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An Evaluation of Public Employment Programmes in the East German State of Sachsen-Anhalt

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Abstract:

In East Germany, active labour market policies (ALMPs) have been used on a large scale to contain the widespread unemployment that emerged after unification. This paper evaluates the effects for participants in public employment programmes (PEPs), an important component of ALMP in the East German States (Länder). The paper focuses on individual unemployment probabilities. By concentrating on the state of Sachsen-Anhalt, the econometric analysis can use a large new panel data set available only for that state, the *Arbeitsmarktmonitor Sachsen-Anhalt*. We aim at nonparametric identification of the effects of PEPs by combining the use of comparison groups with differencing over time to correct for selection effects. Our results indicate that PEP participation reduces participants' probability of unemployment.

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JEL classification: C 14, C 23, H 43, J 64, J 68

1 Introduction

After unification, the East German economy changed dramatically. In the labour market, under- and unemployment rose. Programmes of active labour market policy (ALMP) were set up and quickly attracted a large number of participants, while at the same time absorbing considerable amounts of money. Public employment programmes (*Arbeitsbeschaffungsmassnahmen*, PEPs) constitute an important part of ALMP in the former East Germany. PEPs create additional jobs in the public or private sector through a wage subsidy paid by the labour office. More than two million people have participated in some kind of PEP since 1990. The main goal of PEPs is the reduction of unemployment, both directly and indirectly by improving the employability of PEP participants in the regular labour market through work experience and informal training. In this microeconometric evaluation study we estimate the effects of PEPs on participants in the period after they leave the PEP.

Only a few econometric evaluations have studied PEPs in East Germany.[1] Steiner and Kraus (1995) evaluate PEPs in East Germany with data from 1990 to 1992 using duration models. They find that, relative to non-participants, male PEP participants have a higher employment probability twelve months after PEP participation. In more recent work using more data and improved methods, Kraus, Puhani and Steiner (2000) find a significant negative effect of PEP participation. Both papers rely on the East German

[1] Many evaluations of active labour market policies have been conducted in the USA (see, e.g., the surveys by Heckman, LaLonde, and Smith, 1999, Heckman, Lochner, Smith, and Taber 1998, or LaLonde, 1995). However, the results they survey are not comparable to the results of our study due to the different institutional setting in the US labour market as well as different programme designs. Furthermore, many US evaluations use data from social experiments. Such data are not available for Germany.

Arbeitsmarktmonitor, a panel study that ceased in 1994. In addition, both papers handle selection effects (i.e., identify the causal effects of PEPs) using a parametric approach. Hence, these studies are subject to the doubts raised in the econometric evaluation literature about whether making more or less arbitrary parametric assumptions is the best way to identify programme effects.

In his research on employment policy in East Germany, Hübler (1997) also evaluates the effects of PEP participation on individual unemployment probabilities. Among the several approaches he uses, including a multinominal logit model and a random effects probit model combined with different methods to control for selective programme participation, the method closest to our approach controls for selectivity by selecting the individuals for the non-treatment sample based on a distance measure in observable variables between the treatment and non-treatment observations. The decision of how broad a set of individuals (in terms of the similarity of their characteristics to those of the participant sample) to include in the comparison sample is based on a pre-programme-test similar to the one employed by Heckman and Hotz (1989). The random effects probit model with the reduced sample is then used to estimate the programme effects. The results for the effects of PEP participation differ for the various specifications, but overall Hübler (1997) concludes that PEP participation does not create the expected positive effects.

Cockx and Ridder (2001) provides an example of a study in a European setting that is more in the spirit of nonparametric identification of treatment (PEP) effects. They discuss the effects of a wage subsidy for social security recipients on employment in Belgium. They argue that special institutional settings lead to a natural experiment, which is used to control for selection bias. They find that controlling for selection bias reverses the sign of the estimated effects relative to the results they obtain without controlling for selection bias. In their study, the effect of the wage subsidy is negative and statistically insignificant, which suggests that the wage subsidy prolonged welfare participation.

Puhani (1999) provides an example of the evaluation of ALMPs in a transition economy. He estimates the effects of several ALMPs in Poland, including training and PEPs, using a variety of methods that includes a nonparametric approach based on the conditional independence assumption (CIA) combined with a matching estimator. Overall, Puhani (1999) finds that participation in training decreases an individual's probability of unemployment, while participation in PEPs and other subsidised job schemes increases it.

The nonparametric methods employed in several of the studies just reviewed cannot be used in our study due to the institutional setting in East Germany and the nature of our data. As a result, we use other nonparametric assumptions to identify the causal effects of PEP participation. The key issue in identifying the causal effects of PEPs is eliminating the effect of selective participation by individuals. Because our data are not sufficiently informative to make the assumption that selection is only driven by observable variables (the conditional independence assumption or CIA) plausible, we base our analysis on an identification strategy suggested by Heckman, Ichimura, and Todd (1997). They propose using 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 PEPs at different dates, we extend their approach and combine it with a particular matching approach outlined by Lechner (1999).

The dataset available for this study is the *Arbeitsmarktmonitor Sachsen-Anhalt*, a panel based on the population of the East German state of Sachsen-Anhalt. Although the data have some drawbacks, it is, in our opinion, the best data available for evaluating PEPs in (a part of) East Germany. Therefore, we restrict our analysis to Sachsen-Anhalt.

Our results show a significant and substantial reduction in the probability of unemployment due to PEP participation in the period after the PEP ends. This finding is fairly robust to several different specifications and to different subpopulations. Interestingly, for men the reduction results from a higher employment probability among PEP participants, while for women it results from a higher probability of dropping out of the labour force.

The following section of the paper discusses PEPs in East Germany in general and in Sachsen-Anhalt in particular. Section 3 provides information on the institutional regulation of PEPs. Section 4 describes our data, while Section 5 outlines the econometric methods we use. Section 6 presents the results and Section 7 concludes. Several appendices contain further information on the data and the estimation methods as well as some additional results.

2 Stylized Facts

2.1 The Labour Market

In 1990, the economy of East Germany was not prepared for unification and the resulting changes in institutional settings and relative prices. The sudden switch to a market economy and increasing wages brought huge changes in the labour market such as worker displacement and rapidly decreasing labour demand. In reaction to these developments, the government, together with the labour offices, set up large labour market programmes. The major components are short-time work (*Kurzarbeit*, STW),[2] early retirement (ER), continuous training and retraining (CTRT), and public employment programmes (*Arbeitsbeschaffungs-massnahmen*, PEPs). Based on the number of participants as well as on the amount of money spent, ER and STW were the most important programmes in 1990 and 1991. From 1992 on, CTRT and PEPs replaced most of STW. ER remained important, but the number of participants slowly decreased because no new participants were accepted. With some "ups and downs”, due in part to financial restrictions, CTRT and PEPs remain important components of active labour market policy in East Germany.

The German Democratic Republic (GDR) had a labour force of 9.7 million people in 1989. In 1992, only 5.6 million people were regularly employed in East Germany, a drop of 40%.[3] After 1992, the number of employed persons increased but remained below 6 million. In 1991 and 1992, the majority of people leaving regular employment went into some kind of active labour market policy programme, but unemployment rose sharply as well. In 1992, more than one third of the total labour force was financed by the labour offices, either through unemployment benefits or active labour market policy. In 1996, this group was still 30 percent of the labour force. Focusing on the active labour market policies alone, the labour offices spent 32 billion DM in 1992 in East Germany, which had fallen to a still very substantial 16 billion DM by 1996.

The government required some time to set up PEPs, but by 1992 PEPs employed an average of around 400,000 people. Due to policy changes and financial restrictions, PEP participation decreased thereafter – as did participation in active labour market policies in general – to a level of between 235,000 and 315,000 persons at any given time. In total, more than 2 million people participated in PEPs between 1991 and 1997, and more than 40 billion DM were spent on them. Although there might be some differences between the states in East Germany, the general picture drawn in this section holds for Sachsen-Anhalt as well.

2.2 Sachsen-Anhalt

Sachsen-Anhalt is one of the new federal states. It is located to the south and west of Berlin, and borders several other new federal states as well as the former West Germany, but not Berlin. A rough sketch of the economic structure before unification shows an industrialised

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[2] Companies can introduce STW in response to temporary output reductions. Under STW, employees work a reduced number of hours, with the resulting income loss partly made up by payments from the labour office.

[3] This number does not include participants in active labour market policies.

southern part with a focus on the chemical industry, coal mining and energy, and an agricultural northern part. Table 1 provides some information about the economy of Sachsen-Anhalt compared to the average of the economies of the new federal states and the West German average.

Table 1 about here.

Table 1 shows that economic development in Sachsen-Anhalt from 1991 to 1997 was somewhat worse than the average for the new federal states. The share of the GDP in the new federal states produced in Sachsen-Anhalt decreased slightly over time. This pattern is reflected in the unemployment rate as well, with Sachsen-Anhalt having the worst unemployment rate among all the new federal states in 1997. However, the relatively poor economic situation in Sachsen-Anhalt is not reflected in a correspondingly higher share of PEP participants. Instead, the share of PEPs in Sachsen-Anhalt declines over time compared to the rest of East Germany.

2.3 PEP Participation

Tables 2 and 3 provide some data on PEP participation for East Germany and, if available, for Sachsen-Anhalt separately.[4]

Table 2 about here.

The use of PEPs reached a peak as early as 1992. Nevertheless, PEPs continued to be an important part of ALMP in the following years, as measured by both expenditures and participation. From 1992 to 1996 some tendency for the average PEP duration to increase is found when comparing the inflows and the stocks. A rising share of women in PEPs reflects

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[4] Further information and data about the East German labour market in general and PEP in particular can be found, e.g., in Wolfinger (1994, 1995) or Spitznagel (1993).

the unfavourable development of the labour market for women in East Germany as well as an increasing focus by the labour offices on persons with special labour market problems. In general the pattern is the same for Sachsen-Anhalt.

Table 3 about here.

We would expect the type of work a person performs in a PEP to influence his or her employment prospects in the regular labour market. Although there is no direct data available on the type of work PEP participants do, data on the number of PEPs in each sector paint the following picture. A majority of PEP participants work in sectors such as agriculture, infrastructure, building, site development, landscape building and environmental redevelopment. The share of PEPs in these sectors rises from 53% in 1992 to 62% in 1993 and then decreases slowly back to 50% in 1997. PEPs in the social services steadily increase their share from 13% in 1992 to about 22% in 1997. Administrative PEPs are roughly constant at about 8% of the total, while for the remaining PEPs no information is available.

Although complete statistics appear not to be available, there are some hints that PEPs in Sachsen-Anhalt differ from the average for all of the new federal states. Emmerich (1993, p. 6) states that in 1991/1992, 11% of all PEP participants are employed in so-called Mega-PEP, while this proportion is 21% in Sachsen-Anhalt.[5] Furthermore, although all new federal states spent additional money on Mega-PEPs, four fifths of the total amount in 1991/92 came from Sachsen-Anhalt (Emmerich, 1993, Table 7 and 8). Brinkmann and Wolfinger (1994, p. 17 and tables) show that in the second quarter of 1994 only 7% of all PEPs under §249h take place in Sachsen-Anhalt, but they employ 25% of all participants in such PEPs.[6] Furthermore,

[5] Mega-PEPs get unusually high subsidies from the labour offices. They usually employ a relatively large number of workers, and typically the good or service produced is more like investment than consumption compared to the average PEP. For example, workers in mega-PEPs may construct new infrastructure or clean up industrial sites (Emmerich, 1993, p. 6).

[6] PEP according to §249h is a somewhat different kind of PEP in East Germany, introduced in 1993. See Section 3 for more information on the different kinds of PEPs in East Germany.

there is a sectoral concentration of Mega-PEPs in Sachsen-Anhalt in coal mining, the chemical industry and steel, as well as a corresponding regional concentration.

3 Institutional Arrangements for PEPs in East Germany

In early 1990 the German Democratic Republic (GDR) adopted a labour law very similar to the West German one, the *Arbeitsförderungsgesetz* (AFG). After unification, the West German AFG came into force in East Germany as well, although many exemptions were added. The AFG contains several regulations on PEPs.[7] Here we will discuss PEPs according to §§91-96 AFG (*Arbeitsbeschaffungsmassnahmen*) and according to §249h AFG (*Produktive Lohnkostenzuschüsse Ost*), which were introduced in January 1993.[8] In most aspects relevant for this study the two programmes are similar, in formal terms as well as in their informal handling by the labour offices. Furthermore, our data do not allow us to separate them. Therefore, throughout this paper, the phrase public employment programme (PEP) includes both types of PEPs, unless we explicitly state otherwise.

Participants in a PEP are not directly employed or paid by the labour office. Rather they have a regular employment contract with a programme-supporting employer (PSE), which can be a public institution, a private nonprofit organisation, or a firm. This contract automatically ends at the end of the PEP. The labour offices reimburse the PSE for all or part of the wages paid.[9] PEP jobs must not replace existing jobs. Furthermore, it must be the case

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[7] The information given in this section draws on formal sources such as laws, as well as on informal sources, where the latter are important to catch the effects of the daily implementation of the programme as well as the use of exceptions. The major informal source was interviews we conducted in East German labour offices in 1996 and 1997. The interviews were partly questionnaire-led, partly free conversation, and were held in local labour offices and the *Landesarbeitsämter*, the regional head offices. Although there were no interviews in Sachsen-Anhalt, we expect the general results to be true for Sachsen-Anhalt as well. For more (formal) information on the regulations see the AFG, the *Sozialgesetzbuch III*, and corresponding legislation at lower levels.

[8] For convenience we use “regular PEPs” and “original PEPs” for PEPs according to §§91ff AFG and “§249h” as a name for PEPs according to §249h AFG. PEPs for unemployed over fifty-five years of age (*Arbeitsbeschaffungsmassnahmen für ältere Arbeitnehmer, §§97-99 AFG*) are not discussed because this age group is excluded from our empirical work.

[9] Normally between 50% and 75% of the wages are covered, with 100% covered in some special cases. Such special cases appear to be common practice in East Germany. In addition, the labour office could cover the

that the work done in the programme is useful to the public and would not be undertaken without the PEP.[10]

To control for selection bias it is important to know how a PEP gets created and why an individual participates. In practice, there were at least three different procedures through which PEPs were created in East Germany, although a sharp separation is not possible.

