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Continuous Off-the-Job Training in East Germany After Unification: Preliminary Results of an Evaluation of the Effects for Individual Workers

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Abstract

Retraining the labor force to match the demands of a modern economy is seen as an important task during the transition process from a centrally-planned to a market economy. This need was particularly pressing in East Germany, because the transition process has proceeded much faster than in the rest of Eastern Europe. Therefore, substantial resources have been devoted to this purpose.

This paper analyzes the impact of continuous off-the-job training in East Germany from the point of view of individuals who were part of the labor force before German unification in 1990. It tries to answer questions about the average gains from participating in a specific type of training. Typical outcomes considered to measure those gains are income, employment status, job security, and expected future changes in job position.

The methodology used for the evaluation is the *potential outcome approach to causality*. This approach has received considerable attention in the statistical literature over the last fifteen years and it has recently been *rediscovered* by the econometric literature as well. It is adapted to allow for important permanent and transitory shocks, such as unemployment, which influence the decision to participate in the training as well as future labor market outcomes.

The empirical part is based on the first four waves of the Socio-Economic Panel (GSOEP)-East (1990-1993). This panel data set has the advantage that the fourth wave contains a special survey on continuous training and that it allows keeping track of individual behavior on a monthly, respectively yearly, basis.

The econometric analysis focuses on off-the-job training courses that began after unification and were completed not later than in early 1993. Although it is obviously too early to evaluate the long-term implications, the results suggest that there are no positive effects in the short run.

1 Introduction

Retraining the labor force to match the demands of a modern economy is an important task during the transition process from a centrally planned to a market economy. This need was particular pressing in East Germany, because the transition process proceeds much faster than in the rest of Eastern Europe. Therefore, substantial resources have been devoted to this purpose, and the need for evaluating the results of the work-force training efforts is obvious.

This paper concentrates only on one particular aspect of the training part of the active labor market policy, namely off-the-job training. It tries to identify the average individual gain of the workers of the former GDR participating in off-the-job training between July 1990 and December 1992 compared to the hypothetical state of nonparticipation. Furthermore, the paper addresses the issue whether the gains, if any, are the same for the whole population, or whether there are specific groups of individuals for which they are substantially different. The targets of the evaluations are labor market outcomes after the completion of the training, such as current or expected income, labor market status, and career prospects. It is in the nature of the subject, that when this paper was written only short-run effects can be identified.

In typical evaluations of work-force training programs outcomes measured for the sample getting the training are compared to outcome measures for a *comparable* group, sometimes called control group, who does not get the training. In most social experiments such a group consists of individuals who apply for the program, but are denied participation by randomization, that is by the results of a random number generator, for instance. Experiments are feasible in some states, such as the US, but are rejected mainly for ethical reason in others, such as Germany. In an observational study, that is a study not based on experimental data, the researcher should find individuals who are identical to trainees regarding all *relevant* pre-training attributes except for not having obtained the training. Since typically such individuals cannot be easily identified, additional assumptions have to be invoked to - in some sense - adjust for their dissimilarity and avoid potentially serious so called *sample selection biases.*[1]

Various model based procedures are suggested in the econometrics' literature to avoid such biases. However, among others, Ashenfelter and Card (1985) and Lalonde (1986), who compares different estimates for various nonexperimental control groups with results obtained for an experimental control group, come to the conclusion that the results are highly sensible to different stochastic assumptions made about the selection process. Both papers conclude that the econometric adjustment procedures are unreliable, and hence social experiments are necessary to evaluate training programs. Yet, on the one hand, even when social experiments are available, evaluations based on

[1] Holland (1986, incl. discussion) and Heckman and Hotz (1989, incl. discussion) provide extensive discussions on these issues.

them may have other undesirable features.[2] On the other hand, as Heckman and Hotz (1989) correctly observed, the only case you expect adjustment procedures based on different assumptions about the source of the sample selection bias to lead to the same results, is the very case when there is no bias. Consequently, these authors suggest test procedure to chose methods suitable for the particular problem analyzed. Recently, Dahejia, and Wahba (1995a, 1995b) - using an approach very similar to the one chosen here - reevaluate the Lalonde (1986) data. They can replicate the experimental results very closely by using nonparametric techniques, partly to be discussed later. It seems that this issue is not yet settled.

Project (or treatment) evaluation and the related need for a definition of causality have a history in the statistics' literature as well. This literature does not put so much emphasis on modeling specific aspect of various distributions. Instead, it stresses the need for nonparametric solutions to the identification problem, and - once it is solved - on nonparametric estimation of the causal effects. It seems that Rubin (1974) is the first author explicitly suggesting a model of potential outcomes (outcomes if trained and outcomes if not trained for the same individual). This model clarifies that the individual causal effect of training - defined as the difference of the two potential outcomes for example - is never identified. This model and possible necessary identifying assumption for objects like average causal effects show a close resemblance to the experimental context and emphasize the importance of same sort of randomization as an identifying assumption. It is a useful device to point out that testing methods alone are insufficient, because there is basic lack of identification due to the unobservability of the counterfactual outcome. This has to be overcome by plausible, untestable assumption that usually depend heavily on the problem analyzed and the data available. As long as these identifying assumptions do not generate overidentifying restrictions, there is nothing that can be tested, and hence Heckman and Hotz's (1989) conclusions have to be considered with care.

In the following sections I try to convince the reader that the prototypical statistical approach - appropriately adjusted for this specific application - is more suited to the particular problem analyzed here than the model based approaches. Hence, the results are obtained by using the potential outcome approach to causality as a general framework to define causal effects of off-the-job training on individual actual and expected post-training labor market outcomes. The paper argues that due to the specific situation in East Germany after unification and the rich data available, the assumption that the outcomes and the assignment mechanisms are independent (randomized) conditional on observed attributes, including monthly pre-training employment status, is very plausible. Hence, this assumption solves the identification problem inherent in causal analysis.

[2] E.g. Manski and Garfinkel (1992), and papers therein (in particular Garfinkel, Manski and Michalopoulos, 1992), but for a forceful defence of experiments see e.g. Burtless and Orr (1986). However, in this particular case the apriori assumption that additional off-the-job training would be benefical was not in question. Therefore, the cost in terms of time 'lost' for conducting an experiment ahead of any program, which would have to be delayed for a considerable time, appeared to be prohibitively high.

Since this fundamental lack of identification is at the center of every causal analysis, the paper contains a considerable part on nonparametric identification of the causal effects in this setting. Nonparametric methods that are direct extensions of the matched pair methods suggested by Rubin (1979) and Rosenbaum and Rubin (1983, 1985) are then used for the estimations. I will argue that this approach most probably reduced the bias of the estimated causal effects to a minimum.

Finally, it is noteworthy that the results in this paper do not confirm previous positive finding of the effectiveness of work-force training in East Germany.[3] Although there are only a few studies conducted so far, they are different in many aspects ranging from the database to the implementation of the evaluation. However, they share two common features that are absent from this work: They do not use an explicit causality framework, and they are based on modeling the distributions of the outcome variables given certain covariates. This paper explicitly avoids imposing these kinds of restrictions in general and puts emphasis of the particular notion of causality behind the results.

The paper hopes to contribute to the ongoing discussion of the effectiveness of the training in East Germany by trying to understand the participation decision as well as by identifying empirically important factors related to it, before obtaining evaluation results for several outcome measures related to the actual and prospective individual position in the labor market. On a methodological side, standard procedures taken from the statistical literature are extended to allow an accommodation of the specific problems encountered in this study.

The paper is organized as follows: The following section outlines some basic features of the East German labor market after unification. This significant aspect of the economic environment is important to understand the processes leading possibly to an individual participation in training courses. Additionally, it is also important for the interpretation of pre- and post-training labor market outcomes, that are the results of the evaluations. Section 3 introduces the longitudinal data used in this study and presents several characteristics of the sample chosen. It is based on the first four years (1990-1993) of the Socio-Economic Panel study for East Germany. All aspects of the computations of the evaluations are discussed in section 4, which consists of four subsections. The first subsection details the causality framework used and discusses particular conditions for the identification of average causal effects. The following subsection identifies factors influencing (potential) labor market outcomes as well as training participation. It argues that the respective identification condition is met, and discusses the methodology as well as the results of the estimation of a binary choice model for training participation. The third subsection shows that transitory shocks just prior to training, measured on a monthly basis, play an important role for the participation probability. An adaptation of a matching approach is suggested which allows for these important factors to be included in the choice of the control population. The final subsection

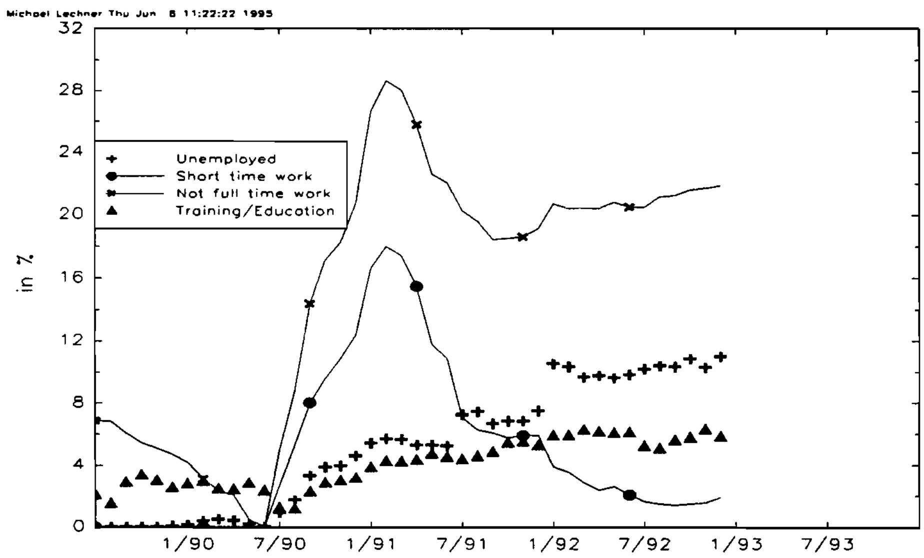
[3] E.g. Pannenberg and Helberger (1994) and the references therein.

defines the outcomes, gives details of the suggested nonparametric estimation approach, and shows several aspects of the results. Section 5 concludes. Appendix A contains additional information about the data used. Appendix B consists of several more technical parts concerning the econometric methods.

2 Some features of the East German labor markets

Unification came as a shock to the East German labor markets.[4] The transformation from the previous centrally planned economic system to a West-German-type market economy led to considerable disequilibria in the labor market.[5] Figure 1 shows monthly pre- and post- unification developments for various indicators, such as unemployment, involuntary short-time work (IST) and full-time employment. Figure 2 depicts respective gender differences. Both figures describe the population that I am most interested in: individuals not younger than 20 and not older than 50 (1990). They worked full-time just before unification, lived in East German at least until 1993, and are not in bad health conditions.

Figure 1: Labor market states



Note: Own calculations based on GSOEP (1990-1993) using panel sampling weights; population is full-time working in June 1990, 20 - 50 years old (1990) and always responding.

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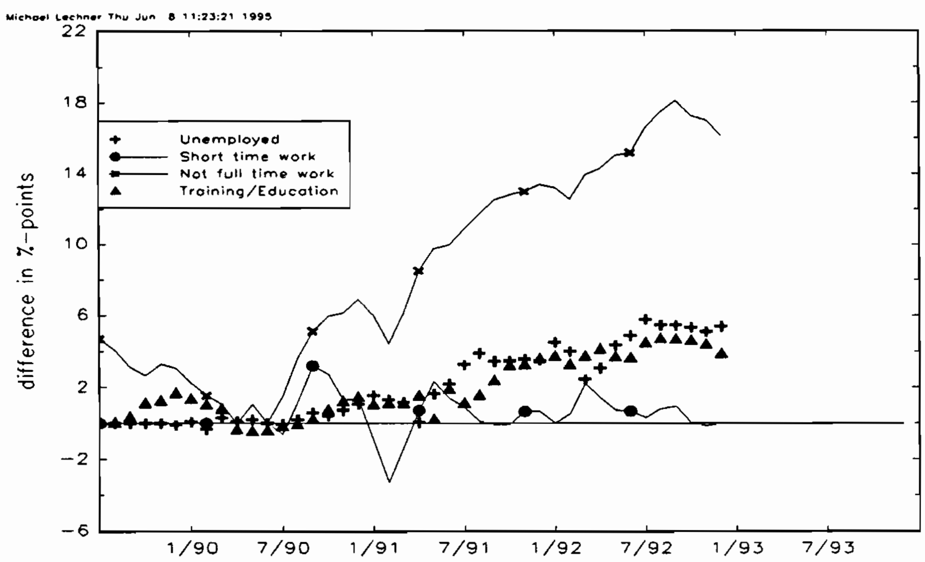
4 This section is based - if not indicated otherwise - on information contained in Statistisches Bundesamt (1994), DIW (1994), Bundesanstalt für Arbeit (1994a, 1994b), Bundesministerium für Bildung und Wissenschaft (1994), and Bundesminister für Arbeit und Sozialordnung (1991).

5 The sharp decrease in fertility rates well below West German levels are an indication that decisive changes occurred not only in the labor market, but also in many other important aspects of daily life.

Figure 1 shows that for this population full-time employment (x) declines from 100% in mid 1990 to about 70% in early 1991 and than stabilizes at around 80%.[6] A very significant proportion of the early fall is absorbed into involuntary short-time work IST (•), which means a reduction of working hours in the firm accompanied by a subsidy from the labor office to compensate employees for the otherwise occurring income loss. In particular in the first year after unification this reduction of working hours could be substantial.[7] However, IST was only temporarily an important tool of active labor market policy. It was unimportant after 1991. As a result of the decline of IST after early 1991 as well as of the worsening general labor market conditions, the unemployment rate (+) - below 2% before unification (total population) - increased steadily up to about 12 % in the end of 1992.[8] Finally, the number of people taking part in some kind of job training (Δ) also increased steadily after unification. It reached a ratio of about 5% in 1992 (of those full-time employed in 1990).

Figure 2: Labor market states: Female - male differences



Note: Own calculations based on GSOEP (1990-1993) using panel sampling weights; population is full-time working in June 1990, 20 - 50 years old (1990) and always responding to survey questions.

Figure 2 shows the difference of the above ratios for women as compared to men. Large differences appear in particular regarding full-time work, but the unemployment as well as the job training rates are significantly higher for women than for men, too. It is also clear that these gender gaps are not just a temporary phenomenon after unification, but it seems that large and per-

6 Full-time work includes Make-Work-Programs (ABM) which account for about 5-10% of full-time employment.

7 In the total population in 1991 (1992, 1993) about 56% (48%, 34%) employees on short time work worked less than 50%, and 27% (26%, 23%) worked less than 25% of their usual hours.

