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ABSTRACT

Evaluating the Dynamic Employment Effects of Training Programs in East Germany Using Conditional Difference-in-Differences*

This study analyzes the employment effects of training in East Germany. We propose and apply an extension of the widely used conditional difference-in-differences evaluation method. Focusing on transition rates between nonemployment and employment we take into account that employment is a state dependent process. Our results indicate that using transition rates is more appropriate and informative than using unconditional employment rates as commonly done in the literature. Training as a first participation in a program of Active Labor Market Policies shows zero to small positive effects both on reemployment probabilities and on probabilities of remaining employed with notable variation over the different start dates of the program.

JEL Classification: C14, C23, H43, J64, J68

Keywords: evaluation of active labor market policy in East Germany, nonparametric matching, conditional difference-in-differences, employment dynamics, Ashenfelter's Dip, bootstrap

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1 Introduction

After the formation of the German "Social and Economic Union" in 1990, the East German economy underwent enormous changes. It had to transform from a command driven backward economy to a market economy at an unprecedented speed. The transformation process brought about high unemployment in East Germany. To increase the employment chances of the unemployed, the German government decided to provide on a high scale Active Labor Market Policies (ALMP) in East Germany. These programs mainly consisted of training and temporary employment schemes. More than a decade after the reunification, the German Federal Employment Service still spends around € 20 Billion ($\approx 0.9\%$ of the GDP) for ALMP (Bundesanstalt für Arbeit, 2003). About 50% of this budget is spent in East Germany with a labor force less than one sixth of Germany as a whole. Quite a significant share of the labor force in East Germany has been participating in programs of ALMP since 1990.

Previous studies on the effect of ALMP in East Germany on individual employment chances provide mainly negative though unclear evidence; see the surveys in Fitzenberger and Speckesser (2002) and Hagen and Steiner (2000). The existing studies suffer from data limitations, either from a small number of participants (e.g. Lechner, 1998, using the German Socio-Economic Panel) or from the data being limited to the early 1990s and lacking the employment history on a monthly basis (e.g. Fitzenberger and Prey, 2000, using the Labor Market Monitor for East Germany).[1]

This paper estimates the employment effect of public sector sponsored training programs in East Germany for the group of individuals who belonged to the active labor force in 1990. This group was fully hit by the transformation shock. In the early 90s, training was often considered to be most effective among the ALMP programs as it was supposed to provide skills that were in demand in a market economy but not in sufficient supply due to the former educational system.[2] Training

[1]Our earlier paper Bergemann, Fitzenberger, Schultz, and Speckesser (2000) estimating the impact of training on employment rates is an exception.

[2]Forecasts of the future labor demand in the early 1990s for both East and West Germany (e.g. Prognos 1993) usually indicated a severe shortage especially for service sector skills in the East if

was the ALMP program with the largest number of participants.

We implement a semiparametric conditional difference–in–differences estimator (CDiD) (Heckman, Ichimura, Smith and Todd, 1998). In light of state dependence in employment status, we extend the CDiD approach to using transition rates between different labor market states as outcome variables instead of exclusively using employment rates in levels as is often done in the literature. We apply propensity score matching in the first stage and then estimate average effects of treatment on the treated. The analysis matches treated individuals to nonparticipants using local linear matching to account for selection on observables. Selection on time invariant unobservable characteristics is controlled for using a conditional difference–in–differences estimator. Our inference uses a bootstrap approach taking account of the estimation error in the propensity score. We perform a sensitivity analysis on the implementation details of the evaluation approach.

Our results indicate that modeling transition rates is more appropriate than using unconditional employment rates. Thereby, we can determine whether ALMP programs help workers to find a job and/or whether they stabilize employment. We find that the employment effects are mostly insignificant but that there are some significantly positive effects for selected start dates with significant variation over time concerning job finding rates and employment stability.

Next to the extension of the CDiD to using transition rates as outcome variables, our paper involves two further methodological innovations: First, anticipation effects regarding future participation or eligibility criteria (Ashenfelter's Dip) requiring a certain elapsed duration of unemployment for participation are likely to affect strongly the results of any difference–in–differences estimator (Heckman and Smith, 1999). Using institutional knowledge to bound the start of the Ashenfelter's Dip, we suggest a long–run difference–in–differences estimator to take account of possible effects of anticipation or participation rules. Second, we suggest a heuristic cross–validation procedure for the bandwidth choice that is well suited to the estimation of conditional expectations for counterfactual variables.

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catching up to the economic situation of the West. Human capital transformation was believed to satisfy the changing labour demand and at the same time to reduce unemployment (OECD 1994).

Some progress has been made in the literature in order to extend standard static evaluation approaches to the dynamic selection issue involved here. Similar to our paper, the timing–of–events approach (Abbring and Van den Berg, 2003; Fredriksson and Johanson, 2003) focuses on transition rates from unemployment to employment by modeling the duration of unemployment as outcome. Sianesi (2004) emphasizes that treatment differs by the elapsed duration of unemployment at the beginning of the program and that future program participants should be used in the comparison group for earlier treatments. Existing empirical studies using the aforementioned approaches are based on an inflow sample into unemployment. Our data set (section 3) is too small to restrict the analysis to an inflow sample into unemployment. Also, our analysis goes beyond the aforementioned studies in two respects: We model the effects of treatment on both the probability of leaving nonemployment and of remaining employed. We allow for unobserved, state dependent individual specific effects, extending the standard, static specification on a CDiD estimator.

The paper is organized as follows: Section 2 gives a short description of the institutional background for ALMP in East Germany and discusses descriptive evidence. Section 3 develops the microeconomic evaluation approach used here. The implementation of the approach is described and the empirical results of the evaluation are discussed in section 4. Section 5 concludes. The appendix includes detailed descriptive evidence and results.

2 Training in East Germany

2.1 Institutional Background

Between 1969 and 1997, training as part of Active Labor Market Policy in Germany was regulated by the Labor Promotion Act (*Arbeitsförderungsgesetz*, AFG). Despite a number of changes in the regulation over this time period, the basic design of training programs remained almost unchanged until the AFG was replaced by the new Social Law Book (Sozialgesetzbuch, SGB) III in 1998. The German Federal Labor Office (*Bundesanstalt für Arbeit*, BA) was in charge of implementing these programs in addition to being responsible for job placement and for granting unemployment benefits. With German unification, these programs were extended to East Germany (§ 249 AFG). Policy makers intended to support the adjustment of human capital in East Germany to Western levels. ALMP was also justified by the goal that the standard of living in East Germany should converge quickly to Western levels in order to avoid large scale outmigration and to foster political stability.

Training programs under the AFG rule basically comprise the following four types: Further Vocational Training (*Fortbildung*), Re–training (*Umschulung*), Short–term training (*Kurzzeitmaßnahmen nach § 41a AFG*) and Integration subsidies (*Einarbeitungszuschuss*, §§ 33 – 52 AFG).

Further vocational training (§ 41 AFG) consists of the assessment, maintenance, and extension of skills. The duration of the courses depends on the characteristics of the participants. The courses regularly take between 2 and 8 months and are mainly offered by private sector training companies.

Re–training enables vocational re–orientation if no adequate employment can be found because of skill obsolescence. Re–training is supported by the BA for a period up to 2 years and aims at providing a new certified vocational training degree.

Short–term training aims at increasing the employment chances by skill assessment, orientation, and guidance. The courses are intended to increase the placement rate of the unemployed. Mostly, they do not provide occupational skills but aim at maintaining search intensity and increasing hiring chances. The courses usually last from two weeks to two months.

Integration Subsidies involve payments to employers providing employment to previously unemployed workers who need a training period. The worker earns a regular wage from the employer. This program is not analyzed in our empirical analysis because it can not be identified in our data.

Except for integration subsidies, all participants in full–time courses are granted an income maintenance payment (*Unterhaltsgeld*). To qualify, participants must have been employed beforehand for at least one year during a certain time period or they must be eligible for unemployment benefits or unemployment assistance.

2.2 Changes in Programs and Participation Incentives

During the 1990's, legislation modified the types of programs, the level of income maintenance payments, and the eligibility criteria. *Short–term training programs* were abolished formally in 1992 and in 1993, a new program started with the same purpose. However, participants were no longer considered as taking part in training programs and were therefore recorded as unemployed. *Income maintenance payments* were reduced after 1993 from 68% (63%) of the net earnings during previous employment for participants with (without) children to 67% (60%).

Participants on training might have been either recipients of unemployment benefit (i.e. those with unemployment of less than one year) or of unemployment assistance (long–term unemployed, receiving lower transfer payments which are means–tested). Both groups requalified for the receipt of unemployment benefit while being on the program and receive a higher benefit while on the program. Consequently, participants might have also started training because of these incentives. For this reason, the legislator abolished benefit renewal in 1998.

Before 1994, participation in a training program was open to participants without having experienced unemployment beforehand as long as the case worker considered participation in training as "*advisable*". This type of training intended to prevent future unemployment, to increase the labor market prospects of the employed in the future, or to foster re–integration of individuals returning to the labor market. Starting in 1994, access was restricted to individuals fulfilling the criteria for "*necessary*" training, which basically restricted the program to formerly unemployed participants. However, especially in East Germany, participation under the weak criterion of "being threatened by unemployment" was still possible.

The reform in 1994 changed the mix of participants in training programs and shifted somewhat the focus of training, both of which a credible evaluation strategy has to account for. The end of explicit short–term training programs made the programs longer and more expensive on average and the program mix become less focussed on immediate placement of participants. After the change, there is a stronger focus on providing additional skills and helping participants to signal their skills. We suspect therefore that, on the one hand, incentives to participate are stronger on average after than before the reform. This may result on average in stronger anticipation effects such that in anticipation of participation unemployed individuals decrease their search effort for a new job. On the other hand, training programs become less attractive, especially for workers who are still employed. Over time, a change in the selection of the program group occurs, with training increasingly targeting problem groups with a priori significantly lower employment chances.

2.3 Aggregate Participation

Training programs were implemented in East Germany immediately after unification (see figure 1): 98,500 persons started to participate during the last three months of 1990. In 1991, the maximum was reached with 892,145 entries. Only in 1992, was there a similar magnitude. Between 1993 and 1997 the number declined considerably, down to 166,000 in 1997. Afterwards participation recovered to a level slightly above 180,000 reflecting the ongoing importance of these programs in East Germany. The share of entries into re–training as a percentage of training in total varies between 15% in 1991 and 28% in 1993, the share for integration subsidies declines from 15% in 1991 to 8% in 1997. Separate figures are available neither for short–term training and further vocational training for the early 1990's nor for the subprograms after 1997 due to the change in the regulation.

Stocks of participants show a similar pattern (see figure 2). The maximum was reached in 1992, amounting to 492,000 participants on average. Participation has been declining afterwards (2000: 139,700, 2002: 129,000 participants). The trends for the subprograms (not reported in figure 2) are analogous.

Direct costs for participation paid by the BA (see figure 2, right axis) – income maintenance, course fees, travel costs etc. – continuously increased over time. In 1991, when short–term training programs still existed, annual costs were at € 8,000 per participant. These costs increased to € 14,600 in 1995 and to € 20,600 in 2002.

3 Evaluation Approach

Our empirical analysis is based upon the potential outcome approach to causality (Roy, 1951, Rubin, 1974), see the survey Heckman, LaLonde and Smith (1999). We focus on estimating the average causal effect of treatment on the treated (TT) in the binary treatment case.[3] TT is given by

$$E(Y^1|D=1) - E(Y^0|D=1) \; , \tag{1}$$

where the treatment outcome Y^1 and the nontreatment outcome Y^0 are the two potential outcomes and D denotes the treatment dummy. Our outcome variable of interest is a dummy variable for employment, possibly conditional on employment in the previous month resulting in a transition dummy. The observed outcome Y is given by $Y = DY^1 + (1-D)Y^0$. The evaluation problem consists of estimating $E(Y^0|D=1)$ since the counterfactual outcome in the nonparticipation situation is not observed for the participating individuals ($D=1$). Thus, identifying assumptions are needed to estimate $E(Y^0|D=1)$ based on the outcomes for nonparticipants ($D=0$). We apply a conditional difference–in–differences (CDiD) approach to control for time invariant selection effects. We also allow participation rules and possible anticipation effects of the treatment (Ashenfelter's Dip) to affect the outcome before the treatment.

3.1 Selection on Observables and Matching

Assuming the Conditional Mean Independence Assumption (CIA)

$$E(Y^0|D=1,X) = E(Y^0|D=0,X) \tag{2}$$

[3]The framework can be extended to allow for multiple, exclusive treatments. Lechner (1999) and Imbens (2000) show how to extend standard propensity score matching estimators for this purpose. Although this would be a natural extension in our application, we do not think that our data are sufficiently rich enough for this purpose. Our analysis is very demanding since we argue that matching on observable covariates will not suffice to control for selection bias and since we model the effects on transition rates between different labor market states. Therefore, we restrict ourselves to estimating TT for training where the comparison group is the group of all individuals who either do not participate in any program or who only participate in other programs where the latter two are weighted by their sample frequencies.

implies that the nontreatment outcomes of the participants and of the nonparticipants are now comparable in expectation when conditioning on X. Then, to estimate the expected nonparticipation outcome for the participants with observable characteristics X, it suffices to take the average outcome for nonparticipants with the same X. Based on the CIA, the popular matching approach estimates the expected nontreatment outcome for a participant i with characteristics X by the fitted value of a nonparametric regression in the sample of nonparticipants at point X (see survey Heckman, LaLonde and Smith, 1999). The nonparametric regression can be represented by a weight function $w_{N_0}(i,j)$ that gives a higher weight to nonparticipants j the stronger his similarity to participant i in terms of X. For each i, these weights sum up to one over j ($\sum_{j\in\{D=0\}} w_{N_0}(i,j) = 1$). The estimated TT is then

$$\text{(3)} \qquad \frac{1}{N_1} \sum_{i\in\{D=1\}} \left\{ Y_i^1 - \sum_{j\in\{D=0\}} w_{N_0}(i,j)\, Y_j^0 \right\},$$

with N_0 the number of nonparticipants j and N_1 the number of participants i.

Matching estimators differ with respect to the weights attached to members of the comparison group. The most popular approach in the literature is nearest neighbor matching which uses the outcome for the closest nonparticipant ($j(i)$) as the comparison level for participant i, see Heckman, LaLonde and Smith (1999) and Lechner (1998). In this case, $w_{N_0}(i,j(i)) = 1$ for the nearest neighbor $j(i)$ and $w_{N_0}(i,j) = 0$ for all other nonparticipants $j \neq j(i)$. Following Heckman, Ichimura, Smith and Todd (1998), we implement instead the local linear matching approach using a nonparametric local linear kernel regression to estimate the expected nonparticipation outcome of participants with certain characteristics, see also Pagan and Ullah (1999). This amounts to specifying the weight function based on a kernel function which has as its argument the distance in terms of characteristics of the individuals.[4] Local linear matching has a number of theoretical advantages compared to nearest neighbor matching. The asymptotic properties of kernel based methods are straightforward to analyze and it has been shown that bootstrapping provides a

[4]We also checked the sensitivity of our results by using nearest neighbor matching without and with caliper (the latter allows only for matches which are sufficiently close). For our application, it turned out that the choice of matching approach had no notable impact on the estimated treatment effects. We only report the results using local linear matching.

consistent estimator of the sampling variability of the estimator in (3) even if matching is based on closeness in generated variables (this is the case with the popular method of propensity score matching which will be discussed below), see Heckman, Ichimura, Smith and Todd (1998) or Ichimura and Linton (2001) for an asymptotic analysis of kernel based treatment estimators. Abadie and Imbens (2004) show that the bootstrap is in general not valid for nearest neighbor matching due its extreme nonsmoothness.

It is difficult to match with respect to a high–dimensional vector of observable characteristics X ("curse–of–dimensionality"), see Pagan and Ullah (1999). Therefore, the evaluation literature uses extensively the result of Rosenbaum and Rubin (1983) that the CIA in equation (2) implies that participants and nonparticipants become comparable in expectation when conditioning on the treatment probability $P(X) = P(D = 1|X)$ (propensity score) as a function of the observable characteristics X, i.e.

$$E(Y^0|D = 1, P(X)) \; = \; E(Y^0|D = 0, P(X)) \qquad (4)$$

for $0 < P(X) < 1$.[5] The result reduces the matching problem to one dimension effectively using the "closeness" in the propensity score as the weighting scheme. The propensity score has to be estimated. We implement local linear matching based on the estimated propensity score. We take account of the sampling variability in the estimated propensity score by applying a computationally quite expensive bootstrap method to construct the standard errors of the estimated treatment effects. To account for autocorrelation over time, we use the entire time path for each individual as the block resampling unit. All the bootstrap results reported in this paper are based on 200 resamples.

For the local linear kernel regression in the sample of nonparticipants, we use the Gaussian kernel, see Pagan and Ullah (1999).[6] Standard bandwidth choices

[5]To estimate TT, it suffices to assume $P(X) < 1$. For X with $P(X) = 0$, condition (4) is not defined, but this part of the support of X is not needed to estimate TT.

[6]A kernel function with unbounded support avoids some of the problems involved with local linear kernel regression, namely, that the variance can be extremely high in areas where there is not a lot of data, see Seifert and Gasser (1996) and Frölich (2004) for a critical assessment of local linear kernel regression.

(e.g. rules of thumb) for pointwise estimation are not advisable here because the estimation of the treatment effect is based on the average expected nonparticipation outcome for the group of participants, possibly after conditioning on some information to capture the heterogeneity of treatment effects. Since averaging pointwise estimates reduces the variance, it is clear that the asymptotically optimal bandwidth should go to zero faster than an optimal bandwidth for a pointwise estimate, see Ichimura and Linton (2001) on such results for a different estimator of treatment effects.[7]

To choose the bandwidth, we suggest the following heuristic leave–one–out cross–validation procedure which mimics the estimation of the average expected nonparticipation outcome for each period. First, for each participant i, we identify the nearest neighbor $nn(i)$ in the sample of nonparticipants, i.e. the nonparticipant whose propensity score is closest to that of i. Second, we choose the bandwidth to minimize the sum of the period–wise squared prediction errors

$$\sum_{t=1}^{T}\left[\frac{1}{N_{1,t}}\sum_{i=1}^{N_{1,t}}\left(Y^0_{nn(i),t}-\sum_{j\in\{D=0\}\backslash nn(i)} w_{i,j}Y^0_{j,t}\right)\right]^2$$

where the prediction of employment status for $nn(i)$ is not based on the nearest neighbor $nn(i)$ himself and $t=1,...,T$ denotes the month ($T=120$ for our data). The optimal bandwidth affecting the weights $w_{i,j}$ through the local linear regression is determined by a one–dimensional search. The resulting bandwidth is typically smaller than a rule–of–thumb value for pointwise estimation, but this is not always the case, see Ichimura and Linton (2001) for similar evidence in small samples based on simulated data. Since our method for the bandwidth choice is computationally quite expensive, it is not possible to bootstrap it. Instead, we use the bandwidth found for the sample in all resamples.

3.2 Employment Model and Ashenfelter's Dip

We specify the econometric model for employment in order to be clear about which treatment parameters are estimated. The dummy variable for employment Y_{it} of

[7] This is also the rationale for researchers using nearest neighbor matching with just the closest neighbor thus focussing on minimizing the bias.

individual i in month t exhibits strong state dependence, i.e. holding everything else constant the probability of remaining employed $P(Y_{it} = 1 \mid Y_{i,t-1} = 1)$ given that i is employed in the previous month is likely to be much higher than the reemployment probability $P(Y_{it} = 1 \mid Y_{i,t-1} = 0)$ given that i is not employed in the previous month.[8] Therefore, the dynamic employment process for individual i is specified using separate outcome equations depending on the state in the previous month as

$$(5) \quad Y_{it} = \begin{cases} a^e(X_i, t) + \delta^e_{i,t,\tau} D_{i,t}(\tau) + c^e_i + u^e_{i,t} & \text{for} \quad Y_{i,t-1} = 1 \quad \text{(employed before)} \\ a^n(X_i, t) + \delta^n_{i,t,\tau} D_{i,t}(\tau) + c^n_i + u^n_{i,t} & \quad\quad\;\; Y_{i,t-1} = 0 \quad \text{(not empl. before)} \end{cases}$$

where $D_{i,t}(\tau)$ is a dummy variable for treatment in period τ, $a^e(X_i, t), a^n(X_i, t)$ are functions describing the state dependent employment probabilities as a flexible function of observed time invariant characteristics X_i and month t, $\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$ are the individual specific, state dependent effects of treatment on the employment probabilities, c^e_i, c^n_i are state dependent permanent individual specific effects, and $u^e_{i,t}, u^n_{i,t}$ are the idiosyncratic, period specific effects. To simplify the notation, we only consider the effects of treatment in one period τ. Furthermore, we assume that the effect of treatment occurs after treatment, i.e. $\delta^k_{i,t,\tau} = 0$ for $t < \tau$ and $k = e, n$.[9] The assumption implies the absence of deterrence effects, which is plausible since training programs are not mandatory. We will discuss below Ashenfelter's Dip as linking treatment and the idiosyncratic error term before treatment. We allow the individual treatment effect $\delta^k_{i,t,\tau}$ $(k = e, n)$ to depend upon observed characteristics X_i and the individual specific effects c^k_i. They are also allowed to vary by i, t, and τ conditional upon X_i and c^k_i. For the idiosyncratic error terms, we assume that $u^e_{i,t}, u^n_{i,t}$ are mean independent of treatment in the past.

Regarding the issue of selection bias, the evaluation approach allows that treatment $D_{i,t}(\tau)$ is affected by the observed covariates (X_i, t), by the treatment effects $\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$, and by the individual specific effects c^e_i, c^n_i. Furthermore, we do not impose functional form restrictions on $a^e(X_i, t), a^n(X_i, t)$. The evaluation approach

[8] In this section, the index i denotes any individual whereas in the remainder of the paper i applies only to treated individuals.

[9] This assumption is similar to the timing–of–events approach (Abbring and Van den Berg, 2003).

attempts to be as nonparametric as possible. However, we choose a parametric model to estimate the propensity score.

It is often observed, that shortly before participation in a labor market program the employment situation of the future participants deteriorates disproportionately. A similar finding termed Ashenfelter's Dip was first discovered when evaluating the treatment effects on earnings (Ashenfelter, 1978). Later research demonstrated that the same phenomenon can also occur regarding employment, see Heckman, LaLonde and Smith (1999), Heckman and Smith (1999), and Fitzenberger and Prey (2000). We argue that in our context Ashenfelter's Dip is caused by participation rules or anticipation effects. Therefore, we allow that $D_{i,t}(\tau)$ can be correlated with $u^k_{i,\tau-s}$ $(k = e, n)$ with $s = 1, \ldots, ad$ and where ad denotes the beginning of Ashenfelter's Dip. Even though no tough participation rules were applied in East Germany in the early 1990s, it is clear that in most cases unemployment must have lasted some time before treatment could start. A reason for anticipation effects can be that unemployed workers or workers at the risk of becoming unemployed reduce their search effort if they know that participation in an active labor market program is an option in the near future. Analogously, unemployed individuals expecting to start a new job in the future are not likely to receive treatment.

