmes raise serious doubts about the effectiveness of these programmes, any definite policy conclusion from this and other studies about active labour market policy in East Germany would probably be premature.

**Keywords:** East German Labour markets, treatment effects, training, panel data, matching

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## 1 Introduction

After unification the East German economy changed quite dramatically. On the labour market under- and unemployment rose. Programmes of active labour market policy (ALMP) were set up to adjust the skills of the East German labour force as well as to combat unemployment directly. Quickly a huge number of people participated in these programmes, but at the same time ALMP absorbed considerable amounts of resources. In some years the *Bundesanstalt für Arbeit*, the German labour offices, alone spent more than 30 billion DM on ALMP in East Germany. In addition to that the private sector also spent considerably on on-the-job training.

In this paper we summarise our evaluation results for public employment programmes (Eichler, Lechner, 1998), public-sector sponsored training programmes (Lechner, 2000) as well as for on-the-job training not sponsored by the public sector (Lechner, 1999a). Let us consider the different parts in turn.

Public employment programmes (*Arbeitsbeschaffungsmassnahmen*, PEP) are an important part of ALMP. PEP create additional jobs in the public or private sector through a wage subsidy paid by the labour office. More than two million people participated in some kind of PEP between 1990 and 1998. The main goal of PEP is the reduction of unemployment, directly as well as by improving employment chances of PEP participants on the regular labour market, by providing work experience and informal as well as some formal training.

Public sponsored training is another part of ALMP. Here we focus on continuous training and retraining (*berufliche Fortbildung und Umschulung*, CTRT). The hope was CTRT could be used to quickly adapt the qualification of a labour force that was considered well trained by COMECON standards, so to smooth the transition considerably. Here as well more than two million participated in CTRT between 1990 and 1998.

The third 'programme' considered here is on-the-job training (OJT). This should not be understood in the way that the form of training is 'on the job', but that the training is related to a current employer and that the employer bears (at least part of) the costs, regardless of the form or the provider of training. Although knowledge about OJT is less complete, there is no doubt that it is important. In the early nineties about a quarter of the employed persons participated in some kind of OJT every year.

Given the considerable expenditures for these efforts it is obvious that there is scope for an evaluation of the effects of these programmes. Here we concentrate on the micro effects, i.e. the individual effects in terms of labour market outcomes for the participants in these programmes. Finding positive effects on the individual level appears like a necessary condition for overall positive effects, but it is of course not sufficient. Displacement effects could over-compensate the positive effects, but this is beyond the scope of this paper.

Since experimental data on the programmes are not available, every microeconomic evaluation faces the problem of selection bias due to a correlation of individual programme participation with the outcomes under investigation. Without assumptions the effects of the programmes cannot be disentangled from the effects of selective participation. For the training evaluations the empirical work is based on an informative panel data set – the German Socio Economic Panel (GSOEP). The informative data together with plausible ‘exogeneity’ assumptions derived from the specific structure of the German unification process leads to the identification of the programme effects. To be specific, the key point is that, conditional on a rich set of observable factors including for example the individual employment histories on a monthly basis, participation in the programmes is a random event (conditional independence assumption, CIA).

Unfortunately, the GSOEP is not very suitable for studying PEP. One alternative data available is the *Arbeitsmarktmonitor Sachsen-Anhalt* (AMM-SA), a panel based on the working age population of the state of Sachsen-Anhalt (only). Although the data has several drawbacks, it is in our opinion currently the best data available for evaluating PEP in (a part of) East Germany. Nevertheless, the data is less informative than the GSOEP in several key respects, so that the above mentioned CIA approach cannot be used directly. Heckman, Ichimura and Todd (1997) propose an alternative that is attractive in our case. The idea is to use comparisons between participants and selected groups of non-participants as well as differencing over time to eliminate selection bias. Because in our case individuals start PEP at different dates, we extend their approach and combine it with a particular matching approach suggested by Lechner (1999b).

Of course there are many alternative ways to identify the effects of programme participation or more generally of ‘treatments’ (see for example the surveys by Angrist and Krueger, 1999, and Heckman, LaLonde and Smith, 1999). Some of them are used for East Germany as well. The advantage of assumptions like CIA that do not involve parametric function form assumptions of conditional expectations or distributional assumptions for error terms is that checking their valid-

ity is much more straightforward. Parametric assumptions are usually difficult to justify by economic reasoning and are often difficult to communicate to non-econometricians. In addition Ashenfelter and Card (1985) and LaLonde (1986) - among others - find that the results are highly sensitive to different (plausible) stochastic assumptions made about the selection process.

When identification is achieved by nonparametric assumptions, it appears to be 'natural' that estimation is also conducted nonparametrically. Matching methods (e.g. Rubin, 1979, Rosenbaum and Rubin, 1983, 1985) have received renewed attention in the literature as a non-parametric estimator useful for evaluation studies (e.g. Dehejia and Wahba, 2000, Heckman, Ichimura and Todd, 1998). It closely resembles the typical estimator used in ideal social experiments: The difference between the mean of the outcome variable in the treatment group and the mean in the comparison group. The comparison group consists typically of individuals who applied for the programme but who are randomly denied participation. Therefore, their only systematic difference compared to the participants is their participation status. Prototypical matching estimators mirror this approach by choosing a comparison group from all non-participants such that this group is - in the ideal case - identical to the treatment group with respect to the variables used in the particular formulation of the CIA. This simple idea can be combined by matching before and after the programmes. The assumption involves that there is no effect of the programme before actually participating. Therefore, any estimated effect of 'programme participation' for a date before the participation took place is considered an estimate of the bias and subtracted from the estimated effect after the programme.

We find some positive earnings effect for on-the-job training and also some positive employment effects for employment programmes. No such effects appear for public sector sponsored (off-the-job) training programmes. Generally, the scope of such analysis is very much hampered by the insufficient quality and quantity of the data available for East Germany. Although in particular the results for public sector sponsored training programmes raise serious doubts about the effectiveness of these programmes, any definite policy conclusion from this and other studies about active labour market policies in East Germany would probably be premature.

This paper is organised as follows: The next section outlines basic features of the East German labour market after unification. Section 3 provides information on the institutional regulations for the evaluated programmes and Section 4 introduces the data used in these studies. Econometric

issues and the empirical implementation are discussed in Section 5. The subsections of Section 6 contain the evaluation results for OJT, CTRT and PEP respectively. Section 7 concludes.

## **2 Labour market and active labour market policy in East Germany**

The centrally planned economy of the GDR was not prepared for the unification in 1990. The institutional settings of the West German market economy, the relative prices and the international competition came as a shock. The GDP per capita, already far below that of West Germany, dropped sharply following the Economic, Monetary and Social Union in July 1990.<sup>1</sup> In 1991 it was about a quarter of the West German one. From 1991 to 1994 the GDP rose with an annual rate of 6-8% and GDP per capita reached approximately 50% of the West German level in 1994. At the same time labour productivity increased from 31% to 51% of the West German level. However, this was offset by the development of wages rising from 48% of the West German level in 1991 to 73% in 1994. This led to economic disequilibria, especially on the labour market.

Figure 1 illustrates the labour market developments. It presents the size of different labour market states and ALMP in relation to the total labour force of 1989. The East German regular (unsubsidised) employment dropped from 9.7 million in 1989 to 5.6 million in 1992 and remained around that level ever since. Those people who lost regular employment either went into that part of the labour market dependent on ALMP, became unemployed, or left the labour force.<sup>2</sup> After unification the majority of people not working was absorbed by ALMP. In 1993 and 1994 the shares of people who went into ALMP, unemployment or out of the labour force were roughly of the same magnitude. In 1992 more than 30% of the number of people working in 1989 were subsidised in one way or another by the labour offices. This share declines but rather slowly, e.g. in 1994 still more than 20% were subsidised. Compared to currently working people the respective shares are close to 40% in 1992 and 30% in 1994.

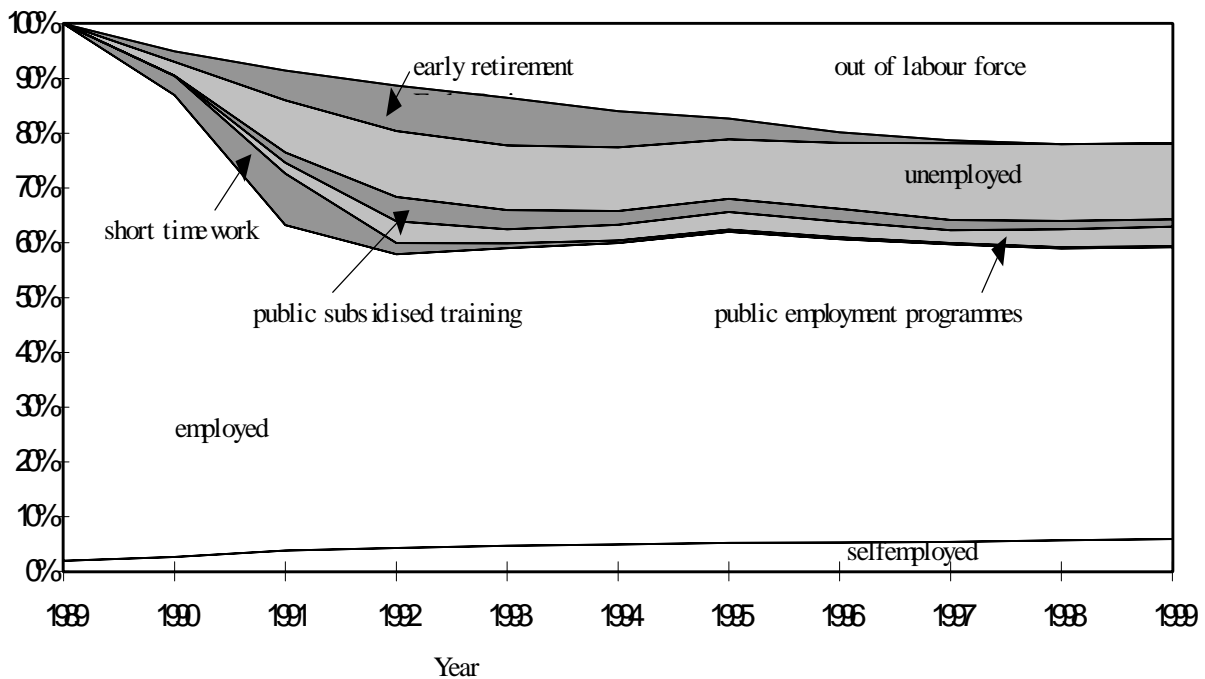
Two major explanations were given for the fast rising under- and unemployment in East Germany after unification. First, there was a decreasing total demand for labour. Second, a huge part of the human capital accumulated by the labour force of the previous GDR was losing its value, while essential capabilities for the new economic situation were missing. ALMP as well as other

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<sup>1</sup> The GDP figures given in this section are contained in Statistisches Bundesamt (1992, 1995), the data on productivity and wages in Bundesministerium für Wirtschaft (1995).

public and private strategies were initiated to counteract this development. Obviously, training was the first and the most important strategy to reduce the qualification mismatch. That includes a wide variety of kinds and contents of training, like 'learning by doing' in a new job, few hours or days 'introduction to PC working' and two years training for a new profession. The same variety can be found in organisation and financing: public, by firms, individually or any combination. Short time work, wage subsidies and public employment programmes are supposed to provide further qualification as well, either explicitly with training parts included or implicitly through work experience. But their major goal was to reduce unemployment directly through the increase in labour demand. Early retirement programmes had the same goal, but by decreasing labour supply.