First, a company or a part of a company is to be closed. To avoid mass layoffs, the whole company, or a part of it, it transferred to a PEP. In this case, the decision about individual PEP participation is not based on individual considerations. Instead, exception regulations are used to transfer the workers into the PEP. Many programmes in 1991 and 1992 were of this kind.

Second, *Arbeitsbeschaffungs- und Beschäftigungsgesellschaften* (ABG) are used.[11] These institutions are 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. While the labour office takes care of financing and supervision, the ABG creates, organizes, and administers the PEP. Furthermore, ABGs provide information and help for the participants. It is not clear how the decisions about the participants are made or how the requirements for individual participation are checked when an ABG creates a PEP. In some cases, all new participants in ALMP are allowed to join an ABG. In other cases, participation is limited to certain groups, e.g., people formerly employed in a certain firm or people who joined the ABG at the founding.

Third, a PEP is created in the "normal" way, where normal means according to the regulations of the labour law and the common usage in West Germany. In this scenario, a future PSE proposes to create a PEP. If the labour office approves it, the labour office decides

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cost to set up a workplace or provide loans to cover that cost. The programme rules were tightened over time. After 1993 the labour office accepted only wages below 90% of the wage for a comparable unsubsidized job. For §249h programmes the subsidy is based on the average unemployment benefits the labour office would have had to pay otherwise.

10 A delay that would occur without the PEP is sufficient as well. For §249h there is a closed list indicating the types of work allowed.

about the participants as well.[12] Here, the individual requirements (described in more detail below) should usually be met.

It is not clear which of these forms is the most important, and there are (to our knowledge) no complete statistics available on ABGs or the use of exceptions in the labour law. It seems that the third possibility is typical for the smaller PEPs with only a few employees while PEPs created through the other ways typically employ many people. Unfortunately, our data do not provide any information on how a particular PEP is created.

The most important formal requirement for participating in a PEP is that the individual is unemployed and entitled to some kind of unemployment payments (or, since 1994, social assistance payments) just prior to participation. Furthermore, the person must have been unemployed for at least six months within the last twelve months.[13] People with special labour market difficulties are preferred. This includes the disabled, unemployed persons over fifty years old, persons below twenty-five without a vocational degree, the long-term unemployed and - a special regulation for East Germany - women. The labour office can depart from all formal requirements for social or labour market reasons.

Typically a PEP is limited to twelve months although both shorter and longer (up to twenty-four months and, under special conditions, up to thirty-six months) durations are possible. During the PEP participants get paid the *Tariflohn*, which in almost all cases is higher than unemployment benefits.[14] Beginning in 1994, payments were reduced to 90% of the *Tariflohn*, but private firms in East Germany often pay less than the *Tariflohn* as well,

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11 Many different names are used for this kind of companies.

12 In the interviews, officials who decide about PEP participation in the labour offices pointed out that neither the employer nor the individual participant has any influence on the participation decision.

13 For §249h the regulations are somewhat less tight. Only three months of unemployment within the last six months are required. Alternatively, being in a regular PEP or in STW (13 or more weeks with 10 percent or less working time) suffices as well.

14 *Tariflohn* is the wage rate trade unions and employers agree on in region- and sector-specific contracts. Unemployment benefits in Germany range from 53 to 68% of the former net income, depending on family status and duration of unemployment. The replacement rate changes over time. Therefore, although the wage rate for the PEP employment being may be much lower than the former personal wage, there is typically a financial incentive to participate in PEPs.

with the result that this change did not increase the incentive to leave PEP very much. Furthermore, during PEP social security contributions are paid on behalf of the participant, which could create new rights for unemployment benefits.

An individual leaves a PEP when the PEP ends. It is possible – and hoped for – that the PSE will employ the participant without subsidy after the PEP. Furthermore, an individual must leave a PEP immediately to take up a job offer on the regular labour market or for a training programme proposed by the labour office. Both the labour office and the participant are required to search for such possibilities during a PEP.

4 Data

4.1 The Arbeitsmarktmonitor Sachsen-Anhalt

The *Arbeitsmarktmonitor Sachsen-Anhalt* (AMM-SA) is primarily designed to provide information about the current situation of the labour market in Sachsen-Anhalt.[15] Creating a (panel) data set for scientific use is only a secondary goal. It contains a wide variety of questions including the respondent's labour market status now and a year ago, information about his or her job and employer, participation in active labour market policies, earnings, education, family and living arrangements, and personal opinions about political and labour market issues.

The AMM-SA is compiled from questionnaires mailed to individuals ages 15 to 65 living in the federal state of Sachsen-Anhalt. The first series of interviews took place in March 1992, and from autumn 1992 on it has been repeated every year. We use data up to the wave of autumn 1997. For each interview, between 10,000 and more than 20,000 questionnaires were mailed out, resulting in 6,000 to 10,000 available observations, equal to

[15] More information on the AMM-SA can be found, e.g., in Wiener (1995) or in Zentrum für Sozialforschung (1998; pages 11-15, 18-20, and in the last appendix).

0.3 to 0.5 % of the population. Of these, between 500 and 1,000 of the observations in each wave have experience with PEPs.

The general structure of the data is a mixture of cross-section and panel. Wave one and two are a panel, but for wave three a new cross-section was drawn. From wave four on, each wave includes both previously interviewed and newly drawn individuals. Panel mortality in the data is high. For example, of 9,252 persons participating in autumn 1993 only 1,268 persons (14%) have a valid interview in 1997.[16] Difficulties when using the panel structure, apart from the panel mortality, arise from changing definitions of the variables in the questionnaire over time and the failure to include all questions in all questionnaires. Nevertheless, for our study we consider the AMM-SA the best data currently available on PEPs in East Germany. Table 14 in Appendix A provides an overview of the variables used in this study.

4.2 Selection Rules and Descriptive Statistics

For people starting their working life by participating in a PEP, the effects of a PEP might be quite different than for people in the middle of their working life. Therefore our sample is restricted to persons who were at least 22 years old in 1993. This restriction allows us to use schooling as a time-constant variable in the estimation. If the first available observation is at age 55 or older, the person is deleted to avoid capturing the effects of early retirement. Participants in PEP of age 52 or older at the start date of PEP are excluded, because our interviews at the labour offices confirmed that PEPs were used to “bridge” the time to early retirement for older unemployed persons. In such cases, the purpose is not to increase the employment prospects of the participants.

[16] The age restriction accounts for the loss of 535 observations. For the remaining 8,717 dropouts no information is available. It could be that some are valid dropouts (e.g., the person died), but it has to be assumed that the major reason is non-response. No detailed non-response study is available, but we could not find any substantial (unplanned) changes in the observable composition of the samples due to either panel mortality or our selection rules (described below).

Further exclusions are made because of data limitations. Due to missing answers or limitations of the questionnaire, for some PEP participants neither the start nor the end date of the PEP is known. These observations are excluded. Because of low numbers of cases or inconsistent information, observations with certain characteristics were deleted from the sample. Table 13 in Appendix A presents more details on the construction of the analysis sample and on the numbers of observations lost due to each sample restriction. In addition we exclude persons with missing information for relevant questions from each estimation step separately.

In the sample of people not participating in PEPs, no additional observations had to be excluded because they were not eligible for PEP. Due to the exemption rules for PEP participation in East Germany all persons could have participated in a PEP, although the estimated probabilities of participation differ substantially among non-participants.

Table 4 (as well as Figure 4 in Appendix A) show that the distribution of start and end dates for PEPs in our sample is not uniform. We observe start dates between November 1989 and September 1997 in the original data set, but the selection rules we applied restrict the observations to those with PEP participation starting between April 1991 and September 1997. The median start date in the restricted sample is January 1992, and 65% of all PEPs start before September 1992. The median end date is June 1993, and 60% of PEP participation spells end between September 1991 and August 1993. In part, this reflects the pattern of PEP participation in East Germany as described above. However, the sample distribution of start and end dates is also driven by the unbalanced panel design of the data with different sample sizes in each year, and by the designs of the questionnaires.[17]

Table 4 about here.

[17] No complete information on start and end dates of PEP are available for September 1994 and 1995, while in September 1993 and 1996 questions about the complete PEP history along with dates were included, although no more than one spell was allowed.

A typical PEP duration is twelve months, and the regular maximum is twenty-four months. Table 4 and Figure 1 reveal that our data are consistent with these rules, with 17% of all PEPs ending after twelve months and only very few lasting longer than twenty-four months.[18] The mean PEP duration in the sample is 14.8 months, the median 12 months. Of all PEPs in the sample, 10% have duration less than or equal to three months (six months: 25%; twelve months: 65%; twenty-four months: 89%). For some observations in the sample PEP participation is right censored. Only a small fraction of these persons is still in PEP in September 1997. Most of the censoring is due either to panel attrition or to insufficient information in the questionnaires. These right censored observations could not be used for the final evaluation, but provided useful information for the estimation of the propensity score.

Figure 1 about here.

Splitting the sample in two groups, one with PEP participants and one with non-participants, we find some differences between them. Table 5 shows that people with a higher degree of formal education are less likely to participate in PEPs. The same pattern appears when comparing the groups according to the level of formal job education or the last known job position. Not surprisingly, unemployment is higher among PEPs participants.

Table 5 about here.

[18] Actually there are even more short PEP spells because the "start date" is coded as the first known start date and the "end date" is coded as the last known end date. Put differently, the data available only allow the identification of some spells for persons with multiple PEP spells.

5 Econometric Methodology and Empirical Implementation

5.1 The Causality Framework and the Targets of the Evaluation

The empirical analysis tries to answer questions such as "What is the average gain for PEP 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 or non-participation in PEPs has an effect on individual outcomes, such as earnings or employment status.[19] Therefore, the framework that guides 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 (PEP), while *n* denotes no PEP).[20] Additionally, denote variables that are unaffected by treatment - called *attributes* by Holland (1986) - by *X*. It remains to define a PEP participation indicator, *S*, that indicates whether person *i* participates in a PEP ($s_i = 1$) or not ($s_i = 0$). The observed outcome is $y_i = s_i y_i^p + (1 - s_i) y_i^n$. Hence, the causal effect, defined as the difference of two potential outcomes, can never be known, because the *counterfactual* $(y_i^p, s_i = 0)$ or $(y_i^n, s_i = 1)$ to the observed outcome y_i is unobservable. However, the quantity of interest required to answer the question raised at the beginning of this section is the average causal effect of PEPs for PEP participants, defined as θ_0:[21]

$$\theta_0 \equiv E(Y^p - Y^n \mid S = 1) = E(Y^p \mid S = 1) - E(Y^n \mid S = 1) = g^p - E(Y^n \mid S = 1). \tag{1}$$

[19] See Holland (1986) and Sobel (1994) for extensive discussions of concepts of causality in statistics, econometrics, and other fields.

[20] 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,...,N*) are assumed to be the result of *N* independent draws from the population.

[21] $E(\cdot|S=1)$ denotes the mean of the respective random variables in the population of PEP participants.

g^p can be consistently estimated by the mean of y_i in the subsample of PEP participants.[22] The problem is the term $E(Y \quad | S=1)$. Much of the literature on causal models in statistics and selectivity models in econometrics is devoted to finding useful identifying assumptions that allow the researcher to predict the unobserved expected nontreatment outcomes of the treated population by using the observed non-treatment outcomes of the untreated population $(y_i, s_i = 0)$ in different ways.[23]

5.2 *Identifying Restrictions*

5.2.1 Conditional Independence Assumption

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

$$Y \ \coprod S \,|\, X = x, \ \forall x \in \chi . \tag{2}$$

The CIA means that participation is independent ($\coprod$) of the non-treatment outcome conditional on the values of covariates or attributes *x* in the space χ. Thus $E(Y \quad | S=1, X=x) = \ E(Y^n \,|\, S=0, X=x)$, and θ_0 is identified. Compared to approaches based on fully specified parametric models, the CIA allows the researcher to estimate treatment effects directly without imposing the functional form or parametric assumptions that are often imposed when estimating a structural model. Subsequently, let θ_0^{CIA} denote the limit of an estimator that is consistent under the CIA assumption ($\operatorname{plim}_{N\to\infty} \hat{\theta}_N^{CIA} = \theta_0^{CIA}$):[24]

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[22] Note that in the application we estimate a slightly different parameter because, due to some non-representativeness in the sample and due to our sample selection rules described in the previous section, the distribution of participants in the population and in the sample could be somewhat different.

[23] This may include the outcomes of participants prior to PEP.

[24] E_X denotes the expectation over the distribution of X for participants.

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

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

To justify the CIA, the important task is to identify and observe all variables that could be mutually correlated with assignment and potential non-treatment outcomes. This implies that no important variable that influences both non-treatment outcomes and assignment given a value of the relevant attributes is left out. For this study the reasoning in the previous section suggests that X should include information on schooling, past job training and work experience, the complete labour market history of the person and individual socio-economic characteristics.

It is thus obvious that in our case the validity of the CIA requires a very informative data set. However, for PEPs in East Germany, such data are not (yet?) available. The data used in this study for Sachsen-Anhalt have several shortcomings. Employment histories are only available on a yearly basis, and information on employment before or at unification is missing. As a result, we might miss employment dynamics in the months just before PEP participation. Since unemployment is one important selection criterion, this omission invalidates the CIA for our data. In addition to this, 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.

A technical problem, related to the choice of variables to be included in X, is the potentially high dimension of X. A high dimensional X complicates the estimation of the conditional expectation. Let $P(x) = P(S=1|X=x)$ denote the propensity score, defined as the nontrivial probability ($0 < P(x) < 1$) of being assigned to a PEP 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 the CIA is valid, Rosenbaum and Rubin (1983) show that $Y^n \amalg S \mid b(X) = b(x), \forall x \in \chi$ holds, hence:

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

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.

5.2.2 The Conditional Bias Stability Assumption

At this point, it is useful to introduce the time dimension explicitly with $Y = \{Y_{-\mathrm{T}},...,Y_{-2},Y_{-1}, Y_1, Y_2, ..., Y_{\mathrm{T}}\}$, $Y = \{Y_{-\mathrm{T}},...,Y_{-2},Y_{-1}, Y_1^p, Y_2^p,...,Y_{\mathrm{T}}^p\}$ and $Y^n = \{Y_{-\mathrm{T}}^n,...,Y_{-2}^n,Y_{-1}^n, Y_1^n, Y_2^n,...,Y_{\mathrm{T}}^n\}$.