It is conceivable to interpret Ashenfelter's Dip as a treatment effect thus violating our timing–of–events assumption. We stick to this assumption since both anticipation effects and participation rules have no bearing on the economic mechanisms at work during and after treatment. Therefore, we assume that these preprogram effects are not linked to the outcome variable once treatment has started, i.e. $u^k_{i,\tau-s}$ $(k = e, n)$ are not correlated with $u^k_{i,t}$ with $s \geq 1$ and $t \geq \tau$.[10]

In our empirical analysis, we allow for a maximum length of time (ad months) for Ashenfelter's Dip, where ad is set according to institutional features of the programs

[10]This is in contrast to Heckman and Smith (1999) who model earnings in the recovery process to be expected (based on nontreatment outcomes) after the treatment being symmetric to the deterioration during Ashenfelter's Dip. The study shows empirically that such a pattern holds based on experimental data. In our context, state dependence in employment results in a sluggish recovery process without treatment which in general is not symmetric around Ashenfelter's Dip. Analyzing transition rates allows us to take account of the sluggishness of recovery.

under consideration. After inspection of the data, we set *ad* conservatively and we let it vary over time (see section 4.3 and 4.4). While it is likely that shortly after German unification the anticipation of program participation occurs only shortly before the beginning of the program and participation rules were applied in a very lax way, *ad* increases with the rise of unemployment during the early 1990s.

3.3 Conditional Difference–in–Differences

While the matching approach addresses selection bias due to observed variables, selection bias due to unobserved characteristics has to be addressed differently. We allow the selection into treatment to be affected by the permanent unobserved effects in our employment model in equation (5). For instance, unobserved characteristics could be due to differences in the motivation of participants or could reflect that programs are targeted to individuals with some particular problems in the labor market.[11] The difference–in–differences estimator can be used when selection effects are additively separable and time invariant. Then, it is possible to use the framework in section 3.1 by merely analyzing the before–after change in the outcome variable instead of its level. We implement a conditional difference–in–differences (CDiD) estimator using preprogram differences in the outcome variable after matching to control for remaining unobservable differences. In order to avoid the "fallacy of alignment" (Heckman, LaLonde and Smith, 1999), we have to take account of possible preprogram effects via Ashenfelter's Dip. We extend the CDiD as used in the literature to fully capture the state dependence in the employment process.

3.3.1 Conditional Difference–in–Differences in Employment Rates (CDiDS)

Following the approach in Heckman, Ichimura, Smith and Todd (1998),[12] we use local linear matching based on the estimated propensity score to match participants

[11]We do not attempt to estimate an econometric selection model because the scarce data do not allow for credible exclusion restrictions in the participation equation, see section 4.1.

[12]See also Blundell, Costa–Dias, Meghir and Van Reenen (2004) for an application of the CDiD, where age and regional variation is used to take account of selection effects.

i and nonparticipants j in the same time period. The simple CDiD–estimator for the treatment effect on the employment rate[13] in period $t1$ is given by

$$\frac{1}{N_1}\sum_{i=1}^{N_1}\left[Y^1_{i,t1} - Y^0_{i,t0} - \sum_j w_{i,j}(Y^0_{j,t1} - Y^0_{j,t0})\right]$$

where period $t1$ lies after and $t0$ before treatment for individual i. $t1$ and $t0$ are defined relative to the actual beginning of the treatment τ. N_1 is the number of participants i for whom the $t1 - t0$ difference can be determined, and due to Ashenfelter's Dip $t0$ must lie before $\tau - ad$.[14] For the alignment in the preprogram period, we effectively define $Y_{m,t0}$ $(m = i, j)$ as the average individual employment rate in the time period considered before Ashenfelter's Dip.

This static specification of the conditional difference–in–differences estimator (in the following: CDiDS) is a valid estimator if the employment process in equation (5) does not exhibit state dependence and if the change in the idiosyncratic error term is conditionally mean independent of treatment status D and covariates X_i, i.e. $E(u_{i,t1}|D = 1, X_i) - E(u_{i,t1}|D = 0, X_i) = E(u_{i,t0}|D = 1, X_i) - E(u_{i,t0}|D = 0, X_i)$ for $t1 \geq \tau$ and $t0 < \tau - ad$, $a^e(X_i, t) = a^n(X_i, t)$, $c_i = c^e_i = c^n_i$, and $u_{i,t} = u^e_{i,t} = u^n_{i,t}$. However, the common individual specific effect c_i does not have to be conditionally mean independent of D and X_i.

3.3.2 Conditional Difference–in–Differences in Hazard Rates (CDiDHR)

Based on the employment model in equation (5), we develop the following Conditional Difference–in–Differences in Hazard Rates (in the following: CDiDHR) estimator as an extension of the CDiDS estimator to a state dependent employment process.[15] We simply estimate the treatment effect on the employment probability via CDiD conditional on employment status in the previous month by

$$\frac{1}{N^l}\sum_{i\in\mathcal{N}^l} g_i\left[Y^1_{i,t1} - Y^0_{i,t0} - \sum_j w_{i,j}(Y^0_{j,t1} - Y^0_{j,t0})\right] \tag{6}$$

[13]Although our model is defined in discrete time we use the word 'rate', as it can be aggregated to a probability in discrete time.

[14]We do not take symmetric differences $\tau_0 - t0 = t1 - \tau_1$ with τ_0 the beginning of the program and τ_1 the end of the program, as in Heckman and Smith (1999), see footnote 10 above.

[15]With the abreviation CDiD we address in the following the conditional difference–in–differences method in general or the CDiDS and CDiDHR estimators jointly.

where l denotes the employment status in the previous month ($l = 1$ if previously employed and $l = 0$ if previously nonemployed), $\mathcal{N}^l$ is the set of treated individuals for whom $Y_{i,t1-1} = Y_{i,t0-1} = l$, where period $t1$ lies after and $t0$ before treatment for individual i. N^l is the number of individuals in the set $\mathcal{N}^l$. Similar to the previous section, we use the average individual employment rate $Y_{m,t0}$ ($m\!=\!i, j$) in the time period before Ashenfelter's Dip conditional on the employment status in the month before. Thus, we use individual average observable transition rates for alignment in the preprogram period. Only nonparticipants j for whom $Y_{j,t1-1} = Y_{j,t0-1} = l$ are considered, i.e. can have a non zero weight $w_{i,j}$. For $l = 0$ and $l = 1$, expression (6) estimates the reemployment probability and the probability of remaining employed, respectively.

The g_i's represent weights accounting for the fact that $\mathcal{N}^l$ does not include the entire treatment sample. For some individuals, there exists no pair of time periods in the 'pre' and 'post' intervals where they are employed or not employed. Under two natural conditions, this would not affect the definition of the estimated parameter. First, the selection effect depends only on observable X_i's and the unobserved individual specific effects c_i^e and c_i^n. Second, the weights g_i control for the selection in employment states. We will discuss below that our implementation of the weights g_i can only control for the effect of X_i. Accordingly, the definition of the estimated treatment parameter changes slightly.

To properly account for selection bias in the nonparticipation outcome, CDiDHR only requires the mean difference in the idiosyncratic error terms conditional on D and X_i to be invariant over time, i.e. $E(u^k_{i,t1}|D = 1, X_i) - E(u^k_{i,t1}|D = 0, X_i) = E(u^k_{i,t0}|D = 1, X_i) - E(u^k_{i,t0}|D = 0, X_i)$ for k=e,n , $t1 \geq \tau$, and $t0 < \tau - ad$. Analogous to CDiDS, the individual specific effects c^l_i do not have to be conditionally mean independent of treatment status D and covariates X_i. Also for CDiDHR, $t0$ must lie before $-ad$, i.e. before anticipation and participation rules can take effect, because of the possibility of Ashenfelter's Dip.

The weights g_i take account of the sorting in covariates X_i conditional on the employment status in the previous month. If we used weights $g_i = 1$, CDiDHR would not identify the unconditional TT $E(\delta^k_{i,t1,\tau}|D = 1)$ but instead the TT

$E(\delta^k_{i,t1,\tau}|D = 1, Y_{t1-1} = l, Y_{t0-1} = l)$ conditional on the employment status l both in the previous month ($l = 0$ if $k = n$ and $l = 1$ if $k = e$) and in the month before the baseline period $t0$. The latter TT is not the same as the unconditional TT with the potential treatment effects $\delta^k_{i,t,\tau}$ being defined irrespective of the employment status of individual i in the previous month. To estimate the unconditional TT, it would be necessary both to account for the differences in the distribution of the X_i characteristics and of the individual specific effects c^k_i with $k = e, n$, since the individual specific treatment effects in the employment model (5) as well as the observed employment status in the previous month presumably depend upon both X_i and the c^k_i's. Differences in X_i and the c^k_i's result in a sorting of high employment individuals into the group of employed individuals in the previous month and vice versa. Ideally, the weights g_i should reweight the treated individuals with a certain employment state in the previous month by the ratio $f(X_i, c^e_i, c^n_i|D = 1)/f(X_i, c^e_i, c^n_i|D = 1, Y_{t1-1} = l, Y_{t0-1} = l)$ where the numerator represents the frequency to appear in the treatment sample and the denominator the frequency both to appear in the treatment sample and to be in employment state l in the previous months.

When defining the weights g_i, we can not take account of the individual specific effects c^e_i, c^n_i. In section 4, the weights g_i only integrate out the distribution of X_i in the treatment sample in a simple way by using a regression model where the mean effect is evaluated at the average of the X_i in the treatment sample. Effectively, we identify the TT

$$E_{X_i,D=1}\{E(\delta^k_{i,t1,\tau}|D = 1, Y_{t1-1} = l, Y_{t0-1} = l, X_i)|D = 1, Y_{t1-1} = l, Y_{t0-1} = l\}$$

conditional on the employment status l in the previous months where the outer expectation $E_{X_i,D=1}$ integrates out with respect to the distribution of X_i in the sample $D = 1$. Thus, conditioning on $(Y_{t1-1} = l, Y_{t0-1} = l)$ only affects the distribution of the individual specific effects and the latter is partly controlled for through the correlation between X_i and the c^k_i's. Our treatment effect weights the individual treatment effects by the frequencies that individuals are employed and not employed in the previous period before and after treatment, respectively.

Our approach estimates the unconditional TT under the following two stringent conditions: First, the treatment effects are conditionally mean independent of the individual specific effects when also conditioning on X_i, i.e. $E(\delta^k_{i,t1,\tau}|c^e_i, c^n_i, X_i) = E(\delta^k_{i,t1,\tau}|X_i)$. Second, we observe each treated individual in both employment states before anticipation and participation rules take effect so that the before–after difference can be calculated for some $t0$ in the past. The second assumption is quite innocuous in our application since we consider the preprogram situation up to 18 months in the past. The preprogram level is then the average transition rate conditional on the employment state in the previous month. For almost all treated individuals, these averages are available for both states. The first condition does not hold when the selection into treatment depends upon the treatment effects $\delta^k_{i,t1,\tau}$ conditional upon X_i via the individual specific effects. We do not think that condition is likely to hold.

There is no ready procedure to estimate the unconditional TT by also integrating out the individual specific effects without imposing further stringent assumptions. Thus, we only integrate out the X_i distribution in the treatment sample. It is quite plausible that, conditional on X_i, both treatment effects $\delta^k_{i,t1,\tau}$ are positively correlated with the individual specific effects and that the two individual specific effects are positively correlated. Then, our approach will overestimate the TT for the probabilities of remaining employed and it will underestimate the TT for the reemployment probabilities. Given this, we will nevertheless be able to draw conclusions on the effectiveness of training programs based on the estimation results.

4 Empirical Analysis

4.1 Data

Our analysis uses the Labor Market Monitor Sachsen–Anhalt[16] (Arbeitsmarktmonitor Sachsen–Anhalt, LMM–SA) for the years 1997, 1998, and 1999. The LMM–SA is a panel survey of the working–age population of the state (*Bundesland*) of Sachsen–

[16]Although the data refer to the state of Sachsen–Anhalt only, the results are likely to be representative for East Germany as a whole (see Schulz, 1998). For further information on the data set, see Ketzmerik (2001).

Anhalt with 7,100 participants in 1997, 5,800 in 1998, and 4,760 in 1999. 1999 is the last year in which the survey was conducted. Only in the three years used, retrospective questionnaires on the monthly employment status between 1990 and up to December 1999 were included. The monthly data provide all possible labor market states, i.e. employment, unemployment, or participated in a program of ALMP, as well as periods in the education system, inactivity, or in the military. Individuals who did not participate in the 1998 survey are recorded until at least September 1997, those who dropped out in 1999 at least until October 1998. Recall error is unlikely to be of particular importance for these data (a further discussion on this issue can be found in appendix B).

Selection of Sample

Unfortunately, in the three survey years used the categories of the labor market states differ. For compatibility, the data set also includes a combined monthly calendar for the three survey years (compiled by the Zentrum für Sozialforschung Halle (ZSH)). This calendar distinguishes the following categories: Education, full–time employed, part–time employed, unemployed, job creation scheme, training, retirement, pregnancy/maternity leave, not in active workforce.

We only consider individuals with complete information on their labor market history between January 1990 and at least September 1997 (i.e. individuals who completed the retrospective question in 1997).[17] The individuals are between 25 and 50 years old in January 1990 and employed before the start of the "Economic and Social Union" in June 1990. This way, only individuals are included who had belonged to the active labor force of the former GDR, who therefore were fully hit by the transformation shock, and who are not too close to retirement. Individuals who are later on in education, on maternity leave or retired are excluded completely from the analysis. The goal is to construct a consistent data base excluding individuals who have left the labor market completely. In addition, we exclude individuals without valid information on those individual characteristics, on which we build the matching. We aggregate the remaining labor market states to the four categories

[17]See Table 1 for the number of observations dropped from the sample for each of the reasons described here.

employment, which comprises part– and full–time employment, *nonemployment*, which comprises unemployment and out of the labor force, *training* and *job creation*.

Our outcome variable employment is defined with nonemployment as the alternative resulting in a binary outcome variable. Modeling transitions between unemployment and being out of the labor force is here an impossible task. People move occasionally back and forth between the two states in the data and it is not obvious whether the individuals precisely distinguish between unemployment and being out of labor force, since no formal definition of unemployment is given in the questionnaire.

The resulting sample consists of 5,165 individuals and it is likely to be quite representative of the labor force in the former GDR. Table 2 summarizes participation in ALMP based on our data. The two most important programs, Training (TR) and Job Creation Schemes (JC), were implemented on a large scale. In total, 27% of our sample participated at least once in one of the two programs. While 13% (689 cases) participated at least once in JC, TR was the most important program with a rate of 20% (1,021 cases).[18] Our data do not distinguish between further training and retraining. Therefore, the estimated treatment effects represent an average of the two programs.

After a first training program, a second treatment in training or JC occurred in 326 cases, i.e. more than 36% of the 889 cases in a first treatment in training participated in at least one other program.[19] This paper restricts the analysis to the effects of a first participation in training only.

4.2 Implementation of the Evaluation Approach

In particular, we estimate the effect for participation in training as the first program. The treatment probability (propensity score) is estimated by a parametric probit

[18]The question in the LMM–SA on training also includes privately financed training. However, calculations based on the German Socioeconomic Panel for East Germany show that a very high share of training is in fact public sector sponsored training (in 1993 more than 88%).

[19]We originally estimated effects both for a first and second treatment. We evaluated sequences or increments of multiple treatments using the evaluation approach suggested under section 3. The results of these estimations can be found in appendix B.

model. Since the data do not provide time–varying information (except for the labor market status), the regressors are the static observable characteristics education, occupational degree, gender, age, residence (at the time of the survey) and interactions of gender and education or occupational degree. The group of "nonparticipants" ($D = 0$) represents the entire sample of individuals who are not participating in the treatment sequence under consideration but who might be a participant in another program. The probit model does not model when the participation in the program actually takes place. We do not think that the data are sufficiently rich to model the timing. We do not match on the employment history shortly before the program (see Lechner (1998) for such an approach) because of Ashenfelter's Dip. Here we only present the evaluation results jointly for men and women. In our case separate estimations of the program effects did not show significant differences by gender.[20] Using a bootstrap estimator for the covariance matrix of the estimated treatment effects, we capture the estimation error in the propensity score.

The results of the probit estimate for the propensity score are reported in table 3. There is a very high degree of overlap in the distributions of the estimated propensity score between participants (Treated) and nonparticipants (Nontreated) for all treatments when conditioning on both employment states in the previous month (graphical results are available upon request). Thus, there is sufficient common support for matching and we match the entire treatment sample.[21]

In this paper, the post-program evaluation period starts with the beginning of the participation in training. This approach views the treatment as a different nonemployment state while searching for a job. Since the participant might be enrolled in training for a duration of several months up to two years, the effects after program beginning include a lock–in effect caused by the program itself, i.e. the time spent in the program is likely to cause an increase of the nonemployment probability for the treatment group in the early months of our outcome period.[22]

[20] The results of these estimations are available upon request.

[21] Results can be found in appendix B.

[22] The first version of this paper considered two different evaluation periods, either after the end of the program or at the beginning of the program. By estimating effects after the end of the program, the treatment period is excluded from the employment history when evaluating the

The start of the evaluation period depends upon the outcome variables considered. For employment rates and reemployment probabilities, the evaluation period starts one month after the first month of the treatment. For probabilities of remaining employed, the evaluation period starts one month later than for the other two outcome variables, since we first have to observe employed former participants. We choose the length of the evaluation period to be 36 months (as far as being observed in the data set – otherwise set to missing). For the alignment of the DiD estimators in the preprogram period, we start 18 months before the beginning of the treatment (excluding Ashenfelter's Dip).

Based on the estimated propensity score, we construct matched samples of participants and comparable "nonparticipants". Alignment occurs in the same calendar month. The characteristics and outcomes of matched nonparticipants are the fitted values obtained by the local linear kernel regression of characteristics and outcomes, respectively, on the estimated propensity score in the sample of nonparticipants as a whole. Table 4 provides evidence on the balancing properties in the matched samples. The first column shows the average characteristics in the whole sample. The remaining columns show the average characteristics conditional upon employment state in the previous month. For example, when calculating the average characteristics for the previously nonemployed, the individual contribution to the mean characteristics is weighted by the number of months the individual's state was nonemployment during the time period under consideration. For the matched nonparticipants, the average reported uses all available observations.

Table 4 shows that participants are younger than the nonparticipants and that women participate at a higher rate in training than men. There is no clear cut difference in the skill distribution. It is evident, that the matching process balances well the characteristics of the participants and the matched nonparticipants conditional upon employment status in the previous month. For example, 27% of the previously nonemployed nonparticipants were aged between 25 and 34 in 1990, whereas 40%

success of the treatment because treatment is viewed as time spent outside of the labor market. This exclusion is somewhat unsatisfactory since labor market history continues, especially so for the nonparticipants. The results for the evaluation period after the end of the program can be found in appendix B.

of the participants belonged to this age group. In the matched sample, 36% of the matched nonparticipants belong to this age group. The balancing works especially well for the previously employed in all cases and for the previously nonemployed in most cases. However, the labor market region does not seem perfectly balanced for the latter group.

Furthermore, table 4 sheds some light on the differences in characteristics across employment states in the previous month. Previously employed participants are younger than previously nonemployed. Male participants were more often previously employed compared to females. Previously employed participants more often have a university education.

4.3 Specification of Outcome Equation

In the matched samples, the CDiDS estimators are based on a flexible linear model for the employment dummy as outcome variable. For CDiDHR, the model is estimated separately depending on the employment state in the month before, thus modeling transition rates. The state of nonemployment includes the participation in ALMP programs so that previous and subsequent participation in a program are both accounted for as nonemployment. We estimate an average employment effect of a program relative to all possible nonemployment states for the treated individuals thus estimating TT (with CDiDHR conditioned on the employment status in the previous month). For CDiDHR, we also control for observed, time–invariant characteristics X_i in the outcome equation. The X_i variables enter the equation as deviations from their averages in the treatment sample.

We assume that individual i begins treatment in period τ and we consider the employment outcome Y before the beginning of treatment $t0 = -18, \ldots, -ad-1$, during the time of Ashenfelter's Dip $t1 = -ad, \ldots, -1$, and during the evaluation period $t1 = 1, \ldots, 36$. Note that in our estimation approach, as described in detail below, the inclusion of Ashenfelter's Dip in the outcome regression does not affect the estimated treatment effect. Its sole purpose is to investigate Ashenfelter's Dip itself.

The definition of $t1$ depends on the success criterion. For the unconditional employment probability or the reemployment probability being the outcome variable, the evaluation starts with the beginning of the program, $t1$ is measured relative to τ , e.g. $t1 = 1$ corresponds to month $\tau + 1$ and $t1 = -1$ to $\tau - 1$. For the probability of remaining employed, $t1$ is measured relative to $\tau + 1$ during the evaluation period.

We estimate the following three steps both for **CDiDS** (sample of all participants) and **CDiDHR** (separately for the two employment states in the previous month):

1. We calculate the average long–run preprogram difference between participant i (treatment starts in τ) and comparable nonparticipants as

$$\hat{a}_{i,\tau} = \frac{1}{18 - ad(\tau)} \sum_{t0=-18}^{-ad(\tau)-1} (Y^0_{i,t0} - \sum_j w_{i,j} Y^0_{j,t0}) \, .$$

2. Then, $\hat{a}_{i,\tau}$ is subtracted from the difference during Ashenfelter's Dip and during the evaluation period resulting in the following model to estimate the treatment effects ($I(.)$ denotes the indicator function, $\nu_{i,t1}$ the error term)

$$Y^1_{i,t1} - \sum_j w_{i,j} Y^0_{j,t1} - \hat{a}_{i,\tau} \; = \sum_{s=-ad(\tau)}^{36} \delta_s I(t1 = s) \tag{7}$$

$$+ (\gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2) I(-ad(\tau) \leq t1 < 0) \; + \; (\gamma_1^{po}\tau + \gamma_2^{po}\tau^2) I(t1 > 0) + \nu_{i,t1} \; .$$

   For CDiDHR, we include deviations of the X_i characteristics from their average in the treatment sample as additional regressors in equation (7).

3. The average long–run preprogram differences $\hat{a}_{i,\tau}$ are regressed on a second order polynomial in the starting month of the treatment (using other flexible specifications makes no substantive difference, results are available upon request). We will report the predictions from this regression

$$\hat{\alpha}(\tau) = \alpha_0 + \alpha_1 \tau + \alpha_2 \tau^2 \tag{8}$$

   to illustrate how the average long–run preprogram differences ($\equiv$ residual selection effect due to permanent individual specific effects) between participants and nonparticipants after matching depend upon the timing of the program.

We define:

$\alpha_0, \alpha_1, \alpha_2$	coefficients measuring the long–run preprogram differences depending upon the month when the program starts τ,
$ad(\tau)$	month before the beginning of the program when Ashenfelter's Dip starts depending upon τ,
$\delta_s, \gamma_1^{ad}, \gamma_2^{ad}, \gamma_1^{po}, \gamma_2^{po}$	coefficients modeling the CDiD effect relative to the long–run preprogram differences $\hat{a}_{i,\tau}$, and
$w_{i,j}$	weights implementing local linear kernel regression on the estimated propensity score.

In equation (7), the estimator subtracts the long–run employment (transition) rates before treatment from the outcomes shortly before and after treatment. The effect of the program depends upon the time since treatment ($t1 > 0$) and the beginning of the program τ. The preprogram employment difference $\hat{a}_{i,\tau}$ proves critical for the alignment of the estimators. Dummy variables for the effect of Ashenfelter's Dip are included to capture the decline in the employment probability shortly before the program. The specification allows the employment differences before and after the program to depend in a flexible way upon τ.

The length of Ashenfelter's Dip $ad(\tau)$ is allowed to depend upon the time when the program starts. During the period shortly after unification, it is likely that the dip is fairly short since program participation could not have been anticipated long before and participation rules were not applied in a strict way. This changed with the occurrence of high unemployment in the mid 90's. A visual inspection of the average employment differences between treated and matched controls before and after the program as a function of the time when the program starts indicates that the dip lasts one to two months in 90/91 and increases over time to at most six months for training. Before November 90, we set $ad(\tau) = -1$. Between November 1990 and July 1994, $ad(\tau)$ increases linearly in absolute value from 2 months to 6 months, where $ad(\tau)$ is rounded to the nearest integer. After July 1994, $ad(\tau)$ remains constant. In order to obtain a lower bound for the employment effect of a program (the employment of the future participants decreases during the dip), we are conservative because taking a shorter period for Ashenfelter's Dip would effectively result in a higher difference–in–differences estimate of the treatment effect.