Figure 1: Development of the East German labour market since unification



Source: IAB (1995), Figure 7.2 C, Figure 2.4.1, phone and fax information from IAB, own calculations.

Note: 100% is equal to 9.7 million people in the active labour force in 1989. 'Employment' is corrected for employment programmes and short time work to avoid double counting. All labour market policies and unemployment are assumed to be zero in 1989. All numbers are average participation throughout the year and full time equivalents. Other ALMPs are excluded because of low participation.

The target of our evaluations are individual gains from participation on labour market outcomes like employment or income after the end of the programme. Therefore, the summery of labour

<sup>2</sup> These are macro data. Of course nothing is said about the individual patterns here.

market measures below focuses on the most important programmes in this respect, i.e. on-the-job training (OJT), public sponsored training (continuous training and retraining, CTRT) and public employment programmes (PEP).

OJT is a very heterogeneous kind of training. It comprises a few hours introduction to a new workplace as well as several months of formal training with an external provider. There is no straightforward definition on what is part of OJT and what is not. It must be related to a current employer, and we use the fact that an employer bears (at least part of) the costs of training as the indicator for OJT, which means here that training took place during working time. It has to be mentioned that in East Germany not all OJT must be related to the participant's (future) job, as would be expected. At the time in questions there were still a lot of GDR firms not privatised. If kept alive by subsidies one of their purposes was to provide training to their employees regardless whether it was expected useful to the firm or not. Training in this case might be quite different from other firm related training.

The other important way to provide training to the East German labour force was training subsidised by the labour offices. In this case, it is the labour office who bears (most of) the cost of training, the direct cost as well as the cost of living during that time. The training separates into four broad categories. First, continuous training (CT) in addition to an occupation someone already holds. Second, retraining for a new occupation (RT), typically more intensive and longer, often ending with an examination of the German apprenticeship system. Third, very short courses to find out about the abilities and interests of the participant, to inform about the subsidies available from the labour office and/or to provide basic job search skills (SHORT). Although many persons participated in this kind of training it is less important with respect to expenditures. Furthermore, the special regulations providing short courses were abolished in 1993. Fourth, the labour office subsidised the introduction to new workplaces. Because this is related to a specific workplace for the individual it is more like a wage subsidy, and the effect might be different from the other kinds of public sponsored training. Therefore, we do not include this in the evaluation of CTRT.

A second large publicly financed programme is PEP. The idea is to create additional jobs by providing a wage subsidy to an employer, either a public employer, a non-profit organisation or – in some cases – a private firm targeted at previously unemployed or at people who might become unemployed. In East Germany the labour offices often carried a very substantial part of the wage,



up to 100%, and in addition some of the set-up costs for the workplace. Apart from the direct reduction of unemployment and the 'social' motivation of income substitution, PEP is also expected to provide some kind of 'training on the job' and 'work experience' to the participants. Furthermore, sometimes formal training parts were included in PEP.

Especially in the years 1991 and 1992 short time work (STW) was an important instrument of ALMP.<sup>3</sup> Not in accordance with the 'temporary shortage' definition of the original regulation, STW in East Germany was also used when the remaining working time was zero and a later lay off was obvious or already scheduled. Generally, we do not include STW in the evaluation for several reasons: It was used only for a brief period, it was very heterogeneous, the purpose of the STW is different, and the necessary information is not available. The exception is training during STW. Often training took place during STW with zero (!) working hours, which was comparable with CTRT. Therefore, it is included in the evaluation.

The last of the major instruments of ALMP to be mentioned are early retirement schemes. But since they do not have any effect on the labour market outcomes other than removing people from the labour force, they are excluded from this evaluation study.

Table 1 provides an overview on the participation in the programmes. Unfortunately, not much consistent and complete information is available for OJT. But even using only the little information available it is clear that OJT provides an important part of training received by the East German labour force. The numbers presented in Table 1 show that in every year from 1990 to 1994 between one and a quarter and two and a quarter of a million people participated in OJT, which is roughly equal to 15% to 25% of all people employed in November 1990. 1990 about 30% of employees participated in OJT within the last 12 months, 8% of them in workplace introduction, 11% in training courses within the firm and 11% in training outside the firm. In 1992 the total rate decreased to 25%, but with an increasing share of external training (4%, 7%, and 14%, respectively). Wahse and Schaefer (1993) have found a comparable amount of OJT in a sample of firms already existing in the former GDR independent on whether the firm was already privatised or not. For 1992 iwd (1994a) reports that for every 100 employees there were 50 participants in OJT.<sup>4</sup> On the one hand this is lower than the corresponding number for West

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<sup>3</sup> In case of a temporary reduction of output a company can introduce STW. Employees work a reduced number of hours. The resulting income loss is partially made up by payments from the labour office.

<sup>4</sup> On employee can participate in more than one OJT. Therefore, this is not equal to every second employee participating in OJT.

Germany, but on the other hand a participant in East Germany spends an average of 96 hours in training, nearly four times as long as in West Germany.

*Table 1: Participants starting a training programme or a PEP in East Germany*

	1990	1991	1992	1993	1994	1995	1996	1997	1998
OJT <sup>a)</sup>	2244	1546	1765	1712	1343	n.a.	n.a.	n.a.	n.a.
Public spon. training	94 <sup>b)</sup>	892	888	294	287	257	269	166	236
thereof: women <sup>c)</sup> (%)	n.a.	60	66	62	67	70	66	59	n.a.
CT	n.a.	443	462	182	199	184	204	128	n.a.
RT	n.a.	130	183	81	69	53	48	27	n.a.
short	n.a.	187	129	---	---	---	---	---	---
workplace	n.a.	133	113	31	19	20	17	11	n.a.
PEP	n.a.	422	296	313	356	279	283	243	528
thereof women <sup>c)</sup> (%)	n.a.	37	44	41	56	58	58	53	52

Sources: ANBA (1992-9) and own estimates based on Hübler (1998), Table 1.

Notes: In thousand persons; n.a.: not available. OJT: On-the-job training; Public Spon. Training: Training sponsored by the *Bundesanstalt für Arbeit*; CT: Continuous training; RT: Retraining; Short: Short courses according to §41a AFG, not possible from 1993 on; Workplace: Subsidy during introduction to a new workplace; PEP: Public employment programmes.

a) Estimated based on results from Hübler (1998), Table 1. Numbers should be somewhat downward biased, because only people employed in November 1990 are included. b) Sep. to Dec. only. c) Share of women in %.

Judged by the number of participants public sector sponsored training was less important, but nearly 0.9 million individuals starting training in the peak years 1991 and 1992 is still a substantial share of the labour force.<sup>5</sup> Measured by average training duration as well as intensity, it is even more important than OJT. For 1990 the average duration is above six months and it is increasing over time. Most of the courses are full time. Observing the development of participation after the massive build-up in 1990/1991, the first feature to notice is a substantial drop from 1992 to 1993 followed by more or less stable participation, with a downward exception in 1997. Yearly participation after 1992 is around 3.7% of the labour force or between 10% and 20% of the unemployed (including participants in ALMP). Because introduction at a new workplace was not very important from 1993 on, the figures for CTRT are about similar. For PEP participation is also substantial, but without a clear pattern over time. Between 13% and 20% of the unemployed took part in a PEP, which is equal to 3% to 5% of the labour force. Altogether there were more than two million entries in a PEP in East Germany after unification.

Expenditures once again highlight the importance of the programmes. For OJT there are again no complete statistics. For 1992 iwd (1994b) reports expenditures of 3.5 billion DM on OJT in East

Germany. Although being substantial, they are small compared to the expenditure of the employment office for training of more than 11 billion DM. This again points to the fact that public sponsored training was much more intensive than OJT. Table 2 shows the development of expenditures for different public labour market programmes.

Table 2: Expenditures of the labour offices in East Germany (*Bundesanstalt für Arbeit*)

Expenditures	1990	1991	1992	1993	1994	1995	1996
Public Spon. Training	181	4735	11281	10396	7032	7314	7234
PEP	56	3075	7784	8900	8137	8230	8155
STW	1171	10006	2653	919	499	424	435
Early Retirement	11	2678	9312	13410	9006	2214	133
total	1419	20494	31030	33625	24674	18182	15957

Sources: ANBA (1992-9).

Note: In million DM. Public Spon. Training: Training sponsored by the *Bundesanstalt für Arbeit*, including CT (continuous training), RT (retraining), short courses according to §41a AFG, and subsidy during introduction to a new workplace; includes course expenditures and living subsidies (*Unterhaltsgeld*). PEP: Public employment programmes. STW: Short time work.

### 3 Institutional regulations

The following sections provides some insight in the regulations and the participation decision process of the programmes evaluated.<sup>6</sup> An understanding of these processes is necessary to assess the identifying assumptions.

During our evaluation periods all public sponsored labour market programmes were regulated by the *Arbeitsförderungsgesetz* (AFG).<sup>7</sup> Public sponsored training as discussed here relates to continuous training and retraining (CTRT, *berufliche Fortbildung und Umschulung*) regulated mainly in §§33-49 AFG. The public employment programmes (PEP) contain regular *Arbeits-*

<sup>5</sup> This and the following number are own calculations based on several issues of the ANBA (1992-9).

<sup>6</sup> Apart from formal sources this section relies on informal sources as well to catch effects of the daily implementation and the use of exceptions. The major informal source were interviews we conducted in East German labour offices in 1996 and 1997. The interviews were partly questionnaire-led, partly free conversation held in local labour offices and the *Landesarbeitsämter*, the regional head offices. For more information on the public sponsored programmes see the AFG, the *Sozialgesetzbuch III*, and corresponding regulations on lower levels.

<sup>7</sup> In early 1990 the German Democratic Republic (GDR) adopted a labour law by and large similar to the West German one. After unification the West German AFG came into force in East Germany as well, although many exemptions for East Germany were added. With a vast number of changes, notably major ones early in 1993 and in January 1994, this law was in force until 1997. It has then be integrated in the German social laws, after an extended revision now the *Sozialgesetzbuch III*. All programmes subject of evaluation in this paper followed the regulations of the AFG. Therefore, the discussion is limited to the regulations contained in the AFG.

*beschaffungsmassnahmen* (§§91-96 AFG) and *Produktive Lohnkostenzuschüsse Ost* (§249h AFG), that were introduced in January 1993.

It should be mentioned that for East Germany the AFG allowed a lot of exceptions and contained several special regulations. A complete discussion is far beyond this paper which only provides a summary on the most important regulations. Therefore, even if a person did not fulfil the conditions stated below, he/she might nevertheless have participated in a programme. Especially if the labour office considered it necessary to reach 'social' or 'labour market policy' goals.