The period between –1 and 1 includes the time in PEP and may be longer than the other periods; it also varies individually. All other periods are equally spaced, with *-T,...,-1* and *1,...,T* indicating points in time where interviews take place, with the assigned value depending on the distance to the beginning and ending of the individual PEP spell, respectively. This notation leads to the following re-definition of the effect of the treatment on the treated in period *t*:

$$\theta_{t,0} \equiv E(Y_t^p - Y_t^n \mid S=1) = E(Y_t^p \mid S=1) - E(Y_t^n \mid S=1) = g_t^p - E(Y_t^n \mid S=1)\,,\; t = ...-1,1,2,.... \tag{1'}$$

In this framework, treatment is defined as participation in a PEP at some time in our observation period (*S* has no time index). Therefore, we allow for a treatment effect before any participation in a treatment (corresponding to a treatment such as “participating in a PEP τ time periods from now”).

In a recent paper, Heckman et al. (1997) propose an assumption that could be applied when there is at least one observation of the outcome before a PEP and one observation after a PEP. The idea is that although the CIA may not hold, it may be reasonable to assume that the bias due to failure of the CIA is the same for at least one period before a PEP (τ, $\tau < 0$) and one period after a PEP (t, $t > 0$). If the true effect of a PEP is zero for periods before the PEP, i.e., if the people do not change their relevant labour market behaviour in anticipation of future PEP participation, than an estimated treatment effect based on the CIA for period τ provides an estimate of the bias. This bias estimate can then be used to correct the estimate of the treatment effect for the period after the PEP.

To illustrate these issues more clearly, we introduce some additional notation. Equation (5) defines the average bias that occurs if the CIA is not true:[25]

$$B_t^{CIA} \equiv E_X[B_t^{CIA}(x_t) \mid S=1]; \quad B_t^{CIA}(x_t) \equiv E(Y_t^n \mid X = x_t, S=1) - g_t^n(x_t); \quad t = -T, \ldots, T . \qquad (5)$$

However, the validity of the CIA implies that $g_t^n(x_t) = E(Y_t^n \mid X = x_t, S=1)$. Therefore, the bias should be zero point-wise in the χ-space ($B_t^{CIA}(x_t) = 0, \forall x_t \in \chi$). If the CIA does not hold, Heckman et al. (1997) suggest an alternative assumption that will be called the Bias Stability Assumption (BSA) in the remainder of this paper:

$$B_t^{CIA}(x_t) - B_\tau^{CIA}(x_\tau) = 0 \quad \text{for at least one pair } (\tau, t),\ \tau < 0;\ t > 0,\ \forall x \in \chi . \qquad (6)$$

The quantity identified by BSA is defined as:

$$\theta_{t-\tau,0}^{BSA} \equiv \theta_{t,0}^{CIA} - \theta_{\tau,0}^{CIA} . \qquad (7)$$

The notation t-τ must not be interpreted to suggest that only the distance between t and τ matters. Rather, the exact values t and τ take do matter. Using the definitions of $\theta_{t,0}^{CIA}$ and $\theta_{\tau,0}^{CIA}$, we obtain the average bias:

$$B_{t-\tau}^{BSA} = B_t^{CIA} - B_\tau^{CIA} - \theta_{\tau,0}. \tag{8}$$

Equation (8) shows that the condition stated in Equation (6) is neither necessary nor sufficient for a zero average bias.[26] The necessary and sufficient condition is:

$$B_{t-\tau}^{BSA} = 0 \Leftrightarrow \underbrace{E(Y_t^n \mid S=1) - E_X[g_t^n(x_t) \mid S=1]}_{B_t^{CIA}} = \underbrace{g_\tau^p - E_X[g_\tau^n(x_t) \mid S=1]}_{B_\tau^{CIA} + \theta_{\tau,0}}$$

$$\text{for at least one pair } \tau,\ t,\ t \neq \tau,\ \forall x \in \chi. \tag{9}$$

From an economic point of view this condition is most easily interpreted when $\tau < 0$ and $\theta_{\tau,0} = 0$ holds, i.e., when there is no effect of a PEP $|\tau|$ periods before a PEP starts. This case implies, together with Equation (9), that the bias due to incorrectly assuming CIA - $E\{[Y_t^n - g_t^n(x_t)] \mid S=1\}$ - is the same for both periods t and τ.

The use of BSA as an identifying restriction has several advantages. First, it nests the intuitively appealing CIA assumption when $\theta_{\tau,0} = 0$ holds. In this case, if the BSA and the CIA are both correct, then a test of whether the estimated $\theta_{\tau,0}^{CIA}$ is zero is a joint test for the

25 For simplicity, in this section we assume that an infinitely large random sample is available for estimation without sampling error. Furthermore, in this and the following arguments we use *b(X) = X* as the balancing score. However, with an appropriate change of notation, the results also hold for balancing scores of lower dimension, such as those we use in the application.

26 It is not necessary to assume point-wise constant bias. On the other hand, (6) would only be sufficient if supplemented by the condition $\theta_{\tau,0} = 0$.

CIA and the BSA. If $\theta_{\tau,0} = 0$ does not hold, then it is conceivable that the CIA is still valid for $t > 0$ even though the BSA is violated.[27]

It is of practical relevance that that the BSA requires less information in period t to identify $\theta_{t,0}$ than does the CIA. The BSA is particularly useful because there is no need to use instruments. Instruments that, for example, satisfy the strong assumptions of Angrist, Imbens, and Rubin (1996), are hard to find in general and not available for this particular application. When X contains past values of variables that vary over time, another practical advantage of the BSA is that $\theta_{t,0}^{CIA}$ and $\theta_{\tau,0}^{CIA}$ need not be estimated using the same sample. Although a panel data set is still useful to keep the two bias terms B_t^{CIA} and B_τ^{CIA} small, the length any individual is required to be observed in the panel is not prolonged by the need to compute both $\theta_{t,0}^{CIA}$ and $\theta_{\tau,0}^{CIA}$.[28] We use this property extensively in our empirical application. Compared to conventional difference-in-differences estimators,[29] the estimator based on the 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.

However, there are also some problems with the application of this approach. It is not only that the computations for an adequate estimator become more burdensome; the more serious problem is that there are many potential “parameters” to choose from. In particular,

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[27] However, in this case the CIA must not involve pre-PEP realisations of outcome variables in the conditioning set.

[28] If we were to use the same PEP participants to calculate both conditional expectations, we would require that each observation remain in the sample for another $(t - \tau)$ periods. (Recall that the distance defined by $(t - \tau)$ is not necessarily equal to the corresponding mathematical calculation due to the variable length of the in-program period). As shown in Section 6, this would be disastrous in our application due to the high attrition rate in our data.

[29] See, e.g., the survey by Meyer (1995).

there is the issue of choosing an appropriate (t, τ, X) combination that makes the BSA valid. Unfortunately, economic theory and the nature of the institutions under consideration often do not provide much guidance in this regard. But, of course, the same problem exists for the widely used conventional difference-in-differences estimator.

5.3 Estimation Procedure

When estimating PEP effects using the nonparametric BSA, it makes sense to use a nonparametric estimation method. Matching methods have recently proved to be useful and flexible estimators for evaluations based on the CIA assumption (see, e.g., Heckman et al., 1997; Lechner, 1999). Since a "natural" estimator under the BSA is $\hat{\theta}^{BSA}_{t-\tau,N}$, the difference of two estimates obtained using the CIA, ($\hat{\theta}^{CIA}_{t,N} - \hat{\theta}^{CIA}_{\tau,N}$), we discuss matching estimators in this section. For simplicity, we stick to simple one-to-one matching, wherein we match only one non-participant observation to each PEP observation. Such an estimator is inefficient, but simplifies computations considerably. In the following we will denote the non-participants matched to the participants as *comparisons*.

Let us compare $\hat{\theta}^{A}_{t-\tau,N}$, $\hat{\theta}^{CIA}_{t,N}$, and $\hat{\theta}^{CIA}_{\tau,N}$ for two cases. First, assume that X is time-constant, so that both estimators $\hat{\theta}^{CIA}_{t,N}$ and $\hat{\theta}^{CIA}_{\tau,N}$ use the same values of the matching variables X. In that case, it makes sense to use a matching protocol that ensures that the same comparison observations are used as matches for both estimates. For ease of exposition, assume that the observations are ordered such that the N^p observations participating in PEPs are followed by the ($N - N^p$) non-participants. Denote a comparison observation j matched to PEP participant i as $j(i)$. Then, $\hat{\theta}^{A}_{t-\tau,N}$ ($= \hat{\theta}^{CIA}_{t,N} - \hat{\theta}^{CIA}_{\tau,N}$) has the following simple form:[30]

[30] We could call this a nonparametric difference-in-differences estimator. Appendix B.2 contains additional information about the estimators used and a discussion of their variances.

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

$$0 < i \le N^p;\ \ N^p < j \le N. \qquad (10)$$

However, as discussed above, if we aim to reduce the bias in both of the estimates $\hat{\theta}_{,}$ and $\hat{\theta}_{\tau,}$ used to compute $\hat{\theta}_{-\tau,}$ (as seems sensible, but not necessary), the conditioning set should also include lagged values of the outcome variables. If $Y_\tau \in X$, then the case $X = X_\tau$ is no longer sensible because it implies by definition that $\theta^{CIA}_{\tau,0} = 0$ and hence $\theta^{BSA}_{t-\tau,0} = \theta^{CIA}_{t,0}$.[31]

Therefore, it is obvious that there are cases in which the set of conditioning variables depends on the calendar time at which a PEP takes place. For example, in our empirical work we consider the unemployment after a PEP as an outcome variable, and unemployment 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 $\hat{\theta}_{\tau,}$ 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 $\hat{\theta}_{,}$. In the *bias correction sample* we assume an artificial PEP period to get $\hat{\theta}^{CIA}_{\tau=-1,N}$, the effect of the artificial PEP. Because of the BSA ($\theta_{\tau,0} = 0$) and because no PEP participation has yet taken place, this is an estimate of the bias. We use it to correct $\hat{\theta}^{CIA}_{,N}$.

Figure 2 about here.

[31] $\hat{\theta}^{CIA}_{\tau,N}$ will only be exactly zero if the sample the comparisons are drawn from is sufficiently rich to allow for "perfect" matches.

The assumptions of random sampling and of large N and N^p allow for the usual asymptotic approximation of the variance of matching estimators:[32]

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

Using standard laws of large numbers and central limit theorems, this estimator is consistent and asymptotically normal. The asymptotic variance can be estimated as follows:

$$\hat{Var}(\hat{\theta}^{BSA}_{t-\tau,N}) = \frac{1}{N^p}[(\hat{S}^{\Delta i}_{t-\tau,N})^2 + (\hat{S}^{\Delta j}_{t-\tau,N})^2]; \quad (12)$$

$$(\hat{S}^{\Delta i}_{t-\tau,N})^2 = \frac{1}{N^p}\sum_{i=1}^{N^p}[(y^p_{i,t} - y^p_{i,\tau}) - (\bar{y}^p_t - \bar{y}^p_\tau)]^2; \qquad \bar{y}^p_t - \bar{y}^p_\tau = \frac{1}{N^p}\sum_{i=1}^{N^p}(y^p_{i,t} - y^p_{i,\tau});$$

$$(\hat{S}^{\Delta j}_{t-\tau,N})^2 = \frac{1}{N^p}\sum_{i=1}^{N^p}[(y^n_{j(i),t} - y^n_{j(i),\tau}) - (\bar{y}^n_t - \bar{y}^n_\tau)]^2; \qquad \bar{y}^n_t - \bar{y}^n_\tau = \frac{1}{N^p}\sum_{i=1}^{N^p}(y^n_{j(i),t} - y^n_{j(i),\tau}).$$

6 Empirical Specification and Results

6.1 General Remarks

In this section the general considerations applied to choose an outcome variable and to set the choice parameters (t, τ, X) are explained. The details of the estimation are then discussed in the following sections. With respect to the outcome variable the labour market states unemployment and employment at specific periods t after a PEP are considered, because they constitute the official targets of the programme. It would also be very interesting to estimate

[32] The following formulas have been obtained using the approximation that the fact that the pairs have been selected by matching on an estimated score (rather than a known score) can be ignored. Appendix B.2.2 contains comparisons to inferences obtained from the bootstrap. It turns out that the differences are minor.

the earnings gains - if any - from PEP participation, but we did not succeed in constructing an earnings variable based on a definition that was consistent across all of the annual interviews.

First, we assume that the true effect of a PEP before the PEP starts equals zero, so that $\theta_{\tau,0} = 0$ for $\tau < 0$. The specific institutional regulations discussed in Section 3 make it difficult for an individual to anticipate participation in a PEP and thus to change their behaviour in light of potential PEP participation. Given the high unemployment rate in East Germany, it is unlikely that someone would leave a job in order to increase his or her chance of getting into a PEP. Therefore, estimating the effect of PEP with an estimator that is suitable if the CIA holds for a point in time (τ) before participation in PEP takes place leaves us with an estimate of the bias due to incorrectly assuming conditional independence. We further assume that this bias is on average identical for the *t* and τ chosen below, so that we can use the estimated bias to correct the estimate we get for *t* using the CIA.

Sample size considerations largely drive our choice of appropriate values for *t* and τ. Table 10 and Appendix D show that choosing *t* larger than 2 typically leads to a very small sample, especially when time-varying variables are included in *X*. This is due to the total length (at most six years) and the high attrition rates of the panel.

The choice of τ is also influenced by considerations of Ashenfelter's dip. It is commonly observed that the mean earnings of participants in active labour market programmes fall during the periods prior to participation. This pattern appears for employment as well. It is not at all clear whether this drop is transitory or permanent, but the choice of τ depends on this knowledge. In recent work, Heckman and Smith (1999) find for the US that the drop in earnings is not permanent. Furthermore, they find that apart from eligibility, participation in programmes depends heavily on changes in labour market status in the months just prior to the participation decision. Using different non-experimental estimators – including a difference-in-differences approach similar to the one used here – they show that the resulting bias can be reduced substantially by conditioning on variables influencing the individual programme participation decision.

Economic conditions in East Germany, especially rising long-term unemployment, suggest that a permanent dip in employment may be more likely in this context. Individual decisions play no role in the participation process, and therefore individual shocks in labour market status may have less relevance. Nevertheless, in the matching process we include variables that take account of labour market status (employment and unemployment) prior to PEP. Furthermore, in this problematic labour market any "temporary" shock leading to unemployment will soon become permanent due to the lack of job offers. When there is no transitory dip, it is intuitively most plausible to choose the relevant points in time (t and τ) as close together as possible in order to make the social and economic environment as similar as possible. A permanent dip would call for a τ near to the start of the PEP as well. Therefore, and also because of sample size considerations, in most cases presented here τ is set to the most recent interview before a PEP ($\tau = -1$).