For a program starting in τ, the following expression captures both the estimate of the disproportionate decline in employment during Ashenfelter's Dip and the estimated TT after the program

$$(9) \qquad CDiD(t1,\tau) \;=\; \begin{cases} \delta_{t1} + \gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2 & \text{for} \quad -ad(\tau) \leq t1 \leq -1; \\ \delta_{t1} + \gamma_1^{po}\tau + \gamma_2^{po}\tau^2 & \qquad t1 = 1, ..., 36. \end{cases}$$

Assuming that the linear specification of the outcome equation in the X_i characteristics holds exactly, $CDiD(t1,\tau)$ estimates the TT conditional on previous employment status while integrating out the distribution of the X_i in the treatment sample, see also section 3.3.

4.4 Estimated Treatment Effects

Before turning to the CDiD estimates, we discuss the outcomes in the matched sample for the outcome variable employment rate. Figure 3 reports the average differences in employment rates for the matched sample with individuals starting treatment in the two–year periods 1990/91, 1991/92, etc. If the CIA $E(Y^0|D = 1, X) = E(Y^0|D = 0, X)$ did actually hold with respect to the time invariant characteristics X_i, then the average differences in employment rates for the matched samples would be a consistent estimate of TT. Right after the beginning of the treatment, employment rates of the participants are between 80 and 100 percentage points (ppoints) lower than for comparable nonparticipants. There is a noticeable recovery for the participants afterwards – basically the time path reflects the changes for participants since employment rates for nonparticipants change fairly little in comparison – but the difference comes nowhere close to zero except at the end for 1997/98 (the latter has to be dismissed since it is based on a very small number of cases). Even three years after treatment, employment rates are still between 20 (90/91) and 40 (mid to late 90s) ppoints lower than for comparable nonparticipants. Thus, under the CIA as stated above, one has to conclude that training results in a considerable reduction in employment rates, which is a common result found in the literature when matching is based on observable characteristics (see the survey in Hagen and Steiner (2000)).

The preprogram effects in Figure 3 raise a number of issues that are addressed by our CDiD estimators. While in 1990/91 there is no preprogram difference 13 to 18 months before the treatment, long–run preprogram differences in the order of 10 to 20 ppoints exist for later years. We take this as an indication of the importance of remaining unobservable differences in the matched sample. Thus, our CDiD estimators take account of possible individual specific effects. It is also apparent here that a simple CDiDS estimate based on the difference between long–run postprogram and long–run preprogram outcomes will result in a negative estimate for TT (as we will see in the following). There is also a strong decline in employment rates shortly before the program starts and the decline starts earlier in the later years. In 1990/91, the decline starts within the last six months before the treatment and the average differences immediately before the start of the program amount to 33 ppoints, whereas in 1997/98 the employment rate of the treated declines already 16 months before the treatment. We take this as an indication for Ashenfelter's Dip which a credible difference–in–differences estimator has to take account of. Basing CDiD on the difference between postprogram outcomes and preprogram outcomes shortly before the beginning of the program would erroneously result in a positive estimate for TT. Finally, analyzing employment rates entails the danger that one misses the state dependence in employment. The continuous decline before the program and the recovery process after the program suggest that employment rates do not adjust instantaneously. Thus, one should allow for state dependence as well.

In the following we discuss the results obtained by CDiDS and CDiDHR for the participation in training as the first treatment. We mainly rely on graphical illustrations of the CDiD estimates in equation (9) and the average preprogram levels $\hat{a}_{i,\tau}$. To avoid estimates being solely based on the extrapolation of the parametric model in equation (7), our graphical illustrations only report point estimates representing at least 10 observations. The complete set of estimated coefficients and graphical illustrations is available in appendix B.

4.4.1 CDiDS Results

Figure 4 depicts the estimated CDiDS employment effects $CDiD(t1, \tau)$ in equation (9) for participation in training as the first program during the evaluation period

$t1 = 1, , \ldots, 36$ and for the period of Ashenfelter's Dip $t1 = -ad(\tau), \ldots, -1$. The evaluation period starts with the beginning of the program.[23] To illustrate the changes over time, the estimates are shown in four separate graphs for the starting dates τ being the month of December in the years 1990, 1992, 1994, and 1996. The thick changing line in the graphs represents the estimated $CDiD(t1, \tau)$ for $t1 = -ad(\tau), \ldots, 36$. The dotted lines around this line represent the 95%–confidence interval. The constant line with dotted lines around it represents the estimated long–run preprogram differences $\hat{\alpha}(\tau)$ ("alpha") with associated 95%–confidence interval. The confidence intervals are based on the bootstrap covariance estimates.[24]

For all cases, the CDiDS employment effects of training prove significantly negative during the post-program period, as to be expected from Figure 3. However, the negative employment effect becomes weaker over time. For the treatment starting in 1990, we estimate an effect of -31 ppoints 36 months after the treatment, the corresponding estimate for the year 1996 is -16 ppoints. Our estimates also clearly show that the employment rates become considerably lower shortly before the program starts (Ashenfelter's Dip) and this effect becomes more pronounced over time. There are also important changes in the long–run preprogram differences over time. For participants starting treatment in 1990, $\hat{\alpha}(\tau)$ is not significantly different from zero. For 1992, we already find significant long–run preprogram differences (-17 ppoints) and this feature becomes more important over time (1996: -22 ppoints). This finding corresponds to training programs becoming more focused on groups with severe problems in finding regular employment during the course of the 1990s, as discussed in section 2.

4.4.2 CDiDHR Results

The CDiDHR estimates explicitly take into account the state dependence in the employment process. The outcome variable used is either the reemployment prob-

[23]The effects for the evaluation period starting after the end of the program are similar in nature and are availabe upon request.

[24]When comparing the bootstrap standard errors to conventional heteroscedasticity consistent standard errors, we find that bootstrap standard errors of both $CDiD(t1, \tau)$ and $\hat{\alpha}(\tau)$ are higher, the increase being stronger for the latter. This is also the case for the CDiDHR estimates.

ability of the previously nonemployed or the probability of remaining employed for the previously employed. Figures 5 and 6, organized in the same way as Figure 4, display the estimated CDiDHR employment effects $CDiD(t1, \tau)$ in equation (9).

Figure 5 summarizes the estimated treatment effect on the treated for participants in training as the first program on their reemployment probability. Evaluation starts after the first month of the program. The first graph of Figure 5 shows the employment effects of participation in training as the first program when beginning in December 1990. We find positive employment effects during the evaluation period, which are, however, rarely significant. For example, one year after the program started the participants have a 4 ppoints higher reemployment probability than they would have, had they not participated. These positive effects of the participation in training vanish for programs starting later. For December 1994 and later, the effect sometimes takes negative values, which are significant shortly after the program started. This is not too surprising because one would expect a reduced search effort when the program has just started. During Ashenfelter's Dip, we find a slight decline in the reemployment probability for the participants. This decline is not significant in most cases and it is much less pronounced than for the CDiDS employment effects. The long–run preprogram difference is significantly negative shortly after reunification (-6 ppoints), becomes less negative over time, and is effectively zero for December 1996. This is in contrast to the CDiDS results where the long–run preprogram differences increase over time.

Figure 6 provides results for the probability of remaining employed when the evaluation period starts two months after the beginning of the program. The estimated effect is close to zero for programs that start in December 1990. However, for later dates, the effect becomes significantly positive. For example, one year after the program started in December 1996 the probability of remaining employed increases by approximately 6 ppoints. Ashenfelter's Dip is very pronounced here with strong significantly negative effects. Anticipation but also participation rules might play a role here. Shortly after reunification, the long–run preprogram difference is slightly negative and significant. It becomes more negative in later periods (-5 ppoints for programs that started in December 1996). The preprogram effects for the probability of remaining employed are similar in nature to the CDiDS results reported above.

Why do the results differ between the two transition rates? We think that this is driven mainly by changes in the content of the training programs over time. Shortly after unification a large part of training consisted of short courses mainly aiming at increasing the participant's placement potential, as described in section 2.2. This could be an explanation for the small positive effect on the reemployment probability. However, later on, the composition of training courses changed towards longer courses intended to provide substantive skills. These additional skills could improve the quality of the match between participants and employers, thus increasing the employment stability, once a participant finds a job. However, these additional skills do not seem to help in finding a job at a faster rate.

Also, changes in the search behavior of East Germans due to a better understanding of the labor market and the benefit system in unified Germany might play a role in the differences. Shortly after unification, unemployed East Germans, not being used to a labor market in a market economy, probably tended to accept new jobs quickly with little regard to the quality of the job (wage and job stability). As a result, a positive effect of training programs might show up in an increase in their reemployment probability rather than in an increase of the probability of remaining employed. Later on, individuals searching for a job perhaps became more aware of the importance of finding a 'good' job, which is not only important for their job stability, but also for the level of potential future unemployment benefits, which depend on the earnings in the last job. In addition, the entitlement for transfer payments is prolonged by taking part in a training program for some time after the program, lowering the opportunity costs of job search for participants compared to other unemployed individuals. Thus, participants tended to search longer to find a 'better' job match resulting in a positive effect on the probability of remaining employed.

An important caveat regarding the interpretation of the CDiDHR results is in order here. Since our estimated TT conditions on previous employment, it is likely that the estimates for the probability of remaining employed overestimate and the estimates for the reemployment probability underestimate the true TT for the Training treatment sample as a whole, as discussed in section 3.3. For this group, it might well be the case that reemployment chances increase on average and the positive effect on employment stability is smaller.

Another feature of the results which should be explained are the changes in the long–run preprogram differences. The CDiDHR estimator matches participants and nonparticipants month by month conditional on having the same employment status in the previous month. Shortly after unification the labor market was quite turbulent. Everybody faced a high risk of becoming unemployed, resulting in a relatively small difference in the long–run preprogram difference in the probability of remaining employed. However, some individuals quickly found another job and did not participate in a training program, leading to a large long–run preprogram difference in the reemployment probability at the begin of the 90's. Later on, unemployment became persistent. The difference in transitions out of nonemployment between participants and nonparticipants became less pronounced.[25] The change in the long–run preprogram differences in the probability of remaining employed most likely reflects the stricter targeting of labor market policy on unemployed individuals.

5 Conclusions

This paper investigates the employment effects of first participation in Public Sponsored Training in East Germany after German Unification. Our study makes some methodological progress, particularly regarding modeling the dynamic employment process in the context of program evaluation. Modeling employment as a state–dependent outcome variable, we develop a new semiparametric conditional difference–in–differences estimator for the treatment effect. We use the transition rates between employment and nonemployment as outcome variables. We account for Ashenfelter's Dip caused by anticipation effects and institutional program participation rules.

[25]Note that this explanation of the changes in the long–run preprogram difference does not violate the assumption of permanent fixed effects since participants change over time.

To start with, we find negative effects of training on unconditional employment rates. However, taking account of state dependency in employment, training shows zero or small positive effects. Concerning training programs which took place shortly after reunification, we find some positive, but small program effects on the reemployment probability - although we have been very conservative in modeling the effects. For programs starting in the mid 90's, we find some positive, but small, program effects on the probability of remaining employed. Our results indicate that modeling transition rates is more appropriate and more informative than using unconditional employment rates. Using only employment rates as a success criterion might result in misleading conclusions concerning the effectiveness of ALMP programs. Further results include that the program effects depend heavily on the time the programs took place, probably as a result of institutional changes during the 1990s.

Overall, our results are not as negative as previous results in the literature and it is unlikely that training on average reduces the future employment chances of participants. We also find noticeable differences among different treatment types. At the same time, it remains questionable whether on average training programs are justified in light of the large costs incurred.

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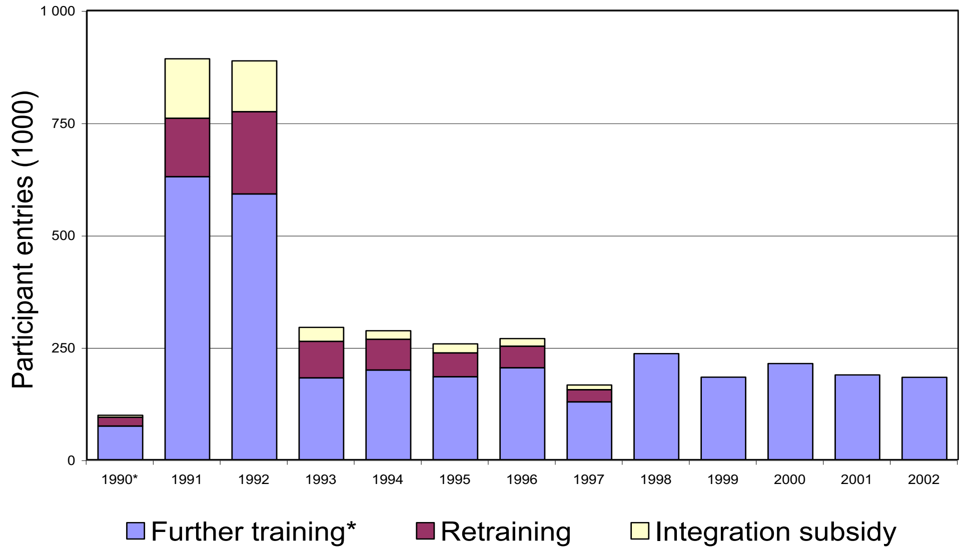
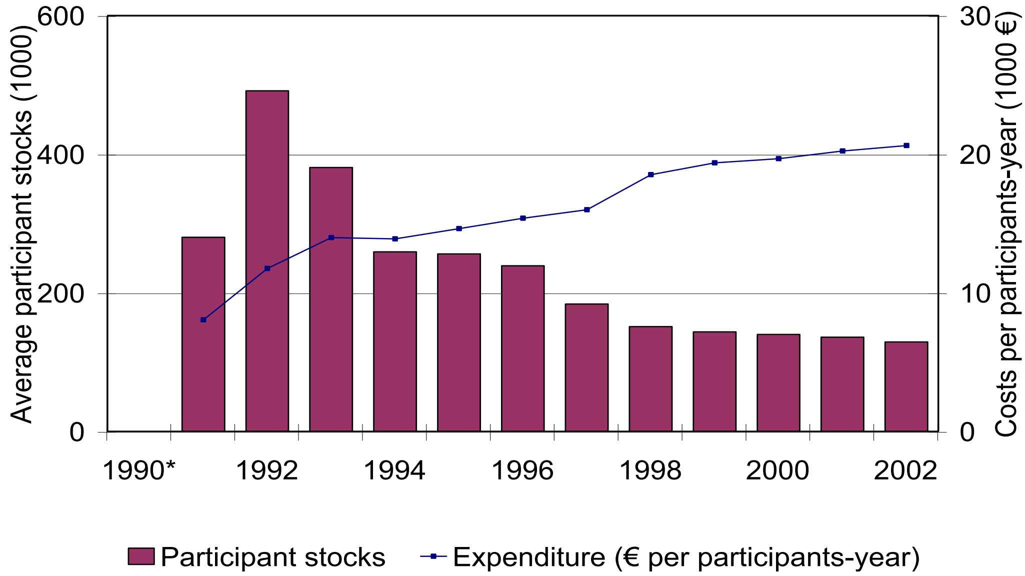
Appendix

Figure 1: Entries into Training in East Germany, Annual Totals



* In 1990, training programs took place only in October, November, and December. Following the 1998 reform, training can no longer be subdivided into three categories. Source: Bundesanstalt für Arbeit (1993, 1997, 2001, 2003), own calculations

Figure 2: Participation Stocks in Training and Expenditure per Participant / Year, Annual Average



* For 1990 no annual stock can be calculated. Source: Bundesanstalt für Arbeit (1993, 1997, 2001, 2003), own calculations

Figure 3: Differences in Outcome Variable (Matched Sample): Training Beginning in Two–Year–Interval 90/91,...,97/98

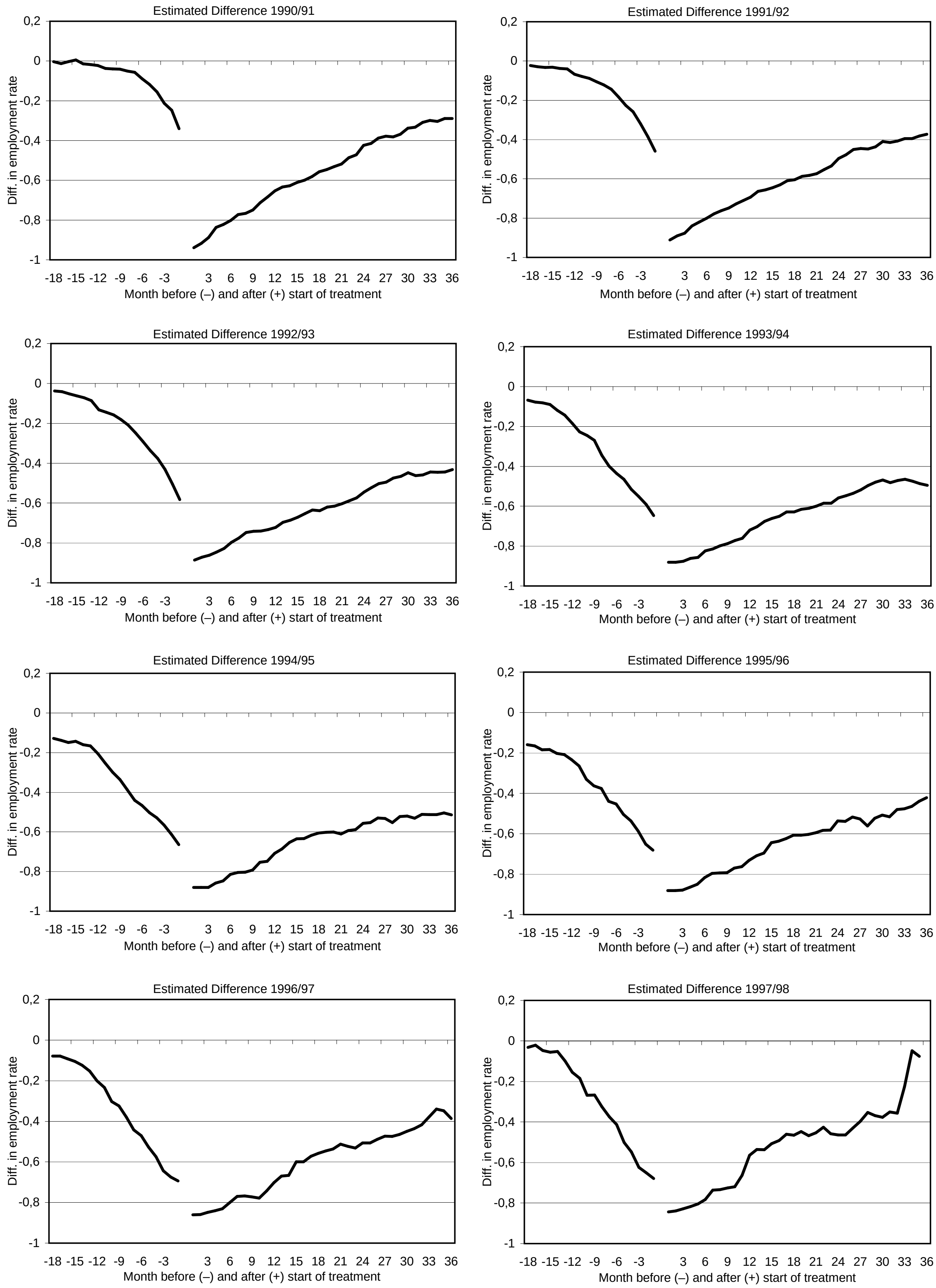


Figure 4: Employment Effects of Training – CDiDS – Evaluation Starts after Beginning of Treatment

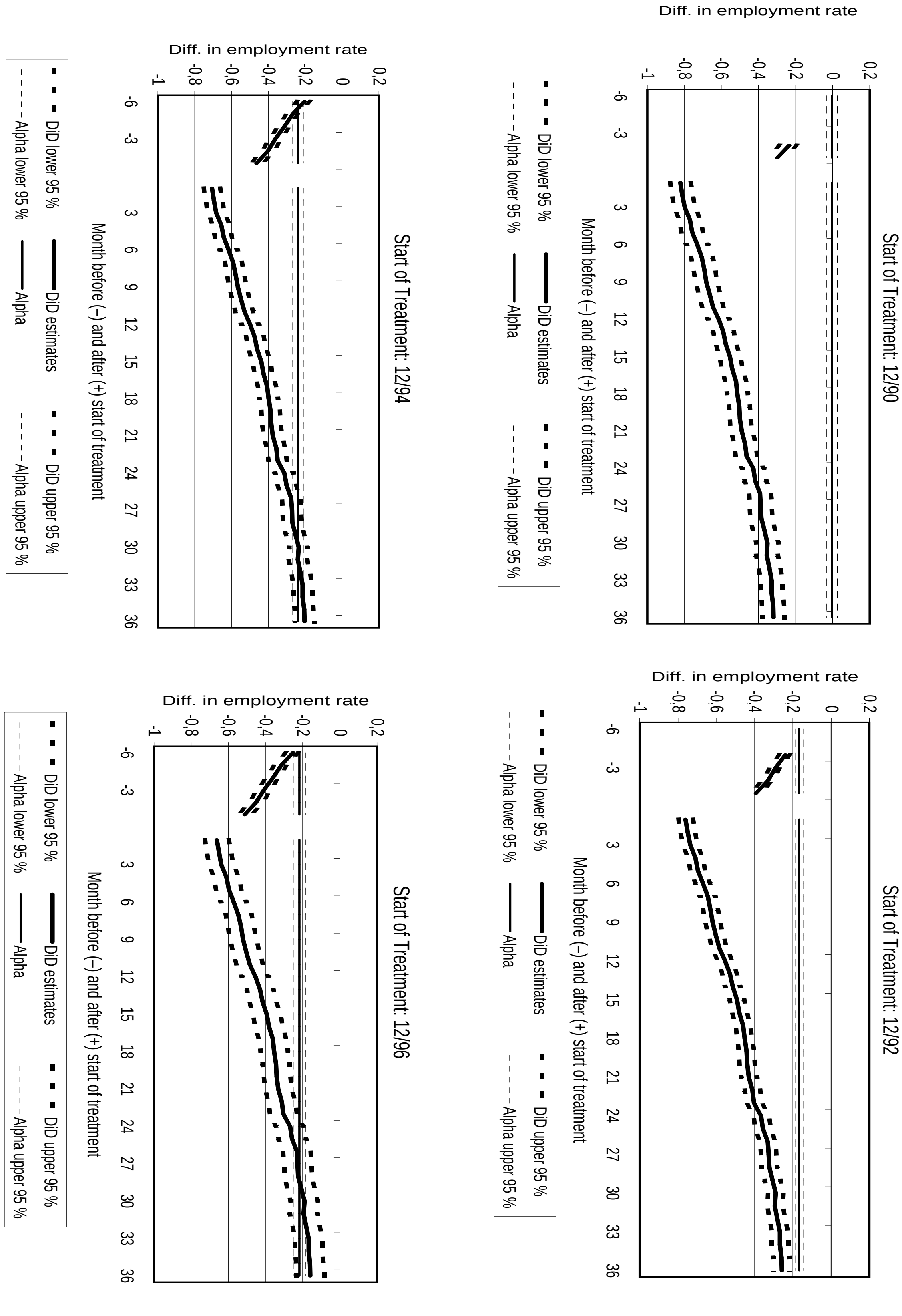


Figure 5: Employment Effects of Training – CDiDHR – Nonemployment in the Previous Month – Evaluation Starts after Beginning of Treatment

