Essays on
Evaluation of Active Labour Market Policy

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General introduction

Over the last three decades, all major industrial societies have been plagued with high levels of structural unemployment. Germany – in a singular situation after the unification – was particularly hit by unemployment, although the government widely implements policies that should balance between supply and demand on the labour market with respect to regions, sectors and qualifications and improve the employment chances for the unemployed and those threatened by unemployment. These policies – mainly the provision of training and job creation to problem groups – are usually referred to as active labour market policy (ALMP) and have been extensively used from the mid 70s in West Germany and even more far–reaching in East Germany after the unification.

However, unemployment still is high and persistent. The seemingly apparent failure of the problem–solving capacities of social and governmental institutions in an era of tight government budgets demands for methods allowing to identify the causal effects of such policies on the labour market.

Certainly, the central question for social scientists as well as for policy makers is whether ALMP actually increases the employment chances of the people they seek to help. This doctoral dissertation consists of four stand–alone papers, each investigating some topics of particular importance to answer this question. The following paragraphs provide a short motivation for the main sections of the thesis and the related research questions that should be answered by this study.

Chapter 1 ("The Evaluation of active labour market policy in Germany: A survey") summarises the state of the art of ALMP evaluation in Germany as described under (i) – (iii) below. Chapter 2 ("Microeconomic evaluation of further training in East Germany based on observational data") analyses how far the choice of the evaluation methodology influences the estimated ALMP effects (iv – vii). Chapter 3 ("Using social insurance data for the evaluation of active labour market policy: Employment effects of further training for the unemployed in Germany") re–examines the effects of ALMP using reliable social insurance data instead of general household panel surveys. Chapter 4 analyses macroeconomic effects of ALMP in Germany and compares the outcomes with programmes in the United Kingdom ("The aggregate impact of active labour market policy in Germany and the UK: Evidence from administrative data")

(i) The institutional design of active labour market policy

At the beginning, this thesis provides an overview of the literature on evaluation of ALMP in Germany. First, this requires an in–depth description of the institutions of ALMP in Germany. We focus on the outcomes these policies intend to achieve with respect to the individual employment situation of the participants in the programmes. The most important insight of the description of the highly diversified policy field of ALMP is that it consists of a wide range of programmes for many different target groups. Some of these programmes should be regarded as functionally equivalent with respect to the integration target, and different institutional arrangements can in principal serve the same problem groups. The implementing body of these policies, the Federal Employment Service (Bundesanstalt für Arbeit) then can chose among several programmes. Other policies are complementary and allow the integration of the participants via different paths, either with training outside the labour market or by providing practical work experience. In practice, policies are combined or reiterated in order to achieve an integration target.

The description of ALMP in Germany points out that a credible evaluation of the employment effect for the lifelike variety of combinable treatments and programme sequences for specific target groups should be provided rather than focusing on effects of programme categories like "job creation" or "further training", which in themselves are too heterogeneous to deduce clear policy recommendations from.

However, the state of the art in evaluation research – both theoretically and empirically – still focuses on programmes. In the subsequent chapters, some attempts are carried out in order to estimate the ALMP effects when taking the institutional design of policies more seriously: ALMP in this study is evaluated by either considering the multiple treatment structure of ALMP (implemented in Chapter 2) or by evaluating explicitly the outcomes of treatments directed to specific problem groups (implemented in Chapter 3).

(ii) Methods to evaluate micro and macroeconomic outcomes of ALMP

Following the description of ALMP institutions, the thesis surveys the methods and the previous empirical evidence of ALMP effects on micro and macroeconomic outcomes in Germany. It is important to know that – with few exceptions– there does not exist experimental evidence for ALMP effects in Germany. Therefore, any empirical evaluation has to address difficult methodological issues in identifying the programme impact for the participants, because the estimation of the non–policy outcome – the outcome one has to contrast the observed policy effect to – remains hypothetical both at the micro and macroeconomic level. The parameters of interest of the hypothetical outcome are either 1) the outcome of an individual who has taken part in a programme if exactly this individual did not participate (microeconomic) or 2) a macroeconomic outcome in a situation without ALMP for a world where ALMP exists and depends exactly this labour market outcome itself.

The solution of the microeconomic evaluation problem can only be achieved by identifying assumptions because the situation of non–treatment is trivially not observable for the treated individual. Therefore, the methodological literature assumes various forms of conditional mean independence, claiming that the outcomes of non-participation in a programme do not differ between participants and non-participants as long as they show comparable observable characteristics. Then, an appropriate non–treatment outcome for a treated is just the most similar non–participant. Apart from observable characteristics, the methodological literature offers only vague recommendations, e.g. it is unclear how selection bias based on unobservable characteristics can be taken into consideration and how the dynamic reduction of the employment rate before treatment should be considered in the evaluation of outcomes (Chapter 2 of the thesis implements some approaches with respect to these issues). For macroeconomic evaluations, the reliability of estimated ALMP effects depends critically on instruments allowing to identify the exogenous variation of ALMP and to solve the endogeneity problem (with respect to this, chapter 4 suggests IV and panel estimators).

(iii) Previous empirical evidence

The first chapter reviews all studies of ALMP in Germany that explicitly pay attention to the difficult issue of identifying the policy effects. Most studies evaluate programme effects of job creation schemes or further vocational training. The previous empirical evidence shows rather a failure of the policies: Only very few studies find positive microeconomic employment effects of ALMP for certain subpopulations, which are not robust to specification issues. Most studies estimate insignificant or negative employment effects. Macroeconomic evaluations find mostly significantly negative employment effects of any of the programmes. In some studies, further training seems to decrease the regional rate of long–term unemployment, whereas job creation programmes show in some case significantly positive effects on the matching.

The description of the previous empirical evidence suggests that the different data sources and the huge differences in the methodological design of the studies might to a certain extent cause the unclear evidence. Besides, most studies usually apply data from panel surveys (such as the German Socioeconomic Panel), which however do not provide a link of the reported individual treatment information to understandable concepts of ALMP. Chapters 2 and 3 of this thesis therefore provide facts how sensitive estimated ALMP effects are with respect to 1) the choice of the evaluation approach, 2) the aggregation of first and reiterated treatments and 3) the underlying data. Certainly, these two chapters are the main contributions of this thesis. They provide new evidence for the microeconomic effectiveness of ALMP in Germany. Essentially, the following three features are analysed (iv – vi):

(iv) Sensitivity of the estimator

Most studies evaluating ALMP outcomes for the 90's implement either parametric evaluation approaches or non–parametric nearest neighbour matching in order to estimate the non–treatment outcome for the treated. In recent years, non–parametric matching approaches based on the propensity score gained importance because these estimators impose less structural assumptions than traditional econometric models, which usually use a parametric specification to account for the impact of observable characteristics and implicitly estimate the potential outcome in the non–treated state as the fitted value on the regression functional.

However, statistical matching estimators offer options among which the researcher has to choose, too, and which can be quite influential with respect to the outcome: The non–parametric estimation of the non–treatment outcome of the treated makes it necessary to decide upon the type of the estimator, the underlying probability distribution and the local area for which the non–treatment outcome is predicted. Chapter 2 shows how the choice of the evaluation approach and of critical parameters influences the estimated policy effect. We report the sensitivity of the estimated effects of treatment–on–the–treated if we vary 1) the local estimator (nearest neighbour, Nadaraya–Watson or local linear estimators), 2) the bandwidth and 3) take into account the sampling variability of the propensity score estimate which forms the basis for matching. Contrary to simulation studies, Chapter 2 brings about evidence for the robustness of estimates for real–world data if we critically consider the changes of these parameters.

As a result of this methodological exercise, we can conclude that the estimated effects are lower for evaluations of matched samples based on kernel regressions compared to the estimators in the case of nearest neighbour matching. The selection of the local area of the non–parametric estimators (the so–called "bandwidth") also affects the results, however – contrary to what the vast literature on the correct choice of the bandwidth in non–parametric estimators suggests – this is of minor importance in our application. The result of this exercise is to gain more knowledge how far the econometric specification influences the evaluation results – even if evaluation applies nonparametric approaches that impute less structure on the models than traditional econometric estimators.

The result of the application of the bootstrap procedure indicates that the error of the confidence intervals increase significantly if we control for estimation error of the propensity score prior to matching.

(v) Sensitivity in case of multiple treatments and if treatment effects vary over time

A second missing aspect in the empirical evaluation of ALMP originates from the fact that basically none of the previous studies distinguishes the effects of a first from those of a second treatment, and that treatments are assumed to exhibit the same outcomes in different years. In order to become sensible how far this aggregation of treatments influences the evaluation outcomes, we

- distinguish the effects of a first from those of a second treatment in further training and estimate the effects of both separately and
- allow the treatment effect to vary over time.

The results reported at the end of Chapter 2 indicate that neither the participation in a first nor in a second further training have positive effects for the participants. The participation in a first further training significantly decreases employment compared to non–participation. Participants in a second programme have an effect of treatment on the treated concerning employment, which is zero. Besides, the negative effects decrease over time.