When choosing the variables to be included in X, our main strategy is to minimise the bias in $\hat{\theta}^{CIA}_{\tau,N}$ and $\hat{\theta}^{CIA}_{t,N}$. Although intuitively appealing, this need not necessarily be the best strategy, because all that is needed is that $\hat{\theta}^{CIA}_{\tau,N}$ and $\hat{\theta}^{CIA}_{t,N}$ are equally biased. There is also a trade-off in our data between sample size and the number of components in X (particularly for time-varying variables). In light of these considerations, we present results with both rich and less rich specifications of X.

Apart from the propensity score and indicators for interviews in particular waves, the richest version includes variables related to labour force status (unemployed or employed), job position, firm size, industrial sector (agricultural, chemical, public) of the current or former employer, highest vocational degree, and regional unemployment rate. Unemployment, especially long-term unemployment, is an important reason for PEP participation, and we use labour force status as one indicator for this. The employer variables are included because of their relationship with the probability of participating in a PEP, especially in one of the large employer-founded PEPs described in Section 3. The agricultural, chemical, and public sectors were hit harder than average by the economic changes and laid off more employees. Furthermore, there were large numbers of PEPs in these sectors, which again justifies their inclusion in the matching. We include the regional unemployment rate because the amount of money the labour offices can spend on PEPs partly depends on it. Due to sample size considerations, the variables measuring the employment history cover at most two periods.

We need to choose a balancing score that takes account of the fact that our richer specifications include time-varying variables in X, and that PEPs start at different dates for different individuals. We rely on a split balancing score, $\left(V\beta_0, M\right)$, that combines an index of the participation probability based on time constant variables ($V\beta_0$, where V denotes the time-constant variables in X and β_0 is a fixed parameter vector) with M, which denotes the time-varying variables in X. In computing the value of M for each matched non-participant observations, the PEP start date of the treated observation to which they are matched is used. The estimation of $V\beta_0$ is discussed below. Some details regarding the matching process are presented in Appendix B.1. Lechner (1999) discusses the use of different starting dates for the computation of $\hat{\theta}_{t,N}^{CIA}$ extensively, including the approach used here. For more details, especially regarding the necessary additional assumptions and issues of computation, the reader is referred to that paper.

6.2 Propensity Score

We specify the propensity score as a probit model and test the specification extensively using tests against heteroskedasticity, omitted variables, nonnormality (score tests) and general misspecification (information matrix test). The probit specification includes variables capturing sex, age, schooling, having a university degree or a degree from a technical school as well as the regional unemployment and PEP rates and disaggregated local information.[33] We also include variables indicating panel participation and interactions of panel participation with sex. Tables 6 and 7 present the results of the estimation as well as the tests.

Table 6 about here.

Table 7 about here.

The estimation results show a lower conditional participation probability for the youngest age group (22 to 25 in 1993, chosen as the reference group) compared to all other age groups. A low school degree (chosen as the reference group) increases the probability of participating in a PEP. A degree from a technical school or a university leads to a lower estimated probability, although the coefficient on university is only significant at the 6% level. Regional heterogeneity is substantively important and significant as well. The positive sign on the unemployment rate corresponds to our expectations, while the PEP rate is insignificant.

The coefficients on the dummy variables for cities and areas show more diversity in PEP participation probabilities than can be accounted for by regional variation in the unemployment rate (note that cities and areas are subdivisions of the regions). Cities with their different and much more diversified economic structure, and their generally lower unemployment rates, have lower participation probabilities. The positive signs among the area and region dummies pick out those areas and regions with either a high concentration of a specific industry or with a single but very large PEP. The concentration of PEPs (especially of

[33] See Table 14 in Appendix A for more information on the definitions of the variables.

large PEPs) differed across industries, and the local concentration of specific industrial sectors was high in the GDR, which explains at least part of the findings. An example is the area of Bitterfeld with a high concentration of the chemical industry as well as an above average PEP concentration. Estimating the propensity score separately for males and females does not substantially affect the results.[34]

6.3 Evaluation Results

We start the discussion of the evaluation results for several specifications of X by considering the least demanding case. This is the case where only the propensity score and indicators for a valid interview in a particular wave are used for matching. The results in Table 8 show how the number of valid observations depends on the length of time the individual is required to remain in the sample before or after a PEP. The estimates for periods prior to PEP participation clearly indicate that this specification leads to a biased estimate of $\hat{\theta}_{t,N}^{CIA}$.

Table 8 about here.

The following tables provide information on the average expected unemployment probability in the sample of PEP participants (employment and out of the labour force probabilities in Table 12), conditional on participation in a PEP, $\hat{E}(Y_t^p \mid S=1)$, and on not participating in a PEP, $\hat{E}(Y_t^n \mid S=1)$.[35] The corresponding estimate of the causal effect $\hat{\theta}_{t-\tau,N}^{\text{BSA}}$ under the BSA is presented as well.

When correcting the bias, there are two ways to proceed. First, one could only use those observations observed at least one period before and on period after a PEP. This reduces

[34] Separate estimates of the propensity score were used for the sex-specific results presented in the following section. These results are available on request from the authors and are also included in Eichler and Lechner (1998).

[35] The first of these values is the actual sample mean while the second is the estimated counterfactual mean from the BSA estimator.

the sample size after a PEP considerably, as can be seen from the results in Table 9. Second, one could use all of the observations available at each time period. Results for this alternative appear in Table 9 as well. In both cases we show results with τ symmetric to t $(\tau = -t)$ and with τ fixed at –1 (τ = –1). For the reasons explained above, we prefer the latter version. The (τ = –1) results for the unbalanced panel in Table 9 correspond (with rounding error) to the estimates constructed under the CIA in Table 8.

For the case of a symmetric choice of t and τ, no significant effects appear. However for t larger than 1 and τ fixed at –1, significant positive effects of PEPs appear in the unbalanced design. With respect to the potential for non-ignorable panel attrition, we note that the results based on the balanced and unbalanced designs are not contradictory.

Table 9 about here.

In order to reduce the bias in both $\hat{\theta}_{t,N}^{\text{CIA}}$ and $\hat{\theta}_{\tau,N}^{\text{CIA}}$, we now add variables measuring labour market status prior to PEP participation to X. From the discussion in Section 3, it is clear that previous unemployment is an important component of the participation decision. In addition to unemployment, we now include now all of the variables discussed in Section 6.1. In light of the issues discussed there, as well as concerns about small sample sizes in the balanced design, we consider only the case of $\tau = -1$ and an unbalanced design.

The right-hand side of Table 10 shows that the bias under the CIA is indeed slightly reduced. Furthermore, the matches with respect to unemployment in both the evaluation sample and the bias correction sample are good, as the values for $\tau = -1$ and $\tau = -2$, respectively, indicate. (Matching in the bias correction sample is done at the last interview prior to the artificial PEP, which in most cases occurs in period $\tau = -2$). Appendix C contains a more detailed analysis of the quality of the matches. The estimate for $\tau = -1$ in the bias correction sample indicates that there remains a substantial bias in the CIA estimates (assuming that the BSA is correct). Figure 3 displays the results from Table 10.

Table 10 about here.

Figure 3 about here.

Table 11 displays our primary results. The estimate of –17 for $t = 1$ indicates a substantial individual gain (i.e., a reduction in the probability of unemployment) 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 52% to 35% at the time of the first interview after the PEP. The results for higher values of t indicate that this is not just a short-term effect, although for t = 3 the number of observations is too small to draw any strong conclusions. Note that the estimates obtained under the BSA reverse the conclusion that would be drawn from the estimates based on the CIA, even when using the richest specification of matching variables available in the data.

Table 11 about here.

The bottom two panels of Table 11 show sex-specific results. Some of the tests we performed on the participation probit (see Appendix C) suggest that such a split is necessary. Furthermore, as described in Section 2.3, the sex composition of PEP participants changed dramatically over time. Although the estimates are less precise due to the reduced sample sizes, the differences in the estimates for men and women are not very large and confirm the previously obtained results. Note that the effects of PEP are similar even though the baseline levels of unemployment differ substantially for the two samples.

Table 12 provides information on the effects of PEP participation on regular employment and on the probability of being out of the labour force. It reveals that the reduced unemployment probability at the first interview after PEP is due to a higher employment probability. For men this result holds (more or less) for longer periods after PEP as well, but for women from $t = 2$ on, a large share of the reduced unemployment probability results from a higher probability of being out of the labour force. This finding is consistent with the hypothesis that women drop out of the labour force when their unemployment benefit period ends.[36]

Table 12 about here.

We also studied the (intermediate) case of using only unemployment and employment as time varying match variables. Other specifications we tried used information on employment histories for two periods. Many specifications with different combinations of these variables and the previously discussed variations (balanced and unbalanced design, choice of t and τ, X variables used in matching, different outcome variables, separate estimates for men and women) have been checked. Some of these results are contained in Appendix D; more can be found in Eichler and Lechner (1998). None of the results from these specifications differ substantially from the results presented in this section.

With the limited information available in the data we could not find any indication of contamination bias arising from participation in other kinds of training by PEP participants. Nevertheless, contamination is a potential problem. Using a preliminary release of the data for 1997 and 1998 with consistent PEP and training information since 1990 and applying our exclusion rules, we found 689 persons who participated in PEP, roughly 10% of the sample. Of these, 321 had some kind of training as well, 115 after the end of their PEP spell. On

[36] PEP participation creates a new claim for unemployment benefits.

average, persons with training before the PEP participation are older, and more likely to be female. These patterns do not hold for people with training after PEP; instead, this group has on average less formal education (both formal schooling and occupational training). These numbers overstate the problem in this study because they use a balanced structure from 1990 to 1997/1998, which differs from the structure of the data used here. Apart from the descriptive information just reported, we examined subsets of our data with consistent training definitions to check for contamination bias. However, what we could do was limited by the fact that both the definitions of, and information on, training in the survey data change substantially between waves and the fact that subpopulations with consistent information get rather small.

7 Conclusion

Using nonparametric difference-in-difference methods, this paper analyses the effects of public employment programmes (PEPs) in East Germany. Because there do not appear to be any suitable data accessible to the scientific community for all of East Germany, we restrict our analysis to the East German state of Sachsen-Anhalt. The available data make the use of difference-in-differences methods almost imperative in order to plausibly identify the effects of PEPs.

Our main findings suggest that individuals participating in a PEP benefit considerably from doing so, as their unemployment risk is reduced by a substantial amount. Furthermore, there is some indication that this is not just a short-term effect, although inference gets difficult because of decreasing sample sizes for longer periods after a PEP. For men, the majority of the reduced unemployment risk results from an increased employment probability. For women, that finding only holds for the first period after PEP; for later periods the effect of

PEP on non-participation in the labour force dominates. These general findings appear when using several different specifications.

However, one should be careful in interpreting these results. Individual gains for PEP participants do not necessarily translate into benefits for the economy as a whole, because we ignore both the costs of the PEPs and any general equilibrium effects of the program. The latter can arise from negative impacts on competing private firms as well as from programme-related changes in the labour market behaviour of non-participants. No estimates of such general equilibrium effects of PEPs exist for East Germany, but Steiner, Wolf, Egeln, Almus, Schrumpf, and Feldotto (1998) find some hints that the positive effects found at the micro level by earlier research on public training programs in East Germany do not correspond to similar effects at the macroeconomic level.[37] If their finding holds for PEPs as well, then a cost-benefit analysis based on the results of this study would not be valid. Furthermore, such a full social cost-benefit analysis would have to take account of other goals and effects of PEP, about which we have no information.[38]

Apart from combining the results of this study with information on costs, other benefits of PEPs, and market interactions in order to undertake a cost-benefit-analysis, future research should consider the robustness of the results. Furthermore, in formulating future labour market policy with regard to PEPs it would be useful to examine the heterogeneity of the effects as a function of the individual characteristics of the participants as well as of the characteristics of the PEP itself. For these tasks, better data would help a lot.

[37] This comparison of micro and macro estimates can provide no more than a hint about possible crowding out or substitution effects. Furthermore, the debate regarding the individual effects of training in East Germany has not yet settled (see, e.g., Lechner 1999, Kraus, Puhani, and Steiner 1999). Puhani (1999) finds some hints of substitution effects of publicly funded training programs in Poland, but his results are sensitive to choices regarding the data and the econometric specification.

[38] A prerequisite for a PEP is that it produces output that is useful for the public, such as local infrastructure or social services. In addition, apart from the improved employment chances of participants, PEP could also increase future wages through improvements in human capital.

Appendix A: Data

Table 13 about here.

Figure 4 presents an overview of the distribution of start and end dates of PEPs in our sample (see also Table 4 and Figure 1 in Section 4.2). The sample used here and in the following descriptive statistics is the sample used for the estimation of the partial propensity score (12,565 No-PEP observations; 1,123 PEP observations); that is, it is the sample that results after all selection rules are applied. See Table 13 for details. The number of observations used for the computations depends on the observability of the information in the sample.

Figure 4 about here.

Table 14 provides information on the definitions of the variables used in this paper, while Table 15 presents some comparable descriptive statistics for PEP participants and non-participants in the sample.

Table 14 about here.

Table 15 about here.

Appendix B: Econometrics

B.1 Matching Protocol

This section gives the details of the matching protocol used for the evaluations. Here V contains the time-constant variables used in the estimation of the propensity score, and M contains the time-varying variables that were separately included in the matching.

Step 1: Split the observations into two exclusive pools depending on whether they participated in a PEP (P-pool) or not (C-pool).

Step 2: Draw randomly an observation from the P-pool (denoted by *i*) and remove it from the P-pool.

Step 3: Compute the time-varying variables *M* in relation to the start date for observation *i* for the observations in the C-pool.

Step 4: Denote these variables (and perhaps other variables already included in *V* as well) as $\tilde{m}_j$ and $\tilde{m}_i$, respectively. Define a distance between observation *i* and each comparison *j* as $d(j,i) = (v_j\hat{\beta}, \tilde{m}_j)' - (v_i\hat{\beta}, \tilde{m}_i)'$, where $v\hat{\beta}$ denotes the estimated partial propensity score. Choose the comparison *j* such that has the smallest Mahalanobis distance $a(j,i) = d(j,i)'Wd(j,i)$, where *W* denotes the inverse of the estimated variance of $(v\hat{\beta}, \tilde{m})'$ in the C-pool (computed for a given start date).