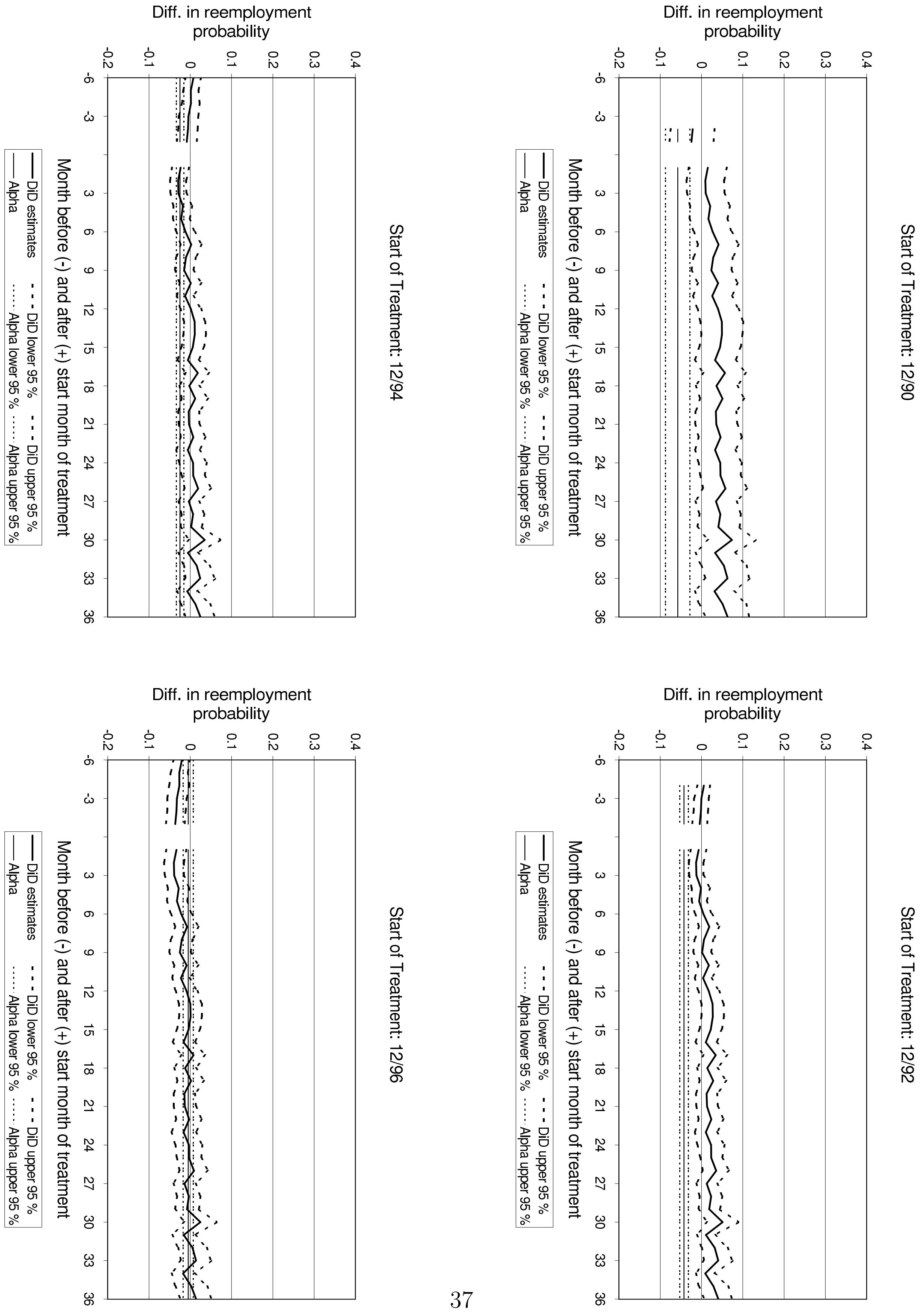
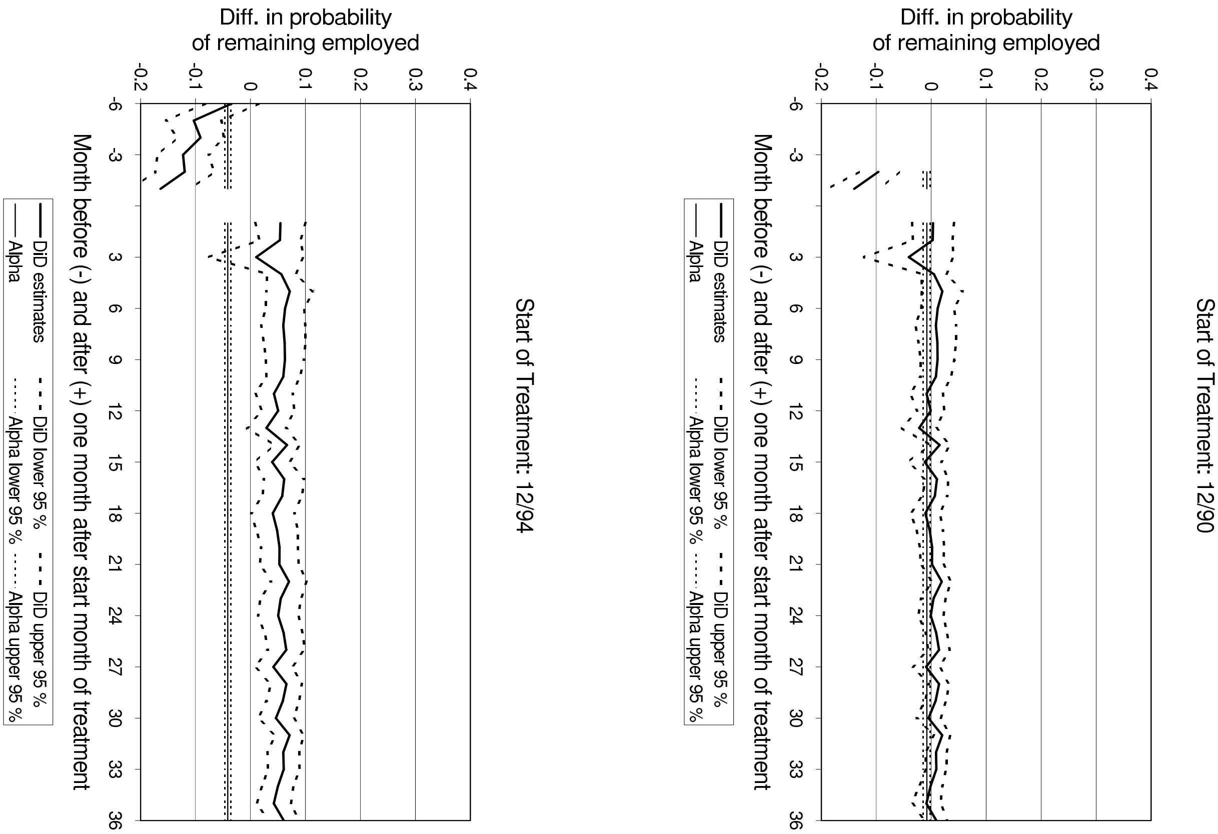
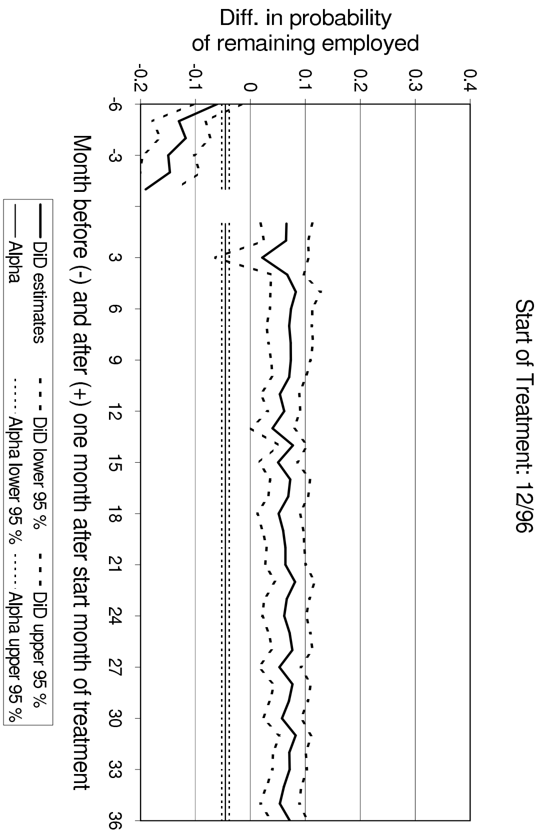


Figure 6: Employment Effects of Training – CDiDHR – Employment in the Previous Month – Evaluation Starts after Beginning of Treatment





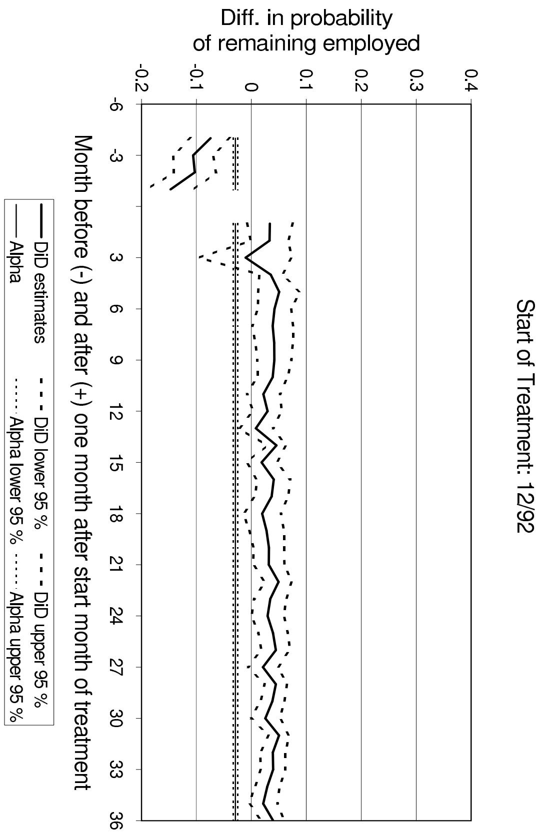


Table 1: Sample Selection

Selection Criteria	Resulting Number of Observations
Fully observed labor market history and year of birth	10,715
Aged between 25 and 50 years in January 1990	6,088
Employed in June 1990	5,529
Not in Education after June 1990	5,480
Not in Maternity Leave after June 1990	5,334
Not retired after June 1990	5,224
Final sample: with valid information on relevant covariates	5,165

Table 2: Program Participation in the LMM–SA during 1990 and 1999[a]

One Program	Job Creation Scheme	Training
At least once	13.3 (689)	19.8 (1,021)
As first program	9.4 (484)	17.2 (889)

Program Sequences[b]	JC–JC	JC–TR	JC alone
First and Second	2.0 (105)	2.2 (113)	5.2 (266)
Program Sequences	TR–JC	TR–TR	TR alone
First and Second	3.4 (176)	2.9 (150)	10.9 (563)

[a] The numbers represent the participation rates and in brackets the absolute number of observation.

[b] For instance, TR–JC indicates that a first participation in training and a second treatment in JC occurred.

Table 3: Propensity Score Estimation

Variable	Participation in Training as a first Program in ALMP Coef.	(s.e.)	mean num. derivative
Constant	-1.036	(0.161)	
Age in 1990: Age 25–34 is omitted category			
Age 35–44	-0.094	(0.047)	-0.023
Age 45–50	-0.311	(0.058)	-0.071
Labor Market Region: Dessau is missing category			
Halberstadt	-0.109	(0.090)	-0.026
Halle	-0.163	(0.077)	-0.038
Magdeburg	-0.126	(0.073)	-0.030
Merseburg	-0.110	(0.082)	-0.026
Sangerhausen	0.009	(0.087)	0.002
Stendal	-0.214	(0.097)	-0.049
Wittenberg	-0.146	(0.111)	-0.034
Professional education (all): Unskilled, semi–skilled or other skills are missing category			
Skilled Worker	0.097	(0.156)	0.024
Craftsman	-0.020	(0.176)	0.022
Technical college	0.271	(0.173)	0.072
University education	0.204	(0.159)	0.052
Professional education (women)			
Skilled worker	0.500	(0.063)	0.140
Craftswoman	0.819	(0.182)	0.267
Technical college	0.035	(0.104)	0.009
University education	0.137	(0.082)	0.036

Table 4: Balancing Properties of Matching for Participation in Training, Evaluation Starts at the Beginning of the Program

Variable	All	Means of Variable in Subgroups					
		Nonpar–ticipants	Parti–cipants	Matched Nonpart.	Nonpar–ticipants	Parti–cipants	Matched Nonpart.
		averaged over prev–iously nonemployed			averaged over prev–iously employed		
Age 25–34	0.37	0.27	0.40	0.36	0.37	0.45	0.43
Age 35–44	0.40	0.36	0.41	0.39	0.40	0.41	0.41
Age 45–50	0.23	0.37	0.19	0.26	0.23	0.14	0.16
Dessau	0.12	0.12	0.14	0.15	0.11	0.14	0.14
Halberstadt	0.09	0.07	0.10	0.06	0.10	0.08	0.09
Halle	0.19	0.17	0.17	0.15	0.19	0.16	0.18
Magdeburg	0.24	0.23	0.23	0.21	0.24	0.25	0.24
Merseburg	0.13	0.16	0.13	0.17	0.13	0.14	0.13
Sangerhausen	0.10	0.11	0.12	0.15	0.09	0.12	0.11
Stendal	0.08	0.09	0.07	0.08	0.08	0.07	0.07
Wittenberg	0.05	0.06	0.05	0.05	0.05	0.05	0.05
Unskilled, semi- or other skilled	0.02	0.09	0.02	0.05	0.02	0.01	0.01
Skilled worker	0.43	0.50	0.50	0.57	0.41	0.46	0.46
Craftsman	0.08	0.08	0.06	0.06	0.08	0.06	0.06
Technical college	0.19	0.16	0.19	0.16	0.20	0.20	0.20
University education	0.27	0.18	0.24	0.16	0.29	0.27	0.27
Female	0.48	0.54	0.64	0.65	0.45	0.55	0.58
Female unskilled worker	0.01	0.05	0.01	0.03	0.01	0.00	0.01
Female skilled worker	0.21	0.30	0.34	0.42	0.17	0.27	0.29
Craftswoman	0.01	0.01	0.03	0.03	0.01	0.02	0.02
Female and technical college	0.13	0.10	0.14	0.10	0.14	0.14	0.14
Female and university education	0.11	0.08	0.12	0.08	0.12	0.12	0.13

Appendix B: Additional Material

Discussion on Recall Error in LMM–SA

Retrospective data, which in our case covers at least 8 years, entails the danger of recall errors. In the following, we will argue that recall errors are less problematic in our analysis than is typically the case with retrospective data.

First of all, note that the individuals were asked about their employment history starting with the year 1990. This year constitutes a turning point in the biography of East Germans, as the political and economic system changed dramatically. The connection of biographic events with historic events, as done here, typically improves the validity of recall data (Loftus/Marburger, 1983, Robinson, 1986). Additionally, starting with the salient year 1990 the individuals had to answer in *chronological* order, which is now commonly viewed as the best technique in collecting life history data in a single survey (Sudman/Bradburn, 1987). Second, our broad definition of employment states circumvents some of the recall errors which are present when analyzing more than two labor market states. It helps especially to merge the states unemployment and out of the labor force. For instance, after some time in unemployment, women tend to label this as having been out of the labor force (Dex/McCulloch, 1998). Third, our evaluation design (CDiDHR estimator) allows for recall errors occurring in the same fashion among treatment and matched comparison group. In particular, if both groups forget to mention transitions in a similar way then the errors simply cancel out.

Thus, recall errors in our analysis might only increase the standard errors of our estimates. However, if we were estimating individual labor market flows, recall errors would be more worrying (Paull, 2002) and it might be useful to change the methodological approach (e.g. following Magnac/Visser, 1999).

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Multiple Treatments and Carousel Effects

The original version of this paper also includes additional estimates for reiterated treatments. In particular, we take into account multiple sequential treatments such that an individual participates in labor market programs more than once. For this purpose, we extend our evaluation approach to the analysis of a first and second treatment. We specify the TT of participation in a second program compared to the situation of not having participated in this specific treatment sequence. The treatment dummy $D2$ is defined such that $D2 = 1$ indicates treatment in this specific treatment sequence and $D2 = 0$ indicates all three other alternatives, i.e. (i) no program participation, (ii) a first training program and no further treatment or another second program not considered here, or (iii) a first treatment other than training. This nontreatment definition allows for the estimation of an average treatment effect assuming that, in the counterfactual situation of nontreatment $D2 = 0$, the treated individual would experience one of the three alternatives with its probability conditional on individual characteristics as observed in the nontreatment sample.

The estimation of the combined effect of the sequence of the first and second treatment is a straightforward application of the single binary treatment case. Individuals with at most one training program $D2 = 0$ are matched to individuals who participate in a second program $D2 = 1$. For CDiDHR, we use the differences between the period after the second treatment ($t1$) and the period before the first treatment ($t0$) for alignment.

To evaluate the incremental effect of the second program we suggest the following heuristic two step procedure. Based on the timing of events, the incremental treatment effect is estimated by CDiDHR using the outcome just before ($t0$) and after ($t1$) the second treatment for alignment in the matched sample. Note, that the combined and the incremental effect differ only by the alignment of $t0$. In line with our analysis above, we treat the participation in the first program as nonemployment. Therefore, the incremental estimator may use the time in the first program and its effects for alignment to estimate the average incremental effect of the second program. The matching procedure uses all nonparticipants of the second program, i.e. the estimated effect relates to the composition of this group. To properly account for selection into the second treatment, we assume that the impact of the individual specific effects enters the individual treatment effects $\delta^k_{i,t,\tau}$ for the first program as an additive constant. Unfortunately, our approach is limited by not allowing for the selection into the second program to depend directly upon the individual treatment effect of the first program (see Lechner and Miquel, 2001, for an approach to deal with this problem based on strong identifying assumption).

Evaluating the combined and incremental effects of multiple program participation, it is possible to investigate whether multiple treatments occur for individuals with particularly bad labor market prospects, whether a further treatment improves the outcome, or whether it just occurs because the participants are unlikely to find a job after the first treatment and this is still the case after further program participation ("carousel effect").

For the multiple, sequential treatments, $CDiD(t1, \tau)$ estimates the incremental employment effect of the second treatment when the beginning of the second program is taken as the beginning of the treatment. The combined effect of the program sequence is obtained using the beginning of the first program. For the incremental effect, the effect of a first treatment is possibly included in the permanent preprogram effect for the participants. Since all TT's are estimated for the specific selection of individuals participating in a certain treatment, it is clear that the TT for a first training and the incremental TT do not have to add up to the combined effect of the treatment sequence.

Starting the evaluation period after the end of the program, figure B.11 naturally shows more positive effects on the reemployment chances of former participants. Also for all cases there is a significantly positive spike in the first month after treatment. This spike can not be interpreted as a pure treatment effect because it also reflects the endogenous, premature termination of the program due to a job offer. However, we also observe smaller but significantly positive program effects after the first month. For example, 12 months after the program the reemployment probability increases by approximately 8 ppoints. For later starting dates, the positive effects are reduced and more often insignificant.

Changing the evaluation period to start two months after the end of the program, the results for the probability of remaining employed do not change qualitatively (see Figure B.12).

Additional Estimation Results

Table B.1: Propensity Score Estimations

	TR–TR		TR–JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Constant	-2.084	(0.140)	-1.625	(0.211)
Age in 1990: Age 25–34 is omitted category				
Age 35–44	-0.078	(0.081)	0.140	(0.084)
Age 45–50	-0.342	(0.109)	0.224	(0.094)
Labor Market Region: Dessau is missing category				
Halberstadt	-0.253	(0.164)	-0.026	(0.144)
Halle	-0.126	(0.128)	-0.423	(0.137)
Magdeburg	-0.121	(0.121)	-0.140	(0.117)
Merseburg	-0.156	(0.140)	-0.176	(0.136)
Sangerhausen	-0.093	(0.149)	0.154	(0.132)
Stendal	-0.414	(0.190)	-0.181	(0.159)
Wittenberg	-0.183	(0.193)	0.036	(0.166)
Professional education (all): Unskilled, semi–skilled or other skills are missing category				
Skilled Worker	-	(-)	-0.645	(0.211)
Craftsman	-0.182	(0.269)	-0.915	(0.312)
Technical college	0.129	(0.221)	-0.391	(0.244)
University education	0.288	(0.144)	-0.295	(0.204)
Professional education (women)				
Skilled worker	0.762	(0.119)	0.747	(0.122)
Craftsman	0.630	(0.397)	1.295	(0.322)
Technical college	0.456	(0.214)	0.074	(0.190)
University education	0.191	(0.143)	0.296	(0.127)

Table B.2: Coefficient estimates for CDiDS

	First Training	
Parameter	Coef.	bootstrap – s.e.
Long–run preprogram difference		
Const	0.109538	(0.031724)
τ	-0.010506	(1.62E-03)
τ^2	7.93E-05	(1.43E-05)
Outcome–equation		
$I(t1 = -6)$	0.015666	(0.051966)
$I(t1 = -5)$	-0.053436	(0.051006)
$I(t1 = -4)$	-0.098527	(0.049093)
$I(t1 = -3)$	-0.147712	(0.047869)
$I(t1 = -2)$	-0.18752	(0.046773)
$I(t1 = -1)$	-0.250343	(0.047172)
$I(t1 = 1)$	-0.331277	(0.072718)
$I(t1 = 2)$	-0.310247	(0.073391)
$I(t1 = 3)$	-0.293056	(0.072545)
$I(t1 = 4)$	-0.287756	(0.07347)
$I(t1 = 5)$	-0.27302	(0.073619)
$I(t1 = 6)$	-0.265125	(0.073884)
$I(t1 = 7)$	-0.254978	(0.074463)
$I(t1 = 8)$	-0.24921	(0.074907)
$I(t1 = 9)$	-0.236731	(0.074903)
$I(t1 = 10)$	-0.222417	(0.074433)
$I(t1 = 11)$	-0.210578	(0.074053)
$I(t1 = 12)$	-0.196867	(0.074775)
$I(t1 = 13)$	-0.181907	(0.074531)
$I(t1 = 14)$	-0.178723	(0.07364)
$I(t1 = 15)$	-0.167247	(0.074247)
$I(t1 = 16)$	-0.162639	(0.073198)

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Table 2: Coefficient estimates <continued>

	First Training	
Parameter	Coef.	bootstrap – s.e.
$I(t1 = 17)$	-0.157038	(0.073196)
$I(t1 = 18)$	-0.150819	(0.072676)
$I(t1 = 19)$	-0.144263	(0.073454)
$I(t1 = 20)$	-0.146938	(0.073612)
$I(t1 = 21)$	-0.148433	(0.074218)
$I(t1 = 22)$	-0.136203	(0.073933)
$I(t1 = 23)$	-0.139326	(0.074613)
$I(t1 = 24)$	-0.149236	(0.074444)
$I(t1 = 25)$	-0.154556	(0.072969)
$I(t1 = 26)$	-0.146676	(0.073267)
$I(t1 = 27)$	-0.145388	(0.073807)
$I(t1 = 28)$	-0.132021	(0.0731)
$I(t1 = 29)$	-0.137155	(0.073106)
$I(t1 = 30)$	-0.133065	(0.073812)
$I(t1 = 31)$	-0.131043	(0.073167)
$I(t1 = 32)$	-0.132572	(0.072657)
$I(t1 = 33)$	-0.125412	(0.072885)
$I(t1 = 34)$	-0.122935	(0.073045)
$I(t1 = 35)$	-0.117068	(0.074169)
$I(t1 = 36)$	-0.113323	(0.073629)
$AD : \tau$	-4.41E-03	(2.01E-03)
$AD : \tau^2$	1.53E-05	(1.72E-05)
$PO : \tau$	-2.89E-03	(3.63E-03)
$PO : \tau^2$	3.63E-05	(3.49E-05)