### **3.1 On-the-job training in East Germany**

Again, only 'anecdotal' information is available for OJT. Because OJT is especially heterogeneous and in general no formal rules for participation exist, we can only identify some variables possibly influencing the participation. Several important kinds of OJT can be separated:<sup>8</sup> Introduction to a new workplace, patent counselling and job rotation, training courses either internal or with an external provider, informal worker exchange and self-teaching programmes. For the individual participation decision the job position and the human capital already accumulated are probably important determinants. Some confirmation is provided by Wahse and Schaefer (1993), who have found a higher participation for persons with higher education / job position (the results are not based on a random sample of East German firms). Furthermore, they have found that participation rates increase with firm size.

### **3.2 Public subsidised training in East Germany**

When discussing the participation process in CTRT one has to separate between the design of a training courses and the individual participation decision. In Germany, in most cases private training institutions provide CTRT. The labour office approves the courses if they are considered cost efficient, of sufficient quality and could be expected to help reaching the goals of the labour offices, namely reducing unemployment. In many cases the private training supplier designs a course, subject to later approval by the labour office. In some cases the labour office requested a certain kind of course for already chosen participants. Courses may offer an officially accepted degree, but must not.

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<sup>8</sup> Compare iwd (1993).

The initiative to take part in CTRT and the selection of a certain course regulations offer no clear guidelines of who is responsible for it. It is possible that the individual wants a specific type of training, gathers the information about the possibilities, selects a course and finally applies at the labour office for support. In other cases, the suggestion might come from the case worker in the labour office. In practise, it is usually a mixture of both.

Before the labour office subsidises an individual to participate in a specific training course several conditions related to the individual and to the contents of the course must be met. The labour office checks if the training is 'necessary' to bring someone unemployed back into work or to avoid unemployment of somebody directly threatened by it. Until 1995 in East Germany a general threat of unemployment was sufficient. Some case workers interpreted that like 'in East Germany everybody was threatened'. Until January 1994, individuals had a *right* to a subsidy if training was necessary.

To participate in CTRT an individual must already hold a profession, otherwise only participation in training for a first profession is possible, a programme not discussed here. To hold a profession it is necessary to have either three years of work experience or a degree of the German apprenticeship training system. Furthermore, from January 1994 on there is an mandatory waiting period of one year after CTRT before the possibility of participating again. Before receiving subsidies a participant must have contributed to unemployment insurance for at least two years within the last three years,<sup>9</sup> but this period can be extended considerably for a variety of reasons.

In addition to the formal 'before' conditions, the case worker must expect the participant to successfully finish the specific training course, to search for employment with mandatory unemployment insurance and to improve the employment chances in the appropriate labour market considerably. In the interviews it turned out that one of the most important indicators for the case workers apart from their personal impression and experience was the individual motivation and activity towards training.

During participation in CTRT the participant can get one or both of two kinds of subsidies. One is an earnings replacement. From 1990 to 1993 it was higher than unemployment benefits, afterwards it was the same as unemployment benefits. The second payment covers part or all of the direct costs of training, among others including course fees, material and travel expenses. During

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<sup>9</sup> Employment in the GDR is treated like a period of contributions if it would have resulted in mandatory unemployment insurance in the West German system.

training the individual as well as the labour office is required to continue job search, but training should only be left if the potential job promises permanent employment. However, it is expected and confirmed by the interviews, that search activity during training is much lower. The duration of CTRT is between two weeks to two years, but exceptions are possible. From March 1993 on the duration of continuous training is limited to one year, again exceptions possible.

Finally, it has to be stressed that a lot of sources provide hints on huge changes in the quality of training during the period in question. That includes the quality of the training providers, the ability for the labour offices to check the provider quality, knowledge about the usefulness of qualifications on the labour market, improved individual counselling, and last not least the experience and smoothing of the process after the build up of the labour offices was finished and the inflow of new persons searching for help decreased. Although all this was a steady process, most sources point to 1993 or early 1994 as 'turning points'.

### **3.3 Public employment programmes in East Germany**

The first step of the participation process in a PEP is the creation of a specific programme. A programme supporting employer (PSE), who in East Germany might be a public employer, a non-profit organisation or a firm, applies at a labour office which has to approve the application. Especially in the early nineties the process in East Germany was often different and the labour offices have been much more involved. To avoid mass layoffs when closing a company or a substantial part of it, the closing part was sometimes completely transferred in a PEP. Another way was the use of *Arbeitsbeschaffungs- und Beschäftigungsgesellschaften* (ABG),<sup>10</sup> organisations set up by a regional or local government or a large company as a kind of 'counterpart' to the labour offices' active labour market policies. Their specific purpose was to create, organise and administer ALMP including PEP and CTRT as well as to provide information and support for participants. PEPs created in different ways might differ substantially with respect to the individual participation process as well as with respect to the effects of participation, but unfortunately there is neither - according to our knowledge - any complete statistics available on PEP founding, nor does our data include any information.

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<sup>10</sup> This kind of companies appear under many different names.

After the creation of a PEP it is the labour office that decides who gets the job.<sup>11</sup> Formally, the most important requirement is that an individual is unemployed (at least 6 of the last 12 months) and entitled to some kind of unemployment payments (or since 1994 social assistance) right before PEP participation.<sup>12</sup> Individuals facing special difficulties on the labour market are preferred. Those are the disabled, the unemployed older than fifty, persons below twenty-five without a vocational degree, the long term unemployed and - a special regulation for East Germany - women. Finally, it is important to notice again that labour offices can depart from all formal requirements for social or labour market reasons.

Typical length of a PEP is one year, the maximum allowed (including extension) twenty-four months.<sup>13</sup> During the PEP the participant has a regular employment contract with the PSE and gets paid by the PSE. The labour office reimburse the PSE for part or all of the wage bill and might as well subsidise workplace set-up costs. The wage is equal to the appropriate *Tariflohn* (90% of it after 1994), which is typically higher than unemployment benefits. Even after 1994 this also exceeds quite often the earnings of a comparable job in the private sector. Furthermore, because participation in PEP is treated as regular employment with respect to social security, it might extend the entitlement to unemployment benefits. If an individual – or the labour office – finds a suitable job offer or training programme during the PEP the wage subsidy will end immediately, regardless of the individual leaving the PEP employment or not. The labour office as well as the participant is required to search for such possibilities during PEP.

## **4 Data and descriptive statistics**

### **4.1 Availability of data**

Several data sets are available for a microeconomic evaluation of ALMP in East Germany. Two of the analysis discussed later use the German Socio-Economic Panel (GSOEP), which is very similar to the US Panel Study of Income Dynamics (PSID).<sup>14</sup> The East German part of the

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<sup>11</sup> Labour offices' staff confirmed in the interviews that neither the employer nor the individual participant has any influence on the participation decision, although the above discussion might cause some doubts, at least for some cases.

<sup>12</sup> §249h regulations are somewhat less tight, especially having participated in a regular PEP or in intensive STW right before the start is sufficient.

<sup>13</sup> Under special conditions an extension to three years is possible. Maximal duration in §249h is greater.

<sup>14</sup> For a more comprehensive English language description of the GSOEP see Wagner, Burkhauser, and Behringer (1993).

GSOEP started in 1990 with a sample of close to 2000 households. The major advantage for the evaluation is that the GSOEP is very informative with respect to socio-demographic characteristics and especially with current and past employment. Both employment status and sources of income are available monthly although interviews take place only yearly. Furthermore, the 1993 survey included a special section concerning continuous vocational training. The disadvantage is the sample size. Moreover, PEP are not very well covered in the GSOEP.

The data used to evaluate PEP is the *Arbeitsmarktmonitor Sachsen-Anhalt* (AMM-SA), a panel of all individuals of age 15 to 65 living in the federal state of Sachsen-Anhalt. It started in spring 1992 and is repeated once a year since autumn 1992. For each interview, 6,000 to 10,000 observations are available. One difficulty arises from the structure over time, which is actually a mixture of cross-section and panel data. The first and the second survey are a panel, but for the third survey a new cross-section was drawn. After that in every survey previously interviewed as well as newly drawn individuals are included. Panel mortality in the data is high. The AMM-SA is also very informative, but with disadvantages compared to the GSOEP. Any information on the labour market status before 1991 is missing, and from 1991 on labour market status is only available half-yearly or yearly.<sup>15</sup> Finally, the information available differs substantially from one survey to the other. A further advantage is the comparable high number of observations in smaller and heterogeneous region like a federal state. Of course, this is also a disadvantage because it is far from clear that results for Sachsen-Anhalt could be generalised to all new federal states.<sup>16</sup>

## 4.2 The evaluated programmes

In general we are not interested in any effects of a first education but of training during the working live. Similar, we do not want to catch effects of (planned) early retirement. Therefore, Lechner (1998), Eichler and Lechner (1998), Lechner (1999b) and Lechner (2000) use an upper and a lower age limit. For the studies based on the GSOEP it was required that people worked full time just before unification.

To capture OJT participation Lechner (1999b) uses the 1993 special GSOEP section on training. It contains specific questions about the last three continuous vocational training courses that were either completed in the last three years or are still going on at the time of the interview. The information provided for these courses includes the starting month of the training, the duration, the

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<sup>15</sup> Later surveys of the panel include a monthly labour market status as well.



number of weekly hours etc.. Training is only considered if it takes place at least partly during regular working hours. Its goal is qualification other than retraining for a different occupation and familiarisation with a new work place. Its duration is 16 hours or more. OJT starts before spring 1993 because of the use of the special survey. Mean duration in full time equivalents is 1.8 months.

For CTRT monthly calendars are used to define participation. Individuals are considered to participate in CTRT if they receive benefits or obtain continuous vocational training during STW. CTRT starts not later than March 1993. The time limit has been chosen to avoid catching effects of the substantial changes in regulations in 1993 and 1994. The mean of the duration is just below 12 months with only very few observations with more than 24 months.

For PEP the sample structure enforces the restriction to start after March 1991. The beginning of PEP spells in the sample is concentrated between April 1991 and September 1992 (65% of all spells). A typical PEP duration is one year, which is confirmed by the sample with 17% of all PEP spells with an duration of 12 months. The mean of the duration is 14.8 months, the median 12 months, and only very few PEP spells are longer than 24 months.

### 4.3 Descriptive statistics

Table 3 provides some descriptive statistics for the different samples used, for participants and non-participants respectively.<sup>17</sup> Notice that the underlying populations differ in all three cases due to differences in selection rules. The higher average age in the population used to evaluate PEP is due to different age restrictions. The same is true for the share of females, because the AMM-SA sample is not restricted to persons working in 1989.

Turning to the differences between participants and non-participants, OJT participants are on average older and more likely to be males than non-participants. For CTRT it is just the opposite. For PEP participants and non-participants are more alike, but at least for gender this hides the fact that early PEP participants were more likely to be male, while later the share of females increased up to two thirds. The different indicators for education and job position indicate that training participation increases with human capital already accumulated and higher job position,

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<sup>16</sup> See the discussion in Eichler and Lechner (1998).

<sup>17</sup> For a more comprehensive description of the samples, see Lechner (1998) for training and Eichler and Lechner (1998) for PEP.

regardless of the kind of training. The opposite can be observed for PEP, probably due to the focus of PEP on persons with special labour market problems.