8 Unemployment and IST numbers are lower than the official rates, because of the age restriction and because different definitions of the populations appearing in the denominator of the ratios.

manent differences have emerged. It is perhaps not surprising that women experience more labor market problems than men, because nonparticipation rates in West Germany are much higher than they have been in the GDR. After unification the East German economy operates under institutional conditions that are very similar to the West German institutional arrangements that are associated with these relatively low participation rates in the West.[9]

While employment went down, wages increased considerably after unification. The yearly average of wages for blue collar workers was about 47% higher in 1993 than in 1991. For white collar male employees the respective increase was about 66%, and for the respective female employees about 59%.[10]

To smooth the transition to a market economy and to adjust the East German stock of human capital to the needs of the new economic system, the state at various levels and its agencies, in particular the labor office, conducted an active labor market policy. This policy not only provided significant funds for training and retraining opportunities (about 26 bn DM until 1993), but also supplied subsidies for IST (14 bn DM) and make-work-programs (Arbeitsbeschaffungsmaßnahmen, ABM, 26 bn DM). However, a discussion of the latter two policies is beyond the scope of this paper.

The following brief description of the continuous training in East Germany is based on official data from the labor office. Therefore, it concentrates on types of measures that are in some way subsidized by means provided in the Work Support Act (Arbeitsförderungsgesetz, AFG). In particular in East Germany, they form the biggest and most important part of the continuous training and retraining taking place after unification. There are three broad types of training and retraining that are supported: continuous training to increase skills within the current profession (CT), learning a new profession, and employers are subsidized for a limited period to provide on-the-job training for individuals facing difficult labor market conditions in to order to allow them to familiarize with the new job. The focus of this paper is on the first group, which accounts for about two thirds of all participants in these subsidized courses, but I also include nonsubsidized courses in this area.

In an increasing number of cases (1991: 53%, 1993: 84%) the labor office does not provide the training, but pays for it, when certain conditions are met. These conditions are related to the employment history, the approval of the course by the labor office, and the potential termination of unemployment or the avoidance of a possibility to become unemployed soon. The last principle has been applied using a broad interpretation in East Germany, so that it includes more groups of the labor force than in the West. The payments cover in most cases almost all the costs for the provision of the course as well as usually more than two thirds of the previous net income. More than 97% of these courses are not provided by the employer.

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[9] There are several other issues explaining these gender differences, but they are beyond the scope of this paper.

[10] Source: Bundesministerium für Arbeit und Sozialordnung (1994).

Table 1: Participants in continuous training (CT) subsidized by the labor office

	Entering CT[1)]		Unemployed before entering CT[1)]	Leaving CT[1)]		Total employment CT[2)]
year	men	women		men	women	
1990	n/a	n/a	n/a	n/a	n/a	8,820,000
1991	252,352	377,304	51% [3)]	n/a	n/a	7,219,000
1992	201,120	389,896	77 %	151,498	292,590	6,344,000
1993	68,489	113,103	74 %	110,970	202,928	6,128,000

Notes: 1) BA (1994b), 2) Bundesministerium für Sozialordnung (1994);
3) Includes other types of training with higher unemployment shares in 1993;
n/a: not available.

Table 1 shows the number of participants entering and exiting training courses.[11] Women are more likely to participate in CT, mainly because their unemployment probability is higher than the one for men. Note also the high proportion of people who were unemployed before the start of CT and the dramatic fall in course entrants in 1993. The latter is due to an accumulation of previous entrants who have not yet finished their courses as well as to a cut in the budget for CT.

3 Data

The sample used for the following empirical analysis is drawn from the German Socio-Economic Panel (GSOEP). The GSOEP is very similar to the US Panel Study of Income Dynamics (PSID). About 5000 households are interviewed each year beginning in 1984. A sample of just under 2000 East German households was added in 1990. The GSOEP is very rich with socio-demographic information, in particular concerning current and past employment status. The attrition and item nonresponse rates seem to be reasonable low for such a panel study: the attrition rate for the East German sample (1990-1993) for households is 22% and for individuals it is 24.6%. For a more comprehensive English description of the GSOEP see Wagner, Burkhauser and Behringer (1993).

A very useful characteristic of this panel survey is the availability of monthly information between yearly interviews. It covers different employment and income states. The information is obtained by retrospective questions about what happened in particular months of the previous year. Figure 3 shows an example for this type 'calendar' that will figure prominently in the following empirical analysis. Although the monthly calendar contains also questions about training and retraining, too many different types of training are aggregated. For example, a distinction between on-the-job and off-the-job training is not possible. Therefore, the training information is taken from a special survey on continuous training included in the 1993 survey.

[11] Missing entries and the lack of gender-differentiation for 2 columns are due to insufficient data.

Figure 3: Selected items of the retrospective questions about employment status in the 1993 questionnaire (calendar)

	1992											
	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
full-time employed	□	□	□	□	□	□	□	□	□	□	□	□
involuntary short-time work	□	□	□	□	□	□	□	□	□	□	□	□
registered unemployed	□	□	□	□	□	□	□	□	□	□	□	□
...[1)]	□	□	□	□	□	□	□	□	□	□	□	□

Note: 1) Other states include part-time work, vocational training and retraining, education, and out-of-the-labor-force, among others (see Infratest Sozialforschung, 1990, 1991, 1992, 1993).

This special survey contains specific questions about the last three continuous training courses that were either completed in the last three years or are still going on during the time of the interview. The information provided for these courses include the first month of the course, the (approximate) duration, the number of weekly hours, the objective of it, whether it took place during working hours, and finally whether some kind of certificate of participation considered useful for future job applications has been obtained. Considerably more information is provided for the one particular course that the respondents consider to be the most important one for their own professional careers. However, the use of this information in an evaluation exercise could lead to biased results, since the 'unproductive' courses are screened out by the respondents.[12] Additionally, there is another problem related to the use of this special survey: About 19% of training participants attended more than 3 courses. No information is available on these additional courses. However, I conjecture that the 'lost courses' have to be rather short and/or began very early (that is before unification) to fit into the three year time span used by the special survey. Hence, they are unimportant for this study. Another data problem relates to an imprecise measurement of the duration and, therefore, also of the ending date of the training, because there is only categorical information available.[13] In the empirical analysis the monthly durations are computed by using the mid-point of the duration intervals multiplied by the appropriately rescaled hours per week. The computation of the ending dates of the courses uses the end of the duration interval instead, to avoid attributing a part of the training to the post-training period. However, this problem is reduced by combining the information in the calendar variables (figure 3) with the special-survey variables to adjust the duration and ending dates.

This version of the paper is based on the first four East German waves of the GSOEP (1990-1993). This leads to a censoring problem: Only courses that are completed by the date of the 1993 interview can be evaluated. This kind of censoring will more likely eliminate courses with a long duration - in many cases the most expensive and therefore the most interesting ones - rather than

[12] This could be an empirically important consideration, because more than 60 % participated in more than one course, and of those 47 % stated that all courses were of equal importance.

[13] Categories: 1 day, up to 1 week, up to 1 month, up to 3 months, up to 1 year, up to 2 years, more than 2 years.

short courses. When the next wave (1994) becomes available, which should be in the second half of 1995, it will be included in the analysis and a substantial part of this specific problem will disappear.

To be able to use the special survey as well as information concerning the employment status in the GDR, a balanced sample of all individuals born between 1940 and 1970 who responded in all four wave is selected. The upper age limit is set to avoid the need of addressing early retirement issues. Since the population of interest is the one that formed the labor force of the GDR, it is required that all selected individuals work full-time just before unification. Furthermore, the self-employed in the former GDR (2%), which form a very different group compared to employees,[14] are not observed taking part in off-the-job training, so they are deleted from the sample. Additionally, individuals reporting severe medical conditions are not considered either, because evaluating the specific kind of training they receive would be beyond the scope of this paper.

Table 2: Descriptive statistics of selected socio-economic variables

	No OFT (1205 obs.)	OFT (110 obs.)
Variable	mean or share*) in %	mean or share*) in %
Age	35.2 years	35.9 years
Gender: female	42	65
Federal states (Länder) in 1990		
Berlin	7	12
Sachsen-Anhalt	21	15
Years of schooling (highest degree)		
12	17	27
10	60	65
8 or no degree	22	8
Highest professional degree in 1990		
university	11	25
engineering, technical college	16	31
skilled worker	64	37
Job position in 1990		
highly qualified, management	19	41
skilled blue and white collar	56	44

Note: *) Mean of indicator variable * 100 in subpopulation.

Table 2 displays some selected descriptive statistics for those who received off-the-job training (OFT) and those who did not receive it. Table A.1 in Appendix A gives a complete description of all variables used in the empirical analysis. Individuals who did not complete the OFT until Dec. 92 are deleted from the sample. The definition of OFT used in this table and all the following empirical analysis is the following: The purpose of the course is qualification other than retraining for a different profession with a duration of more than three months. Its duration is 16 hours or more,

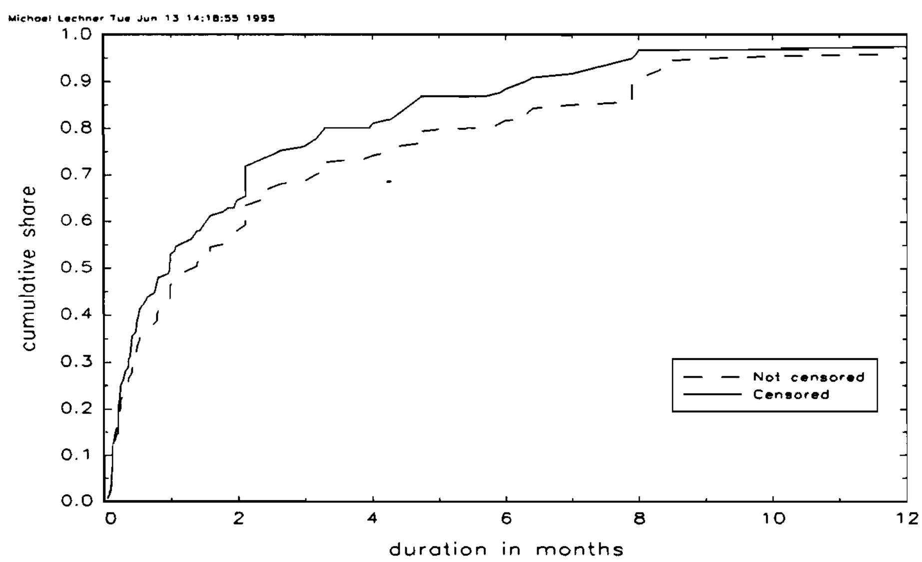
[14] See also Lechner (1993) and Lechner and Pfeiffer (1993).

or longer than one week. Furthermore, it does not take place during regular working hours (if employed). The purpose of the definition is to obtain a not so heterogeneous group of trainees by excluding very short courses, on-the-job-training and retraining for a different profession. Those are all very different kinds of courses with very heterogeneous objectives.

Table 2 shows clearly that OFT-trainees are not a random sample from the population of interest. There does not appear to be a large age difference, but there are far more women in OFT than men. Regarding schooling degrees, professional degrees and job positions in 1990 a very similar pattern appears. Individuals who accumulated more human capital and who reached a higher job position in the former GDR are more likely to obtain OFT. Furthermore, regional aspects seem also to be of importance: Individuals living in East Berlin are more likely to be observed taking part in OFT than for example people living in Sachsen-Anhalt. As section 5 will show, participants in OFT are also more likely to be unemployed or on IST before the beginning of the course as compared to nonparticipants in the same period of time.

Figure 4 shows the sample distribution function for the duration of OFT. It appears that about 50% of the courses have a duration of one month or less. Only a very small proportion (less than 5%) of the courses last longer than 12 months.

Figure 4: Empirical distribution functions for durations of training



Note: *Censored* refers to the sample subject to a selection rule that requires the course to be completed by early 1993. The remaining part (12 to 18 months) of the cdf is omitted. For a complete pdf see Appendix A.

The impact of censoring can be seen by comparing the dashed and the solid lines. It seems that although the durations in the censored samples are lower, the difference is not very large. The more import impact is on the starting dates, because the courses beginning in the second half of 1992 are underrepresented (see figures A.1 and A.2 in Appendix A for more details).

The goal of 4 % of the courses was retraining for another profession (maximum duration less than 3 months), another 37 % was intended to qualify for promotion, and 71 % intended to adjust skills to new circumstances.[15] 85 % of the individuals obtained a certificate that they would use when applying for another job. Finally, about 30 % of the OFT participants stated that they were either unemployed or out of the labor force during OFT. Note that this number is less than the official unemployment rates reported in table 2. The reason for this is not entirely clear. It is however very likely that OFT in this sample includes several types of (short) courses that are not funded by the employment office and that could be attended parallel to a job.

As mentioned before, all information about costs to the individual and received subsidies are only available for the one course, the individual believes is the most important for the own career. Nevertheless, the following statistics provide information about these issues. About 16% of the individuals declared that they obtained financial support, such as a continuation of their wage or salary, by their employer. 44% got such a financial support from the labor office, whereas about 42% declared that they got nothing. It is important to note this implies that the definition of OFT used here includes a substantial part of courses that were not subsidized by the labor office. 35% of those participants getting this kind of support would not have participated in OFT otherwise. About 60% had no costs at all for OFT. For those participants who had costs, the median is 300 DM and the mean is 800 DM. 72% paid less than 1000 DM. Another issue that arises in this context is the portability of the acquired human capital when changing jobs. When the individually most valuable course is OFT, 6% of participants state that they acquired nonportable skills and 23% limited portable skills. 39% obtained skills that are portable to a high degree, and 32% got completely portable skills.

4 Econometric methodology and empirical implementation

This section begins with a brief discussion of causal modeling and the restrictions that are used to identify the training effects. Subsection 4.2 shows that this identifying assumption is reasonable with the problem analyzed in this study and the data at hand. Then it discusses the estimation and test framework as well as the results of the estimation of the probability of OFT participation. Subsection 4.3 is devoted to specific issues related to the chosen nonparametric estimation approach. Finally, subsection 4.4 contains the econometric methods used for and the results of the actual evaluation. Several technical aspects are relegated to Appendix B.

[15] 19 % had another objective. Numbers add to more than 100, because categories are not exclusive.

4.1 Causality, potential outcomes, identification and the propensity score

"What is the average gain for OFT participants compared to the hypothetical state of nonparticipation?" This question is at the center of the empirical analysis of this paper. It refers to potential outcomes or potential states of the world, which never occur. The underlying notion of causality requires the researcher to determine whether participation or nonparticipation in OFT effects the respective outcomes, such as income or employment status. This is very different from asking whether there is an empirical association, typically related to some kind of correlation, between OFT and the outcome. Therefore, I do not try to answer the question whether OFT is associated with a higher income for example, but whether the effect of OFT is a higher income (does OFT cause a higher income in this sense?).[16] The framework that will serve as guideline for the empirical analysis is the potential-outcome approach to causality. Rubin (1974) seems to be the first paper that has explicitly suggested that framework. This idea of causality is very much inspired by the set-up of experiments in science. Its main building blocks for the notation are *units* (here: individuals, i), for which I will assume that they belong to the large population defined in the previous section, *treatment* (participating in OFT or not participating in OFT[17]) and potential *outcomes*, which are also called *responses* (income, labor market states, either at a particular time, or at a particular span of time after having completed OFT). Y_i^t and Y_i^c denote the latter.[18] Additionally, denote variables that are unaffected by treatments, called *attributes* by Holland (1986), by X_i. It remains to define a binary *assignment* indicator S_i, which determines whether unit i gets the treatment ($S_i = 1$) or not ($S_i = 0$). If the unit participates in OFT the actual (observable) outcome (Y_i) is Y_i^t, and Y_i^c, otherwise. This notation points to the fundamental problem of causal analysis. The causal effect, for example defined as difference of the two potential outcomes, can never be estimated, even with an infinite sample, because the *counterfactual* (Y_i^t or Y_i^c) to the observable outcome (Y_i) is never observed. However, it is the important contribution of this literature to show under what conditions objects like average causal effects can be identified from a sample of the population.