AD: Ashenfelter's Dip $\equiv I(ad(\tau) \leq \tau < 0)$

PO: After end of program $\equiv I(\tau > 0)$

Table B.3: Coefficient estimates for CDiDHR – First Training – Nonemployment in Previous Month

Start of Evaluation:	One Month after Start Month		One Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.065	(0.023)	-0.065	(0.023)
t	5.45E-04	(8.11E-04)	5.45E-04	(8.11E-04)
t^2	1.93E-06	(6.61E-06)	1.93E-06	(6.61E-06)
Outcome–Equation				
$I(t1 = -6)$	-0.027	(0.049)	-0.035	(0.050)
$I(t1 = -5)$	-0.034	(0.050)	-0.041	(0.050)
$I(t1 = -4)$	-0.033	(0.049)	-0.042	(0.049)
$I(t1 = -3)$	-0.039	(0.048)	-0.048	(0.049)
$I(t1 = -2)$	-0.040	(0.048)	-0.049	(0.048)
$I(t1 = -1)$	-0.043	(0.048)	-0.052	(0.048)
$I(t1 = 1)$	0.029	(0.037)	0.314	(0.050)
$I(t1 = 2)$	0.023	(0.037)	0.090	(0.046)
$I(t1 = 3)$	0.023	(0.036)	0.097	(0.047)
$I(t1 = 4)$	0.034	(0.038)	0.091	(0.046)
$I(t1 = 5)$	0.030	(0.037)	0.099	(0.047)
$I(t1 = 6)$	0.041	(0.036)	0.110	(0.046)
$I(t1 = 7)$	0.055	(0.038)	0.099	(0.046)
$I(t1 = 8)$	0.042	(0.037)	0.114	(0.049)
$I(t1 = 9)$	0.037	(0.038)	0.120	(0.048)
$I(t1 = 10)$	0.054	(0.037)	0.092	(0.047)
$I(t1 = 11)$	0.040	(0.037)	0.101	(0.047)
$I(t1 = 12)$	0.053	(0.038)	0.107	(0.048)
$I(t1 = 13)$	0.063	(0.039)	0.125	(0.048)

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Table 3: Coefficient estimates <continued>

Start of Evaluation:	One Month after Start Month		One Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 14)$	0.063	(0.038)	0.091	(0.048)
$I(t1 = 15)$	0.058	(0.038)	0.109	(0.045)
$I(t1 = 16)$	0.046	(0.038)	0.105	(0.047)
$I(t1 = 17)$	0.070	(0.039)	0.117	(0.049)
$I(t1 = 18)$	0.050	(0.038)	0.094	(0.047)
$I(t1 = 19)$	0.064	(0.039)	0.102	(0.045)
$I(t1 = 20)$	0.048	(0.038)	0.097	(0.046)
$I(t1 = 21)$	0.049	(0.039)	0.089	(0.047)
$I(t1 = 22)$	0.060	(0.039)	0.103	(0.047)
$I(t1 = 23)$	0.046	(0.037)	0.092	(0.047)
$I(t1 = 24)$	0.059	(0.038)	0.092	(0.046)
$I(t1 = 25)$	0.059	(0.037)	0.098	(0.046)
$I(t1 = 26)$	0.071	(0.039)	0.115	(0.048)
$I(t1 = 27)$	0.048	(0.038)	0.101	(0.045)
$I(t1 = 28)$	0.059	(0.038)	0.113	(0.048)
$I(t1 = 29)$	0.054	(0.039)	0.098	(0.045)
$I(t1 = 30)$	0.087	(0.040)	0.098	(0.048)
$I(t1 = 31)$	0.046	(0.037)	0.097	(0.045)
$I(t1 = 32)$	0.068	(0.040)	0.095	(0.045)
$I(t1 = 33)$	0.076	(0.038)	0.115	(0.046)
$I(t1 = 34)$	0.045	(0.037)	0.099	(0.045)
$I(t1 = 35)$	0.065	(0.041)	0.107	(0.048)
$I(t1 = 36)$	0.077	(0.039)	0.098	(0.046)
AD:τ	1.83E-03	(1.98E-03)	2.21E-03	(1.98E-03)

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Table 3: Coefficient estimates <continued>

Start of Evaluation:	One Month after Start Month		One Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
AD:τ^2	-2.08E-05	(1.76E-05)	-2.40E-05	(1.76E-05)
PO:τ	-1.19E-03	(1.31E-03)	-2.57E-03	(1.65E-03)
PO:τ^2	5.28E-06	(1.05E-05)	1.61E-05	(1.35E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	6.54E-03	(1.65E-02)	-7.19E-03	(1.69E-02)
Age 45–50	-1.40E-02	(1.62E-02)	-2.81E-02	(1.81E-02)
Halberstadt	5.19E-04	(1.74E-02)	-1.90E-02	(1.90E-02)
Halle	-2.33E-02	(2.48E-02)	-3.49E-02	(3.17E-02)
Magdeburg	5.95E-03	(1.53E-02)	-4.47E-03	(1.70E-02)
Merseburg	-5.56E-03	(1.82E-02)	-2.23E-03	(1.98E-02)
Sangerhausen	1.42E-02	(1.72E-02)	2.92E-03	(1.96E-02)
Stendal	-2.51E-02	(2.95E-02)	-4.48E-03	(1.98E-02)
Wittenberg	-8.81E-02	(7.32E-02)	-1.15E-01	(8.92E-02)
Skilled Worker	-2.78E-02	(3.32E-02)	2.46E-02	(2.79E-02)
Craftsman	5.39E-05	(2.38E-02)	3.03E-02	(3.50E-02)
Technical college	-2.69E-02	(3.04E-02)	1.11E-02	(4.19E-02)
University education	-3.35E-02	(3.40E-02)	-1.89E-02	(4.15E-02)
Female skilled worker	1.86E-02	(2.60E-02)	-2.01E-02	(2.09E-02)
Craftswoman	2.37E-02	(4.28E-02)	1.74E-02	(4.94E-02)
Female and technical college	2.12E-02	(2.88E-02)	-5.28E-03	(3.83E-02)
Female and university education	2.70E-02	(3.25E-02)	3.04E-02	(4.04E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table B.4: Coefficient estimates for CDiDHR – First Training – Employment in Previous Month

Start of Evaluation:	Two Month after Start Month		Two Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.005	(0.006)	0.005	(0.006)
t	-1.22E-03	(2.69E-04)	-1.22E-03	(2.69E-04)
t^2	7.33E-06	(2.48E-06)	7.33E-06	(2.48E-06)
Outcome–Equation				
$I(t1 = -6)$	-0.011	(0.050)	-0.008	(0.051)
$I(t1 = -5)$	-0.080	(0.048)	-0.078	(0.048)
$I(t1 = -4)$	-0.068	(0.043)	-0.068	(0.044)
$I(t1 = -3)$	-0.099	(0.039)	-0.100	(0.040)
$I(t1 = -2)$	-0.096	(0.033)	-0.097	(0.033)
$I(t1 = -1)$	-0.141	(0.038)	-0.151	(0.038)
$I(t1 = 1)$	-0.016	(0.024)	-0.007	(0.016)
$I(t1 = 2)$	-0.016	(0.025)	-0.008	(0.016)
$I(t1 = 3)$	-0.060	(0.043)	-0.035	(0.018)
$I(t1 = 4)$	-0.014	(0.020)	-0.013	(0.016)
$I(t1 = 5)$	0.001	(0.025)	-0.011	(0.015)
$I(t1 = 6)$	-0.007	(0.022)	-0.028	(0.017)
$I(t1 = 7)$	-0.010	(0.025)	-0.032	(0.016)
$I(t1 = 8)$	-0.008	(0.024)	-0.019	(0.016)
$I(t1 = 9)$	-0.007	(0.022)	-0.012	(0.015)
$I(t1 = 10)$	-0.010	(0.021)	-0.009	(0.014)
$I(t1 = 11)$	-0.027	(0.022)	-0.009	(0.015)
$I(t1 = 12)$	-0.020	(0.020)	-0.022	(0.016)
$I(t1 = 13)$	-0.041	(0.023)	-0.013	(0.016)

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Table 4: Coefficient estimates <continued>

Start of Evaluation:	Two Month after Start Month		Two Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 14)$	-0.004	(0.019)	-0.009	(0.015)
$I(t1 = 15)$	-0.031	(0.023)	-0.022	(0.017)
$I(t1 = 16)$	-0.009	(0.016)	-0.014	(0.016)
$I(t1 = 17)$	-0.012	(0.018)	-0.004	(0.015)
$I(t1 = 18)$	-0.030	(0.019)	-0.009	(0.015)
$I(t1 = 19)$	-0.022	(0.018)	-0.017	(0.015)
$I(t1 = 20)$	-0.017	(0.018)	-0.021	(0.016)
$I(t1 = 21)$	-0.018	(0.017)	-0.007	(0.015)
$I(t1 = 22)$	0.000	(0.017)	-0.015	(0.016)
$I(t1 = 23)$	-0.015	(0.018)	-0.033	(0.017)
$I(t1 = 24)$	-0.020	(0.018)	-0.031	(0.017)
$I(t1 = 25)$	-0.010	(0.017)	-0.017	(0.016)
$I(t1 = 26)$	-0.005	(0.017)	-0.011	(0.016)
$I(t1 = 27)$	-0.028	(0.020)	-0.016	(0.016)
$I(t1 = 28)$	-0.005	(0.018)	-0.013	(0.016)
$I(t1 = 29)$	-0.011	(0.018)	-0.013	(0.016)
$I(t1 = 30)$	-0.024	(0.018)	-0.019	(0.016)
$I(t1 = 31)$	0.001	(0.017)	-0.016	(0.016)
$I(t1 = 32)$	-0.010	(0.018)	-0.007	(0.016)
$I(t1 = 33)$	-0.010	(0.018)	-0.011	(0.016)
$I(t1 = 34)$	-0.020	(0.019)	-0.008	(0.015)
$I(t1 = 35)$	-0.028	(0.021)	-0.011	(0.016)
$I(t1 = 36)$	-0.010	(0.019)	-0.014	(0.016)
AD:τ	1.27E-04	(1.66E-03)	2.62E-04	(1.67E-03)

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Table 4: Coefficient estimates <continued>

Start of Evaluation:	Two Month after Start Month		Two Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
AD:τ^2	-8.62E-06	(1.38E-05)	-1.06E-05	(1.38E-05)
PO:τ	1.67E-03	(1.00E-03)	1.95E-03	(8.69E-04)
PO:τ^2	-8.36E-06	(9.53E-06)	-1.15E-05	(8.49E-06)
Variables as deviation from their mean value over all treated:				
Age 35–44	-3.48E-03	(9.25E-03)	-6.46E-03	(8.75E-03)
Age 45–50	2.68E-02	(2.66E-02)	2.41E-02	(2.21E-02)
Halberstadt	2.14E-02	(1.99E-02)	1.81E-02	(1.79E-02)
Halle	2.78E-02	(2.55E-02)	2.27E-02	(2.05E-02)
Magdeburg	1.29E-02	(1.48E-02)	1.05E-02	(1.22E-02)
Merseburg	2.01E-02	(1.54E-02)	3.12E-02	(1.44E-02)
Sangerhausen	9.00E-03	(1.60E-02)	5.61E-03	(1.39E-02)
Stendal	1.61E-02	(1.73E-02)	1.52E-02	(1.81E-02)
Wittenberg	8.23E-03	(1.97E-02)	5.98E-03	(1.68E-02)
Skilled Worker	-3.27E-02	(3.50E-02)	-1.22E-02	(3.92E-02)
Craftsman	-2.92E-02	(3.61E-02)	-1.65E-02	(3.95E-02)
Technical college	-3.31E-02	(3.57E-02)	-2.07E-03	(4.08E-02)
University education	-3.84E-02	(3.43E-02)	-2.12E-02	(3.89E-02)
Female skilled worker	4.50E-04	(1.84E-02)	-2.71E-04	(1.55E-02)
Craftswoman	-2.15E-02	(3.59E-02)	-2.87E-03	(2.85E-02)
Female and technical college	-2.11E-03	(2.15E-02)	-8.30E-03	(2.28E-02)
Female and university education	2.12E-03	(1.64E-02)	7.07E-03	(1.40E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table B.5: Coefficient estimates for CDiDHR – TR–TR – nonemployment in Previous Month

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.066	(0.037)	-0.011	(0.029)
t	1.62E-03	(1.46E-03)	-5.12E-04	(7.82E-04)
t^2	-1.28E-05	(1.31E-05)	5.53E-06	(5.35E-06)
Outcome–Equation				
$I(t1=-6)$	0.013	(0.067)	-0.062	(0.059)
$I(t1=-5)$	0.008	(0.066)	-0.052	(0.059)
$I(t1=-4)$	0.013	(0.066)	-0.043	(0.059)
$I(t1=-3)$	0.012	(0.065)	-0.059	(0.058)
$I(t1=-2)$	0.009	(0.065)	-0.060	(0.058)
$I(t1=-1)$	-0.003	(0.068)	-0.069	(0.061)
$I(t1=1)$	-0.017	(0.089)	-0.235	(0.204)
$I(t1=2)$	-0.015	(0.088)	-0.232	(0.204)
$I(t1=3)$	-0.017	(0.089)	-0.234	(0.206)
$I(t1=4)$	-0.020	(0.089)	-0.211	(0.206)
$I(t1=5)$	-0.014	(0.089)	-0.225	(0.207)
$I(t1=6)$	-0.015	(0.089)	-0.244	(0.208)
$I(t1=7)$	-0.019	(0.089)	-0.232	(0.208)
$I(t1=8)$	0.014	(0.090)	-0.232	(0.207)
$I(t1=9)$	-0.013	(0.089)	-0.230	(0.207)
$I(t1=10)$	0.018	(0.093)	-0.230	(0.207)
$I(t1=11)$	-0.015	(0.089)	-0.222	(0.208)

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Table 5: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 12)$	-0.018	(0.089)	-0.215	(0.208)
$I(t1 = 13)$	0.002	(0.093)	-0.211	(0.208)
$I(t1 = 14)$	-0.011	(0.090)	-0.229	(0.209)
$I(t1 = 15)$	0.019	(0.098)	-0.237	(0.209)
$I(t1 = 16)$	0.014	(0.096)	-0.208	(0.209)
$I(t1 = 17)$	0.021	(0.094)	-0.228	(0.209)
$I(t1 = 18)$	-0.014	(0.091)	-0.216	(0.209)
$I(t1 = 19)$	-0.013	(0.091)	-0.203	(0.208)
$I(t1 = 20)$	-0.010	(0.090)	-0.214	(0.206)
$I(t1 = 21)$	-0.009	(0.090)	-0.210	(0.207)
$I(t1 = 22)$	-0.010	(0.090)	-0.237	(0.208)
$I(t1 = 23)$	-0.010	(0.090)	-0.223	(0.208)
$I(t1 = 24)$	0.022	(0.090)	-0.195	(0.209)
$I(t1 = 25)$	-0.012	(0.090)	-0.210	(0.211)
$I(t1 = 26)$	0.025	(0.099)	-0.237	(0.210)
$I(t1 = 27)$	-0.012	(0.090)	-0.224	(0.211)
$I(t1 = 28)$	0.005	(0.092)	-0.224	(0.211)
$I(t1 = 29)$	0.026	(0.097)	-0.223	(0.212)
$I(t1 = 30)$	0.009	(0.092)	-0.237	(0.209)
$I(t1 = 31)$	-0.010	(0.089)	-0.223	(0.210)
$I(t1 = 32)$	-0.007	(0.089)	-0.206	(0.211)
$I(t1 = 33)$	0.012	(0.093)	-0.220	(0.210)

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Table 5: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 34)$	-0.005	(0.089)	-0.221	(0.213)
$I(t1 = 35)$	-0.008	(0.089)	-0.241	(0.211)
$I(t1 = 36)$	-0.013	(0.089)	-0.223	(0.210)
AD:τ	1.34E-05	(2.95E-03)	1.82E-03	(1.57E-03)
AD:τ^2	-2.12E-06	(2.80E-05)	-1.28E-05	(1.03E-05)
PO:τ	3.62E-04	(3.68E-03)	6.61E-03	(5.77E-03)
PO:τ^2	-3.23E-06	(3.36E-05)	-4.39E-05	(3.84E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	-1.56E-02	(4.56E-02)	-1.29E-02	(2.32E-02)
Age 45–50	5.54E-03	(5.43E-02)	8.92E-03	(2.95E-02)
Halberstadt	3.16E-02	(4.70E-02)	5.70E-02	(5.30E-02)
Halle	2.90E-02	(4.80E-02)	3.84E-02	(4.32E-02)
Magdeburg	3.22E-02	(4.53E-02)	4.28E-02	(5.30E-02)
Merseburg	2.60E-02	(4.53E-02)	5.02E-02	(5.05E-02)
Sangerhausen	2.95E-02	(5.16E-02)	5.49E-02	(4.33E-02)
Stendal	2.79E-02	(4.62E-02)	3.58E-02	(5.61E-02)
Wittenberg	-1.74E-01	(2.02E-01)	2.84E-02	(3.81E-02)
Skilled Worker	-	(-)	-	(-)
Craftsman	-8.01E-02	(1.34E-01)	-1.52E-02	(5.37E-02)
Technical college	-7.34E-02	(1.19E-01)	-4.57E-02	(5.38E-02)
University education	-7.28E-02	(1.24E-01)	-1.35E-01	(9.07E-02)
Female skilled worker	-	(-)	-3.95E-02	(3.19E-02)

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Table 5: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Craftswoman	-	(-)	-7.93E-03	(5.48E-02)
Female and technical college	-9.49E-02	(1.26E-01)	9.53E-03	(3.35E-02)
Female and university education	3.06E-02	(6.58E-02)	1.20E-01	(6.71E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table B.6: Coefficient estimates for CDiDHR – TR–TR – Employment in Previous Month

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.000	(0.017)	-0.159	(0.055)
t	-1.19E-03	(7.88E-04)	1.58E-03	(2.02E-03)
t^2	9.24E-06	(8.27E-06)	-7.81E-06	(1.61E-05)
Outcome–Equation				
$I(t1=-6)$	0.134	(0.179)	-0.280	(1.371)
$I(t1=-5)$	0.111	(0.184)	-0.374	(1.368)
$I(t1=-4)$	0.027	(0.169)	-0.350	(1.367)
$I(t1=-3)$	0.011	(0.157)	-0.257	(1.364)
$I(t1=-2)$	-0.106	(0.146)	-0.331	(1.343)
$I(t1=-1)$	-0.058	(0.137)	-0.351	(1.364)
$I(t1=1)$	0.111	(0.130)	0.207	(0.442)
$I(t1=2)$	0.108	(0.125)	0.257	(0.443)
$I(t1=3)$	-0.147	(0.233)	0.243	(0.442)
$I(t1=4)$	0.075	(0.109)	0.254	(0.439)
$I(t1=5)$	0.039	(0.103)	0.283	(0.440)
$I(t1=6)$	0.028	(0.103)	0.278	(0.440)
$I(t1=7)$	0.034	(0.101)	0.180	(0.449)
$I(t1=8)$	0.032	(0.102)	0.279	(0.446)
$I(t1=9)$	0.030	(0.101)	0.184	(0.460)
$I(t1=10)$	0.056	(0.104)	0.278	(0.451)
$I(t1=11)$	-0.004	(0.107)	0.288	(0.452)

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Table 6: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 12)$	-0.058	(0.129)	0.291	(0.446)
$I(t1 = 13)$	-0.103	(0.131)	0.366	(0.447)
$I(t1 = 14)$	0.030	(0.102)	0.241	(0.454)
$I(t1 = 15)$	0.030	(0.103)	0.324	(0.441)
$I(t1 = 16)$	-0.019	(0.102)	0.298	(0.431)
$I(t1 = 17)$	0.064	(0.102)	0.286	(0.427)
$I(t1 = 18)$	-0.005	(0.110)	0.351	(0.434)
$I(t1 = 19)$	-0.050	(0.108)	0.344	(0.435)
$I(t1 = 20)$	0.012	(0.106)	0.345	(0.436)
$I(t1 = 21)$	0.014	(0.105)	0.345	(0.436)
$I(t1 = 22)$	0.004	(0.109)	0.345	(0.436)
$I(t1 = 23)$	0.006	(0.095)	0.336	(0.437)
$I(t1 = 24)$	-0.113	(0.119)	0.275	(0.436)
$I(t1 = 25)$	0.033	(0.098)	0.332	(0.434)
$I(t1 = 26)$	-0.025	(0.109)	0.333	(0.435)
$I(t1 = 27)$	0.042	(0.102)	0.330	(0.433)
$I(t1 = 28)$	0.048	(0.102)	0.377	(0.448)
$I(t1 = 29)$	0.005	(0.110)	0.402	(0.447)
$I(t1 = 30)$	-0.016	(0.112)	0.405	(0.448)
$I(t1 = 31)$	0.066	(0.103)	0.397	(0.445)
$I(t1 = 32)$	0.007	(0.106)	0.330	(0.462)
$I(t1 = 33)$	-0.027	(0.111)	0.391	(0.443)

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Table 6: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 34)$	-0.041	(0.109)	0.390	(0.443)
$I(t1 = 35)$	-0.033	(0.119)	0.395	(0.443)
$I(t1 = 36)$	-0.031	(0.117)	0.392	(0.448)
AD:τ	-4.09E-03	(6.84E-03)	6.09E-03	(3.77E-02)
AD:τ^2	1.04E-05	(6.51E-05)	-2.41E-05	(2.40E-04)
PO:τ	-1.40E-03	(5.62E-03)	-4.71E-03	(1.35E-02)
PO:τ^2	2.49E-05	(6.86E-05)	4.54E-05	(1.02E-04)
Variables as deviation from their mean value over all treated:				
Age 35–44	1.83E-02	(4.14E-02)	-7.00E-02	(1.77E-01)
Age 45–50	2.07E-01	(1.94E-01)	-	(-)
Halberstadt	3.78E-02	(8.53E-02)	-4.41E-01	(6.82E-01)
Halle	1.75E-01	(1.07E-01)	-3.28E-01	(6.53E-01)
Magdeburg	2.40E-02	(6.62E-02)	-1.88E-01	(6.70E-01)
Merseburg	4.97E-02	(7.21E-02)	-3.11E-01	(6.69E-01)
Sangerhausen	-1.47E-02	(7.70E-02)	-3.23E-01	(6.57E-01)
Stendal	-1.17E-02	(1.06E-01)	-4.14E-01	(6.63E-01)
Wittenberg	1.51E-01	(1.78E-01)	-4.84E-01	(7.57E-01)
Skilled Worker	-	(-)	-	(-)
Craftsman	-7.76E-02	(1.12E-01)	-	(-)
Technical college	-1.96E-01	(2.16E-01)	-	(-)
University education	-2.74E-01	(1.67E-01)	3.53E-01	(3.29E-01)
Female skilled worker	-1.05E-01	(7.43E-02)	2.11E-01	(1.65E-01)

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Table 6: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Craftswoman	-1.11E-01	(1.00E-01)	-	(-)
Female and technical college	1.21E-01	(1.55E-01)	1.07E-01	(1.66E-01)
Female and university education	1.72E-01	(1.07E-01)	-1.51E-01	(3.64E-01)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table B.7: Coefficient estimates for CDiDHR – TR–JC – nonemployment in Previous Month