(vi) Sensitivity of the treatment effect when using social insurance data instead of surveys

Practically all previous studies were based survey data of the German Socioeonomic Panel and the Labour Market Monitor for East Germany. These two panel surveys however suffer from severe shortcomings with respect to 1) the quality of the treatment information and to 2) the precision of the employment history before and after treatment. Furthermore, 3) mainly policies for East Germany are evaluated for the early 90's and 4) the evaluation results usually refer to very small samples sizes of the treatment group.

The conclusion of all these deficiencies of previous studies can only consist of using alternative data offering precise information about the employment history of the individual as well as treatment and providing a larger sample size the panel surveys. Therefore, chapter 3 provides an evaluation analysis of a specific type of further training based on integrated register data from the unemployment insurance. These data have been generated in a research project, to which the author of this thesis contributed important parts, and offer extensive and detailed information about the legal regulation under which the treatment was carried out. Using these data allows identifying clear–cut treatments with respect to the type of courses, the intended integration objectives and the contents of the courses.

However, social insurance data are not directly applicable to evaluation questions because the information provided does not correspond to any socioeconomic concept of work, treatment or unemployment. Consequently, an extensive recoding is implemented in order to identify informative treatment groups in the sample (first part of Chapter 3).

The evaluation then implements multiple procedures to overcome the microeconomic evaluation problem: It relies on a conditional independence assumption and restricts participants and non-participants to individuals experiencing the same employment history prior to treatment or non-treatment. Again, matching approaches are implemented. We extensively test whether the matching approach generates appropriate evaluation data.

Like in previous studies, ALMP outcomes are significantly negative immediately after the beginning of the treatment. Some months later, training seems to initiate a positive employment dynamic. However, the effect of the treatment remains negative or insignificant for long–time after the beginning of the treatment, when the training itself is supposed to have ended for the treated. These insignificant employment effects are found for different years and different target groups starting treatment after short–term, medium–term and long–term unemployment. And they are surprisingly similar for East and West Germany.

(vii) Evaluating macroeconomic outcomes

Finally, Chapter 4 provides some evidence for the aggregated outcomes of ALMP. Evaluation studies of the macroeconomic effects of ALMP can be seen as a complementary for the understanding of positive or negative microeconomic effects: If a positive microeconomic effect of treatment–on–the–treated suggests a positive outcome of the programme, macroeconomic evaluations could indicate whether this positive effect is partially counteracted by negative effects on the non–treated. In contrast, a negative microeconomic effect with respect to employment could exhibit positive outcomes on the economy as a whole because it could lower the aggregate wage pressure and then lead to higher employment on the macroeconomic level.

Empirical evaluations of macroeconomic effects of ALMP face identification problems because of the endogeneity of the ALMP: The political system answers to unemployment by allocation of ALMP, so that we observe the simultaneous occurrence of high levels of ALMP and high unemployment. Without the explicit control for the endogeneity of ALMP, the estimated policy effect would be biased. In order to estimate the macroeconomic effect of ALMP, the analysis either controls for endogeneity by instrumental variables with the intention of identifying the exogenous variation of ALMP irrespective the level of unemployment or by implementing dynamic panel models.

The macroeconomic analysis finds a reducing effect of further training on the extended unemployment rate in the short run in fixed effects models, which is however not confirmed by estimations which explicitly instrument for ALMP. The estimations of the dynamic panel models of the matching function shows that some specific job creation programmes improve matching in the short run, however, in the long–run, the effect is exactly zero.

1 The Evaluation of active labour market policy in Germany: A survey

1.1 Introduction

In 98, the Federal Employment Service (Bundesanstalt für Arbeit, BA) supports active labour market policy (ALMP) with total expenditure of 27.7 Billion DM (Bundesanstalt für Arbeit 1999: 23). Additionally, 10.9 Billion DM are spent for "other Programmes of ALMP" amounting to a total of 36.6 Billion. ALMP consists of a number of different job–creation and further training programmes for different target groups. Participants in the main programme areas (further training, job creation schemes and targeted wage subsidies for the employment of long–term unemployed) make up to 3.8% of the total labour force and are especially numerous in East Germany with a participation rate of 13.8% of the labour force (Bundesanstalt für Arbeit 1999). The dimension of the policy intervention into the labour market itself, but also the high and persistent level of unemployment especially in the East justifies a continuous evaluation of ALMP and demand for methods that allow identifying the causal effects of such policies on employment outcomes.

ALMP intends to reintegrate participants into regular employment. This integration however cannot be answered by the statistical effect of ALMP on the unemployment figures, but necessitates evaluating the policy effect on the basis of an adequate situation of "policy non–appearance", i.e. the hypothetical outcome of non–treatment either on the microeconomic or in the macroeconomic level. This "non–appearance of policy" can either be modelled by identifying (i) the outcomes of an individual who has taken part in a programme if this individual did not participate (microeconomic modelling of non–appearance of policy) or by (ii) modelling an exogenous allocation of ALMP in a world where ALMP allocation is actually endogenous and depends on the labour market outcome itself. Both strategies are assumed to provide the only satisfactory measure one can correspond the observed policy effect to.

In the following, we review the evaluation studies of ALMP in Germany that explicitly pay attention to the difficult issue of identifying the policy effects in this sense. In Germany, most studies evaluate the effects of (i) job creation schemes and (ii) the support of further vocational training on the micro and macroeconomic outcomes. These are also the most important programmes.

The following essay is subdivided into three main parts: In the second section, we recapitulate the catalogue of the different programmes of ALMP in Germany. Although the basic regulation of ALMP changed in 98, it is worth discussing the institutional framework according to the new regulation, because most evaluation results can be linked to the new regulation, too, since the principal programmes of ALMP remained in place. This section describes which policy outcomes are intended and how the different programmes should increase individual employment opportunities. The third section presents the methodological issues of the evaluation of ALMP. It is well known that the empirical evaluation of programmes has to address difficulties in identifying the programme impact for the participants based on non–experimental data: The situation of non–treatment is not observable for the treated individuals and the situation of non–treated individuals as well as the situation of the treated individuals before the participation in the programme do not provide an appropriate estimate for the non–treatment outcome. On the macroeconomic level, the evaluation of ALMP faces severe identification problems because of the endogeneity of ALMP caused by the response of the political system to unemployment by the allocation of ALMP. In macroeconomic studies, it is crucial to control for endogeneity because the simultaneous occurrence of high levels of ALMP and high unemployment could otherwise lead to biased estimates about policy effects.

The fourth part of the paper provides an overview of the scientific evaluation of ALMP in Germany on the micro and macroeconomic level. There is only little consensus in the microeconomic studies whether the correction of selection bias based on observable characteristics is sufficient or how far panel data could be of help to overcome remaining selection bias based on unobservable characteristics. Maybe due to the very different methodological strategies implemented in the evaluation studies of the last years, we do not find clear empirical evidence of the microeconomic effect of further training, which however is negative or insignificant in most cases. The macroeconomic studies, too, apply relatively dissimilar data and refer to different methods for the evaluation of the effects of training and job creation. In the field of macroeconomic evaluations, there have also been found mainly negative or insignificant employment outcomes. The last section offers a conclusion of the evidence found in the evaluation studies.

1.2 Active labour market policy in Germany

1.2.1 ALMP interventions

Active labour market policy is assumed to be a useful instrument in the search of a balance between supply and demand on the labour market with respect to regions, sectors and qualifications and shall improve employment chances for the unemployed and those threatened by unemployment. According to the ILO definition, ALMP is a selective intervention by the government in the pursuit of efficiency and/or equity objectives, acting indirectly or directly to provide work to, or increase the employability of people with certain disadvantages in the labour market. In a narrower sense (following the OECD), ALMP consists of five different policy areas. These are (i) Public employment service and administration, (ii) labour market training for unemployed and employed adults, (iii) youth measures, (iv) subsidised employment (temporary job creation measures) and (v) measures for the disabled (OECD 1993: 39 ff.). In accordance to this, ALMP only includes government–financed services and programmes. It does not comprise programmes in the private sector except if they are publicly financed (i.e. no policies based on collective agreements). ALMP only consists of selective public interventions for the benefit of *special categories of individuals* and not general employment policy such as changes in the taxation and social security contributions for certain groups and explicitly excludes policies of the temporary or permanent reduction of workforce for employment security of other groups (i.e. early retirement for labour market reasons or short–time work allowance) and special industrial policies (e.g. the employment maintenance by general industrial subsidies as in mining in Germany) (cf. OECD 1993: 39 ff.).

Applying this definition to the German situation, ALMP are predominantly policies of the Federal Employment Service (Bundesanstalt für Arbeit, BA) under the regulatory framework of the Sozialgesetzbuch III (Social law book III, SGB III). Other programmes of ALMP exist at the level of local municipalities, of the German Länder and at the European level. These policies however co–finance in most cases ALMP of the BA. In municipal ALMP, programmes that integrate recipients of social assistance into regular employment are predominant: Since the cities carry the financial burden of social assistance, they seek to reintegrate these individuals into employment subject to mandatory social insurance payments by creating temporary public sector jobs, so that the participants re–qualify for unemployment benefits and leave to social assistance register. In 98, these programmes offered temporary employment for over 200,000 individuals and entailed a financial volume of ten billion DM (Deutscher Städtetag 1999). However, the regulations of the local employment promotion programmes differ widely and are hardly comparable, so that we focus on country–wide programmes according to the SGB III regulation in the following.