Step 5: Remove *j* from the C-pool.

Step 6: If there are any observations left in the P-pool, start again with step 2.

This matching algorithm is similar to the "partial propensity score" algorithm suggested by Lechner (1999). See that paper for more details and a comparison to other ways of handling time-varying variables within a matching framework when the programme start date varies among participants. When matching is done for the bias correction samples (τ-samples, $\tau = -1$), step 3 has to be changed as follows:

Step 3-τ: For all treated observations compute an artificial start date equal to the month of the last interview before PEP minus the duration of the PEP. Then, take the comparisons and compute the time-varying variables *m* in relation to the artificial start date for observation *i*.

B.2 Variance of $\hat{\theta}^{BSA}_{t-\tau,N}$ for Unbalanced Panels

B.2.1 Asymptotic Approximation

In this appendix, $\hat{\theta}^{BSA}_{t-\tau,N}$ is generalized to the case of random attrition. In that case, there are not only different comparisons in periods t and τ, but also different treated observations. Denote the number of treated observations observed in period t (τ) by $N^p_t \leq N^p$ ($N^p_\tau \leq N^p$). Furthermore, denote the set of observations observed in period t (τ) by D_t (D_τ).

$$\hat{\theta}^{BSA}_{t-\tau,N} = \frac{1}{N^p_t}\sum_{i=1}^{N^p} \underline{1}(i \in D_t)(y^p_{i,t} - y^n_{j(i),t}) - \frac{1}{N^p_\tau}\sum_{i=1}^{N^p} \underline{1}(i \in D_\tau)(y^p_{i,\tau} - y^n_{j(i),\tau}); \qquad N^p < j, j' \leq N. \quad (13)$$

Let $\underline{1}(\cdot)$ denote the indicator function that equals one if its argument is true and zero otherwise. Of course, in practice no comparison observation is matched to a treated observation that is not observed in the particular sample. To obtain the variance we may rewrite Equation (B.1) as follows:

$$\hat{\theta}^{BSA}_{-\tau,N} = \sum_{i=1}^{N^p}\left\{\left[\underline{1}(i \in D\ )\frac{y^p_{i,}}{N^p} - \underline{1}(i \in D_\tau)\frac{y^p_{i,\tau}}{N^p_\tau}\right] - \left[\underline{1}(i \in D\ )\frac{y^n_{j(i),}}{N^p} - \underline{1}(i \in D_\tau)\frac{y^n_{j(i),\tau}}{N^p_\tau}\right]\right\}. \quad (14)$$

Since the treated and comparison observations are independent, we can estimate the variance of the two expressions in the square brackets separately.

$$Var(\hat{\theta}^{BSA}_{t-\tau,N}) = Var(P_{t-\tau}) + Var(C_{t-\tau}); \qquad (\hat{\theta}^{BSA}_{t-\tau,N} = P_{t-\tau} - C_{t-\tau}); \quad (15)$$

$$Var(p_{t-\tau}) = N^p Var\left[\underline{1}(i \in D_t \wedge i \in D_\tau)\left(\frac{y_{i,t}}{N_t^p} - \frac{y_{i,\tau}}{N_\tau^p}\right)\right] +$$

$$+\frac{N^p}{N_t^p} Var\left[\underline{1}(i \in D_t \wedge i \notin D_\tau)\left(y_{i,t}\right)\right] + \frac{N^p}{N_\tau^p} Var\left[\underline{1}(i \notin D_t \wedge i \in D_\tau)\left(y_{i,\tau}\right)\right]. \qquad (16)$$

The three variances needed to compute $Var(P_{t-\tau})$ can be estimated by the corresponding empirical variances in the three sub-samples defined by the observability of the treated. The estimation of $Var(C_{t-\tau})$ follows exactly the same rule.

B.2.2 A Comparison to Bootstrapped Standard Errors

Because the asymptotic approximation of the standard errors ignores the estimation of the propensity scores as well as the mechanics of matching algorithm, Table 16 compares the estimates obtained using the asymptotic approximation to estimates obtained from 300 bootstrap samples for our most preferred specification given in the upper part of Table 11. It turns out that the approximate asymptotic standard errors are somewhat conservative. The distribution appears to be symmetric. Overall, the differences between the two sets of estimates are very small, and confirm our basic conclusions based on the asymptotic approximation.[39]

Table 16 about here.

Appendix C: Match Quality

A basic requirement for a successful implementation of a matching algorithm is a sufficiently large overlap between the distributions of the conditioning variables in the treated and untreated sub-samples. Figure 5 shows that this overlap exists for the balancing score

[39] The only large change occurs for period 3, presumably because of the small number of observations. However, the imprecision of the estimates for this period is already clearly indicated by the large standard errors obtained from the asymptotic approximation.

employed in our analysis. The fact that matching is successful for key time-varying variables in Section 6 indicates that the overlap condition also holds for those variables.

Figure 5 about here.

Table 17 shows the differences between the treated and comparison observations for the variables appearing in the propensity score. The final two rows of Table 17 contain two summary statistics related to match quality based on the variables mentioned in the table and some further variables. MSB denotes the median of the absolute biases of the means (i.e., the median difference in means) normalised by the average standard deviation (see, e.g., Rosenbaum and Rubin, 1985, for a motivation for this statistic). JW denotes a quadratic distance measure for the mean biases (differences between the two sub-samples), weighted by the inverses of their covariance matrix (see the note on Table 17 for details).

The table shows that the PEP participants and corresponding matched comparison samples do not differ significantly with respect to the variables included in the propensity score (here for the case using only time-varying variables at the last interview before a PEP). However, it can also be seen that the means of the evaluation sample and the bias correction sample differ in particular with respect to sex. This is as expected because the latter requires more of the individual labour market "history" than the former, with the result that PEPs tend to take place later in the bias correction sample than in the evaluation sample. Since the policy of the labour office changed over time, this in turn leads to the increase in the fraction of women in PEPs that is reflected in the numbers in Table 17. This does not necessarily imply that the analysis given in the main body of the text is flawed, because all that is needed is a time-constant bias. However, this assumption appears to be more reasonable if the distribution of characteristics is also constant over time. The sex-specific results, presented in Table 12 in the main body of the paper, indicate that the unequal distribution of sex in the two samples does not have a significant impact on the results.

Table 16 about here.

Appendix D: Evaluation Results for Different Specifications

In an attempt to further reduce the bias under the CIA, we examined a specification of X that includes the same variables as the X used for the results presented in Tables 10 and 11, but adds values for the time-varying variables for additional interview prior to the PEP. The results from this specification appear in Tables 18 and 19. The match quality with respect to unemployment is again good. The results for the bias correction sample again indicate a positive bias for the CIA results, although the bias is reduced through the addition of the additional conditioning variables. At the same time, probably due to the reduced sample size, $\hat{\theta}_{t,N}^{\text{CIA}}$ is not significantly different from zero. Therefore, it is not surprising that $\hat{\theta}_{t-\tau,N}^{\text{BSA}}$ is not significantly different from zero as well.

Table 18 about here.

Table 19 about here.

Tables 20 and 21 contain results similar to Tables 10 and 11 in the text. However, the variables related to the (last) employer and to vocational degrees, as well as the regional unemployment rate, are not used in the matching.

Table 20 about here.

Table 21 about here.

We examined other specifications as well, such as combinations of the above specifications, splitting the sample by sex, and/or using employment and out of labour force as the dependent variables. The results did not differ substantially from the results presented in the paper.

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Table 1: Comparing Sachsen-Anhalt with the New Federal States

	GDP					Unemployment (in %)		Participants in PEPs[c)]		
	Total			Per head[b)c)]				Total		
	SA[a)]	NFS[a)]	SA in % of NFS	SA	NFS	SA	NFS	SA	NFS	SA in % of NFS
1991	36	206	18 %	19	19	10	n.a.	35	183	19%
1992	46	263	18 %	24	25	15	15	88	388	23%
1993	55	305	18 %	29	29	17	16	n.a.	250	n.a.
1994	61	355	17 %	32	33	18	16	61	280	22%
1995	64	381	17 %	34	36	17	15	64	312	21%
1996	66	397	17 %	35	37	19	17	58	277	21%
1997	70	422	17 %	37	39	22	18	n.a.	234	n.a.

Sources: *Statistisches Jahrbuch für die Bundesrepublik Deutschland*, several issues; several special issues of *Zeitreihen für ausgewählte Arbeitsmarktindikatoren*, *Landesarbeitsamt Sachsen-Anhalt/Thüringen;* ANBA (*Amtliche Nachrichten der Bundesanstalt für Arbeit)*, several issues; *Arbeitsmarkt in Zahlen: Arbeitsbeschaffungsmaßnahmen, Berichtsmonat September 1998*, *Bundesanstalt für Arbeit*, Tables 1 and 2; *Arbeitsmarkt in Zahlen: aktuelle Zahlen, September 1998, Bundesanstalt für Arbeit*, p. 52; *Institut für Arbeitsmarkt- und Berufsforschung, Zahlenfiebel 1995*, Table 7.2; *Statistisches Jahrbuch des Landes Sachsen-Anhalt 1997;* information from IWH Halle July 1998.

Notes: SA = Sachsen-Anhalt; NFS = all new federal states (including East Berlin and Sachsen-Anhalt). 1.9 million people live in SA, 18% of the population of the NFS.

See Section 3 for a definition of the different kinds of PEPs and a description of the PEP measure employed in this paper.

n.a. = not available.

a) In billion DM.

b) In relation to the population of persons ages 15 to 65 (NFS does not include East Berlin).

c) In thousands.

Table 2: Expenditures on PEPs by the Labour Offices

	NFS			SA		
	Total	PEP[a)]	§249h[b)]	Total	PEP[a)]	§249h[b)]
1991	3,075	3,075	---	612	612	---
1992	5,083	5,083	---	1,664	1,664	---
1993	6,905	6,811	94	1,388	1,359	29
1994	4,722	3,397	1,325	1,680	1,359	321
1995	7,109	5,832	1,277	1,734	1,416	318
1996	8,156	6,835	1,321	1,701	1,422	279
1997[c)]	6,703	5,322	1,381	1,422	1,136	286

Source: ANBA, several issues.

Notes: In million DM per year; SA = Sachsen-Anhalt; NFS = all new federal states (including East Berlin and Sachsen-Anhalt).

See Section 3 for a definition of the different kinds of PEPs and a description of the PEP measure employed in this paper.

a) Regular PEP only.
b) §249h was introduced in 1993.
c) The data include West Berlin starting in September 1997.

Table 3: Participants in PEPs

	In East Germany						In Sachsen-Anhalt					
	Inflow in PEPs		Average number of persons employed in PEPs				Inflow in PEPs		Average number of persons employed in PEPs			
	Original PEP		Original PEP		§249h		Original PEP		Original PEP		§249h	
Year	Total	Percent female	Total	Percent female	Total	Percent female	Total	Percent Female	Total	Percent female	Total	Percent female
1991	422	37%	183	36%	0	---	n.a.	n.a.	35	n.a.	0	---
1992	296	44%	388	41%	0	---	59	n.a.	88	n.a.	0	---
1993	243	53%	237	48%	23	n.a.	51	51%	50	44%	n.a.	n.a.
1994	293	60%	192	60%	88	36%	58	n.a.	40	62%	21	33%
1995	222	63%	206	65%	106	39%	43	n.a.	41	69%	23	33%
1996	235	62%	191	65%	86	43%	50	n.a.	40	66%	18	39%
1997[a)]	142	58%	154	64%	80	44%	n.a.	n.a.	33	n.a.	n.a.	n.a.

Source: ANBA, several issues; *Arbeitsmarkt in Zahlen: Arbeitsbeschaffungsmaßnahmen, Berichtsmonat September 1998*, *Bundesanstalt für Arbeit*, Tables 1 and 2; *Arbeitsmarkt in Zahlen: aktuelle Zahlen, September 1998, Bundesanstalt für Arbeit*, p. 52; *Institut für Arbeitsmarkt- und Berufsforschung, Zahlenfiebel 1995*, Table 7.2; *Statistischer Bericht A VI 7 i/96 des Stat. Landesamts Sachsen-Anhalt*, 1996: Table 14, 15, 1994: Table 8, 9, 1995: Table 14, 15, 1993: Table 9; *Statistisches Jahrbuch des Landes Sachsen-Anhalt 1997*; information from IWH Halle July 1998.

Notes: In 1,000 persons.
See Section 3 for a definition of the different kinds of PEPs and a description of the PEP measure employed in this paper.
n.a. = not available.
a) The data include West Berlin starting in July 1997.

Table 4: Start and End Dates of PEPs and PEP Durations in the Sample

	Start of PEP (date)	End of PEP (date)	Duration of PEP (months)
Mean	October 1992	November 1993	14.8
10th percentile	June 1991	February 1992	3
25th percentile	August 1991	July 1992	6
Median in sample	January 1992	June 1993	12
75th percentile	November 1993	July 1995	20
90th percentile	August 1995	August 1996	30
Number of observations	1123	778	778

Source: Own calculations.

Notes: The sample is similar to the sample used to estimate the partial propensity score.

Table 5: Descriptive Statistics of Selected Variables for Persons With and Without PEP

	No PEP	PEP
	Mean in subsample or share in subsample in %	
Age (1993)	38.5 years	38.8 years
Sex: Female	52	52
Schooling: Highest degree		
University entrance degree (*Abitur*)	28	23
Medium (*Klasse 10*)	54	54
Low (*Klasse 9*)	18	22
University degree	18	15
Unemployment: Registered as unemployed in:		
March 1991	6	16
September 1992	10	15
September 1994	9	25
September 1996	10	30
September 1997	11	42
Unemployment rate in region (mean)[a)]	21	22
PEP rate in region (mean)[b)]	4	4
Living in:		
City of Dessau	4	2
City of Halle	12	5
City of Magdeburg	11	8
Area of Aschersleben-Straßfurt	4	6
Area of Anhalt-Zerbst	3	3
Area of Bitterfeld	4	7
Area of Bördekreis	3	3
Area of Quedlinburg	3	6
Area of Wernigerode	4	3
Maximum number of observations	12,565	1,123

Note: Some statistics are generated with fewer observations because of missing information. See Table 14 in Appendix A for the definitions of the variables.

a) Mean over time (91-97) of unemployment rates in eight regions in Sachsen-Anhalt weighted according to the relevant regional distribution.

b) Mean over time (91-97) of PEP participation rates in eight regions in Sachsen-Anhalt weighted according to the relevant regional distribution.