As emphasized for example by Rubin and others, in order that the model's representation of outcomes is exactly adequate SUTVA, the *stable-unit-treatment-value assumption* has to be invoked for all members of the population. SUTVA implies that the value of the potential outcomes for

[16] See Holland (1986) for an extensive discussion of concepts of causality in statistics, econometrics, and other fields.

[17] Since the group aggregated in 'not OFT' is very heterogeneous, the reader may rightly wonder whether a disaggregation would be more informative. Similar considerations apply to the aggregation of the OFT group. In particular for the latter, the current aggregation is mainly driven by sample size considerations. In future work this kind of extension will be attempted with a larger or more specific data set. However, note that the causal estimands can be interpreted as the average effect of the different OFT courses weighted by their distribution in the treated population.

[18] As a notational convention big letters indicate quantities of the population or of members of the population and small letters denote the respective quantities in the sample. The units of the sample (n=1,...,N) are supposed to stem from N independent draws in this population.

unit i will be the same no matter what mechanism is used to assign OFT and no OFT to unit i[19] and no matter what treatments the other units receive (e.g. Rubin, 1986, 1991). Furthermore there should be no unrepresented treatments. A particular important case of the independence of the outcomes from the assignment is when individuals are randomly assigned to the treatment (*randomization*).[20]

The framework can be seen as helpful device to design 'informative' social experiments, or - if this is not possible or not desirable - to set up the problem under investigation in such a way that it approximates closely the design of an experiment, and to point out possible departures. Unfortunately, in economic applications there are typically many such possible departures. In this particular case there might be worries that the treatments are too broadly aggregated, because they do take place at different times and have different durations (to be addressed later). Furthermore, there might be interactions between individuals through the market mechanism, because the supply (total number) of treated units should have some impact on their labor market outcomes when trained as well as when not trained. Another advantage of this approach is that it enforces clear distinctions for three different stages of the empirical analysis: the set-up of the problem using an appropriate notation, the assumptions necessary for the identification of the desired quantities, and the final estimation stage.

Finally, the potential outcome approach to causality emphasizes the need to explicitly choose a control group and discuss its characteristics. Ideally, members of this control should be like *clones* of the members of treatment group. This means that they should be identical in all aspects effecting the training decision as well as the potential outcomes (technical definitions of similarity appear later). If it is not possible to find such individuals, additional assumptions have to be invoked to - in some sense - adjust for their dissimilarity.

Before briefly discussing more aspects of this framework, a quick comparison with standard econometric approaches is in order. When a typical regression approach is used, based on modeling particular moments of the potential outcomes (e.g. Heckman and Hotz, 1989, Heckman and Robb, 1985, Maddala, 1983), the same issues as mentioned above need to be addressed to make causal instead of associational inference. The wording will then invoke assumptions relating unobserved error terms to regressors. One tends to speak about various sorts of *exogeneity*, *functional forms*, and *distributional assumptions*, etc., to overcome *selectivity* and *endogeneity* problems. I think that this indirect approach is more likely to hide important issues related to the causal or noncausal nature of the intended inference. Furthermore, basing identifying assumptions on unobserved components of the assumed models has the 'advantage' of immunifying ones work at least in some respects from criticisms. It is generally easier to defend some assumptions on unobserved,

[19] This part of SUTVA can be relaxed in many ways, some of them will be discussed below.

[20] Recently, this framework has also received attention in the econometrics literature, e.g. Angrist and Imbens (1992) and Imbens and Angrist (1994).

unknown and anyhow artificial things like error terms - only in rare cases has the researcher a precise idea what the error term really embodies - than on substantive relationships between important components of the analysis, such as assignment mechanisms. This paper goes the latter route, and, consequently, I hope that it should attract much more informed criticism, which is based on the real problem at hand. Therefore, this criticism can be used in subsequent revisions to obtain more reliable results than the discussion of relationships between unobserved error terms and explanatory variables would ever yield. Finally, another way - perhaps a bit too extreme - to put some of the aspects mentioned above is "... the primary justification for model-based repeated sampling inference appears to be its richness of mathematical results rather than its practical relevance" (Rubin, 1991, p. 1225).

Although there is no answer to the questions from the beginning of this section, in the following I try to answer questions of the sort "What is the *average* gain for those individuals participating in OFT - or subgroups of them - compared to potential nonparticipation of these individuals?" Using the previous notation the estimand of interest, which is the average causal effect of OFT, is denoted by θ^0 and defined in equation (1):

$$\theta^0 := E(Y^t - Y^c | S=1) = E(Y^t | S=1) - E(Y^c | S=1). \tag{1}$$

The short hand notation $E(\cdot|S=1)$ denotes the mean in the population of all units i, for which S=1. If the objective is to draw inference only in a subpopulation of S=1, defined by attributes contained in X, then this and the following expressions are changed in an obvious way.

The question now is how this expression can be identified from a large random sample of the population. The problem is the term $E(Y^c|S=1)$, because the pair $(Y_i^c, S_i = 1)$ is not observed for any individual. Much of the literature on causal models in statistics and selectivity models in econometrics is devoted to find reasonable (depending on the problem at hand) identifying assumptions to predict the unobserved expected nontreatment outcomes of the treated population by somehow using the observable nontreatment outcomes of the untreated population. If participation in OFT would have been decided by a random number generator (random assignment), than the potential outcomes would be independent from the assignment mechanism and it would be true that $E(Y^c|S=1) = E(Y^c|S=0)$. In this case the untreated population could be used as control group, which implies that the expectations of their observable outcome would be equal to $E(Y^c|S=1)$. Given a large enough sample, the corresponding sample moments converge towards these population moments under standard regularity conditions. However, a brief look at table 2 shows that the assumption of random assignment is hardly satisfied. There appear to be several variables which influence assignment as well as outcomes (gender, schooling, etc.).

Using the law of iterated expectations to rewrite the crucial part of equation (1) as:

$$E(Y^c|S=1) = E[E(Y^c|S=1, X=x)|S=1], \quad (2)$$

leads to another identifying restriction, called random assignment conditional on a covariate (Rubin, 1977). The assumption is that the assignment is independent of the potential outcomes conditional on the value of a covariate or attribute (CIA). If this assumption is true, then $E[E(Y^c|S=1, X=x)|S=1] = E[E(Y^c|S=0, X=x)|S=1]$, which can be estimated in large samples using respective sample analogues. The next section will show that this powerful restriction is reasonable in the context under investigation. The important task will be to identify (and observe) all variables that could be correlated with assignment and potential outcomes. This implies that there is no important variable left out which influences outcomes as well as assignment given a fixed value of the relevant attributes.[21] There are many different possible other restrictions (e.g. Angrist and Imbens, 1991, Imbens and Angrist, 1994, Heckman and Hotz, 1989, Heckman and Robb, 1985), but this one appears to be the most fundamental in its close resemblance of the experimental context.

Rosenbaum and Rubin (1983) showed that if CIA is valid, then the estimation problem simplifies further. Let $0 < P(x) = P(S=1|X=x) < 1$ denote the propensity score that is defined as the nontrivial probability of being assigned to the treatment conditional on characteristics x. Furthermore, let b(x) a function of attributes such that $EP[S=1|b(x)] = P(x)$, or in their words, the balancing score b(x) is at least as 'fine' as the propensity score. Their most important result is that if the potential outcomes are independent of the assignment mechanism conditional on X=x, then they are also independent of the assignment mechanism conditional on b(X)=b(x), hence:

$$E(Y^c|S=1, X=x) = E(Y^c|S=0, X=x) = E[Y^c|S=0, b(X)=b(x)]. \quad (3)$$

The major advantage of this property is the reduction of the dimension of the (nonparametric) estimation problem. The disadvantage is that the probability of assignment has to be estimated. However, this estimation may lead to a better understanding of the assignment process itself. Details of this estimation are relegated to section 4.2.2. Section 4.2.2 will also discuss a very particular form of balancing score 'finer' than the propensity score that is especially useful for the specific problems encounter in this evluation study.

4.2 Estimation of the propensity score

4.2.1 Variables potentially influencing the training decision and outcomes

Variables that might influence the decision to participate in OFT as well as future potential outcomes should be included in the conditioning set X and, therefore, in the propensity score to avoid

[21] In the language of regression type approaches such a variable would lead to simultaneity bias.

biased estimates of the causal effects. Variables only influencing the participation decision may also be included to increase efficiency. To judge what variables this may be, it is necessary to have a definition of OFT (see section 3) as well as of the potential outcomes. Typical outcomes considered are gross monthly income for individuals employed or unemployment benefit for the unemployed, employment status, such as full-time employment, unemployment, involuntary short-time work, expected unemployment and expected changes in job positions soon. Two concepts of timing are used for these outcomes, which specify either a date or a specific time span after the completion of the course (see section 4.4.1 for details).

In the following I identify reasons for participation in OFT by supposing that individuals maximize some sort of future utility, or more precisely, the difference of the present value of future income streams for both states. It seems plausible that at least factors influencing both income and participation in OFT can be identified in this fashion. It is not necessary to develop any formal behavior model. Considering the broad building blocs of such a model is sufficient to identify potentially important attributes.[22] In principle one would like to condition directly on these expected income (utility) streams, but since they are unobserved, they have to be decomposed into the cost of OFT and the additional returns of OFT. These factors have to be uncovered, because they are potentially important determinants of the training decision.[23]

There are at least two hypotheses why income with OFT should be higher when without it, everything else being equal. First of all, the additional human capital should increase individual productivity and, therefore, workers should be able to obtain higher wages. Secondly, OFT can act as a signaling device for an employer who has incomplete information on the workers' productivity. Participation in OFT might signal in particular higher motivation, and the successful completing of longer OFT courses may also signal higher ability, and hence the employer will be prepared to compensate for the expected higher productivity. In the first case the additional human capital will yield returns - ignoring effects on pensions - until retirement, or until it is depreciated. This implies at least for older individuals that the remaining period until retirement could be smaller than the depreciation period for the human capital. Therefore, age should not increase the participation probability, but should most likely decrease it. The magnitude of the effect of age under the signaling hypotheses depends crucially on the ability of the employer to learn quickly the true productivity of the worker. When the signal is too positive, employers will try to adjust wages towards true productivity, et vice versa. People sending the 'wrong' signal will only gain a temporary advantage until the employer understands their true productivity. However, by getting employed

[22] For an introduction in this field of labor economics the interested reader is referred to any modern text book, such as Ehrenberg and Smith (1994).

[23] Note that for these considerations, it does not matter how the labor market really works, but how the individual (and/or the labor office) believes it to work. There might be substantial differences between actual and expected outcomes, when considering that individuals are used to the rules of the command type economy of the former GDR. Furthermore, the high speed of changes after unification makes correct predictions difficult.

due to a too positive signal, they may still obtain additional experience that may increase their income as well as employment prospects until retirement. This implies again a negative impact of age on OFT participation.[24] Another factor is how the individual subjectively estimates the own future income streams. For this analysis it is not so important to formulate the exact type of expectation formation as long as it is known what kind of subjective expectations about the own labor market prospects the individual holds.

It is useful to divide the potential costs of OFT for the individual in two broad groups: direct costs and indirect or opportunity costs. Potential direct costs depend mainly on the availability of subsidies (see section 2 for details). Although direct costs should in principle not have much influence on future outcomes, the labor office tends to give subsidies to individuals with comparatively low (nontraining) labor market prospects, as estimated by the labor office. Therefore, there may be an import indirect effect of the labor market prospects on the potential outcomes through the potential costs. The opposite reasoning applies to employer sponsoring, which, however, is not important for OFT. Opportunity costs basically consist of lost income and / or leisure. Since the marginal utility of leisure should be lower during non-full-time work (a larger amount is available), the actual labor status can be an important factor for its own. It may also differ across individuals according to tastes, as well as other socioeconomic factors such as marital status. The labor office, as well as possibly an employer, provides subsidies to make up most of the foregone earnings under similar conditions that apply to direct costs, so that the same reasoning as before is appropriate.

The above analysis has identified age, labor market prospects, actual labor status, and other socioeconomic characteristics as major factors that could potentially influence the employment decision. Before going in more details about the groups of variables used in the empirical analysis, I will discuss more fundamental issues concerning the admissibility of variables in the conditioning set. Additionally, two assumptions are postulated which are very important in that respect for the particular situation in East Germany after unification, because they make CIA a powerful and justifiable assumption in this context.

From the discussion in the previous section the difference between attributes that cannot be influenced by the treatment[25] and outcomes should be clear. It should be also clear that the conditioning variables should be attributes (which could include the expected potential outcomes if they were observed) to get unbiased estimates of the causal effects. These variables do not change over time, change over time independent from the treatment, or they are dated before any action is taken regarding training participation. The latter point is important: Consider an employee accepting a job that pays less than a comparable job with another firm, but offers the possibility of

[24] The only qualification is that older individuals could in principle retire before the employer learns their true productivity, so that for these people P(x) should not decrease with age.

[25] They would be called *exogenous variables* in regression language.

obtaining employer sponsored OFT. The pre-training income on this job cannot be considered as an attribute or exogenous variable, because it already contains an effect of the future treatment. Conditioning on this kind of pre-OFT variable will in this case almost certainly lead to an upward bias in the estimate of the effect of OFT. It is this kind of reasoning that lead to doubts of the exogeneity of many job-related variables. Thus, it could make CIA an untenable assumption in many cases when long term planning is involved (this might be conjectured for OFT in West Germany for example).

However, the specific situation in East Germany before and after unification makes CIA a far more plausible assumption. The first hypothesis is that the complete switch from a centrally planned economy to a market economy in mid 1990 accompanied by a completely new incentive system, invalidates such long term plans. It was generally impossible for East German workers to predict the impact and timing of this system change.[26] Even when it was partly correctly foreseen, it was generally impossible to adjust behavior adequately in the old system. This assumption, which seems to be highly realistic, allows me to use all pre-unification variables as attributes.

An additional assumption will be invoked which is related to the condition of the labor market in the rapidly contracting East German post-unification economy. Figure 1 and 2 show that the labor market is characterized by rapidly and continuously rising unemployment as well as declining full-time employment. Furthermore, only about 10% of those working full-time in mid 1990 were sure that they might not lose their job within the next two years. I assume that no individual - having only slim chances of rehirering once being unemployed - will voluntarily give up employment to get easier access to training funds (which may not even be the case before 1993, given the official guidelines for obtaining assistance from the labor office). This assumption allows me to consider monthly pre-training information on full-time employment, involuntary short-time work and unemployment as attributes. Additionally, a pre-training change to self-employment is assumed not to be done to obtain training. The risk of self-employment is far too high to be plausible to occur to get such a comparatively small gain. Therefore, pre-training self-employment, which is measured on a yearly basis, is considered an attribute.