*Table 3: Participants and non-participants in the different samples: selected variables*

Variable	OJT	no OJT	CTRT mean / share in %	no CTRT	PEP	No PEP
<i>Age in 1990<sup>a)</sup></i>	36	35	33.4	35.1	38.8	38.5
<i>Gender: female</i>	37	45	57	40	52	52
<i>Schooling: highest degree</i>						
university entrance degree (12 years)	39	14	30	18	23	28
medium (10 years)	51	62	61	60	54	54
low (8 years or no degree)	9	24	10	22	23	18
<i>Highest professional degree</i>						
university	30	9	19	13	15	18
technical school	27	16	17	15	21	24
skilled worker	37	66	58	65	---	---
<i>Job position in 1990</i>						
highly qualified, management	44	17	24	22	n.a.	n.a.
master of a trade / craft	7	8	5	7	n.a.	n.a.
skilled blue and white collar	45	56	57	55	n.a.	n.a.
<i>Employer characteristics (industrial sector)<sup>b)</sup></i>						
agriculture	6	13	14	13	10	5
heavy industry	8	10	11	10	16	8
light ind., consumer goods, electronics, printing	9	17	30	19	17	5
machine building, vehicle construction	5	5	9	7	---	---
construction	8	7	6	8	7	10
trade	4	8	9	8	3	8
communication, transport	8	8	4	9	1	7
education, science	13	10	10	12.4	10	15
government <sup>c)</sup>	---	---	---	---	18	9
other services	26	9	7	13	13	10
<b>observations</b>	<b>222</b>	<b>980</b>	<b>125</b>	<b>1038</b>	<b>1,123</b>	<b>12,565</b>

Source: Own calculations based on Lechner (1998) and Eichler and Lechner (1998).

Notes: a) Age in 1993 for PEP sample.

b) For OJT and CTRT samples in 1989, for PEP sample in 1992; notice that possible sector changes since unification could have taken place already, possibly even into a PEP. Information in the PEP sample is only available for a part of the observations. Due to a lag of information separation into sectors differs somewhat for PEP sample.

c) For OJT and CTRT this is included in other services.

Not surprising, a fundamental difference between OJT and the other two programmes is the experience with unemployment before participating. The share of unemployed persons in the group of OJT participants is below 5% in the last 20 months before participating. For CTRT, the unemployment rate is around 10% from 20 to 10 months before participation, but increases rap-

idly to about 50% in the month before CTRT.<sup>18</sup> For PEP, a somewhat similar picture emerges, although it is less precise with only yearly information available. At the last interview before PEP, the unemployment rate climbed up to 40%. In contrast to CTRT PEP participants face an unemployment rate of 30% already 3 interviews (2-3 years) before PEP. This reflects the special focus of PEP on persons with special labour market problems and especially long term unemployed. CTRT on the other hand is supposed to provide necessary training to improve employment chances, which might be determined rather quickly once an individual gets unemployed and seeks consultation in the labour office. The above discussion points out the fact that the populations taking part in the three programmes are rather different.

For all three kinds of treatments the participation probabilities have been estimated, all based on probit models but with different specifications. In the case of OJT the population has been split into three different groups before the estimation, men with job position 'high qualified, management' in 1990, all other men and women. It turns out that age has only a minor influence on participation in all cases. It significantly decreases participation probability only for two special cases, for the group of men not working in a job position in 1990 characterised as 'high qualified, management' for higher ages and for PEP for persons below the age of 25. As expected from the descriptive statistics there is no significant influence of gender on PEP participation. For CTRT we find the expected strong and significant positive coefficient for females. The coefficients for education and job position reflect in general the expectations as well. For OJT a higher job position and a university or technical school degree have a positive effect on participation. For CTRT and PEP participation it is the school achievement which is more influential, more schooling leading to a higher CTRT participation and holding only the lowest school degree leading to a higher PEP participation probability. There are some significant effects of different regions and the industrial sector employed in in 1990, but for more information the reader should refer to the original studies.<sup>19</sup> Finally, notice that this estimation includes time constant individual characteristics only. As seen from the descriptive statistic the samples differ a lot in their individual employment history. For computational reasons this is not included here but will be taken account for further in the estimation when 'matching'.

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<sup>18</sup> Actually, this number even understates the real facts, because persons in STW before CTRT (e.g. taking part in training during STW) are not unemployed, but the 'zero STW' in east Germany can be considered just like unemployment here.

<sup>19</sup> Lechner (1999b) for OJT, Lechner (2000) for CTRT and Eichler and Lechner (1998) for PEP.

## 5 Econometric methodology and empirical implementation

### 5.1 The causality framework and the targets of the evaluation

The empirical analysis tries to answer questions like "What is the average gain for programme participants compared to the hypothetical state of non-participation?" This is the so-called treatment effect on the treated. The underlying notion of causality requires the researcher to determine whether participation in treatment has an effect on the respective outcomes, such as earnings or employment status.<sup>20</sup> Therefore, the framework that appears to be most useful as a guideline for the empirical analysis is the *potential-outcome approach to causality* suggested by Roy (1951) and Rubin (1974).

To facilitate the discussion, the following notation is useful.  $Y^p$  and  $Y^n$  denote the potential outcomes ( $p$  denotes participation in treatment).<sup>21</sup> Additionally, denote variables that are unaffected by treatment - called *attributes* by Holland (1986) - by  $X$ . It remains to define a treatment participation indicator  $S$ , that determines whether person  $i$  participates in a treatment ( $s_i = 1$ ) or not ( $s_i = 0$ ). The observable outcome is  $y_i = s_i y_i^p + (1 - s_i) y_i^n$ . Hence, the causal effect, for example defined as the difference of two potential outcomes, can never be estimated, because the *counterfactual* ( $y_i^p, s_i = 0$ ) or ( $y_i^n, s_i = 1$ ) to the observable outcome  $y_i$  is unobservable. However, the quantity of interest to answer the question placed on the beginning of this section is the average causal effect of treatment participation for treatment participants,  $\theta_0$ :<sup>22</sup>

$$\theta_0 := E(Y^p - Y^n | S = 1) = E(Y^p | S = 1) - E(Y^n | S = 1) = g^p - E(Y^n | S = 1). \quad (1)$$

$g^p$  can be consistently estimated by the sample mean of  $y_i$  in the subsample of treatment participants. The problem is the term  $E(Y^n | S = 1)$ . Much of the literature on causal models in statistics and selectivity models in econometrics is devoted finding useful identifying assumptions to predict the unobserved expected nontreatment outcomes of the treated population

<sup>20</sup> See Holland (1986) and Sobel (1994) for an extensive discussion of concepts of causality in statistics, econometrics, and other fields.

<sup>21</sup> As a notational convention capital letters indicate quantities of the population or of members of the population and lower case letters denote the respective quantities in the sample. The units of the sample ( $i=1, \dots, N$ ) are assumed to be the result of  $N$  independent draws from this population.

<sup>22</sup>  $E(\cdot | S=1)$  denotes the mean of the respective random variables in the population of treatment participants.

by using the observable nontreatment outcomes of the untreated population  $(y_i^n, s_i = 0)$  in different ways.<sup>23</sup>

## 5.2 Identifying restrictions

### 5.2.1 Conditional independence assumption

The so-called conditional independence assumption (CIA; Rubin, 1977) can be used to identify  $E(Y^n | S = 1)$ :

$$Y^n \perp\!\!\!\perp S | X = x, \quad \forall x \in \mathcal{X}. \quad (2)$$

CIA means that participation is independent ( $\perp\!\!\!\perp$ ) of the non-treatment outcome conditional on the values of covariates or attributes  $x$  in the space  $\mathcal{X}$ . Thus  $E(Y^n | S = 1, X = x) = E(Y^n | S = 0, X = x)$ , and  $\theta_0$  is identified. Compared to model-based econometric approaches, CIA allows to estimate treatment effects directly without imposing functional form or parametric assumptions that are often imposed when estimating a structural model. Subsequently, we will denote by  $\theta_0^{CIA}$  the limit of an estimator that is consistent under the CIA assumption

$$(p \lim_{N \rightarrow \infty} \hat{\theta}_N^{CIA} = \theta_0^{CIA}):$$

$$\theta_0^{CIA} = E(Y^p | S = 1) - E_x \left[ E(Y^n | S = 0, X = x) | S = 1 \right] = g^p - E_x \left[ g^n(x) | S = 1 \right]; \quad (3)$$

$$g^p = E(Y^p | S = 1); \quad g^n(x) := E(Y^n | X = x, S = 0).$$

To justify CIA, the important task is to identify and observe all variables that could be mutually correlated with assignment and potential nontreatment outcomes, without being influenced by the realisation of the treatment status (endogeneity problem). CIA implies that there is no important variable left out that influences nontreatment outcomes as well as assignment given a value of the relevant attributes. For most evaluation studies of ALMP and training standard reasoning

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<sup>23</sup> This may include the outcomes of participants prior to treatment.

suggests that  $X$  should at least include information on schooling, job training and experience, the complete labour market history of the person and individual socio-economic characteristics.

A technical problem related to the choice of variables to be included in  $X$  is the potential high dimension of  $X$  that complicates the estimation of the conditional expectation. Let  $P(x) = P(S=1/X=x)$  denote the propensity score that is defined as the nontrivial probability ( $0 < P(x) < 1$ ) of being assigned to a treatment conditional on  $X$ . Furthermore, let  $b(x)$  be a function of attributes such that  $P[S=1/b(x)] = P(x)$ , i.e. the balancing score  $b(x)$  is at least as 'fine' as the propensity score. If CIA is valid, Rosenbaum and Rubin (1983) show that  $Y^n \perp\!\!\!\perp S \mid b(X) = b(x), \forall x \in \mathcal{X}$  holds, hence:

$$E(Y^n \mid S = 1) = E_x \{E[Y^n \mid S = 0, b(X) = b(x)] \mid S = 1\}. \quad (4)$$

The major advantage of this property is the reduction of the dimension of the estimation problem. The disadvantage is that the probability of assignment - and consequently any balancing scores that reduce the dimension of the estimation problem - is unknown and has to be estimated.

It is obvious that the validity of CIA typically requires a very informative data set. It is argued that the German Socio Economic Panel has the potential of being such data. Thus the evaluation of the effects of OJT and CTRT using the GSOEP are based on the CIA to identify the effect of training.

However, the GSOEP does not provide sufficient information on PEP participation, and no other such informative data is available (yet?). The AMM-SA used in this study has several shortcomings. Employment histories are only available on a yearly basis. Any information on employment before or at unification is missing. Therefore we might miss employment dynamics in the month just before PEP participation. Since unemployment is one important selection criterion, this will invalidate CIA with our data. In addition to that, the considerable amount of panel attrition, the redrawing of the sample in 1993 and the refreshing of the sample thereafter results in a very unbalanced panel design. This leads to relatively short observation periods for many people and thus for many people we have only short labour market histories. Therefore, it is necessary to use an alternative identification strategy in that case.