1.2.2 Regulation of ALMP

In 98, the basic regulation on ALMP in Germany, the former Labour Promotion Act (Arbeitsförderungsgesetz, AFG), was replaced by the SGB III. For the general targets as well as for the organisation and implementation of ALMP in Germany, this reform had far reaching consequences, which are briefly described in the following. The programmes of the former AFG however remained in place (see section 1.2.4 below), and we decided to discuss the empirical evidence of the following sections with respect to the current regulation.

The aims of the former AFG were formulated under good economic conditions, full employment and labour shortage and they were embedded in an institutional framework of both economic and social policy. When the AFG was introduced in 69, two other important regulations were simultaneously launched and reinforced the importance of ALMP in the area of economic policy: The Law on stability and growth (StWG) and the Law on vocational training (BBiG) were introduced as complementary to the AFG. With respect to this motivation, the first paragraph of the AFG stressed the role of ALMP for the allocation of workforce (AFG, § 1 and 2), the optimisation and adjustment of qualifications and that a "lack of qualified workforce should be prevented". Thus, ALMP was intended to equalise between demand and supply of workforce and between cross–regional imbalances. It should promote structural change and increase the productivity of the workforce.

With the changing labour market situation in Germany since the mid 70's, the AFG was being revised by 115 amendments until 97 taking into consideration the labour market situation after the first oil crisis when structural change, persisting unemployment, increased female participation in the labour market and new financial constraints of the unemployment insurance forced the legislator to restrict ALMP to problem groups.

A complete revision of the AFG regulation became inevitable after the German unification when an ongoing labour market intervention for a long period and an extended group of participants was regarded as an appropriate instrument for absorbing the shocks on the labour market after the breakdown of the GDR economy. The widespread implementation of subsidised employment indicated the end of ALMP as a "growth policy" and stressed the role of ALMP as social policy for groups particularly affected by unemployment. The federal legislator adapted ALMP to these new labour market conditions: With the beginning of 98, the new SGB III was implemented.

The SGB III defines the objectives of ALMP as the integration of disadvantaged groups and the improvement of the labour market situation of these target groups by increasing their placement potential with advice, training measures, and special subsidies for professional integration or business start ups. The instruments of ALMP in Germany are now "clearly more subsidiary" (Sell 1998: 545). Furthermore, the SGB III accentuates more the principle of an insurance, and underlines that the promotion of employment opportunities for disadvantaged groups primarily aims at the reduction of income maintenance payments (SGB III, §§ 1).

1.2.3 Institutional change

The introduction of the SGB III significantly modifies the allocation of ALMP by means of organisational and financial reforms of the internal structure of the BA[1]. These institutional changes are briefly discussed in this section.

With the reform of the organisational structure, integration of formerly separate budgets for the support of further training and job creation programmes was introduced at the level of the local employment offices. Those were no longer forced to implement a specific quantity of training or job creation and were enabled to implement programmes according to local requirements. The formerly homogeneous labour market policy designs on the disaggregated level gave way to an ALMP characterised by more diversity in the design and the local implementation. Additional to the flexible shift between the different schemes of the SGB III, the new regulation allows that 10% of the regional budgets of ALMP can be used for an "experimental" ALMP on the regional level (§ 10, SGB III). With these funds the local employment offices can supplement ALMP in order to establish regional specific solutions for individuals and special problem groups (Sell 1998: 541).

However, the introduction of the SGB III shows only small changes the programmes themselves: in general, the programmes of the former AFG[2] remained in place. Nevertheless, two important changes in the instruments indicate the new role of ALMP measures: (i) The introduction of the "integration plan" strengthens the position of counselling and guidance in ALMP: For special problem groups, the SGB III introduces early and short–term training programmes for their job–search activity. (ii) A new "integration contract" offers a temporary subsidy to employers for the creation of new employment probabilities with a special emphasis on training on the job.

To summarise, with the introduction of the SGB III, only few changes in the design of instruments were implemented, but the local employment offices gained a wider flexibility in planning and implementation of the programmes. This is supposed to cause a wide regional variation in outcomes in future.

[1] Changes in the regulation of passive labour market policy, especially the income maintenance function of the unemployment compensation, are not discussed here (for a summary, see Sell 1998).

[2] A detailed description of the instruments follows in the next part.

1.2.4 ALMP programmes

ALMP in Germany consists of three main policy areas, of which the most important is the integration or re–integration of problem groups by the support of individual vocational and further training.[3] The second policy is the creation of temporary or permanent employment opportunities with a broad variety of wage–cost subsidies[4]. The third area is grants for occupational or regional mobility.[5] The most important programmes of these areas are described in the following section[6] and are summarised in Table 1.1.

1.2.4.1 Training

In Germany, most labour market entrants pass through the cooperative dual system of first vocational training, so that youth employment programmes are of minor importance in Germany compared to many other countries, except to some extend in East Germany. Thus *Support for vocational training* makes up only a small quantity of ALMP and basically consists of an allowance for trainees who are not living with their parents.

Among the ALMP programmes for adults, *further training* is the most important. It aims at the integration of unemployed persons and those at risk of becoming unemployed by providing recognised vocational qualifications. It consists of measures for individuals who completed first vocational training and aims at the assessment, maintenance, extension or adaptation of vocational skills to technical developments or changing employment opportunities. Participants may be granted an "income maintenance payment" (Unterhaltsgeld) if they have been previously in employment, which was subject to social contributions for a minimum length during a set period of time or if they have received unemployment benefits or assistance. Under certain conditions, these payments can be extended to persons who return to the labour market. The income maintenance payment is

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[3] This target area contains the following programs: Vocational training (Förderung der beruflichen Ausbildung, SGB III: §§ 59–76), further vocational training (Förderung der beruflichen Weiterbildung, SGB III: §§ 77–96 and §§ 153–159), support for training institutions (Institutionelle Förderung der beruflichen Bildung, SGB III: §§ 248–251), preparatory vocational training measures for young people (Förderung berufsvorbereitender Bildungsmaßnahmen für Jugendliche, SGB III: §§ 59 ff.), vocational training for those with learning difficulties and trainees at a social disadvantage (Förderung der Berufsausbildung von lernbeeinträchtigten oder sozial benachteiligten Auszubildenden, SGB III: § 235 and §§ 240 ff.), vocational rehabilitation (Berufliche Rehabilitation, SGB III: §§ 97–99, §§ 236–239 and §§ 248–251) and the improvement of the prospects of integration by training (Maßnahmen zur Verbesserung der Eingliederungsaussichten [Trainingsmaßnahmen], §§48 – 51 SGB III)

[4] More detailed, these are job creation measures (Förderung von Arbeitsbeschaffungsmaßnahmen [ABM], SGB III: §§ 260–271, § 416), structural adjustment measures (Förderung von Strukturanpassungsmaßnahmen, SGB III: §§ 272–279, § 415.), integration subsidies (Eingliederungszuschüsse SGB III: § 218), recruitment subsidies for businesses start–ups (Einstellungszuschuß bei Neugründungen, SGB III: § 225) and integration contracts (Eingliederungsvertrag, SGB III: §§ 229–233).

[5] These programmes do not strictly match to the definition of ALMP as described under section 1.2.1, but can be understood either as a special category of wage–subsidies or as a part of the placement activity of the employment service. Here, we summarise the programmes bridging allowance (Übergangsgeld, §§ 57–58 SGB III) and mobility allowance (Mobilitätshilfen, §§ 53–55 SGB III)

[6] Placement services and counselling are also areas under the definition of ALMP, but are not taken into consideration in this paper. They are available to the whole active labour force and not only to specific target groups. To our knowledge, these services have so far not been subjected to any scientific evaluation of the type discussed here.

equal to unemployment allowance, i.e. 60–67% of the previous net wage. The training courses are usually carried out by private training centres, which offer programmes for specified target groups in accordance to the requirements of the local employment offices. The selection of appropriate participants among the unemployed lies in the responsibility of the local employment office. The duration of training varies between 3 and 8 months for further training and up to 24 months for re–training.

The programme *improving the prospects of integration* supports short–term training courses or practical activities that improve the prospects of unemployed workers for integration by the assessment of the suitability of the unemployed person for employment or training. They can include job–application training, counselling on job– search possibilities or treatment, which investigate the unemployed person's willingness and ability to work and are intended to promote the individual job–search activity.

1.2.4.2 Subsidised employment

The second main group of ALMP instruments summarises targeted wage subsidies. There are numerous programmes promoting employment opportunities for hard–to–place both in temporary, additional jobs and by subsidising permanent employment contracts. Especially programmes aiming at the integration in regular employment can often be considered as equivalent, with respect to the integration purpose: Irrespective under which regulation the treatment is carried out, any of these programmes aim at the integration of the same target group with basically the same programme design.