Given the institutional framework outlined above, it is tempting to include the latest pre-training expectation about future job-security in the set of attributes. On the one hand, this variable may very well capture threats to the current employment - important to obtain AFG-subsidies - and, therefore, it may be considered to be an attribute. On the other hand it may also be considered to be an outcome: For instance, an employer may offer an employee a future training possibility. This will change the expectation of the employee, and the employee will now assume that the current job is safe. Therefore, future OFT alters pre-training expectations, which could no longer be con-

[26] This is even more plausible, when one considers what a miserable job West and East German experts did in predicting the impact of these changes.

sidered an attribute. Hence, they cannot be used as conditioning variables in this framework. Fortunately, figures 13 and 14 below strongly suggest that this is not a problem in the context considered.

The groups of variables that are used in the empirical analysis to approximate and describe the above mentioned four broad categories of determining factors are age, sex, marital status, educational degrees as well as regional indicators. Features of the pre-unification job position are captured by many indicators including wages, profession, job position, employer characteristics such as firm size or industrial sector, among others. Individual future expectations are described by individual pre-unification predictions about what might happen in the next two years regarding job security, a change in the job position or profession, and a subjective conjecture whether it would be easy to find a new job or not. Details of the particular variables, which are mostly indicators, as well as their means and standard errors in the treatment and control group are contained in table A.1 of Appendix A. Furthermore, monthly employment status information, as mentioned before, is available from July 1989 to December 1992.

Having discussed potentially important factors and variables available for the empirical analysis, the question is whether some important variable might be missing. One such variable can be described as motivation, ability and social contacts. I approximate these kind of attributes by the subjective desirability of selected attitudes in society in 1990, such as 'performing own duties', 'achievements at work', and 'increasing own wealth', together with the accomplishment of voluntary services in social organizations and memberships in unions and professional associations before unification, as well as schooling decrees and professional achievements. Additionally, there are variables indicating that the individual is not enjoying the job, that income is very important for the subjective well-being, that the individual is very confused by the new circumstances, and optimistic and pessimistic views of general future developments. Another issue is the discount rate implicitly used to calculate present values of future income streams. I assume that controlling for factors that have already been decided by using the individual discount rate, such as schooling and professional education, will be sufficient. Other issues concern possible restrictions of the maximization problem, such as borrowing constraints, and a limited supply of OFT. Borrowing constraints can be a serious issue, but there seems to be no sure way to find out using this data set, because it does not contain information on monetary wealth.[27] It seems reasonable to assume that OFT supply is concentrated in larger cities. Unfortunately, the only regional information available refers to the six federal states, because due to data security considerations the agency supervising the panel survey is currently not prepared to release information on the number of inhabitants of cities and regional areas. However, it is possible that some supply factors as well as information about the availability of OFT could be captured by the indicators for memberships in unions, pro-

[27] Property wealth is not informative, because the ownerships of many properties were unclear due to claims of former owners after unification. Therefore, they would be very difficult to sell or to use as collateral for loans.

fessional associations and cooperatives. I conclude that although some doubts could be raised, it seems safe to assume that these missing factors (conditional on all the other observable variables) play only a minor role.

Finally, empirical papers analyzing training programs in the US point to the importance of transitory shocks before training, partly because of individual decision, partly because of the policy of the program administrators. Card and Sullivan (1988) find a decline in employment probabilities before training. Here, the monthly employment status data should take care of that problem. Ashenfelter and Card (1985) observe a decline in earnings prior to training. As will be shown in section 4.4, there is no evidence of this phenomenon in the sample used here. This could be probably due to the short-time span between the start of OFT and unification.

4.2.2 Econometric considerations

The estimation of the propensity score is not straightforward, because there are potentially important variables - monthly pre-training employment status and yearly pre-training self-employment - which are related to the months or years before the beginning of OFT. Since these dates differ across OFT participants, they are not clearly defined for the control group. An approximation, which might be appealing at first sight, is to choose an arbitrary date for the controls and compute the value of these variables regarding this date. However, having the same date for all controls and different dates for the OFT participants leads to a dependence of this variable on OFT participation, the dependent variable. This dependence is aggravated by the rapidly changing labor market conditions. Therefore, such a variable cannot be considered an attribute or an exogenous variable, so that a probit estimation would lead to inconsistent estimates of the propensity score. Consequently, I have to use a particular form of a balancing score that is different from the propensity score for the conditioning.

Partition the vector of attributes in two groups such that $X = (V, M)$, and suppose that $P(S=1|X=x) = P(x) = P[V\beta^0 + f(M,U) > 0|V=v, M=m]$. U denotes some attributes not included in X, which are independent of the potential outcomes, but influence OFT participation. V contains pre-unification and time invariant attributes. β^0 is a fixed parameter vector. M denotes time variant pre-training variables. If the potential outcomes are independent of S conditional on $P(X) = P(x)$, then it is also true that they are independent of S conditional on ($V\beta^0 = v\beta^0$, $M = m$), because $(v\beta^0, m)$ is a balancing score. Note that the use of $v\beta^0$ instead of v can still lead to a dramatic reduction of the dimension of the conditioning set. The rest of this section discusses consistent estimation of $v_n\beta^0, n = 1, \ldots, N$, up to scale (and a constant that does not vary in the population). The application of the 'conditioning', along with statistics showing the importance of M, is referred to section 4.3.

In the following I estimate a conventional binary probit model by maximum likelihood. The basic condition for the consistent estimation of the linear index up to scale is that the conditional expectation of the dependent variable is correctly specified:

$$E(S=1|V\beta^0 = v_n\beta^0) = \Phi(v_n\beta^0), \qquad n=1,\dots,N. \tag{4}$$

$\Phi(v_n\beta^0)$ denotes the cumulative distribution function of the standard normal distribution evaluated at $v_n\beta^0$. The first of two sufficient conditions for (4) to hold is that the propensity score has the additive form $P(x) = P[V\beta^0 + f(M,U) > 0|V=v, M=m]$. This assumption is not so restrictive, because V may contain flexible functional forms for the attributes, such as polynomials or interaction terms. The crucial assumption is that:

$$f(M,U)|V\beta^0 = v_n\beta^0] \sim N(0,\sigma). \tag{5}$$

$N(0,\sigma)$ denote the normal distribution with mean 0 and variance σ^2. Neither the assumption of mean zero nor of unit variance is a problem, because required identification is only up to scale and location. The crucial assumptions are normality and independence with respect to $V\beta^0$. Conditional homoscedasticity (implied by independence) and normality is tested using conventional specification tests (similar to Bera, Jarque, and Lee, 1984, and Davidson and MacKinnon, 1984) described in detail in Blundell, Laisney, and Lechner (1993) and in Lechner (1995).[28] A second way to check for the independence of $V\beta^0$ and M is to compute empirical correlations of the estimated index and the observable m_n. Furthermore, the consistency property of the specification tests will eventually detect any other dependence of $V\beta^0$ and f(M,U).

4.2.3 Results

Table 3 presents the results of the probit estimation and the specification tests.[29] All variables that are not contained in table 3, but described in table A.1, as well as different functional forms for the (approximately) continuous variables, and interaction terms between *Gender* and variables related to job position and education, are subjected to score tests against omitted variables. None of them appears to be significantly missing at the 4% level. Most results are above the 10% level.

[28] The use of semiparametric methods, such as SNP estimation suggested by Gabler, Laisney and Lechner (1993) has been considered. However, it is not necessary, because the specification tests indicate no violation of the distributional assumptions necessary for the probit model.

[29] A table for the tests against missing variables is omitted for reasons of space. The results are available on request from the author.

Table 3: Results of the estimation and the specification tests for the participation probit

	estimation		heteroscedasticity test	
Variable	coef.	std.err.	$\chi(1)$	p.-val.
Sex: female	0.12	0.14	1.2	0.28
Federal states (Länder) in 1990: Berlin	0.37	0.20	2.9	0.09
Years of schooling (highest degree) in 1990				
12	-0.04	0.30	3.2	0.08
10	0.33	0.17	2.6	0.11
Highest professional degree in 1990				
university	0.12	0.37	0.1	0.73
university and female	**0.98**	0.31	0.1	0.77
engineering, technical college	0.31	0.18	0.2	0.63
master of a trade / craft	0.40	0.22	0.1	0.75
Job position in 1990: highly qualified, management	**0.44**	0.20	1.9	0.17
Job characteristics in 1990				
real wage or salary per month / 1000	**-2.12**	0.79	4.4	0.04*)
ln (real wage or salary per month)	**3.04**	1.35	3.4	0.06*)
temporary job contract	-0.53	0.35	0.3	0.61
training (unspecified) while full-time employed	**0.47**	0.17	0.5	0.49
Profession in 1990 (ISCO)				
scientific, technical, medical	-0.31	0.18	0.5	0.48
production	**-0.75**	0.17	0.7	0.42
services, incl. trade, office	-0.21	0.17	0.80	0.37
Employer characteristics in 1990: industrial sector				
agriculture	**-0.64**	0.32	1.7	0.19
mining	-0.70	0.46	0.1	0.72
heavy industry	-0.53	0.32	0.6	0.44
light industry, consumer goods, electronics, printing	-0.32	0.28	0.0	0.95
machine building and vehicle construction	-0.08	0.29	8.0	0.005*)
construction	-0.32	0.33	0.1	0.73
trade	**-0.84**	0.33	2.0	0.15
communication, transport	**-1.05**	0.41	1.4	0.23
other services	-0.51	0.28	0.5	0.47
education, science	**-0.72**	0.29	0.9	0.35
health	-0.59	0.31	3.0	0.08*)
Optimistic about the future in general in 1990	**0.34**	0.14	0.2	0.69
Expectations for the next 2 years in 1990				
redundancies in firm: certainly not	-0.49	0.30	0.2	0.66
Score test against nonnormality			0.4	0.80

Note: **Bold** letters: t-value larger than 1.96.
*) Different covariance estimates lead to very different conclusions.

t-values and test results presented in table 3 are computed using the GMM (or PML) formula given in White (1982).[30] Cases when other ways of estimating the covariance matrices of the tests lead to very different results are marked by an asterix.

[30] Five versions are computed: based on matrix of the outer product of the gradient (OPG) alone, on the empirical hessian alone, on the expected (under the null) hessian alone, and on combining the hessian, respectively the expected hessian (under the null), and the OPG. Previous Monte Carlo studies (e.g. Davidson and MacKinnon,

The situation in East Berlin - now part of a single federal state with West Berlin - is quite different to the situation in the rest of East Germany. On the one hand, there is easier access to already existing OFT facilities in the West Berlin, and on the other hand, the direct and almost immediate exposure of East Berliners to the Western system with the more adequately qualified Western workers may have increased the pressure to obtain additional qualifications. Furthermore, the skill composition of population differs somewhat from the rest of the country, because East Berlin was the capital and the admistrative center of the former GDR. Therefore, it is not surprising that living in East Berlin is a (weakly) significantly positive factor for OFT participation.

The effect of gender as well as of education manifests itself c.p. basically through a large and significantly higher participation probability for the relatively small group of women with university education (5% of the sample). Other female-education and female-job-position interaction terms are not significant. Furthermore, the reason for the insignificance of *12 years* of schooling, which is the university entrance requirement, may very well be due to its high correlation with professional degree *university* and the respective female interaction term. Taken together, the results in the first part of table 3 suggest that having a low educational and professional level in the former GDR reduces the probability of OFT participation. This finding is confirmed by the significantly positive effect of a high job position.

The estimated effect of gross income (in 1993 DM) is nonlinear. It attains its maximum at 1434, which implies that the income effect is positive for the first third of the income distribution and negative for the remainder part. Individuals who obtained some kind of training while being full-time employed in 1990 have a significantly higher OFT probability. Although there is a correlation with age, the mean in this group of 31.7 years is too high to justify the assumption that this variable captures vocational training which - due to a reporting error or an ambiguity in the question - has been described as full-time work by the respondent. The more likely interpretation is that people who were more likely to get some kind of training on the job in the former GDR are also more likely to receive OFT after unification. The results in table 3 show also marked differences regarding professions and sector of the economy: production workers and people working in agriculture, trade and most service sectors are c.p. significantly less likely to be observed in OFT.

It is noteworthy that except for two variables - no redundancies expected (only 6% of the sample) and a general optimistic outlook - none of the subjective expectation variables (in 1990) play any role in the propensity score. This could either be due to expectations changing so rapidly that those held in mid 1990 had no implication for later OFT decisions, or that OFT participation is much more a reaction to temporary and unexpected shocks, like actual unemployment. The results

1984, Lechner, 1991) as well as theoretical papers (e.g. Dagenais and Dufour, 1991) show that tests based on the latter at least avoid some undesirable properties which can occur with other versions (a brief survey of these issues are contained König and Lechner, 1994, see also Davidson and MacKinnon, 1993). Therefore, the results given in table 3 are computed using these estimates of the covariance matrix.

in section 4.4 will show that the former explanation is not supported by the data, since when taking into account the latest pre-training expectation about job security, no significant difference appears between OFT and control group (conditional on the propensity score and the monthly pre-training employment status). Furthermore, the insignificance of the subjective indicators of the difficulty of finding a new job and the objective 'job-danger' indicators, like having only a temporary contract or being already fired lends support to the claim that expectations concerning the security of the own job did not matter much for the OFT decision. However, the importance of shocks will be demonstrated in the next section by showing that the probability of being unemployed in the month just before OFT is much higher for OFT participants than for the control group in the same month (see figure 6).

A comparison of table 3 and table A.1 reveals that many variables related to marital status, the federal states, motivations and general attitudes, memberships in job related organizations, finer groupings of job positions, professions and professional degrees are all 'not necessary' in the estimation of the propensity score.

Unfortunately, a comparison with other results concerning training participation is difficult, because some of the studies investigate the training participation in very different environments and/or use very different econometric methods and approaches, etc. (e.g. Lynch, 1992, Hübler, 1994, O'Higgins, 1994, Helberger and Pannenberg, 1994).

It remains to check some of the stochastic assumptions implied by the mutual independence of the error term $f(M,U)$ and $V\beta^0$, and the normality of $f(M,U)$. First of all, note that the last two columns of table 3 largely do not seem to contradict the assumption of conditional homoscedasticity. In the cases for which a rejection occurs, statistics based on different estimates of the covariance matrix of the test indicators suggest entirely different decisions regarding rejecting the null of no misspecification or not. This behavior could suggest that in these cases the $\chi(1)$ distribution, which is only valid asymptotically, may be a poor choice in small samples. Resolving this puzzle is left to future work. The normality test does not reject the probit.

Table 4: Correlation of the estimated propensity score with potentially omitted time variant variables

	unemployment			involuntary short-time work			full-time employed			self-employed
	last m.	4 m.	all m.	last m.	4 m.	all m.	last m.	4 m.	all m.	last (yearly)
$V\hat{\beta}$	-1.2	-1.2	-1.3	0.2	1.3	-0.2	-2.0	-3.1	-2.5	-1.2

Note: *last m.*: last month; *4 m.*: four months' average (weighted towards the last month by using the weights: 0.173, 0.217, 0.271, 0.339); *all m.* : average of all months after unification and before OFT. The reference month for the control group is Dec. 1991.

Checking the correlation of $v\hat{\beta}$ with the potentially observable part of the error term does also not reveal any particular problem (see table 4). In conclusion, the results of the various tests can be interpreted as not providing enough evidence to reject the maintained model.