Table 6: Propensity Score: Estimated Coefficients and Results from the Score Test Against Heteroscedasticity

	Estimation				Heteroscedasticity test	
Variable	Coef.	Std. Error	P-value	Average derivative[a)]	$\chi^2(1)$	P-value
Constant	**– 2.47**	.53	.000	---	---	---
Sex: Female	0.09	.09	.30	.013	7.5	.006
Age: Between 25 and 35	**0.33**	.07	.000	.057	0.0	.99
Between 35 and 45	**0.37**	.07	.000	.050	0.3	.56
Between 45 and 55	**0.27**	.08	.001	.042	1.5	.22
Only answered questionnaires in:						
Mar. '92, Sep. '92	**– 0.36**	.08	.000	- .050	0.6	.46
Sep. '93	**– 0.36**	.09	.000	- .045	0.0	.99
Sep. '93, Sep. '94	**– 0.46**	.10	.000	- .052	2.2	.14
Sep.' 93, Sep. '94, Sep. '95	– 0.23	.15	.12	- .029	0.4	.53
Sep. '95, Sep. '96	– 0.11	.18	.53	- .015	0.0	.98
Sep. '95, Sep. '96, Sep. '97	0.08	.15	.58	.012	0.0	.86
Sep.' 96	– 0.04	.10	.69	- .006	1.9	.16
Sep. '96, Sep. '97	– 0.15	.10	.15	- .020	0.0	.87
Female and answered questionnaires in:						
Mar. '92, Sep. '92	– 0.17	.11	.097	- .023	7.9	.005
Sep. '93	**– 0.30**	.12	.014	- .037	3.8	.052
Sep. '93, Sep. '94	– 0.14	.14	.34	- .018	0.5	.46
Sep.' 93, Sep. '94, Sep. '95	– 0.11	.20	.60	- .014	0.6	.43
Sep. '95, Sep. '96	– 0.12	.26	.64	- .016	0.5	.46
Sep. '95, Sep. '96, Sep. '97	0.02	.20	.91	.003	0.1	.73
Sep.' 96	0.16	.13	.23	.025	1.2	.27
Sep. '96, Sep. '97	**0.29**	.14	.033	.049	0.0	.88
Schooling: University entrance degree (12 years)	**– 0.18**	.07	.007	- .025	1.4	.25
Medium (10 years)	**– 0.19**	.05	.000	- .027	0.1	.78
Technical school (*Fachschule*)	**– 0.10**	.04	.019	- .014	0.6	.45
University	– 0.12	.06	.055	- .016	1.9	.17
Unemployment rate in region (mean)	**6.25**	3.15	.047	.005	0.6	.45
PEP rate in region (mean)	– 3.04	6.92	.66	- .001	2.9	.087
Cities and areas:						
City of Dessau	**– 0.32**	.10	.002	- .038	0.39	.53
City of Halle	**– 0.38**	.08	.000	- .044	0.0	.96
City of Magdeburg	**– 0.18**	.06	.002	- .023	1.2	.27
Area of Ascherleben-Staßfurt	0.10	.08	.23	.015	0.2	.70
Area of Anhalt-Zerbst	0.07	.10	.47	.011	3.8	.052
Area of Bitterfeld	**0.35**	.09	.000	.061	0.0	.91
Area of Quedlinburg	**0.33**	.09	.000	.058	0.7	.42
Area of Wernigerode	**– 0.29**	.10	.004	- .035	1.2	.28
Area of Bördekreis						
And region of Halberstadt	– 0.27	.20	.17	- .033	0.0	.87
And region of Magdeburg	**0.23**	.11	.035	.038	0.4	.54

Note: Estimated coefficients that are statistically significant at the 5% level appear in bold letters. Standard errors and tests are computed using the expected Hessian as part of the White (1982) approach. The Lagrange multiplier (LM) test is for heteroscedasticity due to single variables (Davidson and MacKinnon, 1984).
Dependent variable: dummy variable for participation in PEP.
Reference group: male, ages 22 to 25, schooling: none or lowest degree, living in any other area, answered questionnaire only in Sep. '93, '94, '95 and '96 or only in Sep. '93, '94, '95, '96 and '97 or only in Sep. '94, '95, '96 and '97 or only in Sep. '94, '95 and '96.
Total number of observations: 13,688 observations (1,123 PEP participants).

a) The average derivative indicates the change in the participation probability in response to a change from 0 to 1 in the case of dummies or to changes of +/- 0.14% and +/- 0.02% for the continuous variables *unemployment rate in region* and *PEP rate in region,* respectively (one standard deviation in each case).

Table 7: Other Specification Tests for the Participation Probit (Partial)

	$\chi^2(df)$	Df	P-value
Score test against non-normality	1.2	2	.55
Information matrix test:			
All indicators	510	447	.020
Only main diagonal indicators	43	36	.20

Note: The score test suggested by Bera, Jarque, and Lee (1984) tests normality against the Pearson family of distributions. The IM tests are computed using the second version suggested by Orme (1990); that version has good small sample properties. The IM tests check the validity of the information matrix equality that holds in a correctly specified model estimated by maximum likelihood. "Only main-diagonal indicators" refers to a test statistic that uses only the main diagonal of the difference between the outer product of the gradient and the expected Hessian.

See also the notes to Table 6.

Table 8: $\hat{\theta}_N^{\text{CIA}}$ Computed with Time-Constant Match Variables Only

Period	$\hat{\theta}_{t,N}^{\text{CIA}}$	P-value	Obs.	Period	$\hat{\theta}_{\tau,N}^{\text{CIA}}$	P-value	Obs.
1	29	.000	743	**– 1**	**29**	.000	228
2	17	.000	459	– 2	25	.000	71
3	15	.000	192	– 3	17	.11	29
4	10	.095	67	– 4	15	.36	13

Note: The *X* variables used for matching consist of the partial propensity score and dummy variables indicating valid interviews in particular survey waves. Results are in %-points of unemployment. A positive sign implies higher unemployment in the PEP group than in the matched comparison group.

Table 9: $\hat{\theta}_{t-\tau,N}^{\mathrm{BSA}}$ Computed with Time-Constant Match Variables Only

Period	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}_{t-\tau,N}^{\mathrm{BSA}}$	P-value	Obs. t	Obs. τ
			Balanced design[a)], τ = –t			
1	36	38	– 2	.64	228	228
2	37	26	11	.44	27	27
3	---	---	---	---	0	0
			Balanced design[a)], τ = –1			
1	36	38	– 2	.64	228	228
2	25	32	– 7	.30	85	85
3	31	52	– 20	.11	25	25
			Unbalanced design, τ = –t			
1	41	40	1	.84	743	228
2	30	38	– 8	.25	459	71
3	23	26	– 3	.82	192	29
			Unbalanced design, τ = –1			
1	41	40	1	.84	743	228
2	30	41	– 12	.010	459	228
3	23	37	– 14	.007	192	228

Note: See the notes to Table 8.

a) In the balanced design, the same observations are used to compute $\hat{\theta}_{t,N}^{\mathrm{CIA}}$ and $\hat{\theta}_{\tau,N}^{\mathrm{CIA}}$.

Table 10: $\hat{\theta}_N^{\text{CIA}}$ Computed with Time-Varying Matching Variables

	Evaluation sample			Bias correction sample				Evaluation sample		
Period	$\hat{\theta}_{\tau,N}^{\text{CIA}}$	P-value	Obs.	$\hat{\theta}_{\tau,N}^{\text{CIA}}$	P-value	Obs.	Period	$\hat{\theta}_{t,N}^{\text{CIA}}$	P-value	Obs.
– 1	0	1.00	231	**28**	.000	144	1	11	.008	231
– 2	7	.37	71	1	.78	143	2	0	1.00	88
– 3	7	.56	29	14	.10	52	3	11	.34	27

Note: The *X* variables used for matching consist of the partial propensity score, dummies indicating valid interviews in particular survey waves, 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, and pre-PEP unemployment rate in region (only last interview before PEP used). The *evaluation sample* is used to compute the effect of participating in PEP under the assumption that the CIA is valid. The *bias correction sample* estimates the bias in the evaluation sample estimate under the assumption that the BSA is valid.
See also the notes to Table 8.

Table 11: $\hat{\theta}^{\text{BSA}}_{t-\tau,N}$ Computed with Time-Varying Matching Variables

Period	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}^{\text{BSA}}_{t-\tau,N}$	P-value	Obs. t	Obs. τ
			Unbalanced design, τ = –1			
1	35	52	– 17	.015	231	144
2	25	53	– 28	.001	88	144
3	30	46	– 17	.21	27	144
			Unbalanced design, τ = –1; women only			
1	42	58	– 16	.086	108	90
2	38	61	– 24	.066	40	90
3	36	79	– 43	.032	14	90
			Unbalanced design, τ = –1; men only			
1	29	46	– 17	.064	123	54
2	14	44	– 29	.003	48	54
3	23	43	– 20	.21	13	54

Note: See the notes to Table 10.

Table 12: $\hat{\theta}^{\mathrm{BSA}}_{t-\tau,N}$ for Employment and Out of the Labour Force Computed with Time-Varying Matching Variables

Period	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}^{\mathrm{BSA}}_{t-\tau,N}$	P-value	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}^{\mathrm{BSA}}_{t-\tau,N}$	P-value
	Employment				Out of the labour force			
	Women and men				Women and men			
1	55	40	14	.043	10	8	2	.58
2	56	42	14	.13	19	5	14	.021
3	52	57	– 5	.74	19	– 3[a)]	---	---
	Women only				Women only			
1	42	25	17	.074	17	18	– 1	.89
2	33	24	9	.48	30	15	15	.16
3	43	14	29	.17	21	7	14	.34
	Men only				Men only			
1	66	51	14	.12	5	3	2	.66
2	75	54	21	.059	10	3	8	.25
3	62	53	8	.62	15	4	11	.41

Note: Results are in %-points of employment and out of the labour force. See the notes to Table 10. The number of observations can be found in Table 11.

a) The negative value is due to a very small sample size.

Table 13: Number of Individuals when Selection Rules Were Applied, Unbalanced Panel

	Observations without any PEP participation	Observations with PEP participation
Raw data	28,490	2,873
Age restriction:		
No or inconsistent age information	– 425	– 30
No observation with age below 55	– 6,074	– 436
Younger than 22 in 1993	– 3,513	– 66
Remaining sample (in % of raw data)	18,478 (65 %)	2,341 (81 %)
PEP information:		
Older than 52 when starting PEP	– 0	– 92
Observations with unknown start and end date of PEP[a)] or with inconsistent PEP information	– 0	– 451
Start date of PEP before March 1991[a)]	– 0	– 266
Remaining sample (in % of raw data)	18,478 (65 %)	1,532 (53 %)
Insufficient number of observations or no useful information available:		
Students[b)]	– 425	– 11
Answered questionnaire only in Mar. 92 or only in Sep. 97[a)]	– 3,545	– 327
Answered questionnaire only in Sep. 94, only in Sep. 94 and 95, or only in Sep. 95[b)]	– 1,551	– 25
Inconsistent regional information	– 11	– 3
Remaining sample (in % of raw data)	12,946 (45 %)	1,166 (41 %)
Observations with missing values of variables used for the estimation of the propensity score	– 381	– 43
Sample for estimating the propensity score (in % of raw data)	12,565 (44 %)	1,123 (39 %)

Notes: a) These observations do not provide any useable information in the context of this paper.
b) These groups lack a sufficient number of PEP participants.

Table 14: Definitions of Variables

Category	Variable	Definition	Time varying?
Age	Age	Age in 1993	Constant
Sex	Female	Women (dummy)	Constant
Schooling	University entrance degree	*Abitur*, highest German schooling degree	Constant
	Medium	*Mittlere Reife*,10th class	Constant
	Low	*Hauptschulabschluss*, 9th class	Constant
	No degree	No degree	Constant
Vocational degree	Vocational degree	Level of highest vocational degree 1: Partly vocational training (*Teilfacharbeiter*) 2: Vocational training (*Facharbeiter*) 3: Advanced vocational training (*Meister*, *Techniker*) 4: Technical school (*Fachschule*) 5: University degree	Variable
	Technical school	Technical school (*Fachschule*)	Constant
	University degree	University degree	Constant
Labour market status	Employed	In regular or irregular employment (including PEPs)	Variable
	Unemployed	Registered as unemployed	Variable
	Out of the labour force	Not employed and not registered as unemployed	Variable
Current employer	Position in firm	Current or last known position in firm 1: In education (*Lehrling*) 2: Blue color worker (*Arbeiter*) 3: White color worker (*Angestellter*) 4: In public services (*Beamter*) 5: Self-employed (*Selbständiger*)	Variable
	Size of firm	Employees of current or last known firm 1: 1 to 19 2: 20 to 49 3: 50 to 99 4: 100 to 999 5: 1000 or more	Variable
	Industrial sector	Industrial sector of current or last known employer; dummies for agricultural, chemical, and public sectors	Variable
Labour market indicators	Unemployment rate in region	Rates for eight regions in the interview month, with unemployment defined as officially unemployed or participating in active labour market policy	Variable
	Unemployment rate in region (mean)	Rates for eight regions, means of 1991-1997; unemployment defined as officially unemployed or participating in active labour market policy	Constant
	PEP rate in region (mean)	Persons employed in PEP per regular employed, rates for eight regions, means of 1991-1997	Constant
Living in	Region	Dummies for living in one of eight regions in Sachsen-Anhalt defined by the boundaries of the labour offices (*Arbeitsamtsbezirke*)	Constant
	City and area	Living in one of 24 local divisions (dummies), 3 cities (larger cities, *kreisfreie Städte*) and 21 areas (rural areas and smaller cities, *Landkreise*)	Constant
When observed in panel	Answered questionnaires only in ...	The date (in month and year) or all dates at which the person answered a questionnaire	Constant

Table 15: Descriptive Statistics of Selected Variables for Persons With and Without PEP