### 5.2.2 The Conditional Bias Stability Assumption

For the further discussion of the methods used by Eichler and Lechner (2000) it is useful to introduce the time dimension explicitly as  $Y = \{Y_{-T}, \dots, Y_{-2}, Y_{-1}, Y_1, Y_2, \dots, Y_T\}$ ,  $Y^p = \{Y_{-T}^p, \dots, Y_{-2}^p, Y_{-1}^p, Y_1^p, Y_2^p, \dots, Y_T^p\}$  and  $Y^n = \{Y_{-T}^n, \dots, Y_{-2}^n, Y_{-1}^n, Y_1^n, Y_2^n, \dots, Y_T^n\}$ . The period between  $-1$  and  $1$  includes the time in treatment and may be longer than the other periods, in particular it varies individually. All other periods are equally spaced.  $-T, \dots, -1$  and  $1, \dots, T$  refer to points in time where interviews take place, with the assigned values depending on the distance to the begin (negative values) and end (positive values) of the individual treatment spell, respectively. This leads to the following re-definition of the effect of treatment on the treated in period  $t$ :

$$\theta_{t,0} := E(Y_t^p - Y_t^n | S = 1) = E(Y_t^p | S = 1) - E(Y_t^n | S = 1) = g_t^p - E(Y_t^n | S = 1), \quad t = \dots, -1, 1, 2, \dots (1')$$

In that framework treatment is defined as participation in a treatment some time in our observation period ( $S$  has no index referring to time). Therefore, we allow for a treatment effect before participation in treatment.

The idea of the identification strategy is that although CIA may not hold, it may be reasonable to assume that the bias due to an incorrectness of the CIA is the same for at least one date before a treatment ( $\tau$ ,  $\tau < 0$ ) and one date after a treatment ( $t$ ,  $t > 0$ ). If the true effect of a treatment is indeed zero for that date before the treatment, than an estimated treatment effect using CIA for period  $\tau$  gives an estimate of the bias. This bias estimate could be used to correct the estimate of the treatment effect for the date after the treatment. This assumption is called the bias stability assumption (BSA) in the remainder of this paper.

The use of BSA as an identifying restriction has several advantages. First, it nests the intuitively appealing CIA assumption when the treatment effect is zero and CIA is valid before the treatment. In this case, if BSA and CIA are correct, then the test, whether the estimated effect assuming CIA before the treatment is zero, is a joint test for CIA and BSA. If  $\theta_{\tau,0} = 0$ , that is part of the BSA assumption, does not hold, then it is conceivable that CIA is still valid for  $t > 0$ , but

BSA is violated.<sup>24</sup> It is of practical relevance that for BSA less information is necessary to identify  $\theta_{t,0}$  than for CIA. It is particularly useful that this generalisation is achieved without the need to use instruments, because instruments that for example satisfy the strong assumptions of Angrist, Imbens, and Rubin (1996) are hard to find in general and not available for these particular applications. When  $X$  contains past values of variables that vary over time it is another practical advantage that the effect before and after the programmes need not necessarily be estimated using the same sample. Although a panel data set is still useful to keep both biases small, the length any individual is required to be observed in the panel data set is not prolonged by the need to observe the same person before and after the programme.<sup>25</sup> Compared to conventional difference-in-difference estimators,<sup>26</sup> BSA has the advantage of being nonparametric so that successful identification does not depend on specific functional forms for the respective expectations. The latter is particularly important for models with qualitative or binary variables as outcomes, because for such models conventional difference-in-difference estimators are very much dependent on the particular functional forms assumed.

### 5.3 Estimation procedures: matching

#### 5.3.1 Matching

Given the choice probabilities or a consistent estimate of them, the terms appearing in equation (4) can be estimated by any parametric, semiparametric or nonparametric regression method that can handle one or two-dimensional explanatory variables. One of possible and recently popular choices of estimators in binary framework was matching (for recent examples see Angrist, 1998, Dehejia and Wahba, 1999, Heckman, Ichimura, and Todd, 1998, Heckman, Ichimura, Smith, Todd, 1998, and Lechner, 1999c, 2000). The idea of matching on balancing scores is to estimate  $E(Y^n | S = 1)$  by forming a comparison group of selected non-participants, that have the same distribution of the balancing score as the group of participants. In the following we will denote the non-participants matched to the participants as *comparisons*. By virtue of the property of

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<sup>24</sup> However, in this case CIA must not involve pre-treatment realisations of outcome variables in the conditioning set.

<sup>25</sup> If we were to use the same treatment participants, we would require that observations remain in the sample for another  $(t - \tau)$  periods (notice that the distance defined by  $(t - \tau)$  is not necessarily equal to the corresponding mathematical calculation).

<sup>26</sup> See for example the survey by Meyer (1995).



being a balancing score, the distribution of  $X$  will also be balanced in the two samples. The estimator of  $E(Y^0 | S = 1)$  is then simply the mean in that selected comparison group. Compared to kernel estimates, a major advantage of matching is clearly its simplicity and its intuitive appeal.<sup>27</sup> Advantages compared to parametric approaches are its robustness to the functional form of the conditional expectations (w.r.t.  $E(Y^0 | X, S = 0)$ ) and that it leaves the individual causal effects completely unrestricted and hence allows arbitrary heterogeneity of the effect in the population.

Note that there is complication compared to standard application of the matching approach. Time variant attributes and their relative distance to the start of the programmes, like the employment status just prior to participation, play an important role to make the identifying assumption credible. However, such a variable could not be observed for non-participants. Lechner (1999b) suggests to use the following step-wise approach allowing for such variables:

- a) Draw a participant randomly from the sample ;
- b) Compute the relevant variables for all control using the start-date of the participant selected in step a).
- c) Add these variables to the balancing and find the closest match.

Steps a) to c) are repeated for all participants and appear to be effective in balancing the matched sample with respect to all relevant variables.

### 5.3.2 Double matching for the BSA

Since a 'natural' estimator under the BSA assumption  $\hat{\theta}_{t-\tau, N}^{BSA}$  is the difference of two estimates using CIA ( $\hat{\theta}_{t, N}^{CIA} - \hat{\theta}_{\tau, N}^{CIA}$ ), we use matching estimators for both quantities separately. In the following we discuss some further issues related to the specific before-after framework that is used under the BSA assumption. Let us compare  $\hat{\theta}_{t-\tau, N}^{BSA}$ ,  $\hat{\theta}_{t, N}^{CIA}$ , and  $\hat{\theta}_{\tau, N}^{CIA}$  for two cases. First, assume that  $X$  is time-constant, so that both estimators  $\hat{\theta}_{t, N}^{CIA}$  and  $\hat{\theta}_{\tau, N}^{CIA}$  use the same values of the matching variables  $X$ . In that case it appears sensible to use a matching protocol ensuring that the same comparison observations are used as matches for both estimations. For ease of exposition

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<sup>27</sup> One might conjecture that matching will be superior in small samples, but as of now, the evidence is very limited.

assume that observations are ordered such that the  $N^p$  observations participating in treatment are followed by the  $(N - N^p)$  non-participants. Furthermore denote a comparison observation  $j$  matched to treatment participant  $i$  as  $j(i)$ . Then,  $\hat{\theta}_{t-\tau, N}^{BSA}$  ( $= \hat{\theta}_{t, N}^{CIA} - \hat{\theta}_{\tau, N}^{CIA}$ ) has the following simple form:<sup>28</sup>

$$\hat{\theta}_{t-\tau, N}^{BSA} = \frac{1}{N^p} \sum_{i=1}^{N^p} (y_{i,t}^p - y_{j(i),t}^n) - \frac{1}{N^p} \sum_{i=1}^{N^p} (y_{i,\tau}^p - y_{j(i),\tau}^n) = \frac{1}{N^p} \sum_{i=1}^{N^p} [(y_{i,t}^p - y_{i,\tau}^p) - (y_{j(i),t}^n - y_{j(i),\tau}^n)];$$

$$0 < i \leq N^p; N^p < j \leq N. \quad (10)$$

However, if it is the aim to reduce the bias in both  $\hat{\theta}_{t, N}^{CIA}$  and  $\hat{\theta}_{\tau, N}^{CIA}$  used to compute  $\hat{\theta}_{t-\tau, N}^{BSA}$  (as seems sensible, but not necessary), the conditioning set should include lagged values of the outcome variables as well. If  $Y_t \in X$ , then the case  $X_t = X_\tau$  is no longer sensible because it implies by definition that  $\theta_{\tau, 0}^{CIA} = 0$  and hence  $\theta_{t-\tau, 0}^{BSA} = \theta_{t, 0}^{CIA}$ .<sup>29</sup>

Therefore, it is obvious that there are cases in which the set of conditioning variables depend on the calendar time a treatment takes place. For example, in the empirical part we consider the indication of unemployment after a PEP as an outcome variable and the indication prior to a PEP as a matching variable (see Section 3 for the importance of unemployment as an eligibility criteria for PEPs). The definition of 'prior to PEP' has to be appropriately changed for the estimation of  $\hat{\theta}_{\tau, N}^{CIA}$  by taking the duration of the PEP into account.

Figure 2 clarifies this issue for the case of  $\tau = -1$  with an example for one PEP participant. In the *evaluation sample* we get  $\hat{\theta}_{t, N}^{CIA}$ . In the *bias correction sample* we assume an artificial PEP period to get  $\hat{\theta}_{\tau=-1, N}^{CIA}$ , the effect of the artificial PEP. Because of the BSA ( $\theta_{\tau, 0} = 0$ ) and that in fact no treatment has taken place yet, this is an estimate for the bias. We use it to correct  $\hat{\theta}_{t, N}^{CIA}$ .

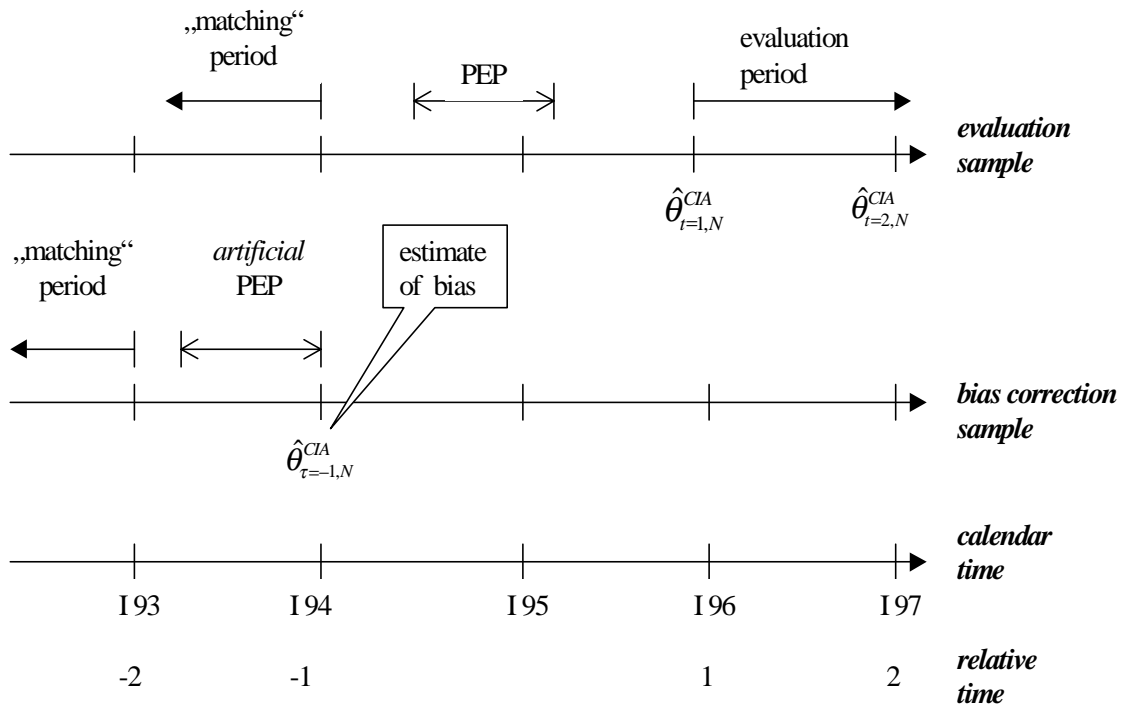
<sup>28</sup> We could call this a nonparametric difference-in-difference estimator.