4.3 Nonparametric estimation of causal effects and matching

The considerations in the previous sections suggest to estimate the causal effects by nonparametric methods in order to avoid potentially incorrect functional form restrictions. To ease notation assume that observations in the sample are ordered such that the first N_t observations receive OFT, and the remaing ($N-N_t$) observations do not. The following two nonparametric regression estimators are obvious choices:

$$\hat{\theta}_N^1 = \hat{E}^1(Y^t - Y^c|S=1) = \frac{1}{N_t}\sum_{n_t=1}^{N_t} y_{n_t} - \hat{g}^c(x_{n_t}), \tag{6}$$

$$\hat{\theta}_N^2 = \hat{E}^2(Y^t - Y^c|S=1) = \frac{1}{N_t}\sum_{n_t=1}^{N_t} \hat{g}^t(x_{n_t}) - \hat{g}^c(x_{n_t}). \tag{7}$$

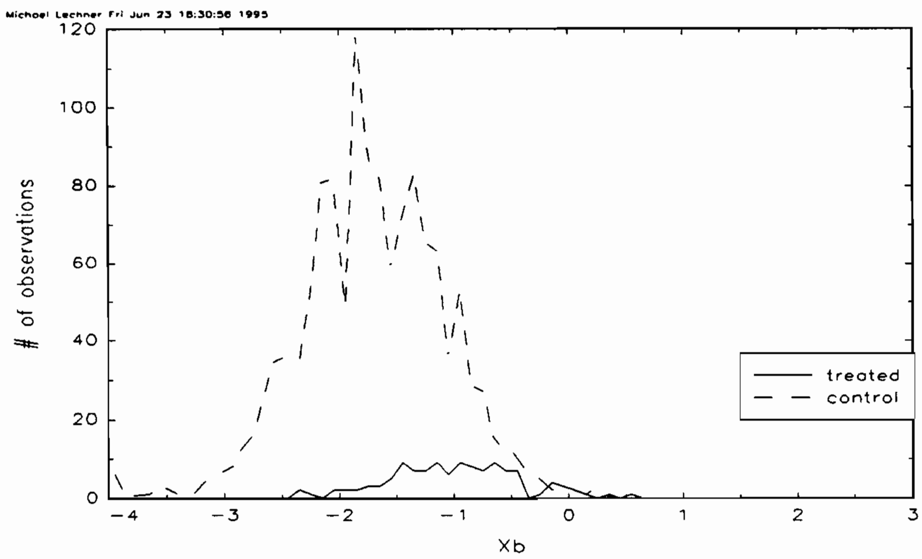
$\hat{\theta}_N^1$ and $\hat{\theta}_N^2$ denote the estimate of the causal effects that are averaged over the sample of the N_t-treated observations only. $\hat{g}^t(x_{n_t})$ denotes a consistent estimate of $E(Y^t|S=1, X=x_{n_t})$ (estimated in the treated pool with observations close to x_{n_t}) and $\hat{g}^c(x_{n_t})$ denotes a consistent estimate of $E(Y^c|S=1, X=x_{n_t})$ (estimated in the control pool with observations close to x_{n_t}), respectively. Although, generally neither $\hat{g}^t(x_{n_t})$ nor $\hat{g}^c(x_{n_t})$ are square root normal, it can be conjectured that under mild regularity conditions both $\hat{E}^1$ and $\hat{E}^2$ are consistent estimates and that both $\sqrt{N_t}\hat{E}^1$ and $\sqrt{N_t}\hat{E}^2$ converge to a normal distribution with a fixed variance.

Generally, the practical problem of the high dimension of x_{n_t}, which in multivariate nonparametric regressions typically requires a large number of observations close to x_{n_t}, can be overcome be using the propensity score property. Instead of computing the multivariate regression to obtain $\hat{g}^t(x_{n_t})$ and $\hat{g}^c(x_{n_t})$, it is sufficient to compute univariate regressions using the estimated propensity score instead of x_{n_t}. However, in the particular case considered in this paper - some attributes are only defined in relation to a particular treated observation - part of the problem of the dimension being too large remains. To capture the employment information appropriately, nine monthly variables (see table 4) and a yearly variable have to be used additionally to the propensity score. Hence, the dimension of the nonparametric regression is so high, that serious small sample problems can be expected for the size of the sample available for this study. Additionally, a separate estimation of $\hat{g}^c(x_{n_t})$ in the control pool is necessary for each different starting date of OFT, which would be a huge computational burden.

For these reasons I choose to use a simpler nonparametric approach that appeared in the statistics' literature (e.g. Rosenbaum and Rubin, 1983, 1985). The basic idea is to find for every treated observation a control observation that is as close to it as possible in terms of a balancing score. When an identical control observation is found, the estimation of the causal effects is unbiased. In cases of 'mismatches', it is often plausible to assume that using local regressions on these differences will remove the bias (see section 4.4 for details). Note that compared to the nonparametric regression described above, there is an efficiency loss, because observation n_t and its closest neighbor n the control population - instead of possibly many close neighbors - are used.

A basic requirement for a successful (i.e. bias removing) implementation of a matching algorithm is a sufficiently large overlap between the distributions of the conditioning variables in both subsamples. Figure 5 shows the overlap for a very important conditioning variable, $v\hat{\beta}$.

Figure 5: Distribution of $v\hat{\beta}$ *for OFT and controls*



Note: 0.1 grid used. Mean (*std*) in OFT (treated) / control sample is -0.98 (*0.56*) / -1.71 (*0.54*).

Although the mass of the distribution of the controls is to the left of the treated, it seems that there is sufficient overlap for most part of the treated distribution. Table 5 contains some descriptive statistics of attributes in the treated and in different control samples. Comparing the first two columns of that table shows that matching on the propensity score alone makes the distribution in the control sample very similar to the distribution in the treated sample.[31] However, it should be noted that conditioning is on $v_{n_t}\hat{\beta}$ instead of $v_{n_t}\beta^0$. The asymptotic standard error[32] of $v_{n_t}\hat{\beta}$ resulting from the estimation of $\hat{\beta}$ can be considerably and ranges from 0.19 to 0.96 in the OFT sam-

[31] The different versions of the matching algorithms are obvious simplifications of the algorithm given in App. B.1.
[32] Computed using the delta method.

ple, and from 0.17 to 1.55 in the control sample. The mean in the OFT (control) sample is 0.31 (0.30), the median 0.29 (0.28), and the empirical standard deviation 0.10 (0.10). Therefore, it can be expected that by matching only approximately on $v_n\hat{\beta}$, but additionally also on some important components of v directly, a better a better match could be obtained.

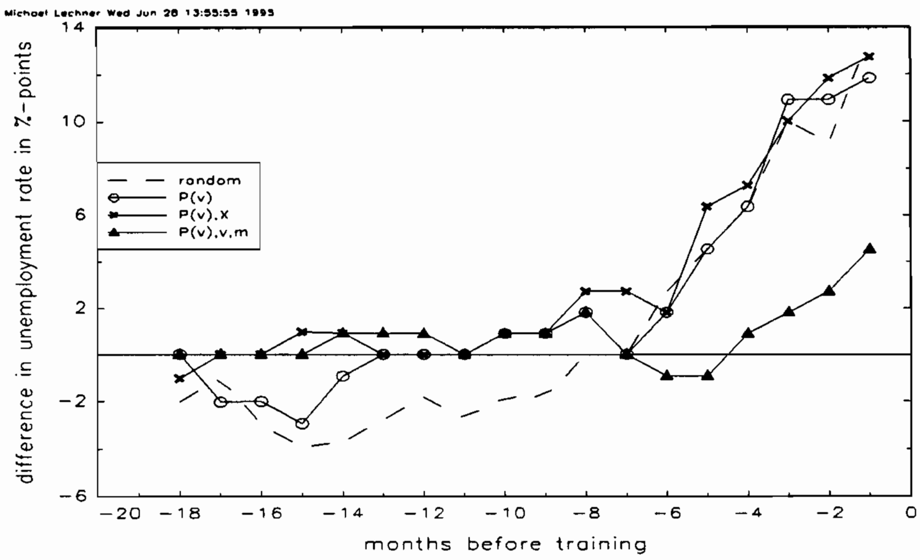
Table 5: Descriptive statistics of selected variables of OFT and control sample: Different matching algorithms

	Controls				OFT (110)
	all (1205)	matched on $v\hat{\beta}$ (110)	and selected v-variables	m-var.	
	(2)	(3)	(4)	(5)	(6)
Variable	mean (std), share in %	mean (std), share in %	mean (std), share in %	mean (std), share in %	mean (std), share in %
$v\hat{\beta}$	-1.71 (.64)	-0.98 (.54)	-0.99 (.54)	-1.00 (.54)	-0.98 (.56)
Age in 1990	35.2 (8.1)	35.6 (8.0)	35.1 (7.0)	33.4 (7.0)	35.9 (7.5)
Gender: female	42	62	69	65	65
Federal states (Länder) in 1990					
Berlin	7	9	13	12	12
Sachsen-Anhalt	21	20	14	14	15
Years of schooling (high. deg.) 1990					
12	17	34	29	25	27
10	60	61	62	66	65
8 or no degree	22	5	9	9	8
Highest professional degree in 1990					
university	11	26	25	22	25
engineering, technical college	16	33	31	29	31
skilled worker	64	35	40	38	37
Job position in 1990					
highly qualified, management	19	49	44	41	41
skilled blue and white collar	56	35	41	40	44
Job characteristics in 1990					
wage / salary per month(defl.)	1708 (524)	1802 (396)	1737 (382)	1700 (373)	1711 (393)
training (unspecified) while full-time employed	7	14	17	17	16
Profession in 1990 (ISCO)					
scientific, technical, medical	20	40	43	37	37
production	42	11	7	11	14
services, incl. trade, administ.	23	24	25	24	25

Note: 1990 relates to the date of interview which for almost all cases was completed before July 1990 (EMSU). Ratio of variance of $v\hat{\beta}$ in OFT sample over variance in control sample is 0.74. Average width of a caliper is 0.42. v-variables used for the additional conditioning are: *gender, Berlin, university, 10 years of schooling, expectation of no redundancies in firm for the next two years* (1990), *highly qualified or management job position* (1990), *monthly wage / salary* (1990), *training (unspecified) while full-time employed* (1990); see also note to tables A.1.

The details of the matching algorithm used are described in Appendix B.1. It follows Rosenbaum and Rubin (1985) suggestion of "matching within calipers of the propensity score" with the exception that window sizes (caliper widths) depend explicitly on the precision of the estimate $v_n\hat{\beta}$. The more precise $v_n\hat{\beta}$ is estimated, the smaller is the width. The additional variables used (col. 4 in table 5) are *gender*, *Berlin*, *university*, *10 years of schooling*, the *expectation of no redundancies in the firm for the next two years* (1990), a *highly qualified or management job position* (1990), *monthly wage / salary* (1990) and *training (unspecified) while full-time employed* (1990). The results that are contained in column (3) of table 5 appear to resemble the distribution of the OFT sample (6) closely.

Figure 6: Difference of registered unemployment between OFT and matched control groups: a comparison of different matching algorithms



Note: See note to tables 4 and 5.

As mentioned in section 4.1 conditioning on monthly employment information to capture the impact of temporary shocks could be important. Figure 6 shows indeed that including only $v_n\hat{\beta}$ in the balancing score is insufficient. The figure displays the difference in the unemployment rate between OFT and different control samples relative to the number of months before OFT. The three lines that are highest in the right hand part of the plot are based on the matching methods mentioned so far plus a random draw in the control pool (col. 2 in table 5). They are almost indistinguishable from each other, but reveal unemployment rates that are about 12-13%-points lower than for the OFT sample. [33] Conditioning additionally on the employment status after unification

[33] The level of unemployment in the month just prior to OFT is 19% for those receiving OFT (involuntary short time work: 11%, full-time work: 67%).

(see table 4 for details on the variables used) reduces the bias significantly. Although there is still some upward bias, figures in the next subsections will show that it is not significantly different from zero. Therefore, all the following evaluations are based on this matched sample.

It is noteworthy that in the first part of their paper Card and Sullivan (1988) choose a very similar approach. They match treated and controls regarding their pre-training employment history. Unfortunately, they are in a worse position, because their data is subject to potentially considerable measurement error concerning these variables. Additionally, the variables are only measured on a yearly basis, so that the employment status just prior to training is unknown. Furthermore, they completely ignore the kind of variables that enter the partial propensity score in this analysis. Therefore, it is not surprising that they decide that this kind of conditioning is insufficient to yield unbiased estimates and switch over to a model based approach.

4.4 Evaluation

4.4.1 Outcomes

This paper is particularly interested in the effects of OFT on post-training changes in actual and anticipated labor market status and prospects. It is due to the nature of the data and circumstances (German unification in 1990) that at the time this paper is written only short-run effects of OFT can possibly be discovered.

The following actual outcomes are measured on a monthly basis by way of the retrospective employment calendar: involuntary short-time work, registered as unemployed, and full-time employment. In addition, the latter two variables are also available for the date of the yearly interview. Another variable capturing characteristics of the actual labor market status - measured once a year - is gross monthly income. For those being employed, it is defined as the gross monthly income in the month before the interview. For those not being employed, imputed unemployment benefits or social assistance - whichever is higher - are used instead (see Appendix A for details). Labor market prospects are measured once a year as individual expectations or worries. They include expectations whether one might lose one's job in the next two years, and whether one is very worried about the security of the current job.[34] Additionally, there is information whether individuals expect an improvement or a worsening of the current job (career) position.[35] It is important to note for the discussions in the following subsection that except for the income variable all other outcome variables are coded as binary indicators.

Finally, there is the issue of comparing outcomes for individuals participating in courses with different end dates. Here, two concepts of comparison are applied. They consist either in specifying a

[34] For non-employed individuals these variables are coded as being very worried and as expecting unemployment.

[35] For non-employed individuals these variables are coded as expecting no improvement and no worsening.

date (early 1993 for yearly information, or a specific month before Jan. 1993 for monthly information) or a specific time span (months or intervals of 0-1, 1-2, 2-3 years for yearly information) after the completion of the course. Note that the number of observations available for the evaluations decreases with the length of the time span considered.

4.4.2 Econometric issues

Define the differences in matched pairs in the sample, which consists of independently drawn observations, as $\Delta y_{n_t} = y^t_{n_t} - y^c_{n_t}$, $\Delta x_{n_t} = x^t_{n_t} - x^c_{n_t}$, $n_t = 1,\ldots,N_t$. $y^c_{n_t}$ and $x^c_{n_t}$ denote values of an observation from the pool of individuals not participating in OFT (controls) that is matched to the treated (OFT) observation n_t. When the outcomes are approximately continues variables, e.g. income, then Δy_{n_t} is approximately continuous. Otherwise, the outcomes are measured with indicators (0, 1) and Δy_{n_t} takes on the discrete values -1, 0 and 1. The estimate of the average causal effect and the respective standard error are computed as:

$$\hat{\theta}_{N_t} = \frac{1}{N_t}\sum_{n_t=1}^{N_t}\Delta y_{n_t}, \qquad \mathrm{Var}(\hat{\theta}_{N_t}) = \frac{1}{N_t}(S^2_{y^t} + S^2_{y^c}). \tag{8}$$

$S^2_{y^t}$ and $S^2_{y^c}$ denote the square of the empirical deviation of y_t in the OFT sample and in the sample matched to the OFT-sample, respectively.[36] As mentioned in the previous section, when a perfect is achieved, implying that $\Delta x_{n_t} = 0$, $n_t = 1,\ldots,N_t$, these estimates are unbiased (cf. Rosenbaum and Rubin, 1983). When the sample is large enough the normal distribution can be used to perform tests and compute confidence intervals. Equation (8) denotes the baseline nonparametric estimate of the causal effect to be discussed in the following subsection. Those are also computed for subpopulations defined by attributes or course characteristics. Note that no assumption is necessary regarding whether or not the treatment effects may differ across the population.