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.050	(0.048)	-0.008	(0.031)
t	8.72E-04	(1.74E-03)	-5.70E-04	(9.03E-04)
t^2	-5.37E-06	(1.44E-05)	4.76E-06	(6.27E-06)
Outcome–Equation				
$I(t1 = -9)$	-	(-)	-0.013	(0.049)
$I(t1 = -8)$	-	(-)	-0.025	(0.049)
$I(t1 = -7)$	-	(-)	-0.032	(0.048)
$I(t1 = -6)$	0.007	(0.080)	-0.027	(0.049)
$I(t1 = -5)$	-0.003	(0.081)	-0.028	(0.047)
$I(t1 = -4)$	-0.036	(0.079)	-0.029	(0.048)
$I(t1 = -3)$	-0.023	(0.081)	-0.029	(0.048)
$I(t1 = -2)$	-0.025	(0.081)	-0.030	(0.047)
$I(t1 = -1)$	-0.031	(0.081)	-0.028	(0.047)
$I(t1 = 1)$	-0.041	(0.084)	-0.007	(0.053)
$I(t1 = 2)$	-0.042	(0.085)	0.005	(0.052)
$I(t1 = 3)$	-0.046	(0.085)	0.003	(0.052)
$I(t1 = 4)$	-0.051	(0.085)	0.013	(0.052)
$I(t1 = 5)$	-0.043	(0.084)	0.002	(0.052)
$I(t1 = 6)$	-0.039	(0.084)	0.003	(0.053)
$I(t1 = 7)$	-0.062	(0.086)	0.006	(0.054)
$I(t1 = 8)$	-0.040	(0.086)	0.002	(0.053)

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Table 7: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 9)$	-0.039	(0.084)	0.010	(0.054)
$I(t1 = 10)$	-0.040	(0.085)	0.009	(0.053)
$I(t1 = 11)$	-0.039	(0.085)	0.006	(0.055)
$I(t1 = 12)$	-0.050	(0.085)	0.031	(0.057)
$I(t1 = 13)$	-0.024	(0.087)	-0.007	(0.052)
$I(t1 = 14)$	-0.037	(0.085)	0.008	(0.052)
$I(t1 = 15)$	-0.041	(0.084)	0.007	(0.055)
$I(t1 = 16)$	-0.045	(0.085)	0.008	(0.055)
$I(t1 = 17)$	-0.005	(0.089)	0.014	(0.055)
$I(t1 = 18)$	-0.034	(0.085)	0.003	(0.054)
$I(t1 = 19)$	-0.040	(0.085)	0.014	(0.054)
$I(t1 = 20)$	-0.029	(0.087)	0.003	(0.055)
$I(t1 = 21)$	-0.033	(0.085)	0.008	(0.052)
$I(t1 = 22)$	-0.037	(0.084)	0.003	(0.053)
$I(t1 = 23)$	-0.037	(0.084)	-0.003	(0.052)
$I(t1 = 24)$	-0.030	(0.085)	0.059	(0.060)
$I(t1 = 25)$	-0.025	(0.086)	0.019	(0.057)
$I(t1 = 26)$	-0.042	(0.085)	0.019	(0.055)
$I(t1 = 27)$	-0.030	(0.086)	0.010	(0.054)
$I(t1 = 28)$	-0.016	(0.091)	0.018	(0.055)
$I(t1 = 29)$	-0.036	(0.085)	0.020	(0.056)
$I(t1 = 30)$	-0.021	(0.088)	0.024	(0.056)

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Table 7: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 31)$	-0.048	(0.086)	0.004	(0.054)
$I(t1 = 32)$	-0.030	(0.087)	0.006	(0.053)
$I(t1 = 33)$	-0.026	(0.086)	0.003	(0.054)
$I(t1 = 34)$	-0.041	(0.086)	0.004	(0.053)
$I(t1 = 35)$	-0.031	(0.086)	0.004	(0.053)
$I(t1 = 36)$	-0.049	(0.085)	0.035	(0.058)
AD:τ	2.07E-04	(2.96E-03)	7.82E-04	(1.39E-03)
AD:τ^2	-1.96E-06	(2.63E-05)	-6.50E-06	(9.68E-06)
PO:τ	1.16E-03	(3.25E-03)	1.53E-04	(1.62E-03)
PO:τ^2	-1.31E-05	(3.00E-05)	-1.98E-06	(1.16E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	-1.96E-03	(3.09E-02)	-1.85E-02	(1.33E-02)
Age 45–50	-3.19E-02	(3.97E-02)	3.59E-03	(1.78E-02)
Halberstadt	-5.33E-02	(4.62E-02)	5.34E-03	(2.69E-02)
Halle	-1.63E-01	(1.47E-01)	-4.34E-03	(1.97E-02)
Magdeburg	-5.78E-02	(4.96E-02)	-2.13E-02	(2.54E-02)
Merseburg	-7.08E-02	(5.43E-02)	-1.21E-02	(2.18E-02)
Sangerhausen	-6.33E-02	(4.99E-02)	1.08E-03	(1.79E-02)
Stendal	-3.89E-02	(5.44E-02)	1.63E-02	(3.07E-02)
Wittenberg	-1.09E-01	(7.63E-02)	2.36E-02	(3.27E-02)
Skilled Worker	1.98E-02	(6.64E-02)	1.24E-02	(1.96E-02)
Craftsman	7.19E-02	(9.11E-02)	2.55E-02	(4.87E-02)

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Table 7: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Technical college	-6.51E-02	(8.33E-02)	-1.71E-01	(1.75E-01)
University education	-1.21E-01	(1.22E-01)	9.95E-03	(1.98E-02)
Female skilled worker	-2.02E-02	(6.82E-02)	-1.39E-02	(1.53E-02)
Craftswoman	-7.82E-02	(1.07E-01)	-1.68E-02	(4.82E-02)
Female and technical college	7.32E-02	(8.06E-02)	1.69E-01	(1.74E-01)
Female and university education	1.10E-01	(1.09E-01)	2.40E-03	(1.33E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table B.8: Coefficient estimates for CDiDHR – TR–JC – Employment in Previous Month

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.021	(0.016)	-0.031	(0.026)
t	-2.03E-03	(8.81E-04)	-2.23E-03	(1.18E-03)
t^2	1.24E-05	(9.29E-06)	1.34E-05	(9.27E-06)
Outcome–Equation				
$I(t1 = -9)$	-	(-)	1.194	(0.639)
$I(t1 = -8)$	-	(-)	1.234	(0.590)
$I(t1 = -7)$	-	(-)	1.160	(0.571)
$I(t1 = -6)$	0.170	(0.138)	1.261	(0.568)
$I(t1 = -5)$	-0.025	(0.150)	0.858	(0.597)
$I(t1 = -4)$	0.031	(0.127)	0.883	(0.366)
$I(t1 = -3)$	-0.053	(0.110)	0.635	(0.423)
$I(t1 = -2)$	-0.040	(0.090)	-0.106	(0.337)
$I(t1 = -1)$	-0.116	(0.107)	-	(-)
$I(t1 = 1)$	0.076	(0.118)	-0.468	(0.162)
$I(t1 = 2)$	0.075	(0.118)	-0.594	(0.208)
$I(t1 = 3)$	0.073	(0.118)	-0.569	(0.176)
$I(t1 = 4)$	0.059	(0.114)	-0.449	(0.190)
$I(t1 = 5)$	0.057	(0.114)	-0.464	(0.177)
$I(t1 = 6)$	0.075	(0.114)	-0.476	(0.172)
$I(t1 = 7)$	0.059	(0.110)	-0.510	(0.167)
$I(t1 = 8)$	0.065	(0.116)	-0.512	(0.163)

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Table 8: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 9)$	0.062	(0.112)	-0.535	(0.159)
$I(t1 = 10)$	0.060	(0.112)	-0.536	(0.159)
$I(t1 = 11)$	-0.074	(0.149)	-0.634	(0.159)
$I(t1 = 12)$	0.055	(0.107)	-0.589	(0.154)
$I(t1 = 13)$	-0.019	(0.111)	-0.588	(0.154)
$I(t1 = 14)$	0.081	(0.104)	-0.572	(0.153)
$I(t1 = 15)$	-0.078	(0.149)	-0.581	(0.153)
$I(t1 = 16)$	0.079	(0.103)	-0.581	(0.153)
$I(t1 = 17)$	-0.054	(0.125)	-0.580	(0.154)
$I(t1 = 18)$	-0.062	(0.149)	-0.652	(0.165)
$I(t1 = 19)$	0.063	(0.108)	-0.584	(0.152)
$I(t1 = 20)$	0.007	(0.127)	-0.575	(0.152)
$I(t1 = 21)$	0.058	(0.108)	-0.576	(0.152)
$I(t1 = 22)$	0.053	(0.107)	-0.577	(0.152)
$I(t1 = 23)$	0.006	(0.108)	-0.571	(0.152)
$I(t1 = 24)$	0.003	(0.120)	-0.535	(0.153)
$I(t1 = 25)$	0.011	(0.114)	-0.533	(0.153)
$I(t1 = 26)$	0.014	(0.118)	-0.537	(0.154)
$I(t1 = 27)$	-0.035	(0.121)	-0.537	(0.153)
$I(t1 = 28)$	0.058	(0.109)	-0.538	(0.154)
$I(t1 = 29)$	0.061	(0.109)	-0.537	(0.154)
$I(t1 = 30)$	-0.058	(0.133)	-0.538	(0.154)

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Table 8: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 31)$	0.063	(0.107)	-0.610	(0.165)
$I(t1 = 32)$	0.066	(0.110)	-0.539	(0.156)
$I(t1 = 33)$	0.064	(0.109)	-0.626	(0.158)
$I(t1 = 34)$	-0.028	(0.121)	-0.594	(0.154)
$I(t1 = 35)$	0.012	(0.115)	-0.598	(0.155)
$I(t1 = 36)$	0.050	(0.108)	-0.615	(0.156)
AD:τ	-1.04E-03	(4.97E-03)	-4.16E-02	(1.61E-02)
AD:τ^2	-3.15E-05	(5.08E-05)	2.65E-04	(1.04E-04)
PO:τ	-1.33E-03	(6.70E-03)	2.92E-02	(7.02E-03)
PO:τ^2	1.04E-05	(7.64E-05)	-2.27E-01	(3 .602536E-04)
Variables as deviation from their mean value over all treated:				
Age 35–44	-3.26E-02	(4.70E-02)	-	(-)
Age 45–50	-1.31E-02	(5.81E-02)	-2.13E-01	(8.14E-02)
Halberstadt	-5.15E-02	(7.68E-02)	4.53E-01	(1.11E-01)
Halle	-5.17E-02	(1.16E-01)	4.29E-01	(1.37E-01)
Magdeburg	-2.09E-02	(4.68E-02)	3.89E-01	(8.11E-02)
Merseburg	-7.97E-02	(6.90E-02)	1.61E-01	(7.59E-02)
Sangerhausen	-4.35E-03	(5.11E-02)	3.15E-01	(7.61E-02)
Stendal	-8.06E-02	(8.23E-02)	3.47E-01	(1.56E-01)
Wittenberg	-9.75E-02	(7.19E-02)	5.02E-01	(1.14E-01)
Skilled Worker	-1.34E-01	(7.43E-02)	-4.80E-01	(1.09E-01)
Craftsman	-9.86E-02	(1.25E-01)	-	(-)

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Table 8: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Technical college	-2.07E-01	(1.12E-01)	-	(-)
University education	-1.29E-01	(8.24E-02)	-	(-)
Female skilled worker	-3.71E-02	(5.94E-02)	5.56E-01	(1.50E-01)
Craftswoman	-	(-)	-	(-)
Female and technical college	-2.24E-03	(9.76E-02)	-	(-)
Female and university education	-5.84E-03	(5.37E-02)	-1.29E-01	(5.93E-02)

Incremental Effect of JC with conventional, heteroscedasticity consistent standard errors due to insufficient number of observations.

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

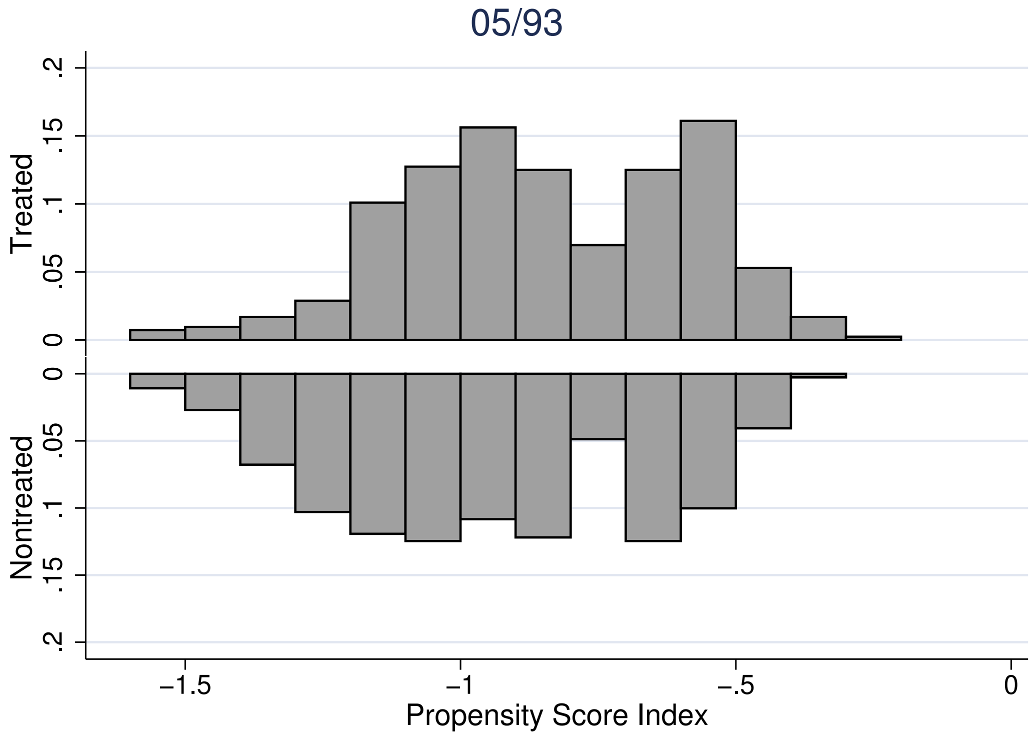
PO: After end of program $\equiv I(t1 > 0)$

Overlap in estimated propensity scores

Figures B.1 – B.4 show the high degree of overlap in the distributions of the estimated propensity score[1] between participants (Treated) and nonparticipants (Nontreated) for the treatments FTR and TR–TR (the graphs are similar in nature for TR–JC). The following graphs are stratified conditional upon nonemployment and employment in the previous month, respectively. Since the employment status changes over time and since after 1997 no complete data is available for all individuals, the overlap can change over time. Here, the graphs show the overlap of the distributions for the two months 5/1993 and 5/1997, being representative for other periods. Only in rare cases, such as first training in 5/1993 and being previously nonemployed, we find a slightly less than perfect overlap.

[1]The graphs depict the fitted values of the latent index for the probit model. The estimated treatment probability is the cdf of the standard normal applied to this index.

Figure B.1: Overlap of Distributions of Propensity Score Index for First Training – Nonemployment in Previous Month



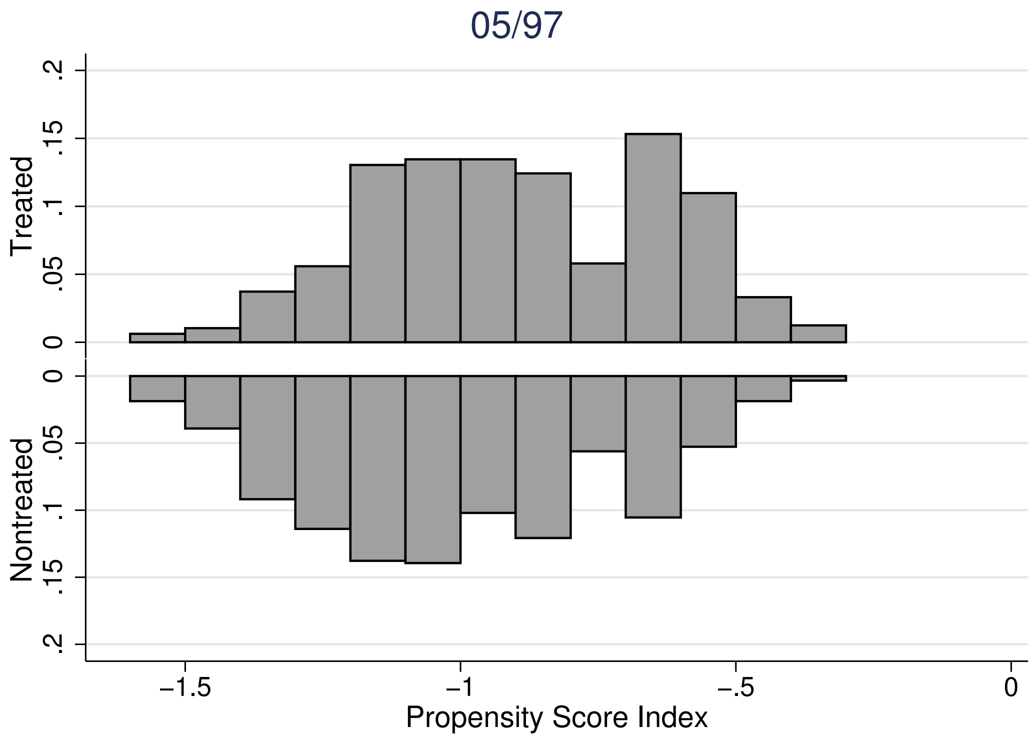
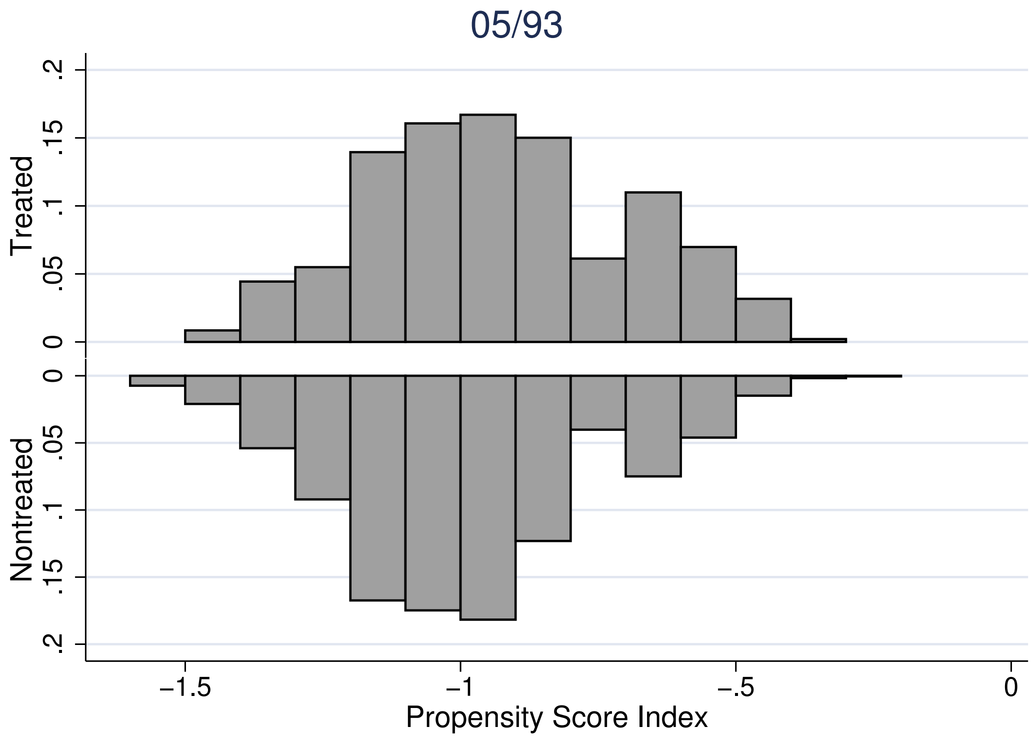


Figure B.2: Overlap of Distributions of Propensity Score Index for First Training – Employment in Previous Month



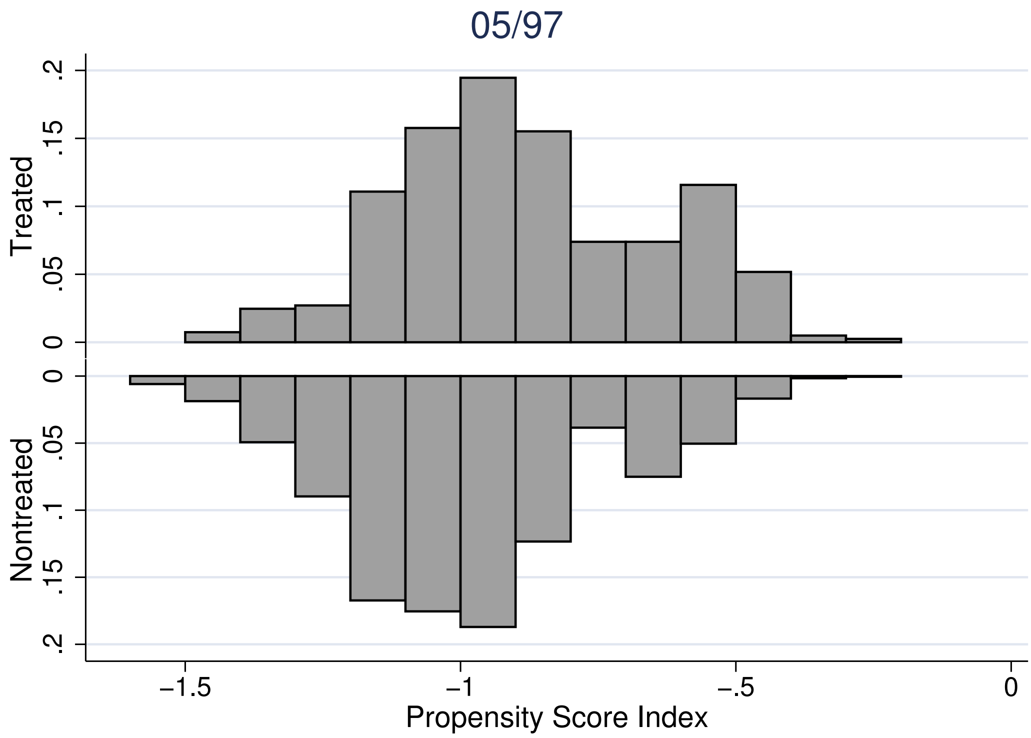
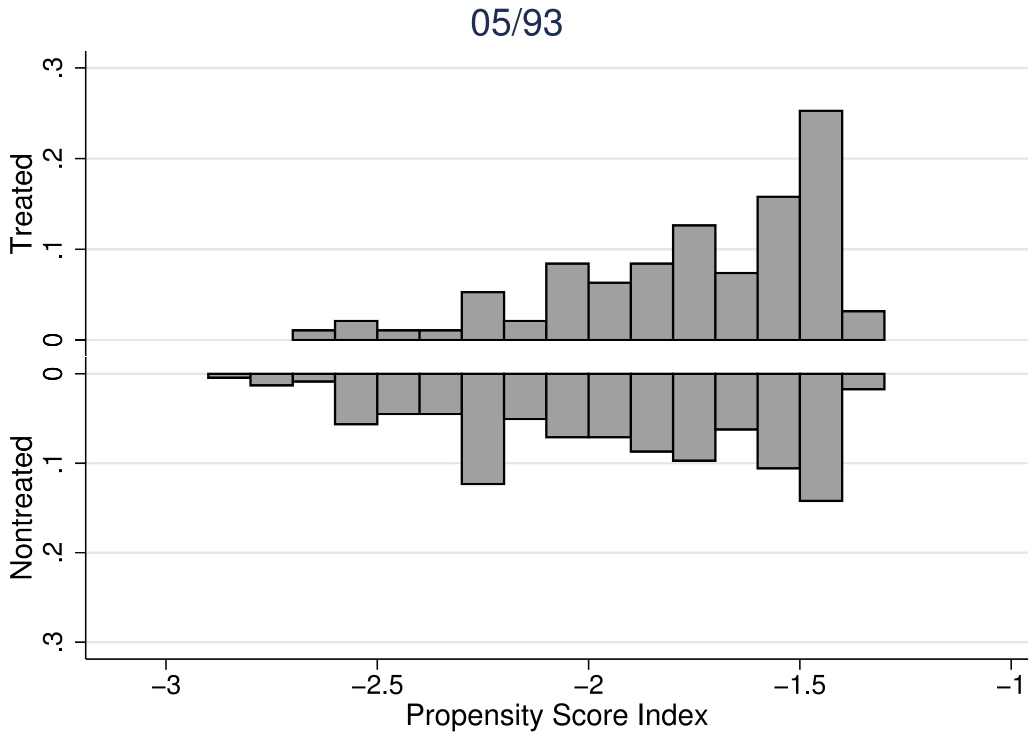