	No PEP	PEP
	Mean in subsample or share in subsample in %	
Age (1993)	38.5 years	38.8 years
Sex: Female	52	52
Schooling: Highest degree		
University entrance degree (*Abitur*)	28	23
Medium (*Klasse 10*)	54	54
Low (*Klasse 9* or no degree)	18	23
University degree	18	15
Technical school (*Fachschule*)	24	21
Current employer: Position in firm (average value)		
March 1991 / Sep. 1992	2.8 / 2.7	2.8 / 2.4
Sep. 1994 / Sep. 1996 / Sep. 1997	2.8 / 2.8 / 2.9	2.6 / 2.6 / 2.6
Current employer: Size of firm (average value)		
March 1991 / March 1992 / Sep. 1992	n.a. / 2.8 / n.a.	n.a. / 2.8 / n.a.
Sep. 1994 / Sep. 1996 / Sep. 1997	2.5 / 2.4 / 2.4	2.5 / 2.2 / 2.1
Current employer: Industrial sector: Agricultural		
March 1991 / Sep. 1992 / Sep. 1994 / Sep. 1996 / Sep. 1997	7 / 5 / 4 / 3 / 3	21 / 10 / 8 / 3 / 5
Current employer: Industrial sector: Chemical		
March 1991 / Sep. 1992 / Sep. 1994 / Sep. 1996 / Sep. 1997	7 / 5 / 4 / 4 / 4	17 / 13 / 9 / 8 / 3
Current employer: Industrial sector: Public		
March 1991 / Sep. 1992 / Sep. 1994 / Sep. 1996 / Sep. 1997	9 / 9 / 12 / 13 / 14	2 / 19 / 13 / 21 / 14
Observability in panel: Answered questionnaire only in		
Mar. '92, Sep. '92	39	30
Sep. '93	19	13
Sep. '93, Sep. '94	11	7
Sep.' 93, Sep. '94, Sep. '95	3	3
Sep.' 93, Sep. '94, Sep. '95, Sep. '96	2	3
Sep.' 93, Sep. '94, Sep. '95, Sep. '96, Sep. '97	6	10
Sep. '95, Sep. '96	1	2
Sep. '95, Sep. '96, Sep. '97	2	4
Sep.' 96	7	13
Sep. '96, Sep. '97	7	12

Table 15 continued

	No PEP	PEP
	Mean in subsample or share in subsample in %	
Employment: Registered as unemployed in		
March 1991 / Sep. 1992 / Sep. 1993	6 / 10 / 16	16 / 15 / 47
Sep. 1994 / Sep. 1996 / Sep. 1997	9 / 10 / 11	25 / 30 / 42
Employment: Employed in		
March 1991 / Sep. 1992 / Sep. 1993	89 / 81 / 81	79 / 78 / 51
Sep. 1994 / Sep. 1996 / Sep. 1997	85 / 84 / 83	68 / 61 / 47
Unemployment rate in region (mean over time 91-97)	21	22
PEP rate in region (mean over time 91-97)	4	4
Living in:		
Region of Dessau	10	10
Region of Halberstadt	10	11
Region of Halle	19	13
Region of Magdeburg	24	25
Region of Merseburg	13	12
Region of Sangerhausen	9	12
Region of Stendal	10	10
Region of Wittenberg	6	7
City of Dessau	4	2
City of Halle	12	5
City of Magdeburg	11	8
Area of Altmarkkreis Salzwedel	4	4
Area of Aschersleben-Straßfurt	4	6
Area of Anhalt-Zerbst	3	3
Area of Bernburg	2	3
Area of Bitterfeld	4	7
Area of Bördekreis	3	3
Area of Burgenlandkreis	5	5
Area of Jerichower Land	3	2
Area of Halberstadt	3	2
Area of Köthen	2	3
Area of Mansfelder Land	4	5
Area of Merseburg-Querfurt	5	5
Area of Ohre-Kreis	3	4
Area of Quedlinburg	3	6
Area of Saalekreis	2	2
Area of Sangerhausen	3	3
Area of Schönbeck	3	3
Area of Stendal	6	6
Area of Weißenfels	3	2
Area of Wernigerode	4	3
Area of Wittenberg	6	7
Maximum number of observations	12,565	1,123

Note: Some statistics are generated with fewer observations than the maximum due to missing information.

Table 16: Bootstrapped $\hat{\theta}^{\text{BSA}}_{t-\tau,N}$ Computed with Time-Varying Matching Variables, Unbalanced design, $\tau = -1$

	Asymptotic Approximation						
			Quantiles in %				
Period	$\hat{\theta}^{\text{BSA}}_{t-\tau,N}$	Std. Error	5	25	Median	75	95
1	– 17	6.8	-28	-21	– 17	-12	-5
2	– 28	8.4	-42	-33	– 28	-22	-14
3	– 17	13.3	-39	-26	– 17	-8	5
	Bootstrap (300 replications)						
	Mean	Std. Error	Quantiles in %				
	$\hat{\theta}^{\text{BSA}}_{t-\tau,N,h}$	$\hat{\theta}^{\text{BSA}}_{t-\tau,N,h}$	5	25	Median	75	95
1	-19	6.0	-30	-24	-20	-16	-9
2	-29	7.5	-42	-35	-30	-24	-17
3	-26	12.8	-41	-35	-26	-17	-2

Note: See the notes to Tables 10 and 11.

Table 17: Descriptive Statistics of Selected Variables for PEP Participants and Matched Comparison Samples

	Evaluation samples		Bias correction samples	
	Matched comparisons	PEP participants	Matched comparisons	PEP participants
(1)	(2)	(3)	(4)	(5)
Variable	Mean, share in %	Mean, share in %	Mean, share in %	Mean, share in %
Sex: Female	49	47	62	63
Age:in 1993	39.4	38.6	39.1	38.8
Between 25 and 35	31	33	32	32
Between 35 and 45	42	42	34	40
Between 45 and 55	23	21	29	23
Schooling: University entrance degree (*Abitur*)	27	26	28	31
Medium (*10. Klasse*)	55	55	53	54
Technical school (*Fachschule*)	22	23	24	26
University degree	18	18	19	25
Unemployment rate in region (mean)	0.22	0.22	0.22	0.22
PEP rate in region (mean)	0.04	0.04	0.04	0.04
Cities and Areas:				
City of Dessau	4	4	3	3
City of Halle	4	4	4	5
City of Magdeburg	9	8	11	10
Area of Ascherleben-Staßfurt	4	6	3	3
Area of Anhalt-Zerbst	5	4	2	2
Area of Bitterfeld	5	6	6	5
Area of Quedlinburg	6	7	3	6
Area of Wernigerode	4	2	3	1
Area of Bördekreis				
And region of Halberstadt	1	1	0	0
And region of Magdeburg	1	3	1	2
Median absolute standardized bias (MSB)	2.83		1.43	
Joint Wald test for paired mean differences (JW), $\chi^2(37)$	12.46	P-value in %: 100	12.02	P-value in %: 100

Note: The comparisons in (2) and (4) are matched on $x\hat{\beta}$, selected *v*-variables and *m*-variables. v-variables used for the additional conditioning consist of dummy variables indicating whether the individual has a valid interview in the respective year. The *m*-variables are unemployment, employment, highest vocational degree, position in (last) firm, size of (last) firm, and the unemployment rate in the region measured for the last interview before PEP for the evaluation samples and the bias correction samples $(\tau = -1)$.

$$MSB = \underset{k}{median}\left(b^k \left[\sqrt{\left(s^2\left(x_i^k\right) + s^2\left(x_{(i)}^k\right)\right)/2} \right]^{-1} \right).$$

$$JW = N'b'\left[s^2\left(x_i^k - x_{(i)}^k\right)\right]^{-1} b \;;\; b^k = \left[N'\right]^{-1} \sum_{i=1}^{N'} \left(x_i^k - x_{(i)}^k\right),$$

where $x_{(i)}$ denotes the value for the comparison observation matched to treated observation *i* and $s^2(a)$ denotes the empirical variance of a. $b = \left(b^1, \ldots, b^k, \ldots, b^K\right)'$. Asymptotically, $\chi^2(K)$ should be a good approximation for the distribution of JW when there are no systematic differences on the *K* attributes given in the table for the matched pairs. More information can be found in Lechner (1999).

Table 18: $\hat{\theta}_N^{\mathrm{CIA}}$ Computed with Time-Varying Matching Variables Two Periods Prior to PEP

	Evaluation sample			Bias correction sample				Evaluation sample		
Period	$\hat{\theta}_{t,N}^{\mathrm{CIA}}$	P-value	Obs.	$\hat{\theta}_{\tau,N}^{\mathrm{CIA}}$	P-value	Obs.	Period	$\hat{\theta}_{t,N}^{\mathrm{CIA}}$	P-value	Obs.
– 1	0	1.00	70	**23**	.015	47	1	4	.60	70
– 2	– 1	.86	71	4	.67	47	2	15	.24	27
– 3	11	.37	28	0	1.00	47	3	10	.67	10

Note: The X variables used for the matching consist of the partial propensity score, dummies indicating valid interviews in particular waves, pre-PEP full-time employment, pre-PEP unemployment, pre-PEP job position, pre-PEP industrial sector (agricultural, chemical, or public), pre-PEP vocational degree, pre-PEP firm size, pre-PEP unemployment rate in region. Only the last two interviews before PEP are used. Results are in %-points of unemployment. See also the notes to Table 8.

Table 19: $\hat{\theta}_{t-\tau,N}^{\mathrm{BSA}}$ Computed with Time-Varying Matching Variables Two Periods Prior to PEP

	Unbalanced design, $\tau = -1$					
Period	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}_{t-\tau,N}^{\mathrm{BSA}}$	P-value	Obs. t	Obs. τ
1	39	58	– 20	.092	70	47
2	37	46	– 9	.59	27	47
3	50	63	– 13	.60	10	47

Note: See the notes to Table 18.

Table 20: $\hat{\theta}_N^{\text{CIA}}$ Computed with a Reduced Set of Time-Varying Matching Variables

	Evaluation sample			Bias correction sample				Evaluation sample		
Period	$\hat{\theta}_{t,N}^{\text{CIA}}$	P-value	Obs.	$\hat{\theta}_{\tau,N}^{\text{CIA}}$	P-value	Obs.	Period	$\hat{\theta}_{t,N}^{\text{CIA}}$	P-value	Obs.
– 1	0	.92	231	**35**	.000	144	1	14	.001	231
– 2	8	.36	67	1	.89	143	2	8	.20	88
– 3	14	.22	29	8	.37	52	3	7	.54	27

Note: The X variables used for matching consist of the partial propensity score, dummies indicating valid interviews in particular waves, pre-PEP full-time employment, and pre-PEP unemployment. On the last interview before the PEP is used.

Table 21: $\hat{\theta}^{\mathrm{BSA}}_{t-\tau,N}$ Computed with a Reduced Set of Time-Varying Matching Variables

Period	Unbalanced design, $\tau = -1$					
	$\hat{E}\left(Y_t^p \mid S=1\right)$	$\hat{E}\left(Y_t^n \mid S=1\right)$	$\hat{\theta}^{\mathrm{BSA}}_{t-\tau,N}$	P-value	Obs. t	Obs. τ
1	35	56	– 21	.001	231	144
2	25	52	– 27	.001	88	144
3	29	57	– 27	.040	27	144

Note: See the notes to Table 20.

Figure 1: Duration of Individual PEPs

Figure 1 here.

Note: X-axis: Time, measured in months; Y-axis: number of observations. Long PEP duration might reflect participation in multiple PEPs, as identification of separate spells is not always possible in the data. The questionnaire was designed in such a way that it is possible to answer either eleven or twelve months for a full year of PEP participation depending on the respondent's interpretation of the relevant questions. Number of valid observations: 778.

Figure 2: Definition of Periods for the Evaluation Sample and the Bias Correction Sample for One PEP Participant when Time-Varying Variables Are Used, $\tau = -1$

Figure 2 here.

Note: The *calendar time* line shows interviews from 1993 (I 93) to 19097 (I 97). The *relative time* line is defined relative to the date of PEP participation for the person in the example. (Note that in this example, the PEP spell includes one interview, which is not necessarily the case). The *artificial PEP* period has the same duration as the PEP and ends just before the last interview prior to 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 the BSA is valid).

Figure 3: $\hat{\theta}_N^{\mathrm{CIA}}$ Computed with Time-Varying Matching Variables

Figure 3 here.

Note: X-axis: Relative time; Y-axis: Unemployment rate in %. *Treated* indicates the estimated unemployment rate for the participants and *comparisons* indicates the estimated counterfactual unemployment rate for participants had they not participated. The latter is given by the unemployment rate of the matched comparison sample. The *diff* indicates the difference between these two, or $\hat{\theta}_N^{\mathrm{CIA}}$.

See also the notes to Table 10.

Figure 4: Start and End Dates of Individual PEPs

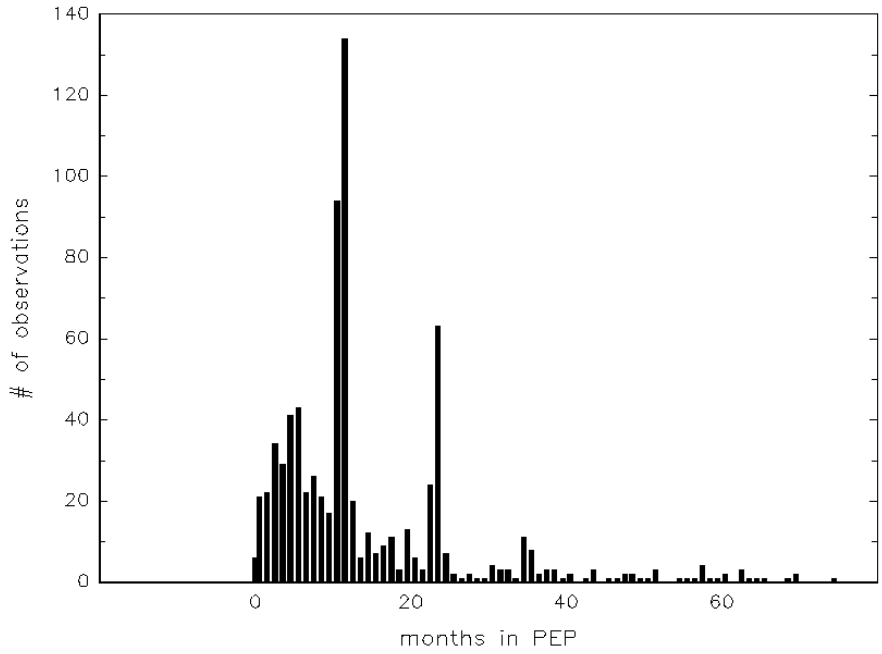
Figure 4 here.