Now, let us consider the case when there is local mismatch, in the sense that although Δx_{n_t} is close to zero - and would be closer if the sample would have been larger ($N_t < N / 2$)- it is actually different from zero. There may be two reasons for local mismatches: On the one hand the coefficients of the propensity score are estimated, and therefore matching on $v_n\hat{\beta}$ could be different from matching on $v_n\beta^0$ in finite samples. On the other hand, the pool of available control observations may be too small to contain exact matches. Again, this problem is less severe with large (control) samples that have a sufficient overlap of attributes with the OFT participants. To correct for biases that could arise from these problems, some modeling is used.

[36] Note the variance estimate exploits the fact that the matching algorithm proposed in App. B.1 never chooses a control observation twice.

Define for the treated subpopulation the variables $\Delta Y_i = Y_i^t - Y_i^c$ and $\Delta X_i = X_i^t - X_i^c$ as the population difference for pairs that would have been matched, if the complete population were available to the researcher. As before, Y_i^c and X_i^c denote the attributes of a control observation matched to the treated observation *i*. Assume for the purpose of illustration, that these matches remain imperfect, so that ΔX_i may be small, but different from 0. In the case of continuous variables it seems reasonable to assume that the conditional expectation of the dependent variable is linear in ΔX_i, because matching has already removed almost (if N is finite) all differences in the X variables, so that in fact the ΔX_i or Δx_{n_t} are local deviations. Local smoothing using a linear conditional expectation is not very restrictive and standard linear regression methods can be used to estimate the average treatment effect θ^0 (cf. Rubin, 1979) by regressing the differences in the attributes and a constant on the differences in outcomes.[37] Appendix B.2 shows what conditions are necessary for the estimated constant term of that regression to be a consistent estimate of θ^0, when the treatment effect actually varies over the population. Suppose now that the outcome consists of only two values, say 0 and 1. Clearly, using a linear approximation for these differences of probabilities is not so attractive as before, except when ΔX_i is very small. Therefore, I use an ordered probit model instead of a linear model to estimate the coefficients of these probabilities based on an underlying latent normal model. Given consistent estimates of the coefficients, the difference of the probabilities is estimated. The standard errors are computed using the delta method (for details see Appendix B.2).

The same approaches are chosen to check whether the treatment effects vary either with characteristics of the courses, such as its duration, or with characteristics of the individuals participating in OFT. Note that this procedure is not nested in the previous one, because now the assumption that either the treatment effect is stable or varies in a particularly specified way is indispensable (Appendix B.2). Therefore, splitting the samples in subpopulations and performing estimations in these subpopulations that do not require such an assumption is an attractive alternative for discrete attributes and characteristics. However, when the attributes and characteristics have too many different values some modeling is required given the size of the sample used in this study.

It should also be remarked that whenever regression type adjustments are used for different dates (time spans) for the same outcome variable, no cross-period-coefficient restrictions are assumed to hold, but the estimations are performed for each date or time span separately. Finally, for the yearly variables all means, variances and regressions are also computed using the appropriate panel weights. Since there are only minor differences among weighted and unweighted estimates, the former are not computed for the monthly data.

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[37] Standard errors are computed using a heteroscedasticity robust estimator. The particular variant is labeled as HC_2 by Davidson and MacKinnon (1993, p.554) and has good small sample properties.

4.4.3 Results

The first set of results is given in figures 7 to 14. It shows the differences between the control and the OFT group for specific time spans before and after the training for a selected group of outcome variables (* 100 for outcomes that are indicators).[38] For variables measured by the calendar (see figure 3) the distance is expressed in months, for those measured only for the month of the yearly interview, the distance is expressed in years.[39] The figures cover up to 18 months or up to 3 'years' before the training and up to 29 months or 3 'years' after OFT. They display the mean effect and its 95% confidence interval based on the normal approximation. The estimates that are not corrected for mismatch are shown as lines.[40] The mismatch corrected estimates (post-treatment only) are displayed as unconnected symbols.

The number of observations available to compute the respective statistics decrease the longer the distance to the incidence of OFT is. The implications of this are threefold: First of all, the variance increases. Although this is reflected in the widening of the confidence band, the accuracy of the estimated band itself may deteriorate, because the normal distribution may be not a very good approximation of the sample distribution when the sample gets too small. Finally, a mismatch correction may be impossible or very imprecise, because there may be too few observations to identify and estimate the parameters of the ordered probit model.[41]

Figures 7 to 8 present the monthly unemployment status for the complete sample and a subsample of individuals being not employed during OFT. The part left to the 0 horizontal mark allows a judgment about the quality of the matches concerning the particular variable.[42] The pre-OFT outcomes here are denoted as unaffected outcomes in his terminology. As already noted in the discussion of figure 6, there is some excess unemployment just prior to beginning of the course, which is however not significantly different from zero.

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[38] The results for those outcomes that are mentioned in section 4.4.1, but do not appear here, are not qualitatively different from the ones presented. Therefore, they are omitted for the sake of brevity.

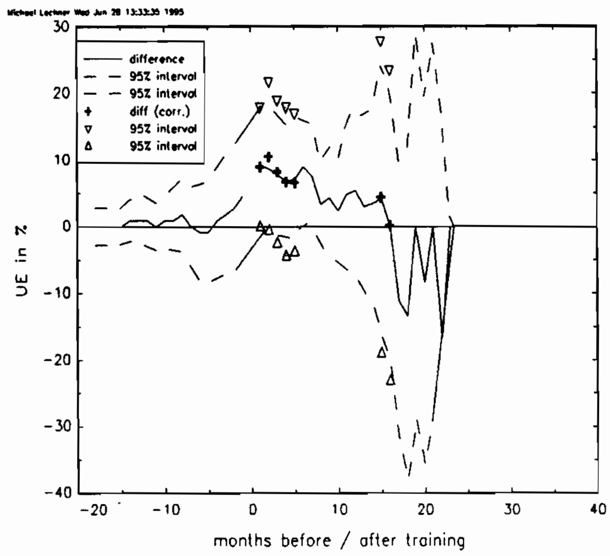
[39] The time span denoted as the first year is actually the time after the end of OFT and the next interview. Therefore, this time span may vary among individuals. Currently, the monthly data available starts in July 1989 and ends in December 1992, whereas the yearly data ranges from mid 1990 to early 1993.

[40] There is no observation at zero. When observations in t = -1 and t = +1 appear to be connected by a line, the reader should ignore this line.

[41] All computations based on less than 5 observations are suppressed. Furthermore, for the plots based on the monthly data do not display any effect or confidence bounds above +40 or below -40.

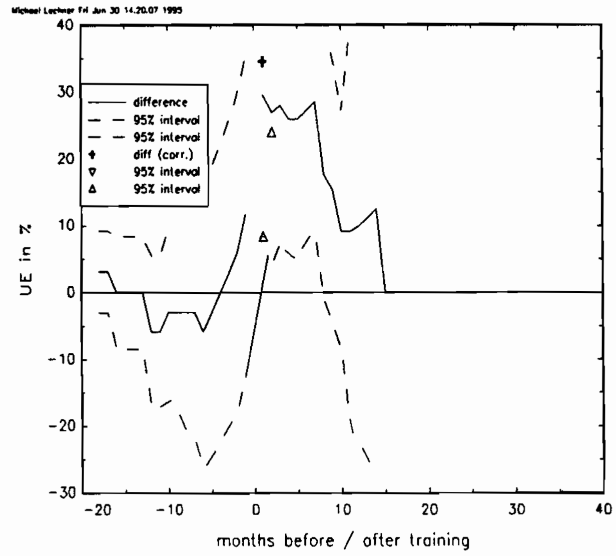
[42] Testing whether these lines deviate significantly from zero is in the same spirit as the tests suggested by Rosenbaum (1984) to use overidentifying restrictions to try to invalidate CIA.

Figure 7: Registered unemployment



Note: $N_t = 110$.

Figure 8: Registered unemployment: only OFT participants not employed during OFT

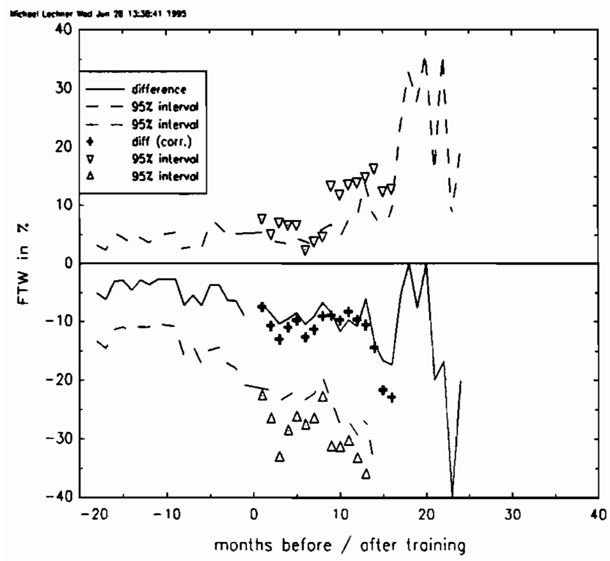


Note: $N_t = 34$.

The effect of training appears to be higher unemployment in the months directly following the end of it.[43] This is a plausible effect when one takes into account that for those unable to keep their previous occupation job search is required. Since this is time consuming, it may not be performed with full intensity until OFT ends. Meanwhile, more members of the control group already found a new employment. This point is particularly obvious when considering only the subsample of individuals not employed during OFT (figure 8). However, given that the mismatch may have introduced some upward bias, these effects may not be so well determined. In any case they disappear entirely after about 8 months. Later on, there are no significant treatment effects. These conclusions are confirmed by the inverted shape (figure 10) of the mean of full-time employment of those OFT participants who are not employed during OFT. However, figure 9 suggests that for the remaining sample, which is employed during OFT, these considerations are - for obvious reasons - not important. Here, OFT does not appear to have any impact whatsoever.

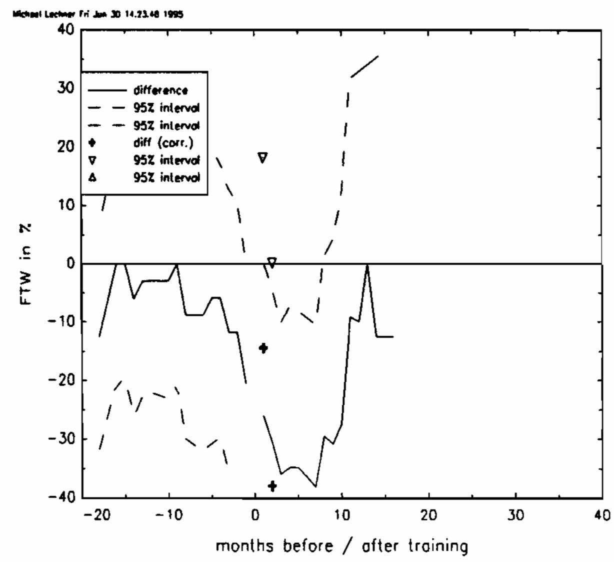
[43] The reader is reminded that the end date is measured with error. Here, it is coded to be never earlier than the true end date. However, there may be a few cases with longer durations for which it could be several months to late (details in App. A).

Figure 9: Full-time employment



Note: $N_t = 110$.

Figure 10: Full-time employment: only OFT participants not employed during OFT

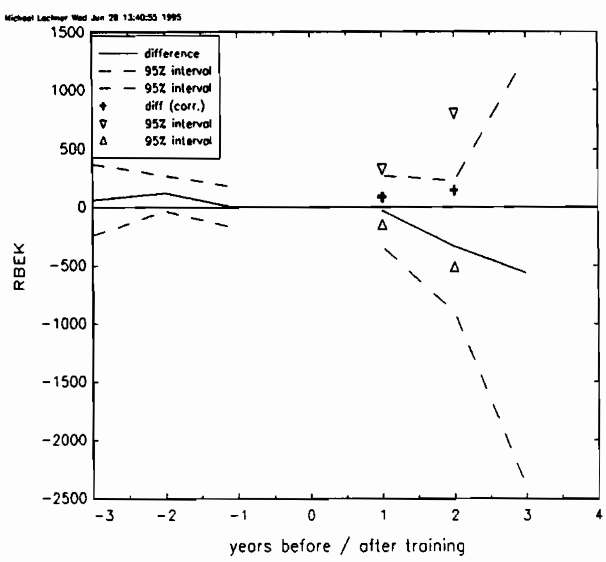


Note: $N_t = 34$.

Figure 11 to 14 feature outcome variables that are only measured once a year, such as gross monthly income, being very worried about keeping one's job, and expected improvement or decline in the professional career in the next two years. On the one hand, there are no significant differences for the pre-training outcomes. On the other hand, the same is true for the post-treatment period. This general result is valid for all yearly variables. It is also robust concerning other functional form (such as logs) of the income variable, for instance.

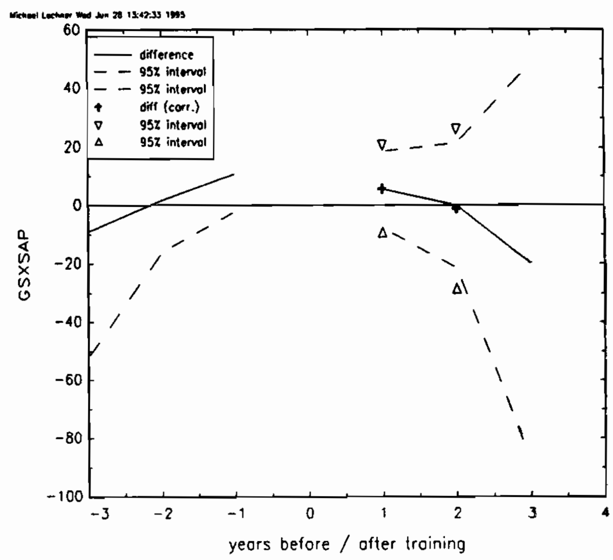
To check whether there might be differences of the average treatment effects in specific subgroups the sample is divided according to gender, job position, professional degree, and as already mentioned, whether the individual was employed during OFT. No significant differences appear. Finally, to check to results for sensitivity with respect to the definition of OFT, the courses used in the estimation are split in several subsamples according to whether (i) they began not earlier than January 1991, (ii) they have a minimum duration of one week, (iii) the objective is qualification for promotion or (iv) the adjustment of skills, and whether (v) a certificate has been obtained by the participant that could be helpful for future job applications. None of the subsamples reveals a substantial difference compared to the results presented above.