Figure B.3: Overlap of Distributions of Propensity Score Index for TR–TR – Non-employment in Previous Month



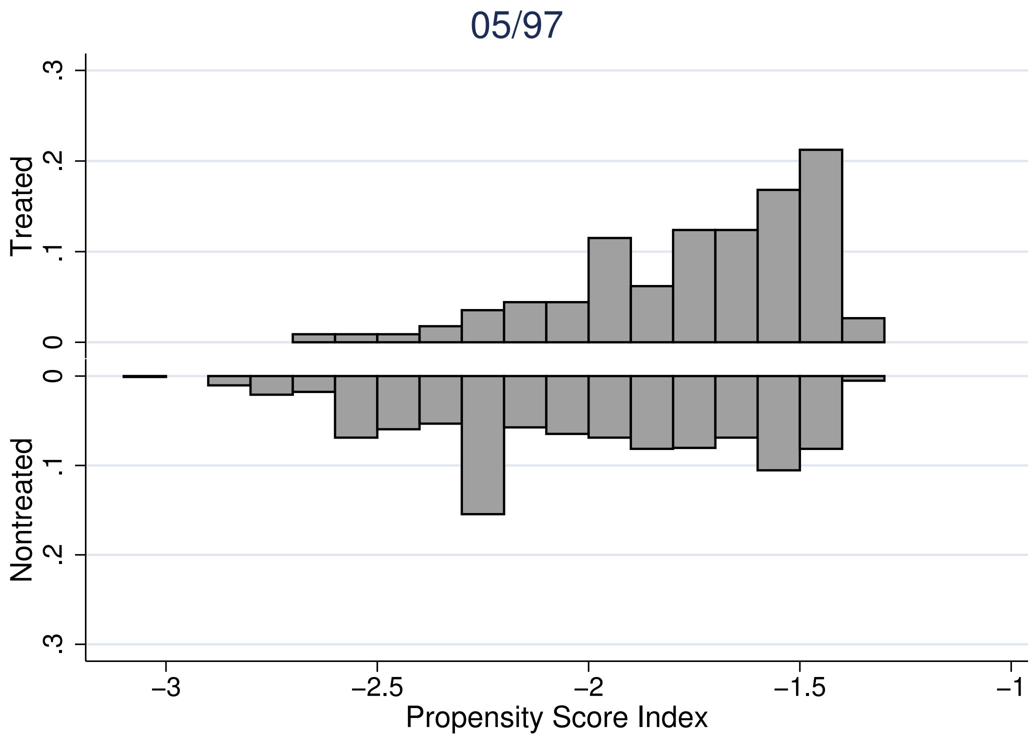
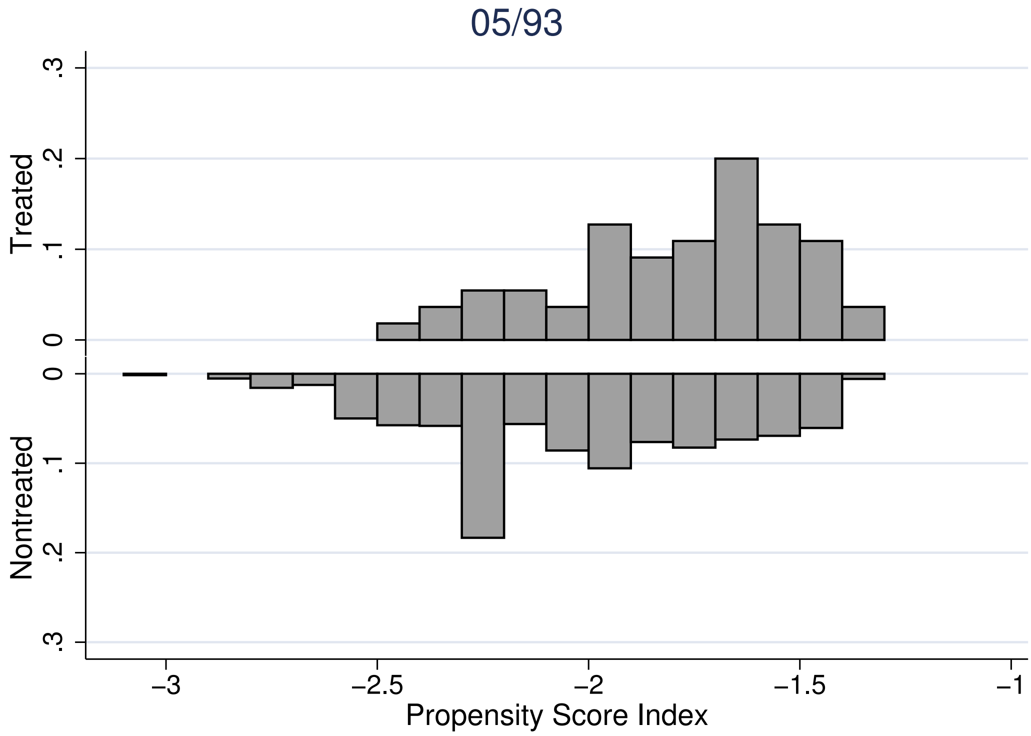
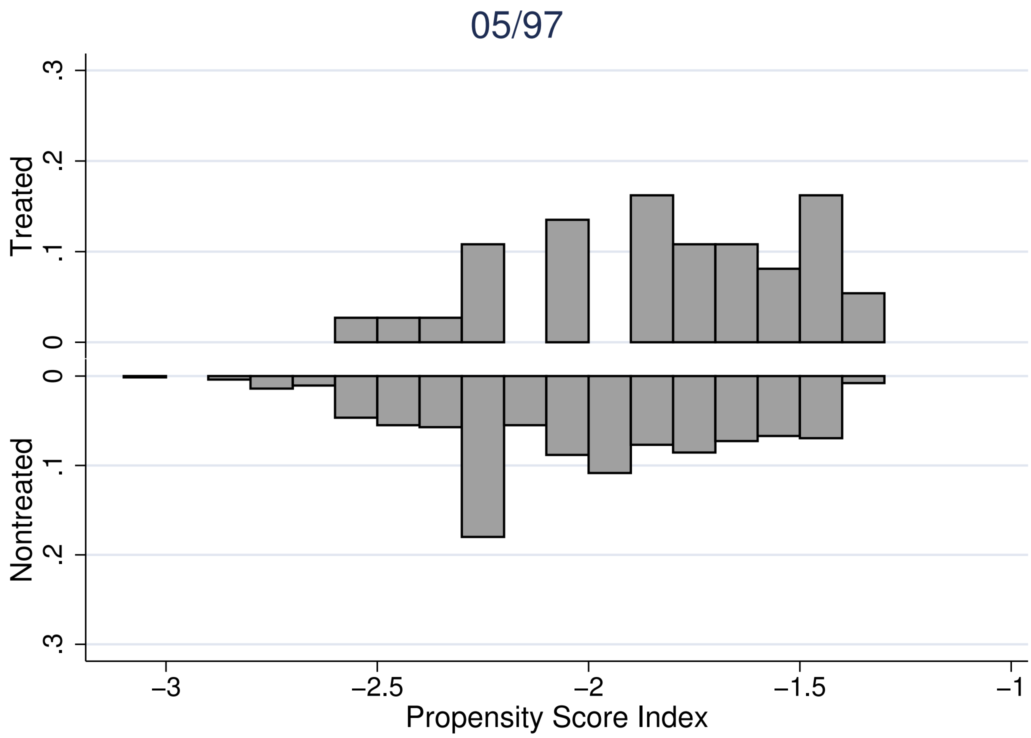


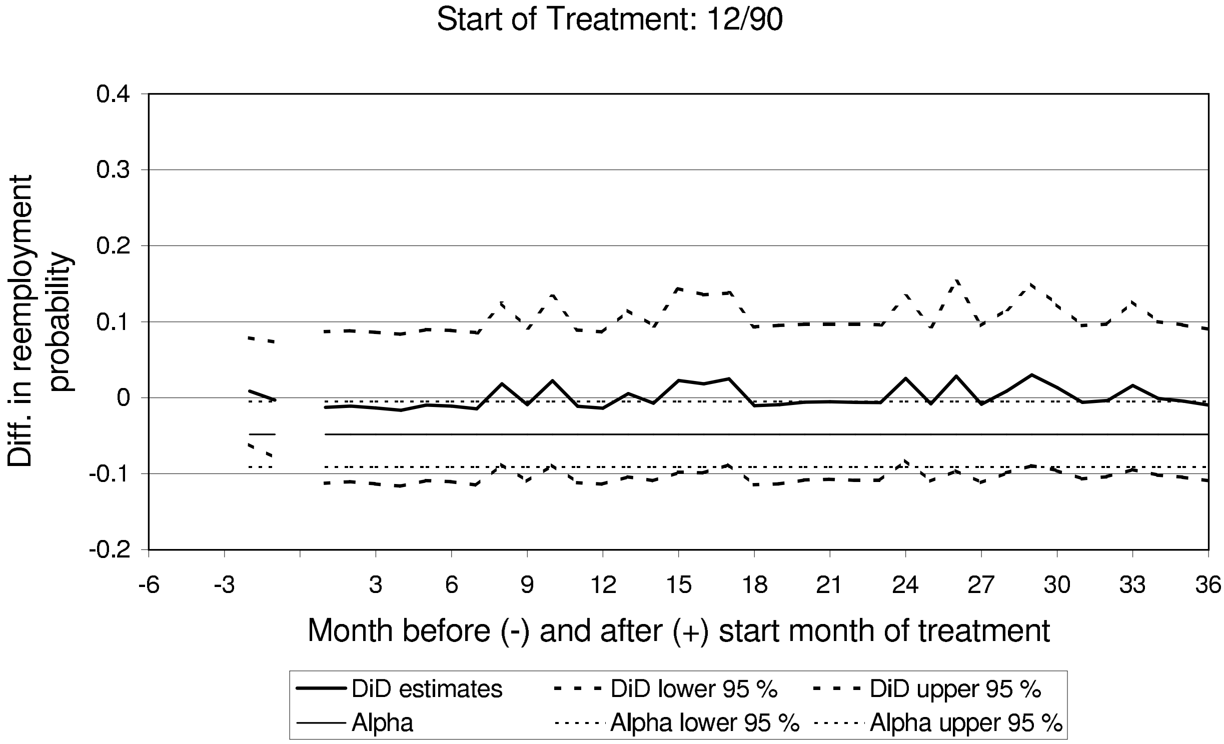
Figure B.4: Overlap of Distributions of the Propensity Score Index for TR–TR – Employment in Previous Month





Additional Graphical Representations of Estimated Treatment Effects

Figure B.5: Combined Employment Effects of TR–TR – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Beginning of Treatment



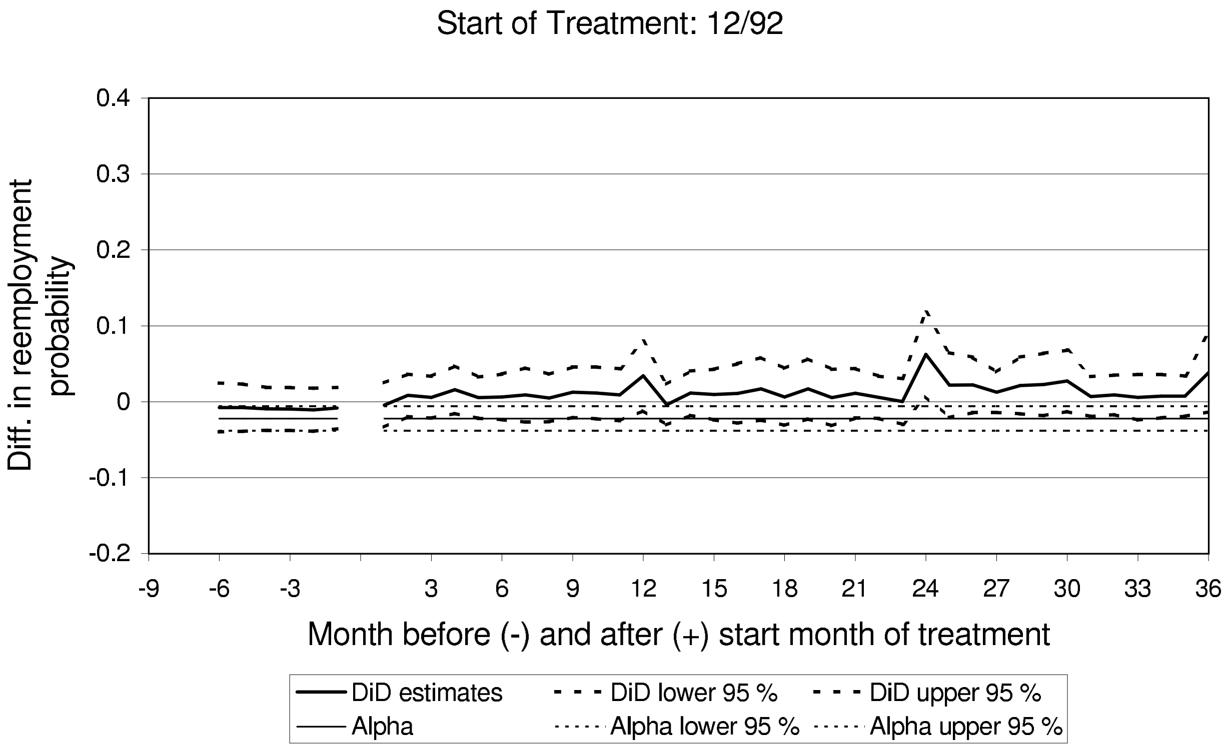
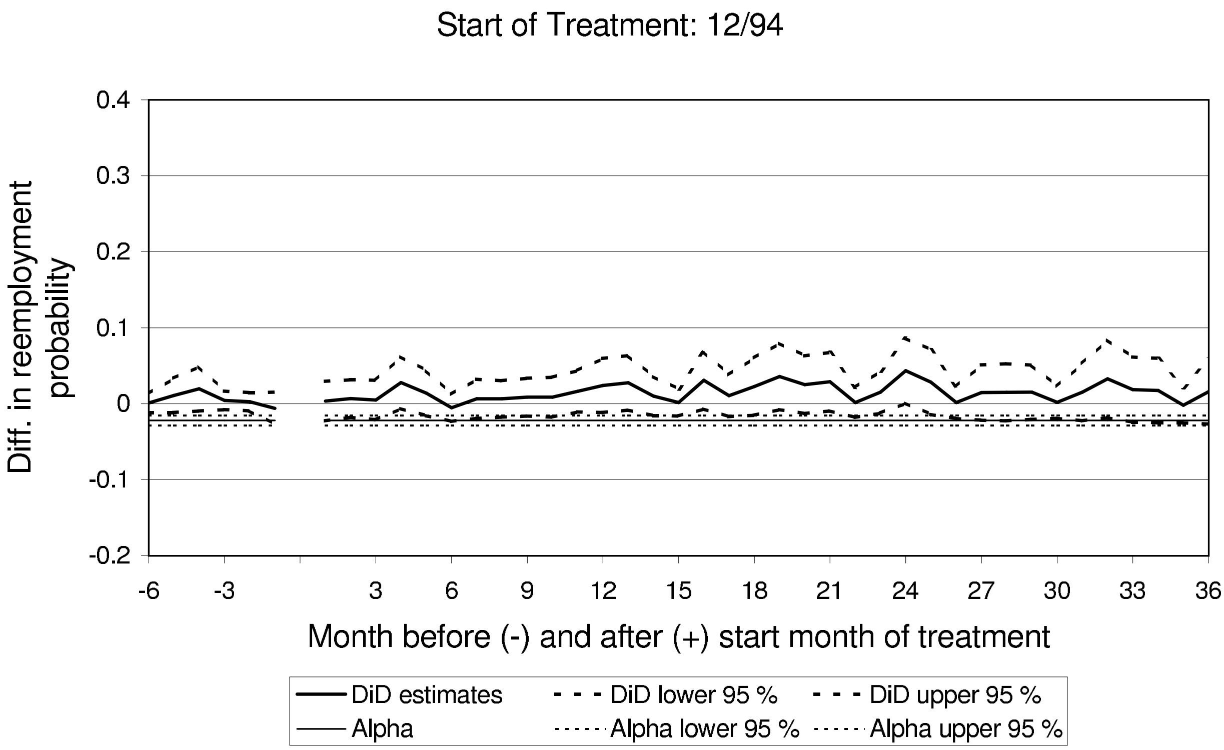


Figure B.6: Incremental Employment Effects of TR–TR – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Beginning of Treatment



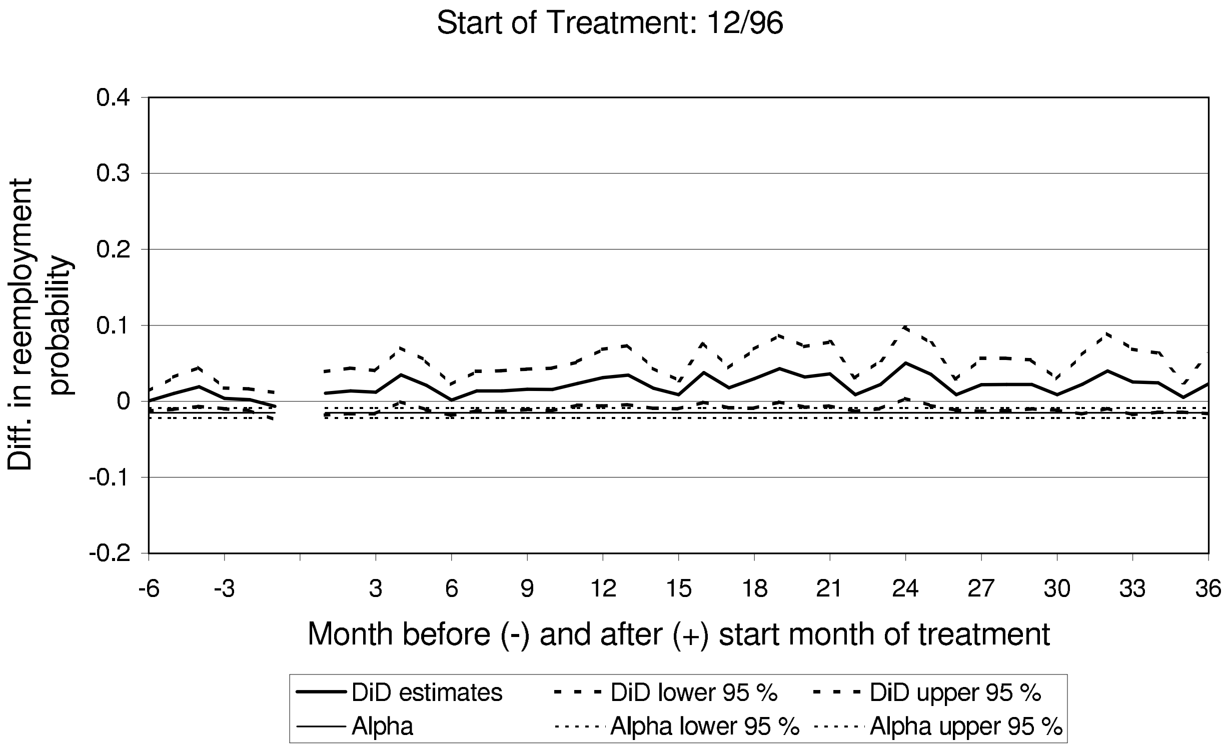
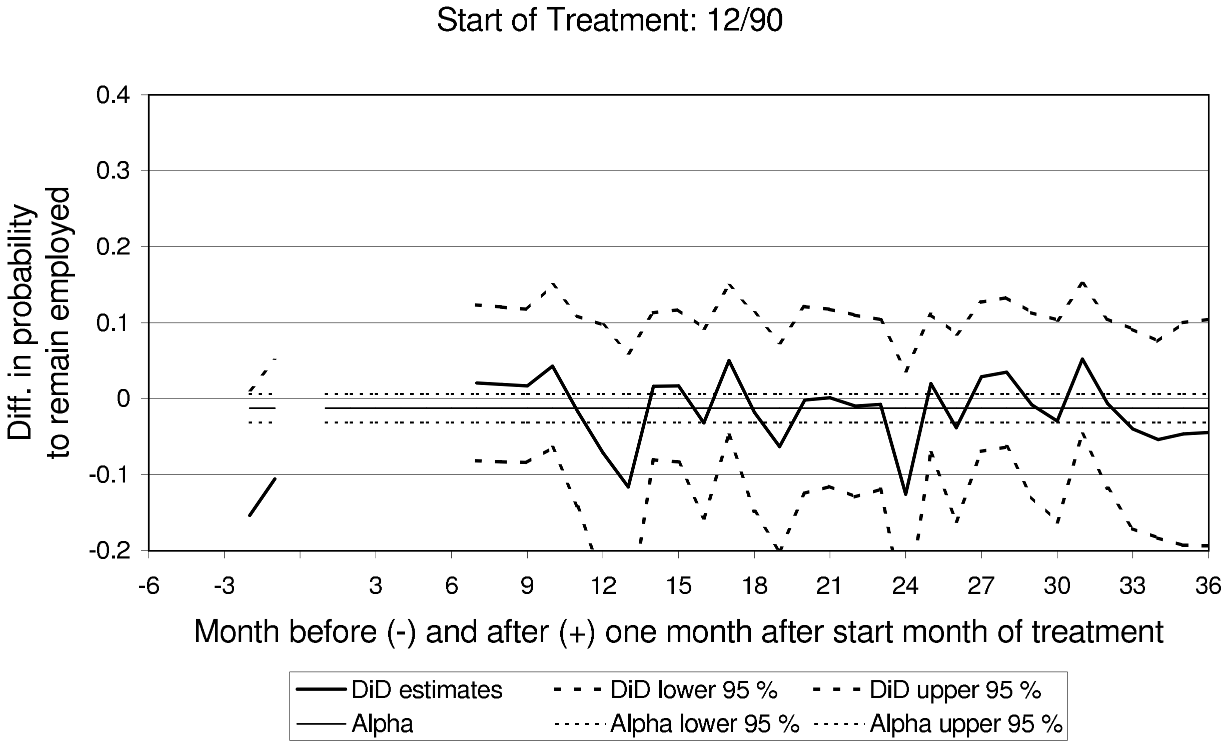


Figure B.7: Combined Employment Effects of TR–TR – CDiDHR – Employment in Previous Month – Evaluation Starts after Beginning of Treatment