<sup>29</sup>  $\hat{\theta}_{\tau, N}^{CIA}$  will only be exactly zero, if the sample to draw the comparisons from is sufficiently rich to allow for 'perfect' matches.

The assumption of random sampling and large  $N$  and  $N^p$  allows for the usual asymptotic approximation of the variance of matching estimators:<sup>30</sup>

$$\text{Var}(\hat{\theta}_{t-\tau, N}^{BSA}) = \frac{1}{N^p} [\text{Var}(y_{i,t}^p - y_{i,\tau}^p) + \text{Var}(y_{j(i),t}^n - y_{j(i),\tau}^n)]. \quad (11)$$

Figure 2: Definition of periods for the evaluation sample and the bias correction sample for one PEP participant when time varying variables are used,  $\tau = -1$



Note: The *calendar time* shows interviews from 193 to 197. The *relative time* is defined according to the PEP participation of the person in the example (note that in this example the PEP spell includes one interview which is not necessarily the case). The period *artificial PEP* has the same duration as the PEP and ends just before the last interview before the PEP ( $\tau = -1$ ). The evaluation sample is the sample used to compute  $\hat{\theta}_{t, N}^{CIA}$  and the bias correction sample is the sample used to compute  $\hat{\theta}_{\tau, N}^{CIA}$  ( $= \hat{B}_{\tau, N}^{CIA} = \hat{B}_{t, N}^{CIA}$  if BSA is valid).

<sup>30</sup> Note that the following formulas have been obtained using the approximation that the fact that the pairs have been selected by an estimated score can be ignored.

## **6 Major results**

### **6.1 Definition of outcome variable**

Our analyses are particularly interested in the effects on post-programme changes in the actual labour market status. It is due to the nature of the data and the circumstances (German unification in 1990) that at the time the analyses were conducted no long run effects could possibly be discovered.

For the analyses of training we can utilise monthly information on employment status as well as yearly information on gross monthly earnings (in the month of the yearly interview). With respect to PEP we have to rely only on yearly information on employment status (again in the month of the yearly interview). It is important to note for the discussion in the following subsection that, except for the earnings' variable, all outcome variables are coded as binary indicators.

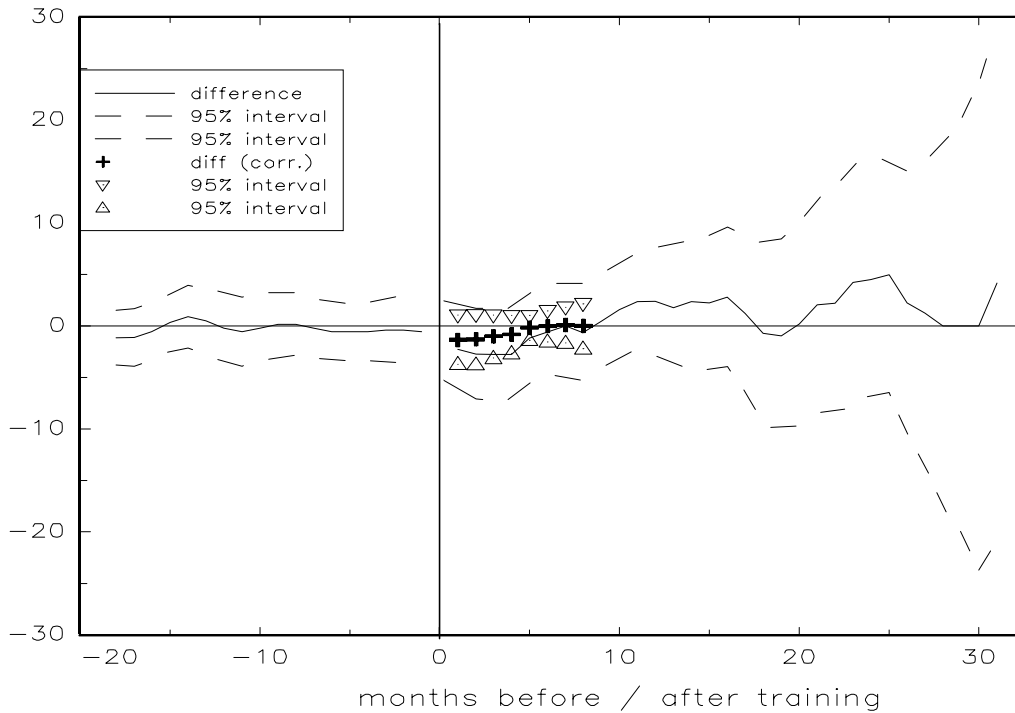
### **6.2 Results for on-the-job training (OJT)**

The results of the evaluations of OJT are given in the following figures. They show the differences between the control and the OJT group for specific time spans before and after the training for a selected group of outcome variables (multiplied by 100 for outcomes that are indicators). For variables measured using the monthly calendar the distance is expressed in months, for those only measured for the particular month of the yearly interview, the distance is expressed in years. The figures cover up to 18 months or up to 3 'years' before the training and up to 30 months or 3 'years' after OJT. They display the mean effect (solid line; + for the mismatch corrected estimate) and its 95% point-wise confidence interval based on the normal approximation (dashed line;  $\nabla$ ,  $\Delta$  for the mismatch corrected estimates).<sup>31</sup>

The number of observations available to compute the respective statistics decreases the longer the distance to the incidence of OJT. This implies that the variances increase over time which is reflected in the widening of the confidence intervals. Additionally, a mismatch correction may be impossible or very imprecise, because there may be too few observations to identify and estimate the parameters of the ordered probit model.

Figure 3 presents the results of the evaluations for the monthly outcome variables unemployment. The part left to the zero vertical mark allows a judgement about the quality of the matches concerning the particular variable.<sup>32</sup>

Figure 3: Difference of unemployment rates in %-points



Note:  $N_{-1}^t = 185$ . Smoothed using 3 month moving averages for  $|\tau| > 1$ . Mismatch correction only available for  $\tau = 1, \dots, 8$ , because of insufficient variation of the difference of the outcome variables for  $\tau > 8$ .

Figure 3 shows no significant positive effects of OJT. The latter is in line with the findings for public sector sponsored training presented in the next section. Apparently, it is difficult to reduce the individual unemployment risk by means of training in a rapidly contracting economy that also adjusts to a new economic environment, for example because of unforeseen changes in firm strategies and technologies used, leading to unexpected changes in the size and composition of the work force, so that even previous OJT may only be of limited value.

Figure 4 shows the average effects of OJT on monthly gross earnings. It appears that there are positive effects of about DM 350 from OJT in the second year after completion of the last OJT

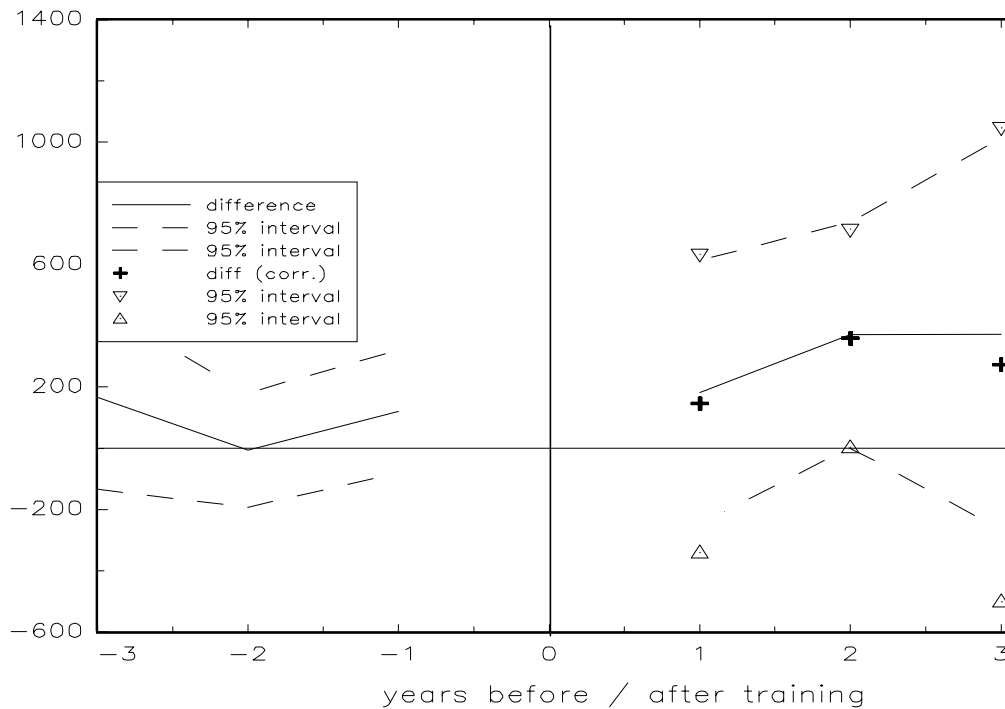
<sup>31</sup> Mismatch-corrected estimate are computed by regressing the difference in outcomes for each matched pair on the difference in observable characteristics for each pair. OLS is used in case of earnings, whereas an ordered probit with three categories (-1, 0, +1) is used for binary outcome variables.

<sup>32</sup> Testing whether these lines deviate significantly from 0 is similar to the tests suggested by Rosenbaum (1984).

spell.<sup>33</sup> Note that the same effect appears for the third year, but that (probably) the reduced sample size leads to its insignificance.

When checking, whether the estimated earnings' effects are stable across the population of OJT participants, a significant difference with respect to job position and occupational degree appears. For those individuals having a university degree and / or being in a highly qualified and / or managerial job position, no significantly positive effects are visible. However, for the complements of these populations, positive effects that already appeared in the population average in year two after OJT are more pronounced. At least for those not in highly qualified and / or managerial job positions, there appears a mismatch corrected positive effect for the third year after OJT. Note that these differences of earnings effects regarding education and job position suggest that OJT training and general training are not complementary. Quite the opposite, there appear to be diminishing returns to education.

Figure 4: Monthly gross earnings (in 1993 DM)



Note:  $N'_{-1} = 185.0$  when unemployed.

<sup>33</sup> The implied average earnings increase is about 9%.