Figure 11: Gross income (in 1993 DM)



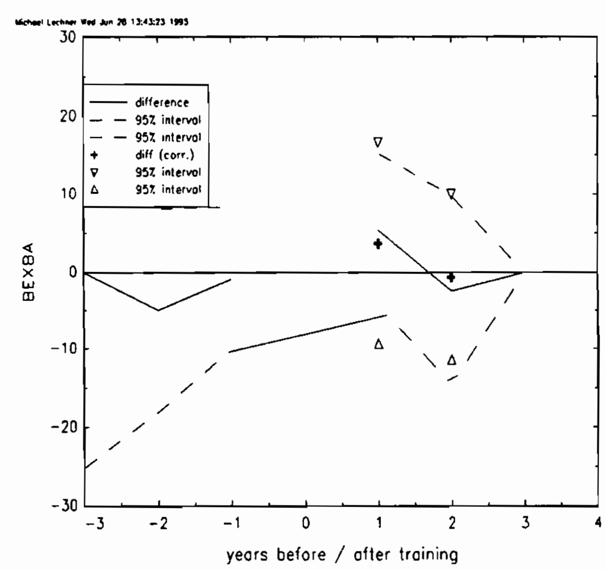
Note: N_t = 110. Income when not employed coded as unemployment benefit or social assistance, whichever is higher. See Appendix A for details.

Figure 12: Very worried about possibility of future job loss (or unemployed)



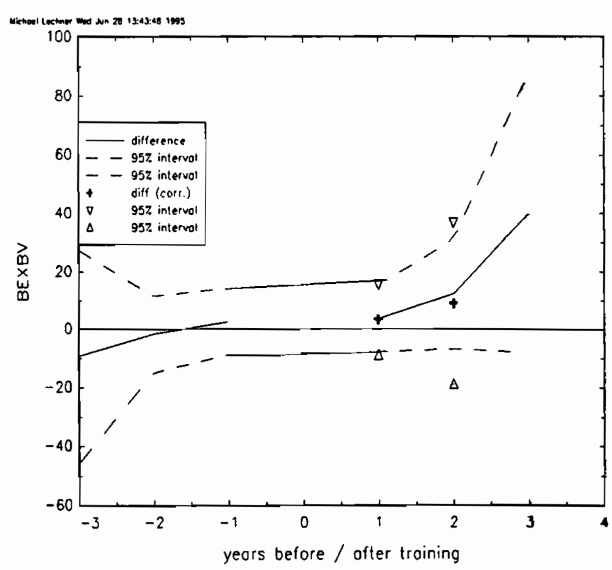
Note: N_t = 110. Nonemployment coded as being very worried.

Figure 13: Expected improvements in the professional career in the next two years



Note: N_t = 110. Nonemployoyment coded as not expecting improvement.

Figure 14: Expected decline in the professional career in the next two years

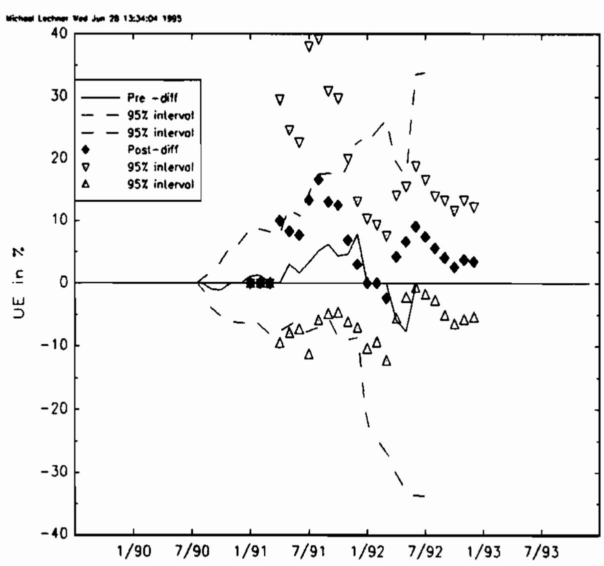


Note: N_t = 110. Nonemployment coded as expecting decline.

Finally, a technical note is in order: The closeness of the mismatch adjusted and not mismatch adjusted results, as well as the statistically nullity of the pre-OFT outcomes, suggest that matching already removes almost all of the bias due to different distributions of the attributes in the OFT and the control sample.

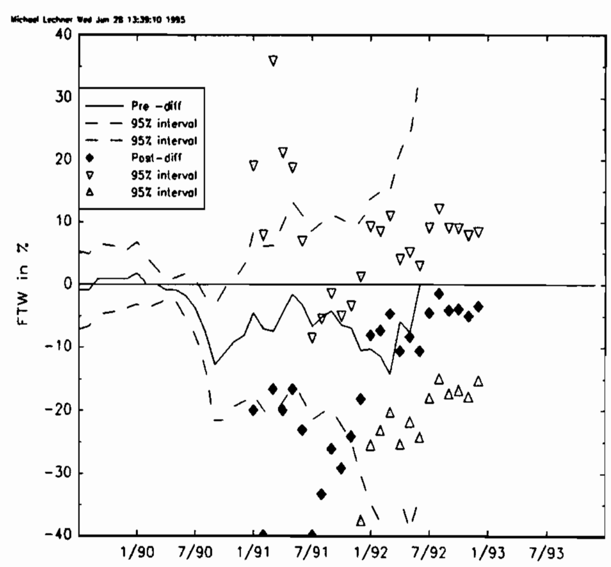
Having discussed results concerning the distance in time to the beginning and ending of a training course, I now turn to the second perspective and consider results for specific dates. Figures 15 and 16 show the development of pre-training (lines) and post-training outcomes (unconnected symbols) over time.[44] Note that when moving from left to right the number of observations is decreasing for pre-training outcomes and increasing for post-training outcomes. However, the conclusions drawn above regarding matching quality and nonexisting OFT effects are adequate for this perspective as well. Since the perspective used above is more informative concerning training outcomes, and because there are no qualitative differences, the results for the other variables are omitted.

Figure 15: Registered unemployment (date)



Note: $N_t = 110$.

Figure 16: Full-time employment (date)



Note: $N_t = 110$.

Finally, several yearly outcome measures are evaluated for the latest possible date available (early 1993). The upper part of table 6 shows various estimates of the average effects of OFT. None of them is significant, which is in accordance with the findings above. Using the techniques discussed in the previous subsection to detect differences among different types of training courses or differ-

[44] Mismatch adjustment is not performed for these two figures.

ent attributes of the participants also reveals little.[45] One of the few effects is a significant positive effect for participants who obtained a certificate compared to those who did not get one. However, this does not imply that there is a positive income effect for the first group, but it merely means that the second group faces a large and significant negative effect.[46] Similar negative effects concerning being full-time employed (or unemployed) or the future professional career can be observed for those who had very low job positions in the GDR. Training effects regarding worries about job security and future declining career prospects differ according to the occurrence and duration of unemployment prior to training (DUPT). The higher DUPT is, the worse are the subjective expectations concerning the career and employment prospectives. This may very well be related to the fact noted above, that the immediate impact of OFT is in many cases some months of unemployment. Regional variations can be found for the same outcome variables. However, comparing them to differences in unemployment rates across the federal states (cf. Statistisches Bundesamt, 1994, table 6.13) reveals only little correlation. Finally, differences according to the age of the participants do only appear for the expected improvement in career prospects. However, the fact that it is larger for older individuals seems hard to explain.

Summarizing the results presented in the figures and tables in this subsection, it should be stressed that no robust positive effects of OFT can be found. There are three possible general reasons for this finding: First of all, the effects can be so small that it is impossible to determine them with size of the sample available. Secondly, there could be positive effects in the longer run that cannot yet be seen. Adding the 1994 data to this study in the immediate future should provide information about the likelihood of this. Finally, it could be that there are no positive effects at all. This conclusion, if also confirmed for more recent courses, would have very serious implications for public policy. It is worth noting that this study has found no evidence whatsoever to rule out that possibility.

[45] Marginal effects are also computed controlling for course duration linearly, and using the panel sample weights, but the qualitative results do not change.

[46] The same effect is significant for ln(income).

Table 6: Evaluation results for several outcomes measured in early 1993

	monthly income in DM		full-time employment		very worried about job		exp. improving profess. career		exp. declining profess. career	
	$\hat{\theta}$	std.er.	$\hat{\theta}^{1)}$	std.er.	$\hat{\theta}^{1)}$	std.er.	$\hat{\theta}^{1)}$	std.er.	$\hat{\theta}^{1)}$	std.er.
mean ($\hat{\theta}_{N_t}$)	24	153	-4	6	5	7	4	5	5	6
weighted mean	30	185	-0	6	6	7	2	5	3	7
mismatch adjusted	65	136	-3	7	9	9	1	5	6	7
Average effect for mean course duration and marginal effect of one month of duration [2)3)]										
average effect	64	146	-3	6	9	9	1	5	6	7
marginal effect	-2	58	0	2	1	2	0	1	-1	2
Separate marginal effects of attributes										
Other course characteristics; reference group: no certificate received and other objectives of course										
certificate receiv.	**1242**	395	r)	-	-10	19	r)	-	r)	-
aim: adjustment	203	306	-4	13	7	16	10	11	-17	15
aim: promotion	-30	351	1	15	3	21	19	12	-11	16
Not employed	-494	424	-25	15	27	19	1	9	18	15
Months in respective labor market status after 6/1990 (/ by number of months prior to OFT)										
reg. unemployed	-1089	1456	-72	60	**163**	31	18	26	**117**	53
inv. short-time w.	326	847	-34	37	7	50	**-38**	26	21	37
Gender: female	-197	342	7	13	1	16	-12	10	-16	14
Age	-71	209	1	8	11	11	**13**	5	-2	9
Years of schooling (highest degree); reference group: less than 10 years										
12 y. of schooling	394	680	37	28	-46	35	-11	17	-43	28
10 y. of schooling	17	559	31	27	-51	34	3	15	38	27
Federal states (Länder) in 1990; reference state: Sachsen										
Berlin	-514	469	-20	24	36	26	-4	14	**49**	24
Thüringen	-165	385	-13	16	26	20	10	11	**37**	18
Mecklenburg - VP	-315	608	r)	-	33	33	16	18	r)	-
Brandenburg	311	463	13	20	16	26	r)	-	-8	21
Sachsen-Anhalt	-365	427	-27	17	**66**	22	6	12	**52**	18
Highest professional degree in 1990; reference group: skilled and unskilled worker										
university	485	459	23	14	-18	19	-16	13	-23	15
engineering, t. col.	-200	359	5	15	1	20	-5	11	-5	17
master of trade/cr.	450	582	-5	30	-24	49	r)	-	10	31
Job position in 1990; reference group: lower job positions										
highly qual., man.	607	457	**51**	15	-9	26	-0	13	**-48**	19
master of tr. / cr.	247	631	r)	-	-19	43	r)	-	11	23
skilled blue, wh. c.	460	461	**48**	16	-15	27	5	12	**-46**	20
Propensity score	-689	1147	4	4	-5	5	-2	3	-5	5

Note: **Bold** letters: t-value > 1.96. [1)] %-points; [2)] Linear models: Regression includes constant (average effect), duration in months normalized to mean 0 (marginal effect), mismatch adjusting covariates. Nonlinear models: see App. B.2. [3)] Evaluated at mean duration; r) Included in reference group, because of small cells and resulting insufficient variation within groups of dependent variable for ordered probit; Marginal effects for binary outcome variables with binary attributes are computed by changing status of members of respective attribute group (e.g. 12 years of schooling) to status of reference group (e.g. 8 y.). Entries in the table are differences of these effects separately for every attribute. They are bounded by -200 and +200. The details of the computations which include also changes in continuous characteristics (age: ± 0.5 years, duration: ± 0.5 m.; duration of unemployment, IST: ± 0.05) are given in App. B.2.

5 Conclusion

The major empirical result of this paper is that no robust positive effects of OFT are found. There are three possible reasons for this: First of all, the true effects can be so small that they are impossible to determine with the current size of the sample. Secondly, there could be positive effects in the longer run that cannot yet be seen. Adding the 1994 data to this study should clear up the likelihood of this possibility. Finally, it could be that there are no positive effects at all. This conclusion, if also confirmed for more recent courses, would have very serious implications for public policy. However, although the study raises serious doubts, one should be cautious to conclude that the training part of the active labor market policy (as defined in the Work Support Act, AFG) in East Germany has no positive impact even in the shorter run. The definition of off-the-job training used in this paper includes several courses, which are not subsidized by the AFG. Immediate future research will be devoted to AFG subsidized courses only, and should provide more direct information about this issue.

The results are obtained by using the potential outcome approach to causality - first explicitly suggested by Rubin (1974) - as a general framework to define causal effects of off-the-job training on individual actual and future post-training labor market outcomes. The paper argues that due to the specific situation in East Germany after unification and the rich data available, the assumption that the outcomes and the assignment mechanisms are independent conditional on observed attributes, including monthly pre-training employment status, is very plausible. Hence, the identification problem inherent in causal analysis, is solved that way. Estimation is performed using a suitably adapted nonparametric matching approach which incorporates the propensity score as well as other attributes that could not possibly be captured by the propensity score, because they depend on the particular date of the beginning of the training. In conclusion, this nonparametric approach appears to be well suited for such an analysis.

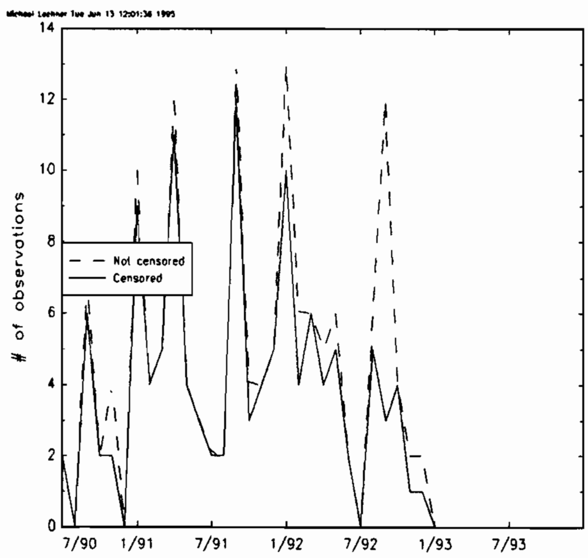
Soon, the 1994 data will be incorporated in the present analysis to determine whether any positive effects can be found over a longer period after the end of the training. Other interesting future research should investigate jointly the effects of different types of training, such as on-the-job training versus off-the-job training versus no training at all. Likewise, it could be an issue whether the quality of the publicly funded training did really improve during the transformation process, as claimed by official sources.

Appendix A: Data

This appendix briefly explains the coding of the start, duration, and end date of OFT courses. It also contains a histogram for the distribution of start dates and the ending dates in figures A.1 and A.2. Furthermore, the exact definition of income variables used in the evaluations are given. Finally, table A.1 shows descriptive statistics for all variables used in the estimation.