The most important programme is *Job creation measures (Arbeitsbeschaffungsmaßnahmen, JC).* JC aim at the creation of temporary employment for long–term unemployed (>12 months) in projects, which "have to benefit the community" and "must be additional", meaning that they would not have carried out without the subsidy. In general, JC is a co–financed programme, in which between 30% and 90% of the whole wage sum of the participant (i.e. the gross wages plus the employers' shares of the social insurance contributions) is subsidised by the BA. The implementing institutions – public or private legal entities – incur further (e.g. material) costs. ABM gives priority to projects that considerably improve the chances for permanent jobs, that support structural improvement in social or environmental services or that aim at the integration of extremely hard–to–place individuals. The wages paid to the participants must not exceed 80% of a comparable unsubsidised job. The duration of JCs is in most cases restricted to 1 year, but can be extended up to 36 months if permanent employment is offered subsequently.

Table 1.1 Main ALMP programmes in Germany

Youth

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 98	Costs, 98
Support for vocational training	Vocational training allowance	§§ 59–76	Trainees not living in the parental home	▪ Refund of course fees and travelling expenses ▪ Vocational training allowance (optional)	36 months (max.)	Total: 165,100 East: 61,200 West: 103,900	Total: 1.043bn DM East: 313m DM West: 730m DM

Training

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 98	Costs, 98
a) Further training b) Re–training	Improving qualification of unemployed	§§ 77–96; §§ 153–156	▪ Unemployed with "training necessity" ▪ Re–entrants from inactivity	▪ Course fees ▪ Income maintenance payment for participants (equal to unemployment benefit) ▪ Costs for accommodation and child care (if necessary)	a) 2 to 8 months b) 24 months	Total: 607970 East: 235959 West: 372011	Total: 12.505 bn DM East: 5.468 bn DM West: 7.038 bn DM
Improving prospects of integration	Improvement of job search	§§ 48–51	Unemployed	▪ Course fees ▪ Costs of accommodation and child care (if necessary)	½ to 2 months	n.a.	n.a.

Mobility

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 98	Costs, 98
Mobility allowance	Financial assistance for entry in regular employment	§§ 53–55	Unemployed	Double household or mobility allowance (up to 500 DM monthly) or relocation subsidy	6 months	n.a.	Total: 51.3m DM East: 24.6m DM West: 26.7m DM
Bridging allowance	Financial assistance for entry in self–employment	§§ 57–58	Unemployed	Income maintenance equal to unemployment benefit	6 months	Total: 97,800 East: 31,600 West: 66,200	Total: 1.248bn DM East: 362.4m DM West: 885.6m DM

Table 1.1 Main ALMP programmes in Germany (cont.)

Subsidised Employment

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 98	Costs, 98
Job creation measures (ABM)	Temporary integration in "additional" employment in the public interest	§§ 260–217; § 416	Long–term unemployed	▪ Wage cost subsidy (30–90% of the wage sum) ▪ Wages paid must not exceed 80% of equal unsubsidised employment ▪ Financial support for institutions	▪ 12 to 24 months ▪ 36 months (if creation of permanent employment follows)	Total: 366,555 East: 271,768 West: 94,787	Total: 7.424bn DM East: 5.453bn DM West: 1.971bn DM
Structural adjustment measures (SAM)	Temporary integration in "additional" employment improving social service and the environment	§§ 272–279; § 415	▪ Long–term unemployed ▪ Unemployed ▪ Persons at risk of becoming unemployed	▪ Wage cost subsidy (equivalent to individual unemployment benefit) ▪ Social insurance contributions ▪ Wages < 80% of equal unsubsidised job	▪ 36 months ▪ 48 months (if creation of permanent employment follows)	Total: 272,178 East: 257,919 West: 14,259	Total: 4.593bn DM East: 4.277bn DM West: 316m DM
Integration subsidies	Permanent integration in regular employment	§§ 217–224	▪ Unemployed and re–entrants if familiarisation required (group a) ▪ Long–term unemployed (b) ▪ Aged at least 55 (c)	Wage cost subsidy ▪ 30% (groups a and b) ▪ 50% (group c, in certain cases b) of wage costs	▪ 12 –24 months (groups a and b) ▪ 36 months (group c)	Total: 70,900 East: 21,600 West: 49,300	Total: 1.142bn DM East: 291.9m DM West: 850.3m DM
Recruitment subsidies for business start–ups	Permanent integration in start–up firms	§§ 225–228	▪ Unemployed ▪ Participants in ALMP measures	Wage cost subsidy 50% of wage costs	12 months	Total: 9,400 East: 2,200 West: 7,200	Total: 171.9m DM East: 37.1m DM West: 134.8m DM
Integration contracts	Permanent integration in regular employment	§§ 229–234	▪ Short–term unemployed with placement problems ▪ Long–term unemployed	Wage cost subsidy (only for periods with training and internal qualification) 100% of wage costs	6 months	Total: 2,800	Total: 4.75m DM
Employment assistance for long–term unemployed	Permanent integration in regular employment	(Federal law)	LTU of duration ▪ 1–2 years (group a) ▪ 2–3 years (group b) ▪ > 3 years (group c)	Wage cost subsidy ▪ 50% (group a) ▪ 60% (group b) ▪ 70% (group c)	12 months	Total: 66,600 East: 17,300 West: 49,300	Total: 878.7m DM East: 194.4m DM West: 684.3m DM

The *structural adjustment measures* aiming at the temporary re–integration of long–term unemployed have less strict eligibility criteria, which are applied to participants, e.g. participation is also possible for individuals starting treatment directly from employment. Nevertheless, priority is given to individuals that cannot be placed in regular employment without subsidies in foreseeable future, i.e. long–term unemployed. The wage cost subsidy is a flat rate equal to the amount of unemployment allowance or assistance the individual would have received if unemployment had continued. Temporary employment is supported by means of this programme mainly if the projects conserve or improve the environment or provide social services. The implementing institutions, public institutions or private companies, pay the remaining personnel and material costs. As for ABM job creations, wages for participants in this programme must not exceed 80% of equal unsubsidised employment. The subsidy is paid for 36 months and can be extended up to 48 months if the participants are regularly employed after the end of the programme.

Integration subsidies serve the same target of integrating long–term unemployed and labour market entrants. This scheme works as follows: Employers receive wage subsidies to compensate for lower performance of the eligible persons for a certain period, in which participants: a) become familiar with the work situation, b) have special integration requirements due to disability or personal circumstances or c) need longer integration periods because of an age above 55 years. The amount and duration of the subsidies depend on the extent to which the employee's performance is reduced and on the individual familiarisation requirements. The subsidy varies between 30% and 50% of the wage costs (i.e. the wages and additionally the employers' share of the social insurance contributions) for a period between 12 and 36 months. Employers have to provide regular employment to participants for at least 12 months after the programme expiration.

Almost the same eligibility criteria are applied in the programme offering *recruitment subsidies for business start–ups*, aiming at the permanent integration of unemployed in the regular labour market. In this programme, the wage–cost subsidy of 50% of the total wage costs is limited to 12 months and regular employment of at least 12 months must follow the programme.

The *integration contract* – another programme for the re–integration of long–term unemployed – offers employers a 100% wage–subsidy for the time a participant needs to qualify for the occupation in training institutions outside the firm or to get familiarised with the work environment. Employers who benefit from integration contracts commit themselves to providing unemployed workers the access to qualification. The integration contract is limited to a maximum of six months.

The fourth wage–cost subsidy scheme aiming at the integration of long–term unemployed into regular employment is the programme *employment assistance for long–term unemployed*. Here, the employer receive a wage–cost subsidy if he offers permanent employment to an employee who has been registered as unemployed for at least one year immediately prior to being recruited. The duration of the programme is limited to a maximum of 12 months, and the level of the subsidy depends on the individual unemployment duration before employment (between 50% and 70% of the wage costs).

1.2.4.3 Mobility

Mobility allowances are traditionally of minor importance in Germany. There are two programmes supporting regional or occupational mobility:

Regional Mobility allowance offers financial aid for the entry into contributory employment: a) A bridging subsistence loan can be given to individuals until payment of the first wage of 80% of the expected first wage, b) an equipment allowance of up to DM 500 for work, clothes and tools or c) an allowance for travelling between the recipient's home and the place of work in another district or alternatively a relocation loan within 2 years of entering employment.

The *bridging allowance* aims at the support of self–employment by subsidising formerly unemployed who become self–employed: The payment can last up to 26 weeks and amounts to the unemployment benefit or assistance, which the claimant had previously received or could have received.

1.2.5 Expenditure and participation

Table 1.2 shows the expenditure on ALMP for the years 95–8. Due to the changes in legislation, the 98 figures are not fully comparable with the figures referring to earlier years and we recalculated subtotals according to the former regulation. One can observe a stable expenditure development for ALMP over the last years. After a tightening of budgets for training and employment promotion in 97, ALMP again grew in 98. The overall budget declined by almost four billion DM, however ALMP gained importance.