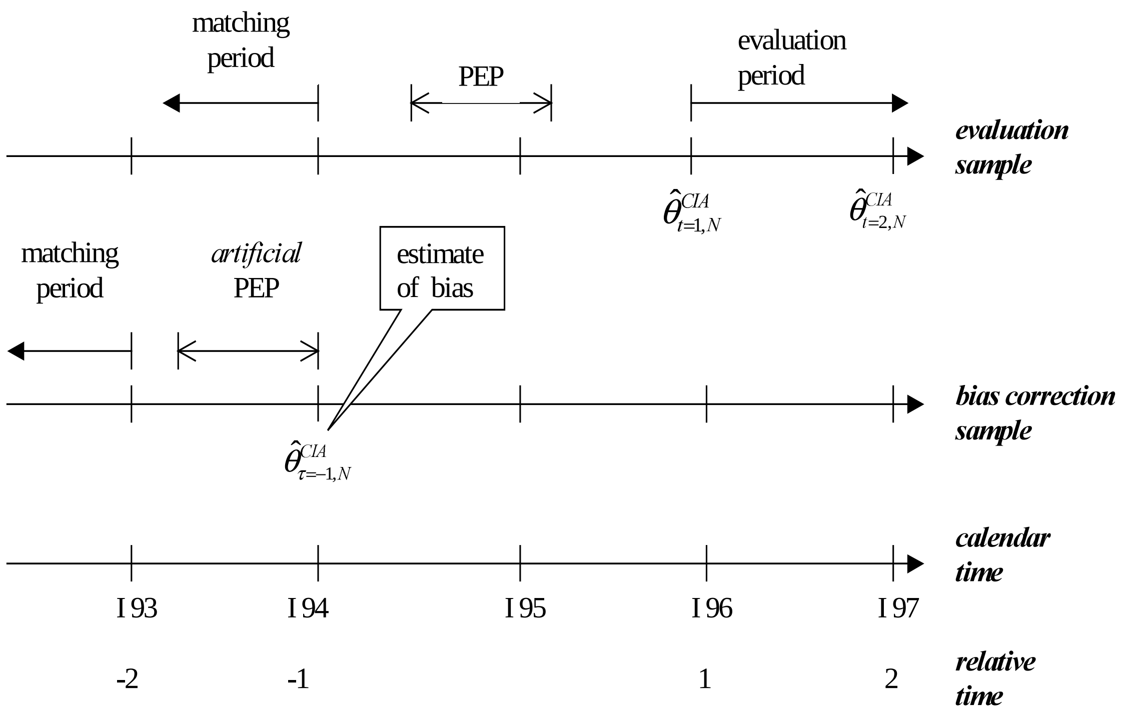
Note: X-axis: Time, measured in months; Y-axis: number of observations. Time is presented in months since January 1989. For example: March '92: 39; September '92: 45; September '93: 57; September '94: 69; September '95: 81; September '96: 93; September '97: 105. Start dates - and therefore end dates as well - earlier than 27 have been deleted (see Section 4.2). Number of valid observations for start dates: 1123; number of valid observations for end dates: 778.

Figure 5: Support of the Propensity Score for Participants and Non-Participants

Figure 5 here.

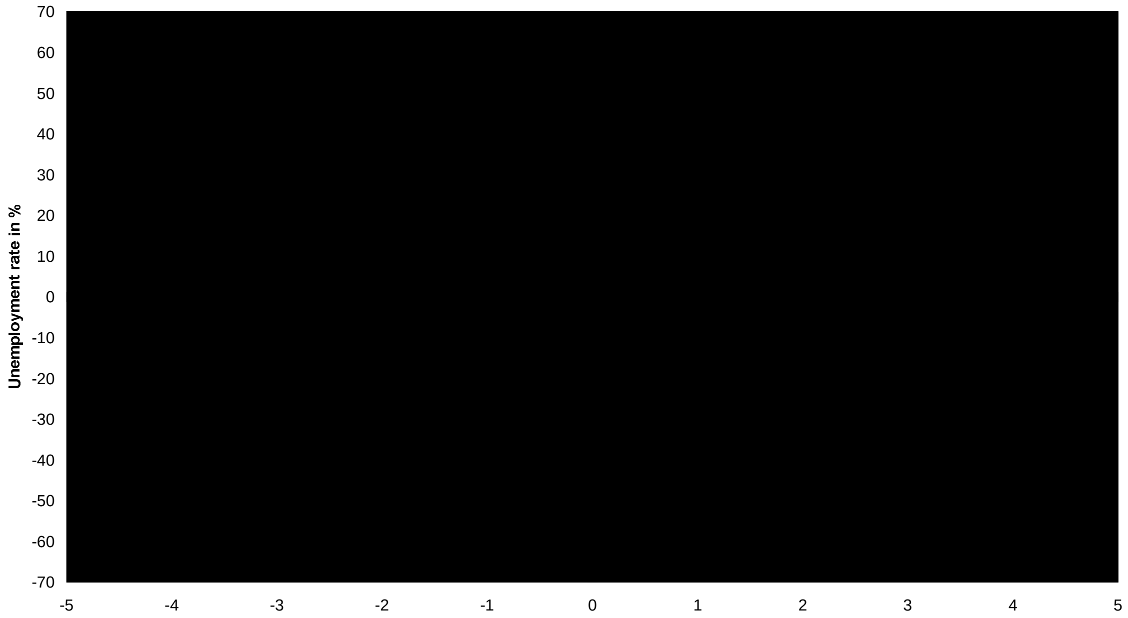
Note: Number of observations: 13,688 (1,123 participants and 12,565 non-participants).


140
120
100
80
60
40
20
0
# of observations
0
20
40
60
months in PEP

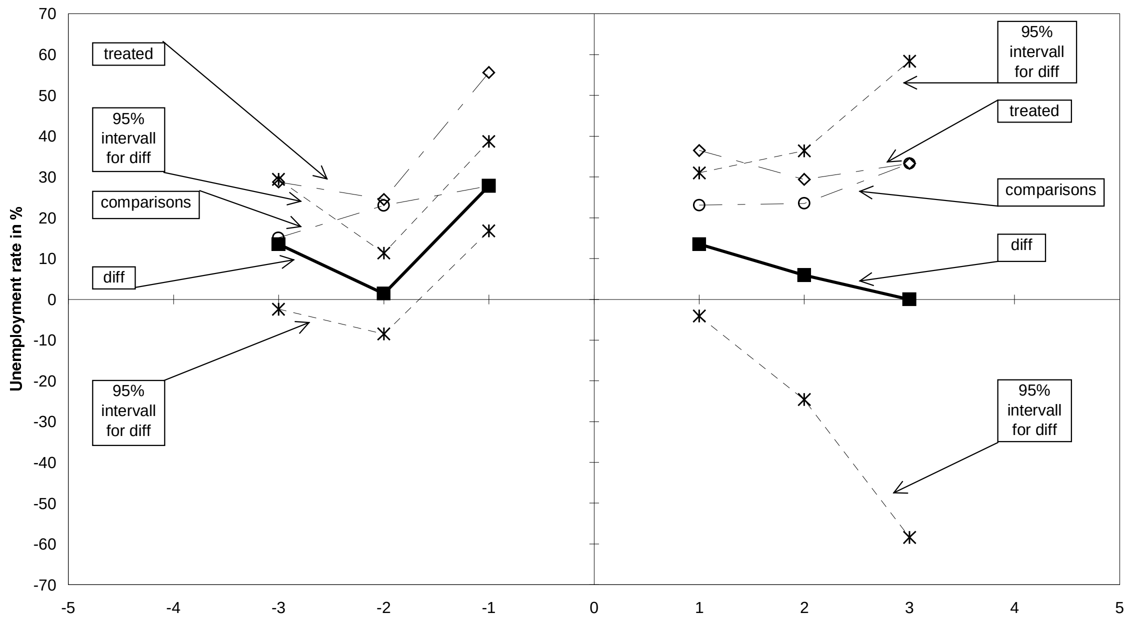


matching period
PEP
evaluation period
evaluation sample
$\hat{\theta}^{CIA}_{t=1,N}$
$\hat{\theta}^{CIA}_{t=2,N}$
matching period
artificial PEP
estimate of bias
bias correction sample
$\hat{\theta}^{CIA}_{\tau=-1,N}$
calendar time
I 93
I 94
I 95
I 96
I 97
relative time
-2
-1
1
2

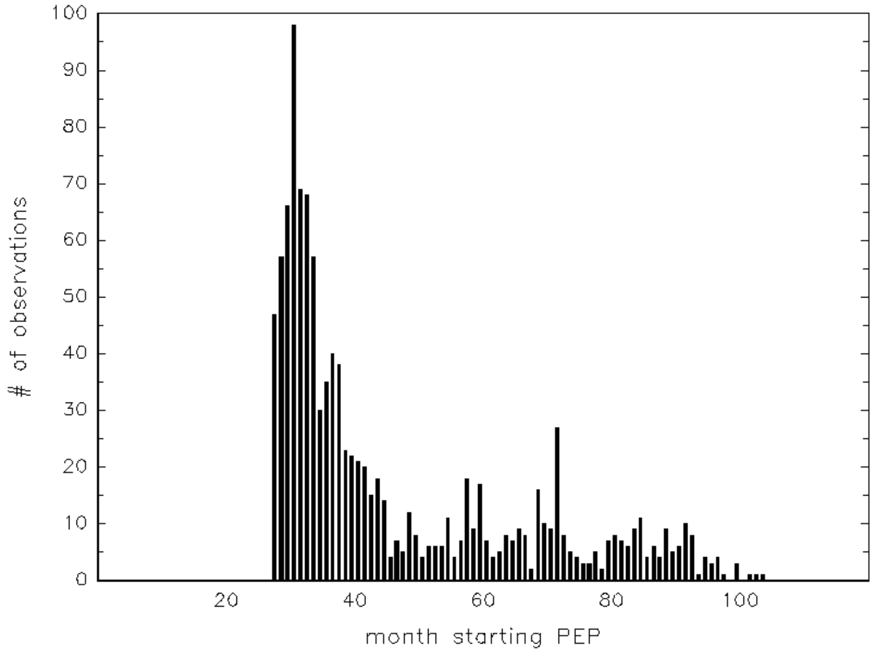
Bias Correction Sample



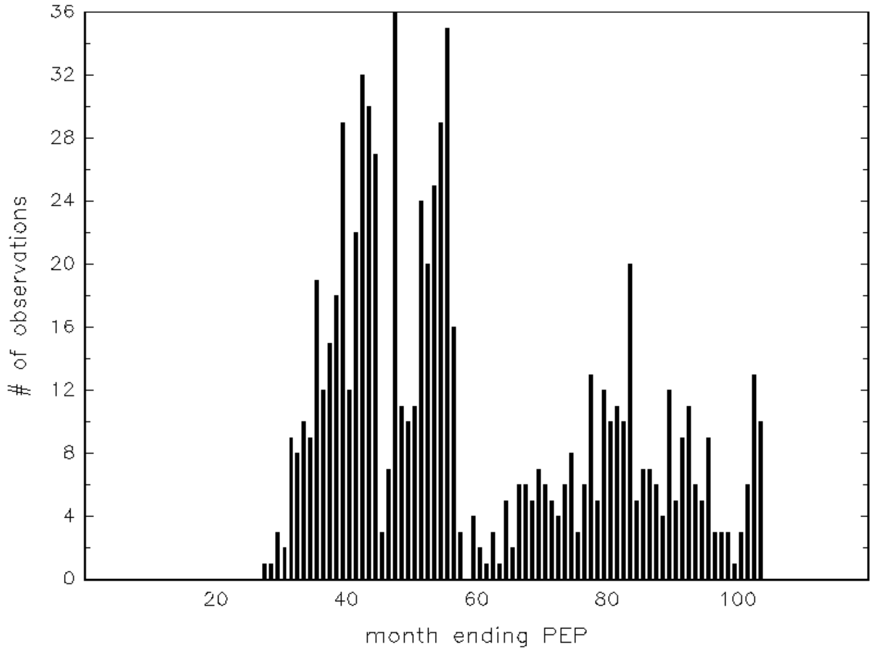
Bias Correction Sample

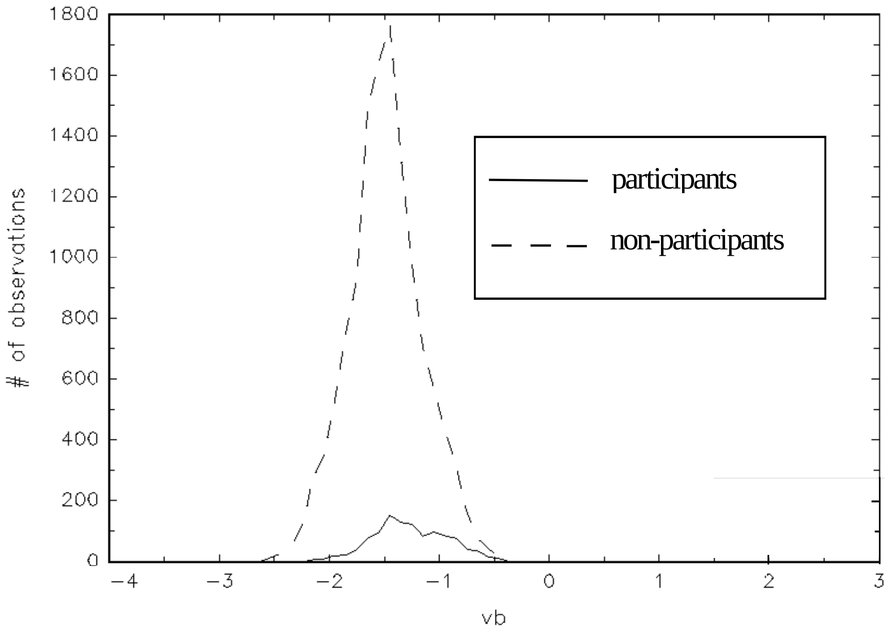


Start Dates



End Dates





participants
non-participants
# of observations
vb
0
200
400
600
800
1000
1200
1400
1600
1800
−4
−3
−2
−1
0
1
2
3