To see whether the significant earnings gains really translate to additional benefits for participants, information about the participants' share of the direct and indirect training costs is necessary. Results in Lechner (1998) suggest that, although the type of training appears not to be too firm specific, most of the cost is paid by the employer. However, it might still be that the firm is reimbursed by a lower pre- or post-OJT wage. Although exact information on these issues is not available, the previous figures can give some clues. For example, one may wonder whether the insignificant effects in the first year may be due to some sort of sharing the additional productivity between the employer and the employee to make up for training costs, or whether it takes some time for OJT to result in additional productivity. Considering the pre-OJT earnings, it appears that there is no significant cost sharing between employers and employees prior to OJT. Otherwise, the respective figures should show a drop when earnings approach OJT from the left, because post-unification pre-OJT earnings are not part of the conditioning set (attribute vector). In conclusion, although there might be some wage restraint in the first OJT year, OJT appears to be very beneficial for earnings. It appears to have no effect concerning the risk of future unemployment. These conclusions are confirmed by considering other outcome variables, in particular those related to future expectations and job position.

### **6.3 The effect of CTRT**

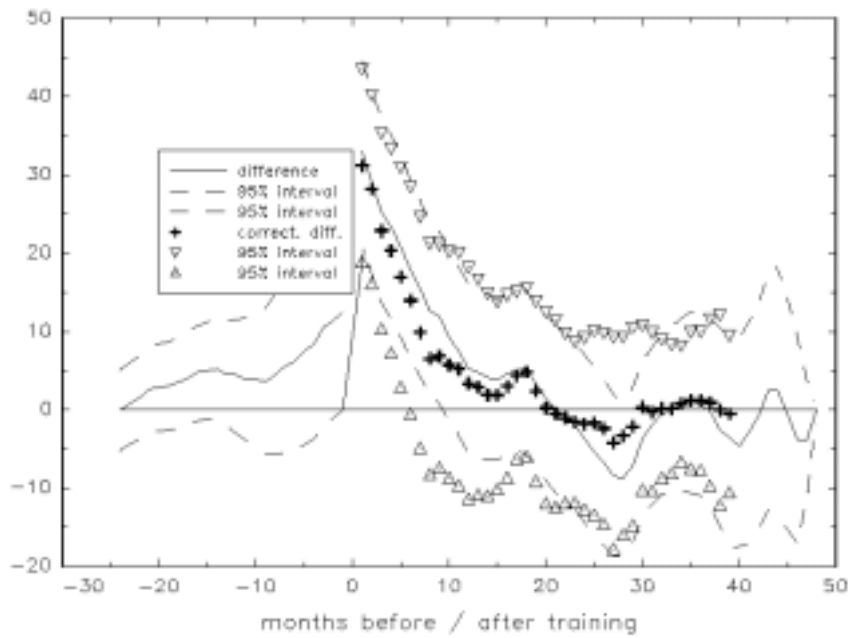
Figure 5 shows the differences of unemployment rates between the CTRT group and the comparison group. The mean effect up to 24 months before CTRT and up to about 40 months after CTRT are displayed.

The parts of Figure 5 to the left to the zero vertical mark again allow a judgement about the quality of the matches concerning that particular variable. Although the number of unemployed persons is generally higher in the CTRT sample, it is only in the month just prior to CTRT that the difference is just significant.

Figure 5 also shows that the immediate effect of CTRT is additional unemployment in the months following the end of CTRT. After some months these negative effects disappear. There is a simple explanation for this effect: More than 50% of CTRT participants are unemployed before CTRT. For an unemployed person the immediate effect of (full-time) CTRT is that during CTRT his or her search efforts will be reduced compared to the comparison non-participants. The results suggest that if there is a positive effect of CTRT it is not large enough to compensate for this

initial negative outcome, and to be detected by the estimator (note that the confidence bands are wide enough to make it difficult to exclude the possibility of medium sized positive effects after about one year as well as of negative effects of CTRT). These general findings are confirmed by other outcome variables.

Figure 5: Difference of unemployment rates for CTRT participants and selected control group in %-points

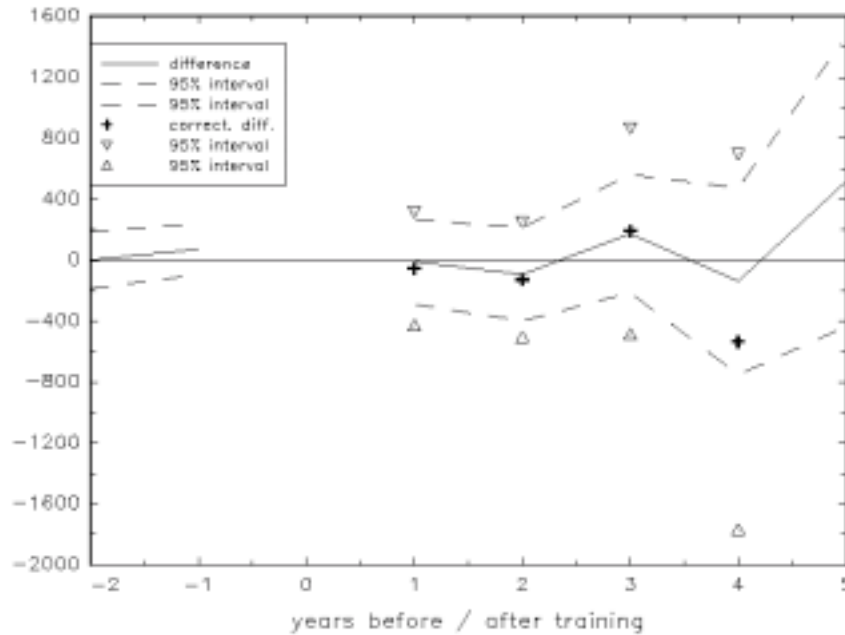


Note:  $N_{-1}^t = 116$ . Smoothed using 3 month moving averages for  $|\tau| > 1$ .

Figure 6 features the results for earnings observed only once a year. For the employed, earnings are defined as gross monthly earnings in the month before the interview. For those not being employed, either imputed unemployment benefits or - if higher - social assistance or zeros are used (not shown). There are no significant differences for the pre-training outcomes in both cases, but there does not appear to be an effect of training either. Note that the estimated earnings effects are largely driven by employment dynamics.



Figure 6: Difference of gross earnings (in 1993 DM, including benefits)



Note:  $N'_{-1} = 116$ .

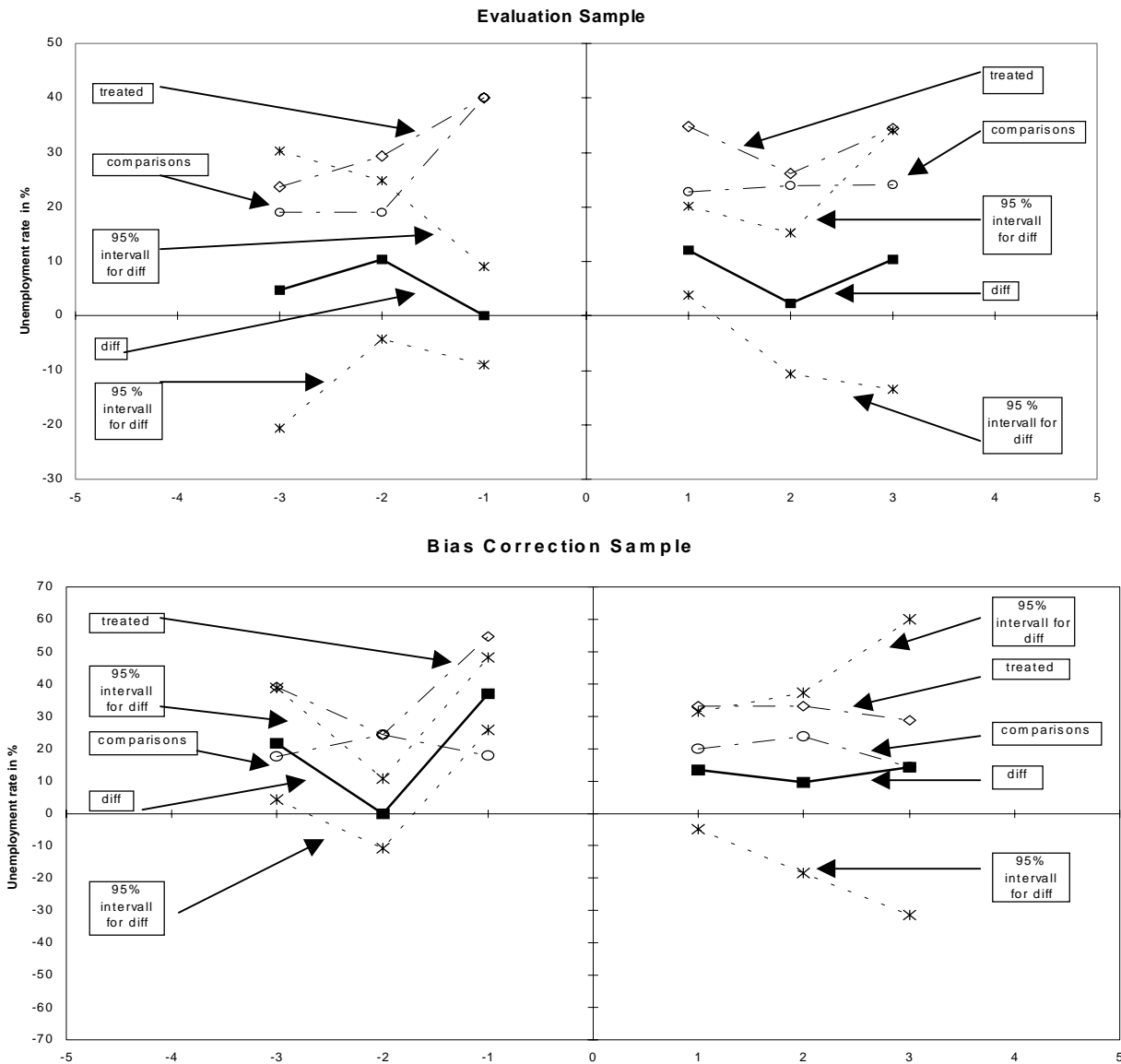
### 6.3 The effect of PEP in Sachsen-Anhalt

The discussion of the results will focus on the case we consider most informative, using time variable matching variables, especially unemployment. Furthermore, this includes the choice of  $\tau$  fixed at the interview before PEP,  $\tau = -1$ , and an unbalanced design, that is that different observations for the evaluation sample and the bias correction sample can be used. Among other reasons one advantage is the reduction of losses of observations to a minimum. Nevertheless, we tested a wide variety of specifications but did not find substantially different results.<sup>34</sup>

Figure 7 presents the first part of the results. The vertical axis is the difference in the unemployment rate between the participants taking part in PEP and the comparisons, the unemployment the participants would have faced had they not participated. A positive number reflects a higher unemployment rate due to participation in PEP. The results prior to PEP ('-1' in the evaluation sample, '-2' in the bias correction sample) demonstrate that matching was successful with respect to unemployment before treatment. The value for  $\tau = -1$  in the bias correction sample indicates that there is still a substantial bias in the CIA results (given BSA is correct).