Table 1.2 Expenditure of the German Federal Employment Service, 95–8[7]

Expenditure in thousand DM	95	96	97	98
1. Training and Re–Integration*	22,937,007	24,959,039	21,860,748	22,805,854
2. Measures for Employment Promotion (incl. short–time work allowance and early retirement schemes)**	14,794,891	13,110,094	10,720,878	12,237,971
3. Income Maintenance for Building and Civil Engineering***	1,585,160	902,674	443,233	471,209
4. Income Maintenance	49,895,084	57,959,263	61,505,145	54,881,647
5. Staff, Equipment, Investments, IT of the Organisation	7,577,327	8,176,006	7,445,872	8,452,025
6. Other Expenditure	313,608	480,716	747,296	N/A,
Total Expenditure	97,103,081	105,587,795	102,723,175	98,848,705

Source: Bundesanstalt für Arbeit, Geschäftsbericht 1995, 1996, 1997, 1998 modified subtotals

The participation figures in ALMP are shown in table 1.3. Considerable changes occurred over the last six years in both East and West Germany: Inflows into job creation and structural adjustment measures in the public sector ("traditional SAM") vary between 0.2% and 0.36% of the total civilian labour force between 93 and 98 in West and 2.5% and 4.4% in East Germany. There are two peaks in 94 and 98, indicating that the participation and the extend of the programme depended on the election to the federal parliament. In total, about 1% of the total civilian labour force start an JC of the ABM programme or a traditional SAM each year and except for the two peak years. The programme size remains relatively stable. The inflows into programmes aiming at the integration of hard–to–place individuals into the regular labour market gained importance over the last years. This category, including integration subsidies, recruitment subsidies for business start–ups, integration contracts and the employment assistance for long–term unemployed as well as special SAMs for the private sector, comprises about 0.44% of the total civilian labour force in West Germany and 3.38% in East Germany in 98. We see a significant increase in the use of this incentive scheme: In 98, five times more participants started regular employment with temporary wage subsides than in 93 in both East and West Germany.

Further training, the biggest ALMP programme, shows participation figures similar to the ABM programme (1.17% of total civilian labour force in 98). In the West, further training involves twice as many participants compared to ABM and traditional SAM. In East Germany this is just opposite:

7 As the BA changed the accounting for expenditure after 98 due to the implementation of the SGB III, we had to relocate the expenditure of the old annual report systematic to ensure comparability.
The results are differently in the sum as the annual report contains a mistake in the accounting of the expenditure for ALMP under its position IV 1. where a total of 24,666,585.976 thousand DM is reported, but where the correct sum would be 24,663,585.976 thousand DM.

* According to the old systematic, we here subsume Unterstützung, Trainingsmaßnahmen, Mobilität, Arbeitnehmerhilfe, Unterhaltsgeld, Maßnahmekosten, Einstellungszuschüsse, Eingliederungsvertrag, Benachteiligte Auszubildende, Sozialplan, Jugendwohnheime, Reha Ersteinstellung, Reha, Wiedereinstellung, Freie Förderung, Berufsausbildungsbeihilfe, Anschlußunterhaltsgeld, Förderung selbständiger Tätigkeit, Eingliederung bei Berufs, Institutionelle Förderung, ESF, Sonstige Ausgaben Kap. 3

** Eingliederungszuschüsse, ABM, Kurzarbeitergeld, Altersteilzeit AN/AG, Strukturanpassungsmaßnahmen

***Wintergeld, Winterausfallgeld, SV–Zuschüsse für umlagefinanziertes WAG

After a decline from 3.87% to 2.42% between 93 and 98, we see that the first and extensive use of further training after unification ended.

Overall, we see a declining use of further training among the German ALMP, no clear trend in the usage of "classical" ABM programme with temporary employment opportunities and an increasing implementation of programmes that offer a temporary wage–subsidies for permanent jobs.

Table 1.3 Participation entries in ALMP as percentage of total civilian labour force

		93	94	95	96	97	98
Job–creation measures (ABMs) and traditional structural adjustment measures (SAM)	West	0.20	0.31	0.30	0.31	0.26	0.35
	East	4.12	4.74	3.74	3.80	2.54	4.39
	Total	0.97	1.17	0.97	0.99	0.71	1.15
Regular employees receiving ALMP subsidies*	West	0.09	0.09	0.20	0.16	0.21	0.44
	East	0.68	0.53	0.84	0.60	1.46	3.38
	Total	0.20	0.18	0.32	0.25	0.46	1.02
Further training	West	1.12	0.98	1.30	1.23	0.89	0.86
	East	3.87	3.82	3.44	3.62	2.20	2.42
	Total	1.66	1.53	1.72	1.70	1.15	1.17
Total	West	1.41	1.38	1.80	1.71	1.37	1.66
	East	8.66	9.09	8.03	8.01	7.17	12.93
	Total	2.84	2.88	3.01	2.94	2.51	3.87

Source: Amtliche Nachrichten der Bundesanstalt für Arbeit (Official Bulletin of the Federal Employment Service), 1, 1999: 14ff.; own calculations

1.2.6 Institutional framework and evaluation

This section described the institutional framework of ALMP in Germany and the main instruments of ALMP. The recent changes towards organisational decentralisation lead to a greater diversity of the instruments with implications for the evaluation of policy outcomes. The evaluation studies should take into account the greater diversity of measures and that a comparison of participants across regions becomes more difficult with respect to the programme design and duration.

Wee see that various options exist for the integration of hard–to–place and problem groups. Training programmes or temporary subsidised employment explicitly focus on the integration of long–term unemployed, and the local employment offices are now enabled to choose between different policy options. Presumably, this recently gained flexibility will change the participants' assignment to programme types in general and the programme impact.

Among the different programmes, the local employment office has in principle a choice between higher or lower integration subsidies of shorter or longer duration for basically the same target group. An assessment of integration subsidies should take into account these differences in programme design.

1.3 Methodological issues in the evaluation of ALMP

There exists a growing literature on the methodological issues involved when trying to evaluate the microeconomic and macroeconomic effects of ALMP, see Heckman et al. (1999) for a recent summary of the state–of–the–art, Calmfors and Skedinger (1995) for the issues involved in macroeconomic evaluations of ALMP. This section cannot provide a comprehensive summary of the methods. Instead, we describe the main methodological issues necessary to discuss the German literature and recent extensions like the timing–of–events approach and further identifying assumptions in the context of different starting dates of the programmes. We present various criteria for the evaluation of how the various authors have resolved the respective methodological problems involved. Since the available evidence for Germany is based on non–experimental data, we restrict our attention to methods for this case.

An evaluation of a specific programme, which is part of ALMP, basically involves four issues: First, one has to define the criteria by which to assess the success of the programme. Second, it is to be investigated whether the programme leads to a causal improvement for the individual participant with regard to the relevant success criterion. Third, one has to analyse whether the individual success of the programme justifies the direct costs of the measure. The second and third issue together form the microeconomic evaluation problem. Fourth, turning to the macroeconomic level one is interested whether the programme leads to an improvement in the aggregate outcome.

1.3.1 Success criteria

Regarding the first issue, we focus here on economic outcome variables such as the (re)employment prospects, the stability of employment or the level of earnings while acknowledging the importance of non–economic goals in actual political decision processes. The economic outcome variables considered here are part of the goals formulated by ALMP in Germany, especially the target of integration into regular employment (see section 1.2). They are defined at the individual level and can be aggregated to the macroeconomic level.

1.3.2 The microeconomic evaluation problem

A microeconomic evaluation investigates whether participation in the programme causes an improvement in the relevant outcome variable for the participants and whether this success is large enough to justify the costs. An assessment of the costs is necessary for a comprehensive microeconomic programme evaluation (see Heckman et al. 1999) but this issue has not been addressed in the recent German literature on microeconomic evaluations of ALMP. This is probably due to the intensive discussion about whether one can even identify positive individual effects of ALMP on the respective outcome variable (without even considering the costs) and to the fact that positive effects at the individual level are necessary (but not sufficient) for a positive overall evaluation.

Microeconomic evaluations typically build upon a potential–outcome–approach to causality, i.e. the causal effect of the programme for individual i is the difference between the outcome variable YT_i when receiving the treatment (i.e. participating in the programme) and the outcome variable

YC_i when the same individual is not receiving the treatment. The evaluation problem lies in the fact that one can only observe one of the two outcome variables (YT_i, YC_i) for the same individual at the same time. In order to estimate the effects of treatment–on–the treated, one has to estimate the outcome variable in the situation of not being treated for the group of treated individuals (those with treatment dummy $D=1$). As average potential outcome, this is given by

$$E\{YC|D=1\}$$

which is typically estimated based on the measured past non–treatment experience of the treated individuals (before–after comparison) or on the experience of other non–treated individuals (D=0, i.e. comparison with control group). Having to rely on plausible though untestable identification assumptions, the non–treatment outcome of other individuals or of the same individuals in other time periods defines the comparison level (when relying on other non–treated individuals one refers to the control group) for the treatment outcome[8].

Put more formally, the average treatment effect on the treated $(D=1)$ is given by

$$E\{YT|D=1\}-E\{YC|D=1\} \quad (1)$$

and the evaluation problem consists of estimating $E\{YC|D=1\}$ since the outcome in the non–treated situation is not given for the treated individual.