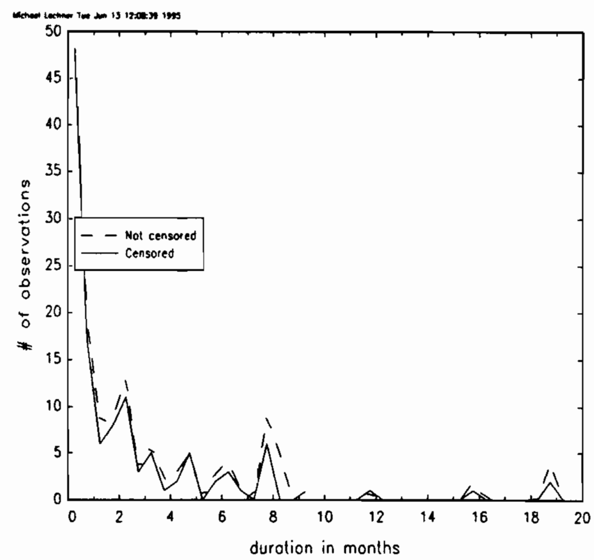
The first month of the course is directly indicated by the individual. When there are several courses classified as OFT, the start date is coded as the earliest one. The duration of each course is computed using the midpoint of the indicated duration interval (see footnote in section 3) multiplied by the weekly hours. In cases of several OFT courses the single durations are added. The last month of each course is computed using the endpoint of the duration intervals to make sure that post-training outcomes are really *post*-training. Note that this is only important for courses with a duration of more than one month. As explained in the main text, the resulting measurement error for these courses (and some of the durations) is reduced by using additionally monthly calendar information on training. In cases of several courses the end date is coded as the end date of the last course.

Figure A.1: Distribution of OFT start dates



Note: Monthly information.

Figure A.2: Distribution of OFT durations



Note: A 0.5 month interval is used.

Table A.1: Descriptive statistics

	No OFT (1205)		OFT (110)	
Variable	mean/share in %	std	mean/share in %	std
Age in 1990	35.2	8.1	35.9	7.5
Gender: female	42		65	
Marital status in 1990				
married	77		78	
single	16		12	
divorced, separated	7		10	
Very desirable behavior / attitudes in society in 1990				
performing own duties	69		66	
achievements at work	72		73	
increasing own wealth	29		20	
Voluntary services in social organizations in 1990:	39		45	
Federal states (Länder) in 1990				
Berlin	7		12	
Brandenburg	15		18	
Mecklenburg-Vorpommern	10		7	
Sachsen	31		28	
Sachsen-Anhalt	21		15	
Thüringen	17		19	
Years of schooling (highest degree) in 1990				
12	17		27	
10	60		65	
8 or no degree	22		8	
Highest professional degree in 1990				
university [1)]	11		25	
engineering, technical college[2)]	16		31	
master of a trade / craft	6		6	
skilled worker[3)]	64		37	
no degree	1		2	
Job position in 1990				
highly qualified, management	19		41	
master of a trade / craft[4)]	8		5	
skilled blue and white collar[5)]	56		44	
Job characteristics in 1990				
wage / salary per month	1236	386	1238	284
tenure in years	10.3		9.9	
temporary job contract	4		2	
professional degree in other than current profession	36		31	
already fired	4		4	
training (unspecified) while full-time employed	7		16	
Profession in 1990 (ISCO)				
scientific, technical, medical	20		37	
production	42		14	
managerial	2		5	
administrative	10		14	
trade	3		1	
agriculture	5		3	
services	8		5	
services, incl. trade, administrative	23		25	

Table A.1 to be continued ...

Table A.1: Descriptive statistics: continued

	No OFT (1205)		OFT (110)	
Variable	mean/share in %	std	mean/share in %	std
Memberships in 1990				
union	74		81	
professional association	7		7	
cooperative (LPG / PGH)	8		4	
Employer characteristics in 1990				
firm size (number of employees)				
0-19	10		9	
20-199	27		25	
200-1999	36		42	
2000 and more	25		24	
industrial sector				
agriculture	11		7	
energy and water	3		5	
mining	3		2	
heavy industry	10		6	
light ind., consumer goods, electronics, print.	16		16	
machine building and vehicle construction	5		9	
construction	7		5	
trade	7		5	
communication, transport	8		2	
other services	11		13	
education, science	10		16	
health	7		10	
redundencies announced	46		47	
Finding a similar new job is (in 1990)				
impossible	10		14	
difficult	70		73	
easy	20		14	
Very worried about job security in 1990	36		40	
Optimistic about the future in general in 1990	17		20	
Not at all optimistic about the future in general in 1990	7		6	
Not enjoying work	5		5	
Very confused by new circumstances	5		3	
Income very important for subjective well-being	65		53	
Expectations for the next 2 years in 1990				
redundancies in firm: certainly	32		35	
redundancies in firm: certainly not	7		3	
losing the job: certainly	5		5	
losing the job: possibly	36		35	
losing the job: certainly not	12		9	
improvements in professional career: certainly	1		1	
improvements in professional career: certainly not	43		37	
decline in professional career: certainly	3		3	
decline in professional career: certainly not	48		44	
new profession: certainly	4		5	
new profession: certainly not	48		42	

Note: 1) University and 'Fachhochschule'; 2) 'Ingenier- und Fachschule', not 1); 3) 'Berufsausbildung', 'Facharbeiter', 'sonstige Ausbildung', not 1), 2) or master of a trade / craft; 4) Includes 'Brigadier', 'Meister im Angestelltenverhältnis'; 5) 'Facharbeiter', 'Angestellte mit qualifizierter Tätigkeit'.
1990 relates to the date of interview which for almost is earlier than July 1990 (EMSU).

Gross monthly income is only measured for those employed. Due to the selection criteria that creates a sample of full-time employees in mid 1990 it is not a problem for 1990, but for the following years. For those unemployed unemployment benefits are computed using 67% of the last *gross* income, which should be a conservative estimate of the value of the gross equivalent for the actual net payment. However, it is assumed that all those unemployed remain eligible for unemployment benefits as opposed to unemployment assistance until 1993. This assumption is plausible, because of the special regulations for East Germans after unification (ratios of people receiving unemployment assistance relative to those receiving assistance or benefits: 1991: 3%, 1992: 8%, 1993: 14%; Statistisches Bundesamt, 1994, table 6.15.4). It is assumed that these benefits increase yearly in line with the price index for private consumption.[47] This should again be a conservative estimate. After performing these imputations, it is ensured that income levels are not below average social assistance levels (Bundesministerium für Arbeit und Sozialordnung, 1994, table 8.16A). Finally, all income variables are converted to 1993 DM by using the private consumption price index for East Germany (Bundesministerium für Arbeit und Sozialordnung, 1994, table 6.9, and Institut der Deutschen Wirtschaft, 1994, table 8).

Appendix B: Econometrics

B.1 Matching protocol

This section gives the details of the matching protocol used for the final evaluations.

Step 1: Split observations in two exclusive pools according to whether they participated in OFT (T-pool) or not (C-pool).

Step 2: Draw randomly an observation in T-pool (denoted by n_t) and remove from T-pool.

Step 3: Define caliper of partial propensity score for observation n_t in terms of the predicted index $v_i\hat{\beta}$ and its conditional variance $\mathrm{Var}(V\hat{\beta}|V=v_{n_t})$. The latter is derived from $\mathrm{Var}(\hat{\beta})$ by the delta method.

Step 4: Find observations in C-pool (denoted by *j*) obeying $v_j\hat{\beta} \in [v_{n_t}\hat{\beta} \pm c\sqrt{\mathrm{Var}(v_{n_t}\hat{\beta})}]$. The constant *c* is chosen such that the interval is identical with a 50% confidence interval around $v_{n_t}\hat{\beta}$.

Step 5: (a) If there is only one or no observation in this interval: Find observation *j* in C-pool that is closest to observation *i*, such that it minimizes $(v_j\hat{\beta} - v_{n_t}\hat{\beta})^2$.

(b) If there are two or more observations in this set: Take these controls and compute the missing variables m in relation to the start date of observation n_t. Denote these and perhaps

[47] It would be preferable to use the wage deflator, but the time series are not complete.

other variables already included in V as $\tilde{m}_j$ and $\tilde{m}_{n_t}$, respectively. Define a distance between the control *j* and *i* as $d(j, n_t) = (v_j\hat{\beta}, \tilde{m}_j)' - (v_{n_t}\hat{\beta}, \tilde{m}_{n_t})'$. Choose control *j* such that it has the smallest Mahalanobis distance $m(j, n_t) = d(j, n_t)' W d(j, n_t)$ within the caliper. W denotes the inverse of the estimated variance of $(v\hat{\beta}, \tilde{m})'$ in the C-pool.

Step 6: Remove *j* from C-pool.

Step 7: If there are any observations in the T-pool left, start again with step 2.

This matching protocol is very close to the one proposed by Rosenbaum and Rubin (1985) and Rubin (1991). They find that this kind of protocol produces the best results in terms of 'match quality' (reduction of bias). The difference is that instead of using a fixed caliper-width (based on considerations about the true propensity score) for all observations, I allow the widths to vary individually with the precision of a monotone function of the partial propensity score (step 4). The (unbounded) linear index $v_{n_t}\hat{\beta}$ is used instead of the (bounded) partial propensity score $\Phi(v_{n_t}\hat{\beta})$. Matching on the latter with this kind of symmetric metric leads to an asymmetry when $\Phi(v_{n_t}\hat{\beta})$ is close to 0 and 1, depending on which side of the control *j* is. This is undesirable. Furthermore, defining the balancing score in terms of $(v_j\hat{\beta}, \tilde{m}_j)$ has also the advantage of making it easier to state under what conditions this type of condition has similar properties as conditioning on the (unknown and not estimatiable) propensity score itself.

B.2 *Correction for mismatches and the modeling of conditional expectations*

B.2.1 The linear case: homogenous effects

The question here is whether the price to pay for the use of the suggested regression methods to adjust for differences in attributes and course characteristics is the assumption of a homogenous treatment effect. This can be seen by considering whether such a regression can identify the mean causal effect θ^0, even when the individual causal effect is not constant in the population. Assume that the following linearity condition holds (given matching has already be performed in an unspecified way):

$$E(\Delta Y | S=1, \Delta X = \Delta X_i; \theta_i^0, \lambda^0) = \theta_i^0 + \Delta X_i \lambda^0. \tag{B.1}$$

Assume that the matches remain imperfect, so that ΔX_i may be different from 0 (for example when some components are continuous so that the probability of a perfect match is zero, even in very large samples).[48] Define the following population means: $\overline{\Delta Y} = E\Delta Y | S=1$, $\theta^0 = \overline{\theta} = E\theta | S=1$, $\overline{\Delta X} = E\Delta X | S=1$, $\overline{\Delta XX} = E(\Delta X' \Delta X) | S=1$ and $\overline{\Delta X\theta} = E[\Delta X'(\theta - \overline{\theta})] | S=1$. $\hat{\theta}_\infty$ de-

[48] Note that, for simplicity, this is again an argument about identification in the population only, so that the respective 'population' notation is used.

notes the population (probability) limit for the constant term of an OLS regression of a constant and the difference in attributes on the difference of an outcome. It can be computed by using the Frisch-Waugh-Lovell theorem (cf. Davidson and MacKinnon, 1993) or by applying the rules for the partial inversion of matrices directly. The result is:

$$\hat{\theta}_{\infty} = \theta^{0} + [1 - \overline{\Delta X}(\overline{\Delta XX})^{-1}\overline{\Delta X'}]^{-1}\overline{\Delta X}(\overline{\Delta XX})^{-1}\overline{\Delta X\theta'}. \tag{B.2}$$

Therefore, generally the estimated OLS coefficient of the constant will not converge towards the population mean, unless $\overline{\Delta X\theta'}$ is zero. This is true when the difference regressors and the causal effects are uncorrelated. A very important case is when θ is the same for all members of the population, another important case is the case of perfect matches.[49] However, note that the bias is reduced when the match quality increases and when effects are more homogenous in the (sub-) population. Similar arguments apply to the nonlinear case. Note that in practice this problem ointl with the linearity of the conditional expectation may be relevant even in large samples, when 'non-vanishing' regressors are considered instead of ΔX, like the characteristics of the courses or attributes of individuals participating in OFT.

B.2.2 The nonlinear case

As mentioned in section 4 of this paper, several of the outcome variables are indicators so that $\Delta Y_i \in \{-1,0,1\}$. The elements of the set either denote a positive effect, not a measurable effect, or a negative effect, respectively. The average causal effect is of the form given in (B.3):

$$\theta^{0} = E(\Delta Y|\Delta X = 0, S = 1) = P(\Delta Y = 1|\Delta X = 0, S = 1) - P(\Delta Y = -1|\Delta X = 0, S = 1). \tag{B.3}$$

A consistent estimate of the average treatment effect can be obtained by substituting sample analogs for the population probabilities (B.4):

$$\hat{\theta}^{M}_{N_t} = \frac{1}{N_t}\sum_{n_t=1}^{N_t}[P(\Delta y_{n_t} = 1|\Delta x_{n_t} = 0) - P(\Delta y_{n_t} = -1|\Delta x_{n_t} = 0)] \tag{B.4}$$

Approximating differences of nonlinear probabilities (expectations) by a linear function as in the previous case may not be - for various reason (e.g. Maddala, 1983) - a preferable option. Therefore, I choose a more parsimonious specification. In a first step a three-group-ordered probit model is estimated with Δy_{n_t} as dependent variable and Δx_{n_t} plus a constant as independent vari-

[49] In this case the notation has somewhat to be changed to allow for noninvertible matrices.

ables.[50] The asymptotic covariance matrix for the estimated coefficients of the ordered probit model are computed using the combination of OPG and expected hessian which has already been discussed in the context of the estimation of the propensity score. In the second step the above probabilities are directly derived from this model and computed for the individual observations using the estimated coefficients of the ordered probit model. Finally, the variance of $\hat{\theta}^M_{N_t}$ is derived from the variance of the estimated coefficients of the ordered probit model by the use of the delta method. Note that the functional form assumption for the conditional mean of ΔY_i is asymptotically unimportant as long as the differences in attribute (Δx_{n_t}) disappear.

The same approach is chosen to check whether the conditional expectations vary with either characteristics of the courses or with characteristics of the individuals having decided to participate in OFT. In the nonlinear case the average marginal effect of a continuous variable W can be defined and estimated as:

$$\begin{aligned}\gamma^M_{N_t} = \frac{1}{N_t}\sum_{n_t=1}^{N_t}[&P(\Delta y_{n_t} = 1|\Delta x_{n_t} = 0, w_{n_t} + a/2) - P(\Delta y_{n_t} = -1|\Delta x_{n_t} = 0, w_{n_t} + a/2) - \\ &P(\Delta y_{n_t} = 1|\Delta x_{n_t} = 0, w_{n_t} - a/2) + P(\Delta y_{n_t} = -1|\Delta x_{n_t} = 0, w_{n_t} - a/2)]\end{aligned} \quad \text{(B.5)}$$

a is an appropriately chosen constant. The particular valuess of it use in the empirical study are given in the note to table 6.

Equation (B.6) gives a similar expression for an average effect of an indicator variable D:

$$\begin{aligned}\gamma^M_{N_t} = \frac{1}{N_t}\sum_{n_t=1}^{N_t}[&P(\Delta y_{n_t} = 1|\Delta x_{n_t} = 0, d_{n_t} = 1) - P(\Delta y_{n_t} = -1|\Delta x_{n_t} = 0, d_{n_t} = 1) - \\ &P(\Delta y_{n_t} = 1|\Delta x_{n_t} = 0, d_{n_t} = 0) + P(\Delta y_{n_t} = -1|\Delta x_{n_t} = 0, d_{n_t} = 0)]\end{aligned} \quad \text{(B.6)}$$

Estimation of these marginal effects defined in (B.5) and (B.6) is accomplished as for the case of mismatch correction, but W or D are used as additional independent variables in the ordered probit estimation.

[50] One bound and the variance of the underlying linear models is normalized (see Maddala, 1983, for details on the ordered probit model).

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