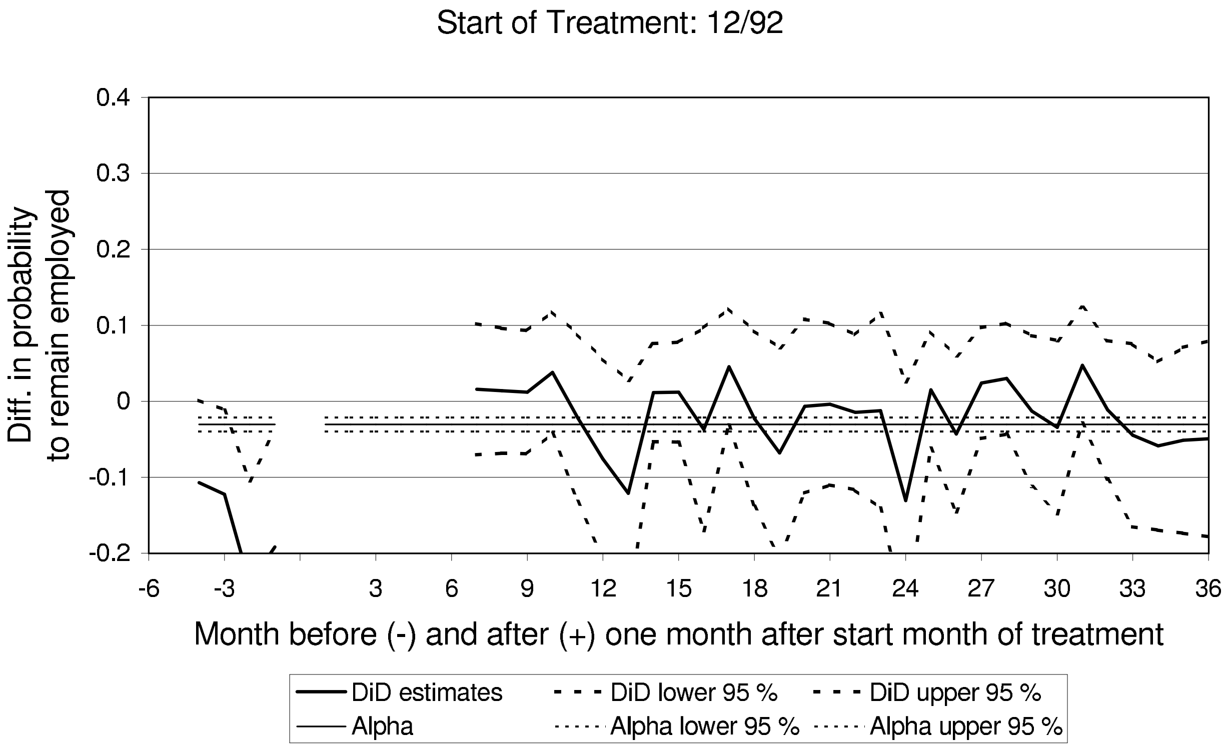
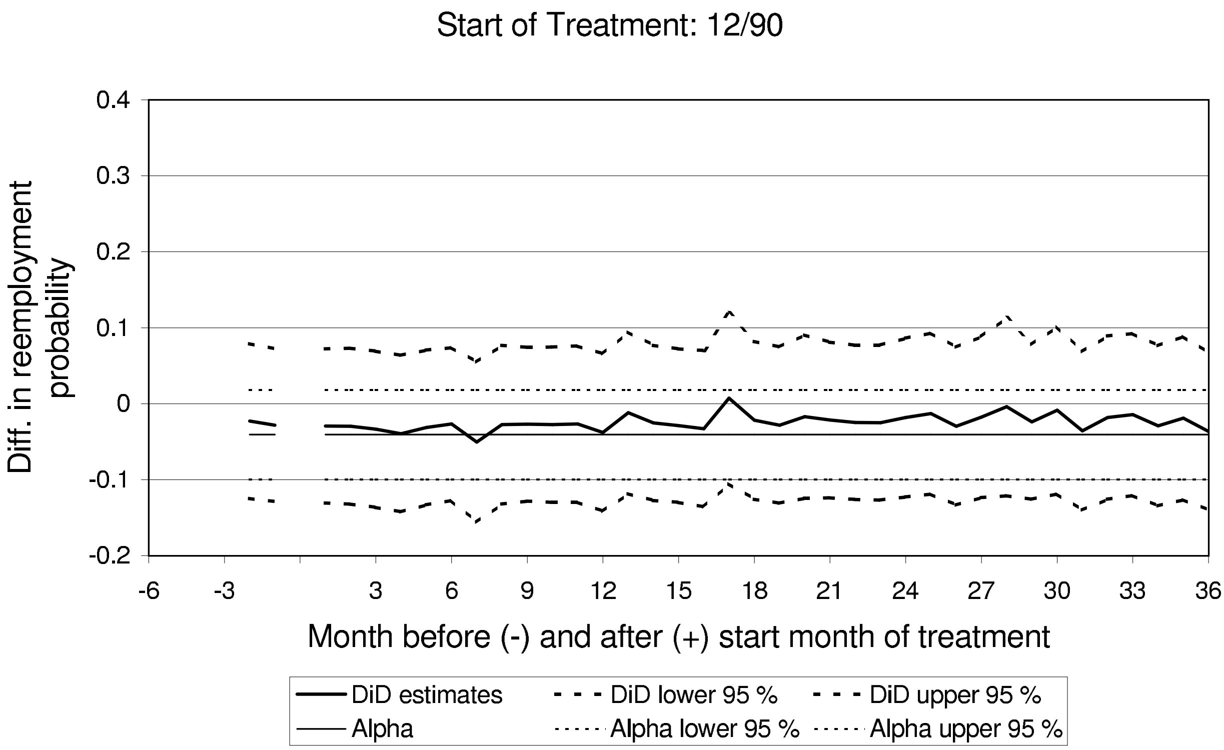


Figure B.8: Combined Employment Effects of TR–JC – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Beginning of Treatment



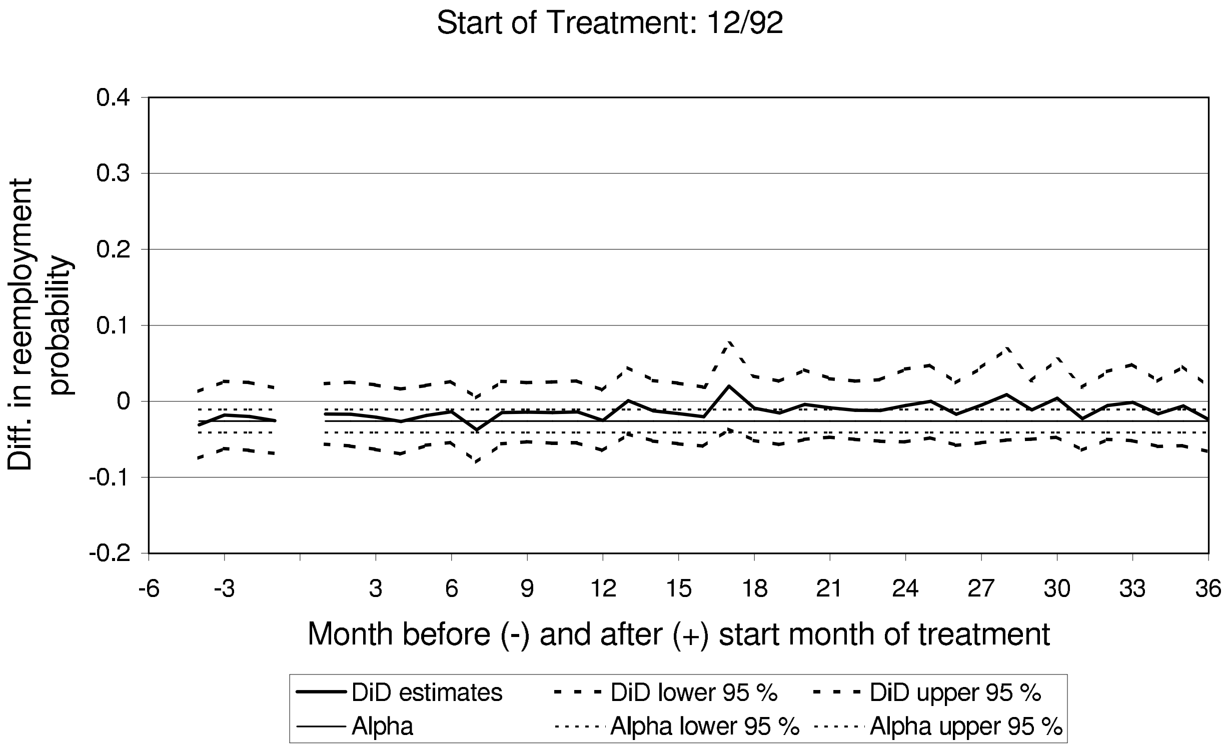
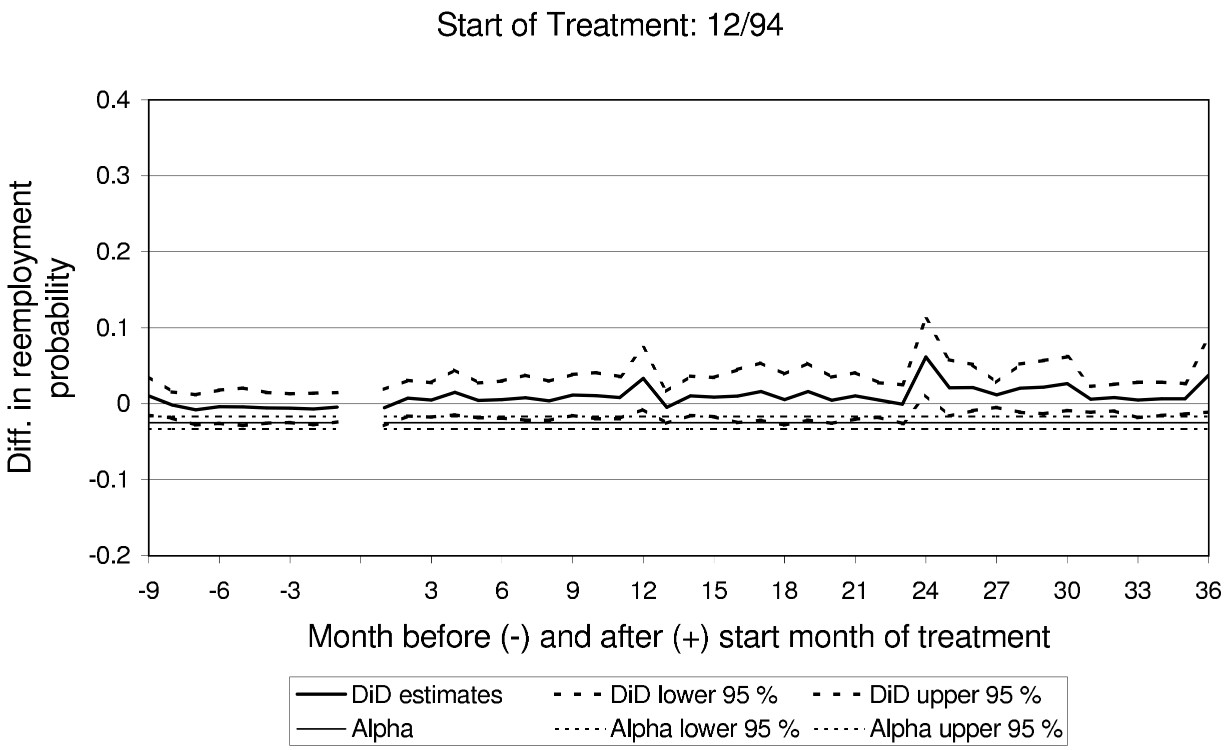


Figure B.9: Incremental Employment Effects of TR–JC – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Beginning of Treatment



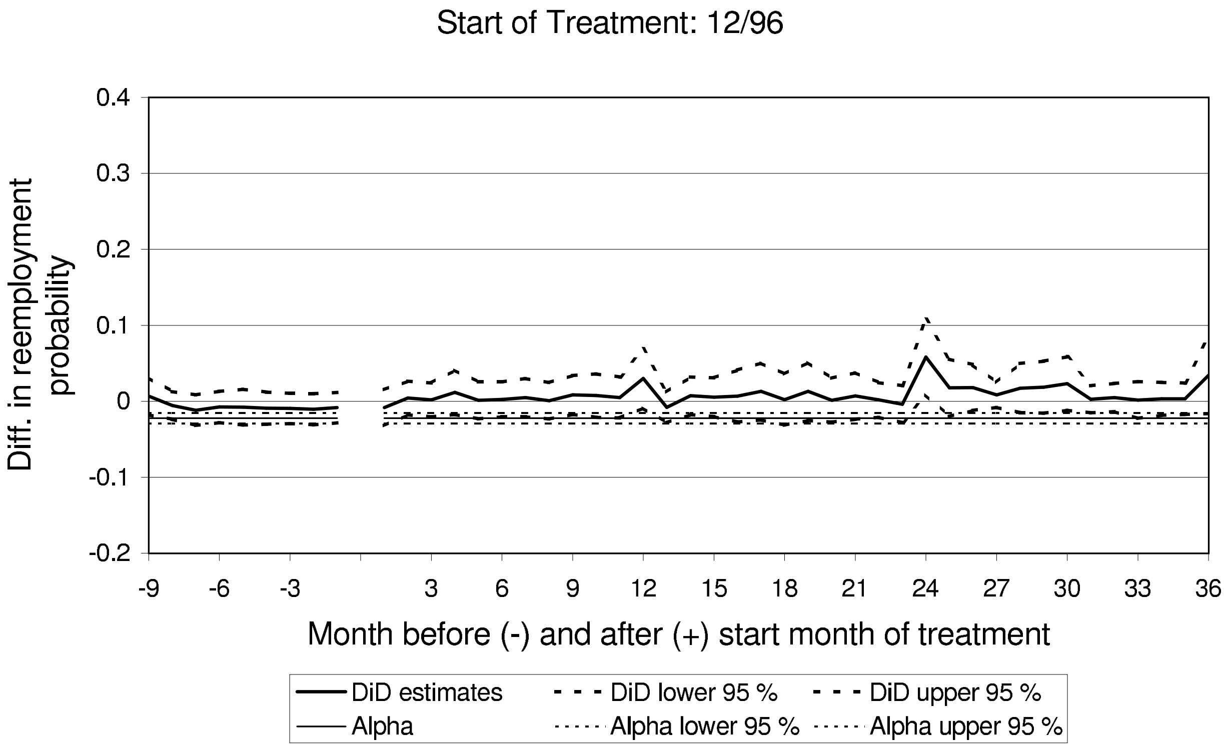
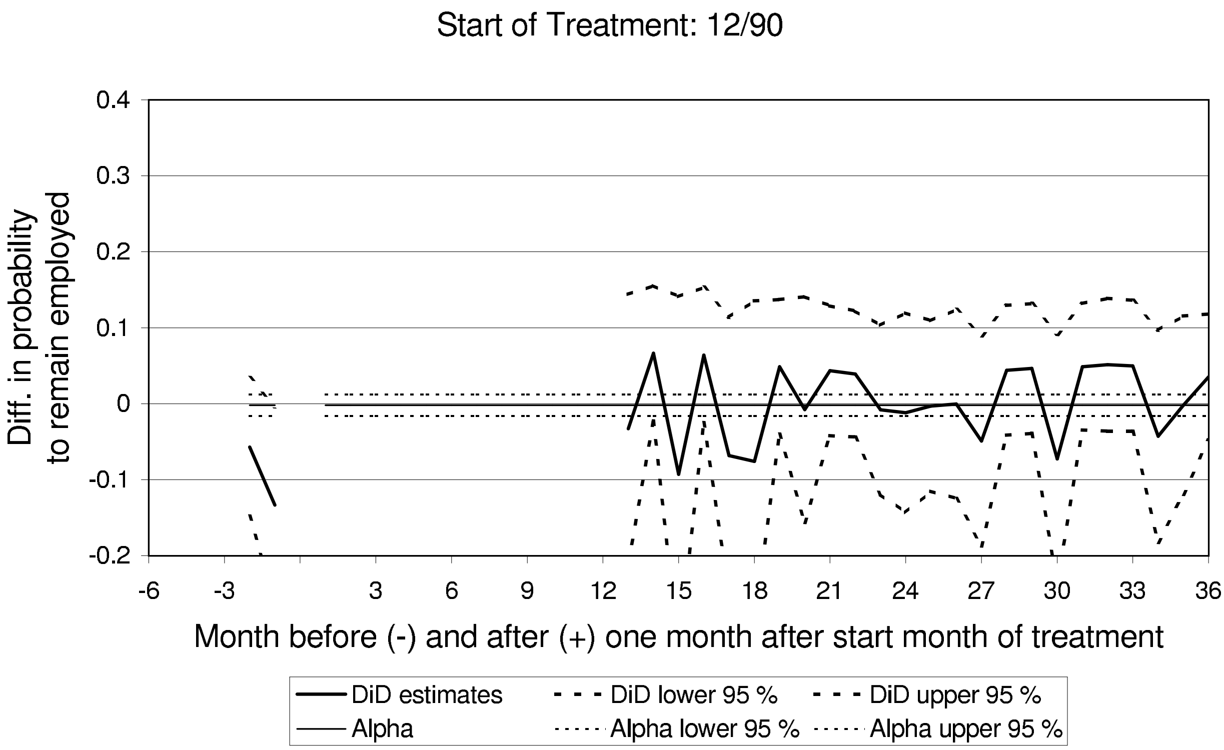


Figure B.10: Combined Employment Effects of TR–JC – CDiDHR – Employment in Previous Month – Evaluation Starts after Beginning of Treatment



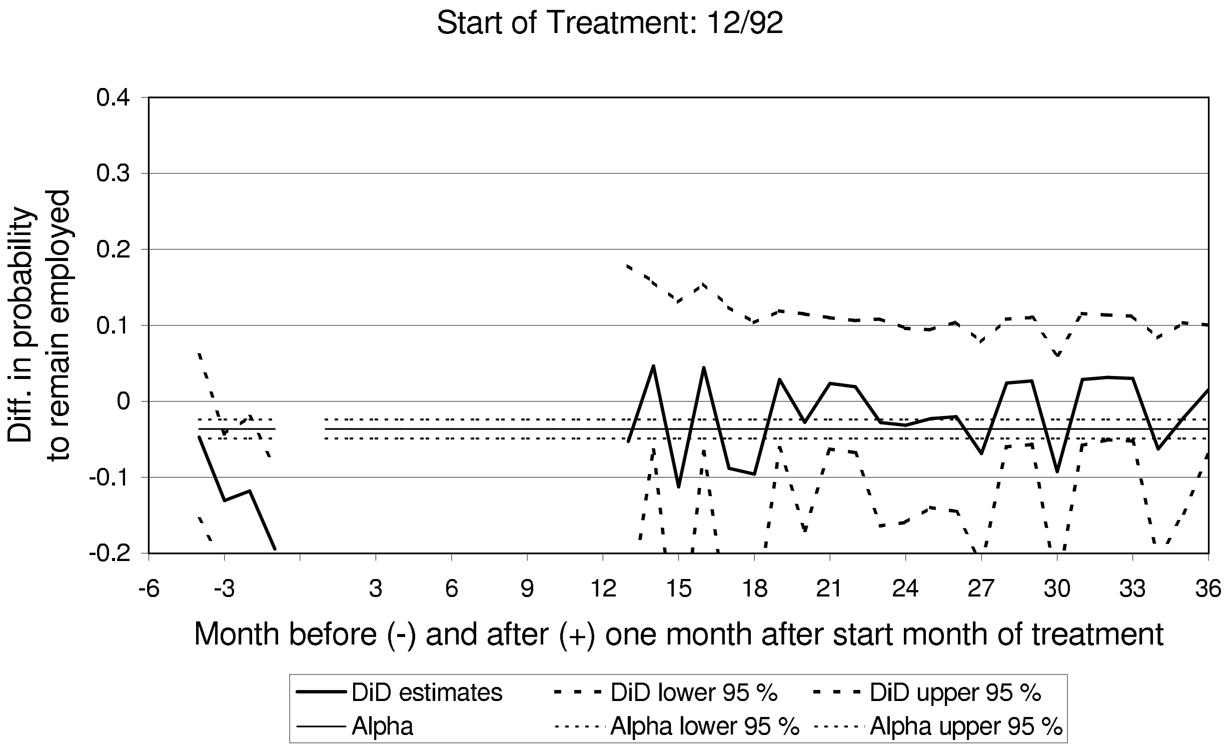


Figure B.11: Employment Effects of First Training – CDiDHR – Nonemployment in the Previous Month – Evaluation Starts after End of Treatment

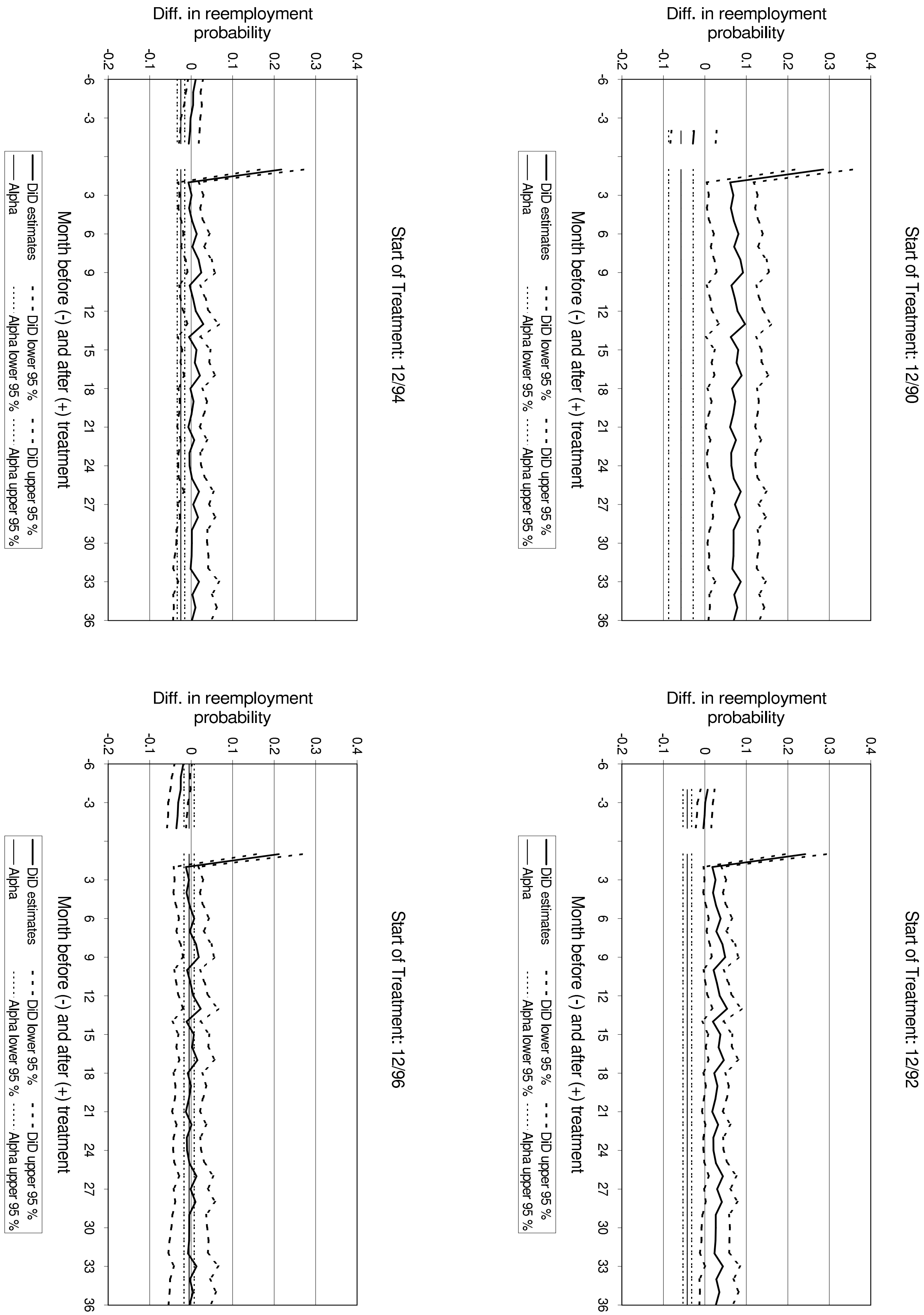


Figure B.12: Employment Effects of First Training – CDiDHR – Employment in the Previous Month – Evaluation Starts after End of Treatment