Typically, the average non–treatment outcome either for the non–treated individuals or for the treated individuals before treatment $(D=0)$ do not provide an adequate estimate of the comparison level, i.e. selection bias of the form

$$E\{YC|D=1\} \neq E\{YC|D=0\} \quad (2)$$

arises due to differences in observable or in unobservable characteristics between treated and non–treated individuals. The recent literature on evaluation methods also points to the possible bias introduced into the analysis by using misspecified statistical or econometric models, see Heckman et al. (1999). In the following, we will describe these issues slightly more formally and sketch the popular approaches to account for selection bias.

1.3.2.1 Selection on observables

Let X be the relevant observable characteristics of treated and non–treated individuals then the conditional independence assumption (CIA)

$$E\{YC|D=1,X\}=E\{YC|D=0,X\} \quad (3)$$

[8] When the evaluation is concerned with the average treatment effect for the entire population of treated and non–treated individuals the potential but unobservable outcomes for both subgroups have to be estimated.

eliminates selection bias conditional on X.[9] Thus, even though treated $(D=1)$ and non–treated $(D=0)$ individuals with different X are not comparable, conditional on X they become comparable in their (potential) outcome in the non–treated state. Note that CIA is not testable. The CIA is the basis of the popular matching approach to programme evaluation, which involves matching treated and non–treated individuals with respect to their observable characteristics such that the treatment effect can be consistently estimated by the average difference in treated and estimated non–treatment outcome based on matched non–treated individuals. In its most rigorous form, matching replicates the idea of a control group from experimental evaluations where in a sample of comparable individuals it is randomly decided who receive treatment and who does not. The latter individuals then form the control group and the treatment effect can be consistently estimated as the outcome difference between treatment and control group.

In a multi–dimensional setting, it is not always the case that one can find for each treated individual one – or even more than one – non–treated individuals corresponding exactly to the treated in all observable characteristics. This is sometimes referred to as a lack of overlap in the distribution of observables between the sample of treated and non–treated individuals. With the treatment effect not being constant, Heckman et al. (1999) emphasise that due to lack of overlap it might not be possible to evaluate the treatment effect for all possible observable characteristics (this can also result in a difference between the effect of treatment–on–the–treated and the average treatment effect). If all individuals with a certain occupation in a certain region receive treatment because a large company terminates its operation and if these characteristics exhibit an impact on labour market outcomes, it will be virtually impossible to find adequate matches among the non–treated population.

In practice, evaluation studies use similar matches to define the comparison level based on three basic approaches, see Heckman et al. (1999, section 7.4.1),

Kernel matching

Under the Conditional Independence Assumption, the average effect of treatment–on–the–treated can be estimated by

$$\frac{1}{N}\sum_{i\in\{D=1\}}\left(YT_i-\sum_{j\in\{D=0\}}w(i,j)YC_j\right) \tag{4}$$

where $j\in\{D=0\}$ is the group of non–treated individuals and the kernel weight $w(i,j)$ defines the "closeness" between the treated individuals i and j in terms of the relevant observable characteristics. Here, one simply estimates the non–treatment outcome of any treated individual i with observable characteristics X by taking an average outcome for non–participants with the same characteristics X – these are the fitted values of nonparametric regressions in the sample of non–participants at the local individual's characteristics X. The nonparametric regression basically can

9 Strictly speaking, our formulation considers only mean independence which suffices for estimating the average treatment effect, see Heckman et al. (1999) for the general case.

be interpreted as a weight function $w(i, j)$: j should have a higher weight for i if the two are more similar. For each treated individual i, the weights sum up to 1 over the whole sample of non–participants. The estimated effect of treatment–on–the–treated can then just be estimated by averaging this difference of the observed treatment outcome and the locally estimated non–treatment outcome over the whole sample of treated individuals N.

Variants of kernel matching are nearest–neighbour–matching where only the "closest" non–treated individual is used for comparison and caliper–matching where a match is only made if there exists at least one individual which is sufficiently close to the treated individual i (problem of overlap).

Propensity score matching

The major disadvantage of kernel matching is the "curse–of–dimensionality", i.e. again it might be difficult to match with respect to a high–dimensional vector of observable characteristics. Therefore, most evaluation studies actually use the result by Rosenbaum and Rubin (1983) that the CIA in equation (3) also holds with respect to the probability of treatment ("propensity score") $P(X)$ as a function of the observable characteristics X, i.e.

$$E\{YC|D=1, P(X)\} = E\{YC|D=0, P(X)\}. \tag{5}$$

This result allows matching upon the one–dimensional probability by using the "closeness" in the propensity score as the weighting scheme in equation (4). This dimension–reduction feature reduces the problem of finding adequate matches but it comes at the cost that the propensity score has to be estimated itself. In general, it is an open question which form of matching is most appropriate; see Heckman et al. (1999). The recent German literature building on a suggestion by Lechner (1998) mostly uses a hybrid approach combining matching on the propensity score with matching on selective important observable characteristics, which often are not time–invariant. One issue, which has not been addressed so far by the German literature is the fact, that the standard error of the estimated treatment parameter should take account of the fact that the propensity score used for matching is a preestimated quantity, see Heckman et al. (1999, section 7.4.1).

Parametric regression

Traditional econometric regression methods typically use a parametric specification to account for the impact of observable characteristics and implicitly estimate the potential outcome in the non–treated state as the fitted value on the regression functional. As the of nonparametric analysis became more popular, these methods have been criticised heavily in the literature. Parametric regression models might not be flexible enough to capture the true relationships and often rely on arbitrary identification assumptions, which allow the researcher to extrapolate into areas of the regressors X for which no observations are available and hide the lack–of–overlap. However, many evaluation studies still use parametric regression methods, as part of their analysis and it has to be investigated whether they suffer from the aforementioned problem. One reason for the popularity of parametric regression methods is that a fully nonparametric analysis involves the curse–of–dimensionality problem.

1.3.2.2 Selection on unobservables

While conditioning properly on a large number of observable characteristics allows to correct for most of the selection bias when contrasting the outcome for treated and non–treated individuals. It is argued that the CIA in equation (3) is not a reasonable identifying assumption for their actual evaluation problem; see Heckman et al. (1999). There exist various plausible channels why unobservable characteristics or differences in the gains from a programme might in fact influence the decision whether to participate, thus violating the CIA:

- Individuals might know more about their labour market prospects with and without treatment.
- The eligibility for programme participation (including the discretion by programme administrators) may depend on variables, which are unobservable to the researcher.
- The labour market behaviour of individuals before treatment might be altered by the prospect of future treatment.

Under these circumstances, it is not possible to infer the adequate comparison level (i.e. the average non–treatment outcome) for the treated population from the outcome of non–treated individuals with same observable characteristics as stated by the CIA. To account for selection on unobservables, the literature has pursued various strategies:

Econometric selection models

Similar to parametric models used to correct for selection on observables the application of the classical econometric selection models[10] in the context of the evaluation problem has been criticised

[10] These models typically specify a separate participation equation for treatment which includes additional regressors compared to the outcome equation. Selection on unobservables involves a correlation between the error term in the outcome equation and the error term in the participation equation. The Heckman correction is the simplest and most widely used on these estimation approaches. Other alternatives are full information maximum likelihood models.

quite heavily in the literature, see Heckman et al. (1999, section 7.4.2), for using restrictive functional form assumptions thus yielding a variety of estimates based on typically misspecified models. Although there has been made a lot of progress in estimating semiparametric selection models, such models have not yet been applied in the evaluation of ALMP in Germany.

Conditional Difference–in–Differences estimation

The difference–in–differences (DiD) estimation approach requires panel data and builds on the assumption of time–invariant linear selection effects. This estimator extends simple before–after comparisons to determine the treatment effect based on the presumption that the outcome variable can also change over time due to reasons, which are unrelated to the treatment. Thus, the change for the treated has to be contrasted to the change for comparable non–treated individuals. Assuming that the employment outcome is given by

$$Y_{i,t} = a_i + D_{it}\left(g_T\left(X_{it}\right) + e_{T,it}\right) + \left(1 - D_{it}\right)\left(g_C\left(X_{it}\right) + e_{C\,i,t}\right) \tag{6}$$

with $YT_{i,t} = Y_{i,t}$ for $D_{i,t} = 1$ and $YC_{i,t} = Y_{i,t}$ for $D_{i,t} = 0$, a general DiD estimator consists of matching individuals *i* and *j* with the same observable characteristics $X_{i,t1} = X_{j,t1}$ and $X_{i,t0} = X_{j,t0}$ where *i* receives treatment between period *t*0 and *t*1 and *j* is a non–treated individual. Further assumptions are that $g_T\left(X_{it}\right)$ and $g_C\left(X_{it}\right)$ are the individual specific outcomes in the treated and non–treated state, respectively, that the permanent unobservable individual effect a_i is correlated with programme participation and that $e_{T,it}$ and $e_{C,it}$ are additional error terms for the treatment and non–treatment state. Thus, a_i captures the effect of selection on unobservables and can be differenced out in order to obtain a constant treatment estimator by

$$\left(YT_{i,t1} - YC_{i,t0}\right) - \left(YC_{j,t1} - YC_{j,t0}\right) = g_T\left(X_{it1}\right) - g_C\left(X_{it1}\right) + e_{T,it1} - e_{C,it0} - e_{C,jt1} + e_{C,jt0} \tag{7}$$

Heckman et al. (1999) term (7) the conditional DiD estimator since individuals *i* and *j* with the same (or reasonably similar) observable characteristics are matched. In a regression specification, the conditional DiD estimator in (7) can be implemented by using both a preprogramme dummy and a postprogramme dummy $D_{i,t}$ in the pooled outcome regression. The preprogramme dummy indicates whether a person receives treatment in the future. Hence, the difference between the regression coefficients for the postprogramme and the preprogramme dummy provides the DiD estimate. Heckman et al. (1999) refer to various studies for the U.S. indicating that conditional DiD combined with nonparametric matching has shown to be a very effective tool in controlling for both selection on observables and unobservables. However, it has to be emphasised that its validity depends critically on the time–invariant nature of the selection effect, see the discussion on preprogramme tests and Ashenfelter's dip in the next section.