<sup>34</sup> Some of the results can be found in Eichler and Lechner (1998). Notice that here we used a revised and extended version of the data. Even for the equal cases there might be some differences in the results. The paper is downloadable from the Internet at: <http://www.sia.w.unisg.ch/lechner/publica/98e01.html>.

Figure 7:  $\hat{\theta}_N^{CIA}$  computed with time-varying matching variables, unemployment

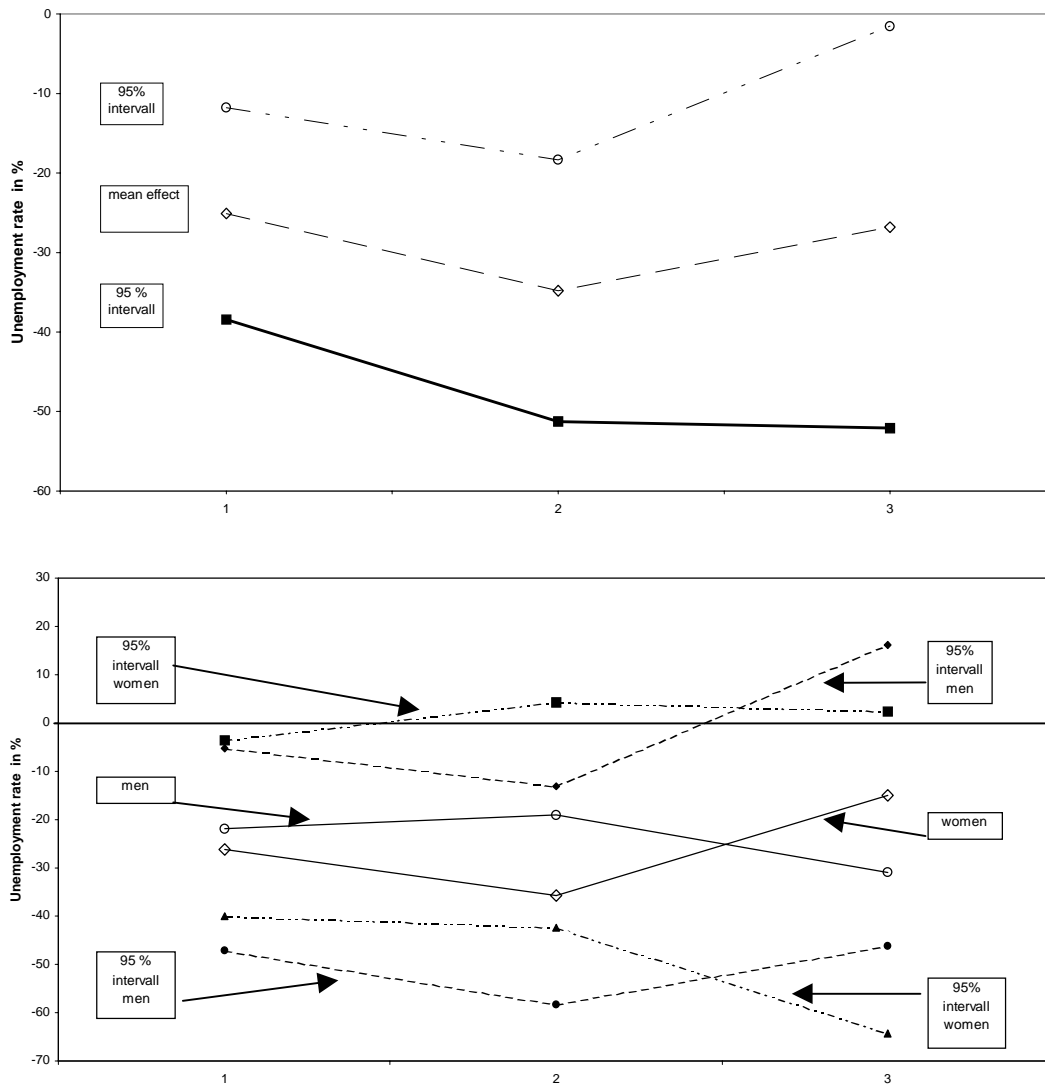


Note: X-axis: Relative time; Y-axis: Unemployment rate in %.  
 Results are in %-points of unemployment. A positive sign indicates that unemployment is XX%-points higher in the PEP group than in the matched comparison group.  
 X used for matching: partial propensity, dummies indicating valid interviews in particular wave, pre-PEP full time employment, pre-PEP unemployment, pre-PEP job position, pre-PEP industrial sector (agricultural, chemical, public), pre-PEP vocational degree, pre-PEP firm size, pre-PEP unemployment rate in region (only last interview before PEP used).  
 The *evaluation sample* is used to compute the effect of PEP if CIA was valid, the *bias correction sample* is the used to estimate the bias. 'Treated' is the estimated unemployment rate for participants, 'comparisons' the estimated unemployment rate for participants had they not participated (the unemployment rate of the matched comparison sample), and 'diff' the difference between these two,  $\hat{\theta}_N^{CIA}$ .

Figure 8 shows the basic result. The value of  $-25$  for  $t=1$  indicates that there appears to be a substantial individual gain (i.e. a reduction in unemployment probabilities) from participating in

PEP. The average probability of unemployment for an individual in the sample of PEP participants - equivalent to the unemployment rate in the sample - is reduced from 60% to 35% at the time of the first interview after PEP ended. The results for higher values of  $t$  indicate that this is not only a short term effect, although the numbers should be considered with care because of the reduced sample size. Note that the bias correction not only changes but reverses the conclusions that would be drawn if CIA was believed to be valid, even when the richest specification of matching possible with the available data is used.

Figure 8:  $\hat{\theta}_{t-\tau, N}^{BSA}$  computed with time-varying matching variables, unemployment



Note: See note on Figure 7.

The lower part of Figure 8 provides gender-specific results. Although the estimates are less precise because of the reduced sample sizes, the differences for the two samples with respect to the effect of participation on unemployment are not substantial. Checking for the reasons of the reduced unemployment, some differences are revealed. For men the reduction in unemployment is mainly due to a increase in employment. For women, the increase in non-employment (out of labour force) is substantial as well (Table 4).

Table 4:  $\hat{\theta}_{t-\tau,N}^{\text{BSA}}$  for employment and out-of-labour-force

Period	$\hat{E}(Y_t^p   S=1)$	$\hat{E}(Y_t^n   S=1)$	$\hat{\theta}_{t-\tau,N}^{\text{BSA}}$	P-value	$\hat{E}(Y_t^p   S=1)$	$\hat{E}(Y_t^n   S=1)$	$\hat{\theta}_{t-\tau,N}^{\text{BSA}}$	P-value
	employment women and men				non employment women and men			
1	54	32	22	.003	12	8	4	.465
2	55	35	20	.029	19	4	15	.018
3	48	40	8	.550	17	-1 <sup>a)</sup>	19	.038
	women only				women only			
1	41	25	16	.090	17	12	6	.434
2	38	37	1	.925	29	11	18	.090
3	41	30	11	.547	18	-3 <sup>a)</sup>	20	.119
	men only				men only			
1	66	40	25	.026	6	5	1	.912
2	70	43	27	.036	11	2	9	.269
3	58	46	13	.566	17	14	3	.882

Note: Results are in %-points of employment and out of labour force. See note on Figure 7.

a) Negative value due to very small sample size.

## 7 Conclusions, shortcoming, and future research

In this paper we summarise our previous results on the effectiveness of different kinds of labour market training programmes as well as employment programmes in East Germany after unification. All the studies use the microeconomic evaluation approach and are based on different types of matching estimators. The evaluation of public and private financed training and public employment programmes (PEP) in East Germany after unification leads to very different conclusions. Participation in training has no (measurable) effect on future unemployment probabilities, with the exception of a short-term negative effect of public sponsored training (continuous training and retraining, CTRT). This is probably due to a reduced search activity during CTRT, which is not the case for the private training considered here, because participants typically hold a job (on-the-job training, OJT). It is surprising, that training does not improve the employment probabili-

ties, especially in the case of OJT where firms appear to cover most of the costs but do not hold onto the employees more frequently than without OJT. An explanation compatible with these results would be that it is very difficult to increase the individual employment probability of workers by means of training in a rapidly contracting economy where changes in the demand for skills are not anticipated successfully. But this explanation is not in line with the result that OJT has a positive effect on earnings. No such effect is found for CTRT. In line with the OJT results would be an OJT that does not provide firm specific human capital but general skills which increased the worker's value on the labour market and hence their wage. The firm could substitute them by other workers for more or less the same costs. Of course in this case there is no other explanation why CTRT can not provide such general skills than that CTRT is an ineffective system to provide them or that the population served is different such in a way that it does not gain from the general skills. In contrast to the above results the effect of PEP participation on future unemployment probabilities is positive, that is it reduces unemployment. One explanation would be the training and work experience provided during the PEP, but such an effect would be rather surprising, especially in the light of the effects for OJT and CTRT on unemployment. Another explanation might be screening by the PEP employer. The PEP employment is used as a 'trial period' to determine the work capacities of the participant. The focus of PEP on persons with special labour market problems and long term unemployed makes this explanation more valuable, because in the labour market situation in East Germany with a huge oversupply, there might be a very low employment chance for this persons without any help. Finally, there is the important issue of the shortcoming of all databases currently available for such evaluation studies that sheds some doubts on all these results.

The problem of no clear-cut policy conclusions is not only a problem for this study but common to evaluations of active labour market policy (ALMP) in East Germany after unification. Several other studies are available, differing from the studies presented here and among each other in the set of identifying assumptions, the estimation methods, the data and finally the definition of the programme. To mention some recent ones, Fitzenberger and Prey (1999) estimate the effects of training participation, Hübler (1997) several different ALMP, Hujer and Wellner (1999) training and Kraus, Puhani and Steiner (1998) evaluate the effects of PEP participation. For a comprehensive survey see Fitzenberger and Speckesser (2000). These and other studies do not come to a common conclusion. Vice versa, results are often contradictory. One of the most important reasons is the data situation, which not only causes the choice of different methods and

treatment definitions, but also make it difficult to draw clear solutions based on the low number of observed cases. Therefore, although most studies appear to be somewhat negative about the effects, any definite policy conclusion from this and other studies about active labour market policy in East Germany would probably be premature.

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<i>Replacing Nos. 1-2 &amp; 4-6: <b>Journal of Comparative Economics</b> Symposium on "Bank Privatization in Central Europe and Russia." Vol. 25, No. 1, August 1997.</i>	<i>No. 1 "Bank Privatization in Transitional Economies" by Roger Kormendi and Edward Snyder. No. 2 "Transactional Structures of Bank Privatizations in Central Europe and Russia" by Anna Meyendorff and Edward A. Snyder. No. 4 "Bank Privatization in Poland: The Case of Bank Slaski" by Jeffery Abarbaness and John Bonin. No. 5 "Bank Privatization in Post-Communist Russia: The Case of Zhilsotsbank" by Jeffery Abarbanell and Anna Meyendorff and No. 6 "The Czech Republic's Commercial Bank: Komerčni Banka" by Edward A. Snyder and Roger C. Kormendi.</i>	<i>August 1997</i>