1.3.2.3 Preprogramme test and Ashenfelter's dip

Pre programme dummies indicating further treatment have also been used as a specification test both in a regression or in a nonparametric matching context to investigate whether the chosen method has properly controlled for time–invariant selection effects. To our knowledge, Heckman and Hotz (1989) had first advocated this approach as the preprogramme test. It is now criticised heavily by Heckman et al. (1999, section 8.4) as the "Fallacy of Alignment". The preprogramme test is motivated by the idea that if the evaluation method corrects properly for all differences – both observable and unobservable – between treated and non–treated individuals then no significant differences in the outcome variables should exist between comparable treated and non–treated individuals before treatment. Put differently, significant differences before treatment indicate remaining time–invariant differences, which are effectively used by the DiD estimator described in the previous section. In a regression context, the preprogramme test is implemented by adding a preprogramme dummy to the outcome regression.

Heckman et al. (1999) argue that the validity of the preprogramme test as a specification test[11] is questioned by a finding termed "Ashenfelter's Dip", which was first discovered when evaluating the treatment effect on earnings but which can also apply to other outcome measures. Ashenfelter's Dip involves a disproportionate decline in earnings before the programme starts. It is likely that this decline in earnings is related to the subsequent participation and, thus, cannot be used to test for "correct alignment" of treated and non–treated individuals before treatment.[12] To address this problem, Bergemann et al. (2000) suggest testing for "alignment" early enough before the start of the programme such that the preprogramme outcome is not affected by future participation.

1.3.2.4 Heterogeneous treatment

The discussion so far has implicitly assumed that all treated individuals participate in one homogeneous programme. However, the different programmes in Germany allow for a lot of heterogeneity in the type of treatment (length of the programme, different contents of training courses, different provider of courses or employers etc.) and it has been observed that hard–to–place individuals often participate in more than one programme over time ("programme career"). It is obvious that the intensity and the quality of a programme should have an impact on the labour market outcome and that complicated dynamic selection effects occur when individuals participate in different programmes one after the other (possibly in order to remain eligible for transfer payments by the labour offices). The survey by Heckman et al. (1999) does not discuss the methodological aspects of these issues and therefore, it does not come as a surprise that applied academic evaluation research has been restricted to the evaluation of single programmes either ignoring their heterogeneity or restricting the analysis to a subset of fairly homogeneous treatments while ignoring the interaction with other programmes. Recently, Lechner (2001) has made theoretical progress in evaluating multiple treatments under the CIA. He extends the method of propensity score matching and shows that the relative outcome effect of different treatments can effectively be evaluated by a bivariate evaluation of the outcome variables of matched individuals receiving different treatments (no treatment effec-

[11] Ultimately, without being acknowledged in this way by Heckman et al., this criticism applies also to the conditional DiD estimator which had been evaluated favourably by these authors.

tively becomes one form of treatment). Interestingly, it provides some ex post justification for those earlier studies in the literature, which evaluated one programme based on the sample of individuals receiving no treatment what–so–ever together with those individuals participating in the programme of interest. Nevertheless, one has to acknowledge that more research is needed on the methodological aspects of programme heterogeneity.

1.3.2.5 Identification and specification issues

The recent discussion on evaluation of ALMP focuses on the (i) timing of treatments and the (ii) identification of causal effects if treatment is restricted to unemployed and may start at any time during unemployment – a situation common in Europe and widely ignored by the American debate usually discussing treatment as a binary choice problem, see Heckman et al. 1999. Most evaluation studies however benefit from panel data, in which both aspects are crucial. Therefore the following section provides a short description of timing–of–events and identification problems in panel data, although there does not exist any application for ALMP in Germany yet (opposite to many other European countries).

Identification without CIA: Timing–of–events

Alternatively to the CIA; Abbring, van den Berg (2003) suggest an identification of the treatment effect using duration data which benefits from the variation in the duration until treatment relative to the duration until the outcome of interest in the case of non–experimental evaluation. This information is usually ignored in applying the CIA as in (3) for the binary treatment problem. The timing–of–events approach[13] models simultaneously the outflows from unemployment into regular work and the programme participation in a mixed proportional hazard model. Both, the participation hazard function and the hazard rate into regular employment depend on observed characteristics X and unobserved heterogeneity u in a multiplicatively separable form elapsed duration t as

$$q_u(t \mid x, D, u) = l_u(t)\exp(X'b_u + Dd + u)$$

where $q_u(t \mid x, D, u)$ is the exit rate from unemployment at time t conditional on observed characteristics, unobserved characteristics and the treatment variable D (Lalive, van Ours, Zweimüller 2002: 14). $l_u(t)$ denotes the effect of the elapsed duration. The treatment, too, is supposed to follow a proportional hazard specification as

$$q_p(t \mid x, D, u) = l_p(t)\exp(X'b_p + v).$$

v introduces unobserved heterogeneity into the hazard to treatment and u and v follow a joint distribution denoted by $G(u,v)$. Assuming randomness oft the unemployment duration, the timing–of–events approach uses the variation in unemployment duration and the variation in the duration until the start of the treatment in order to identify the unobserved heterogeneity distribution. The differ-

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12 Analogously, preprogram earnings cannot be used as "reference level" for DiD estimation.

13 In the following, the methodology is described following the recent application by Lalive, van Ours and Zweimüller for the effect of benefit sanctions on the duration of unemployment in Switzerland

ences in the duration of unemployment until the start of the programme are determined by a combination of the search behaviour of the unemployed and the assignment process of the employment service. Under this identifying assumption, the introduction of unobserved heterogeneity creates homogeneous samples. Within these homogeneous groups, the parameter estimate of the treatment dummy exhibits the difference in the hazard rates related to the treatment for such homogeneous groups after treatment. However, it is necessary to assume that the treatment is not anticipated. This might usually not be the case for long–lasting ALMP programme, but for short–term treatments (e.g. sanctions to unemployed, and most applications of the timing–of–events approach deal with sanctions).

Identification under the CIA if non–treatment is temporary

There are different arguments why neither static matching estimators nor the application of proportional hazard rate models are sufficient to control for selection bias without further assumptions (see Frederiksson, Johansson 2003: 6ff.) and why the estimated effects could be either upward biased or downward biased: First, the effect of treatment–on–the–treated as discussed under section 1.3.2.1 assumes that treatment and non–treatment take place at the same time. This might be true in the case of experiments where treatment and non–treatment are really offered at the same time – in non–experimental evaluation, the non–treatment group has in fact no starting date of the treatment.

Secondly, most participants in ALMP programmes have to experience a certain period of unemployment before any assignment to the programme: Most evaluation studies model the assignment process typically within a certain time window, e.g. the first six months of unemployment, so a potential comparison group usually consists of persons who are unemployed up to this period – but not treated. The problem then is that those who had luck of finding a job quickly are more likely to be found in the control group than in the treatment group (ibd., 9.).

Frederiksson, Johansson (2003) discuss these two lifelike problems extensively and show that the conditional independence assumption as in (3) does not hold: First, individuals who are not treated up to the end of the time window might be participants in a programme after this time, so that a certain conditioning on the future is implemented, and it can be shown that the effect of treatment–on–the–treated is then positively biased. Essentially, it is not possible to create a sample of matching individuals who do not receive treatment at any point in time. It is however possible to consistently estimate the effect of treatment–on–the–treated if one assumes that the timing of treatment matters: e.g. if treatment in one year differs from the treatment in the next year, then a comparison group whose members do not start treatment in the specific period could provide a valid non–treatment outcome; therefore the authors recommend that one should take the timing of the treatment seriously.

The second aspect – a control group usually consisting of individuals that did not participate in a programme because of finding employment before an already planned programme could start – implicitly conditions on the outcome measure, i.e. the employment rate. Consequently, the non–treatment outcome is over estimated. leading in downward biased estimators for the effect of treatment–on–the–treated.

1.3.3 Macroeconomic effects

Now, we turn to the issue of how to determine the effects of ALMP on the outcome at a more aggregated level, i.e. beyond the ceteris paribus effect on the treated. Macroeconomic effects consist of equilibrium price effects, behavioural changes, and other repercussions on the non–treated labour force or on the entire economy. Such indirect effects are likely to be of importance given the extend of ALMP in Germany (in particular in East Germany). Calmfors (1994) reviews the methodological issues involved when trying to estimate the indirect effects of ALMP. He distinguishes displacement effects (treated workers gain their jobs at the expense of non–treated workers), deadweight effects (subsidising a treatment, which would have occurred anyway), substitution effects (replacement of jobs for other types of non–treated workers because of relative wage changes), and tax effects (the effects of financing ALMP). The following discussion neglects fully specified General Equilibrium approaches, as described in Heckman et al. (1999, section 9.2), since we are not aware of applications to ALMP in Germany.

A simple reduced form approach to estimate displacement (or substitution) effects in employment would be to regress the employment of non–treated individuals on the employment of treated individuals (often lagged). A coefficient of zero would suggest that no displacement occurs while a coefficient of minus one suggests full displacement. However, reduced form evidence of this type is not convincing since the extent of ALMP typically depends on the state of the labour market, thus the above regressor is likely to be endogenous and the regression overstates the displacement effect (the coefficient is downward biased). To control for the endogeneity of ALMP, it is necessary to use instruments, which have an impact on the extent of ALMP and which at the same time do not influence directly the state of the labour market.

Most recent analyses for Germany implement a more structural approach to the evaluation of the macroeconomic effects of ALMP. Here, we want to mention two basic approaches that have been implemented in various studies based on regionally disaggregated data. The first type builds on Beveridge–curve–type–models involving the simultaneous occurrence of unemployment and vacancies, see Bellmann and Jackman (1996a). The second type uses the Layard–Nickell–Jackman (1991) labour market framework modelling the aggregate outcome by the interaction of a wage setting relationship and a labour demand equation, see Calmfors and Skedinger (1995). In contrast to the reduced form regressions described above, these two approaches allow to model directly the impact of ALMP within a structural relationship (e.g. on wage setting or on matching efficiency). Thus, it is conceivable to relate the analysis more closely to the microeconomic evaluations and to differentiate effects on the structural rate of unemployment from short–run shocks to the state of the labour market. Obviously, the reliability of the estimated effects of ALMP again hinges critically on the availability of instruments to identify the structural models while making use of an exogenous variation of ALMP. This literature typically addresses two particular issues: First, does ALMP lead to less wage restraints in wage setting since it accommodates the costs of higher wages? And second, does ALMP reduce the mismatch in the labour market, i.e. the discrepancy between the vacancies and the unemployed workers?

1.4 Evaluation results

1.4.1 Microeconometric evaluations

1.4.1.1 Evaluation on the basis of survey data

Evaluations of ALMP in Germany are mainly based on data from household panel surveys.

There are two panel data surveys applied in most of the evaluation studies cited in this synopsis: the German Socioeconomic Panel (GSOEP), which consists of two main subsamples for East and West Germany,[14] and the Labour Market Monitor East Germany (LMM)[15]. In this section, we discuss the information about ALMP available from these two panels and the principal advantages and short–comings of survey based evaluation. The sample sizes of the different evaluation studies differ with respect to the data sources: The GSOEP has an average sample size of 12,000 individuals for both East and West Germany and is available for the period from the year 84 to date (for East Germany: from 90) whereas the LMM was a panel exclusively drawn for the East and limited to the period from 90–94, starting with an overall population of 10,751 for the East.

In addition to these two panel surveys, other data are used for programme evaluations in some few studies. Pfeiffer, Reize (1999) evaluate the promotion of self employment using data of the firm start–up panel of the Centre for European Economic Research (ZEW start–up panel), and add additional programme data taken from administrative records. Besides, there are evaluations of further training on the basis of alternative data: the IAB–BiBB Qualification and Occupational Career data (QOC, cited in Pfeiffer and Reize 1999), the retrospective data of German Life History Study (GLS, Schömann, Becker 1998)[16] and subsample of the official "contributory employment record" (Bender and Klose 2000)[17]. For the evaluation of job creation schemes and further training, two evaluation studies are based on the regional data set of the Labour Market Monitor Sachsen–Anhalt (LMM–SA, Eichler, Lechner 1998, Bergemann et al. 2000)[18]. The most recent study about the effectiveness of job creation by Hujer, Caliendo, Thomson (2003) uses the official programme data and data drawn from the unemployment insurance and job seeker records. Another recent study by Hujer, Caliendo, Radic (2001) evaluates the effects of different types of wage–subsidies with data from the establishment panel of the German Institute for Employment Research (*IAB–Betriebspanel*).

Evaluation studies that explicitly discuss the evaluation problem exist only for Job Creation schemes (JC) and public sector sponsored further training (PSFT). Although information about the subpro-

[14] Information of the concept, design and basic frequencies of the GSOEP can be obtained from the GSOEP web site at the DIW (http://www–soep.diw–berlin.de/).

[15] The LMM was conducted for Eastern Germany only in the period 1990– 1994 (Bielenski, Enderle, von Rosenbladt 1991).

[16] The GLS is a retrospective data set for three birth cohorts with an overall sample size of around 2,200 (Mayer, Brückner 1989).

[17] A summary of the information available in these data sources can be obtained on the web sites of the ZUMA (http://www.zuma–mannheim.de/data/microdata/).

[18] The Labour Market Monitor Sachsen–Anhalt (LMM–SA) is a panel survey with information for the period 1990–97 and with a population of around 8,000 individuals in 1997.

grammes of e.g. further training (consisting of short–term training, the provision of limited professional skills or retraining) is generally available, these have hardly been subject to any evaluation so far. To our knowledge, there exists only one evaluation with LMM data that provide information about different types of PSFT (Fitzenberger, Prey 2000). All other studies follow a broad treatment definition.

For the evaluation of job creation programmes, there are also very few studies for the subprogrammes because the panel surveys of GSOEP and LMM always aggregate two separate programmes: the structural adjustment measures (SAM) and the classical job creation scheme (ABM). As far as we know, there is only one very recent evaluation that explicitly evaluates the effects of job creations following the ABM regulation (Hujer, Caliendo, Thomsen 2003).

Information about wage subsidy programmes targeted at the reintegration of the hard–to–place into regular employment can neither be obtained from any of the panel surveys and was only evaluated at the macroeconomic level. However, it could be difficult to obtain valid information on these programmes by surveying individual labour market participants as these programmes are usually given directly to the companies. Besides, although significantly growing over the last years, wage subsidy programmes in general are smaller scale programmes in both East and West Germany and subsamples from the survey data of GSOEP and LMM would be too small for credible evaluation.

Therefore, this survey reports mostly findings for ALMP micro–evaluations of PSFT and JC. Besides, there are some few other evaluation studies for the support of self–employment (Pfeiffer, Reize 1999), the effects of different wage subsidies on the employment performance of firms (Hujer, Caliendo, Radic 2001) or the regional programme of non–profit temporary work for reintegration (Almus et al. 1999), which are considered in this survey.

Table 1.4 summarises the data sources of existing evaluation studies for German ALMP and indicates the high importance of GSOEP and LMM for the evaluation research of the last years.

Table 1.4 Data used for the microeconomic evaluation of ALMP

	Socioeconomic Panel (GSOEP)		Labour Market Monitor East (LMM)	Labour Market Monitor Saxony Anhalt (LMMSA)	ZEW Start–up Panel	Qualification and occupational career data (QOC)	German Life History Study (GLS)	Employment register and participation data	IAB–establishment panel
	East	West	East	East, only Sachsen Anhalt	East and West	East and West	East and West	East and West	East and West
Job Creation Schemes (JC)	X		X	X					
Classical Job Creation (CJC)								X	X
Structural Adjustment Measures (SAM)									X
Further training (FT)	X	X	X	X		X	X		
Public sector sponsored further training PSFT)	X	X	X					X	
Public sector sponsored further training with income maintenance (PSFT–IM)	X	X	X						
On–the–Job Training (OJT)	X	X	X			X			
On–the–Job Training with income maintenance (OJT–PIM)									
Off–the–Job Training (OFT)	X	X	X			X			
Off–the–Job Training with income maintenance (OFT–PIM)									
Mobility Incentives									
Mobility Allowance									
Bridging Allowance					X				
Wage–sudsidy programmes									X

X Evaluations cited in this survey

1.4.1.2 Training programmes

Most evaluation studies published so far are evaluations of public sector sponsored further training (PSFT). These studies are quite heterogeneous concerning the evaluation methods and with regard to the underlying data and usually analyse an aggregation of different programmes or types of training.

Despite of this variety, evaluations of all types of public sector sponsored further training (PSFT) can hardly be achieved by any of these evaluations. First, both sources (GSOEP and LMM) refer to panel data of the active population and survey only information of the individuals. They offer data for relatively broad categories of training, either further training in general or retraining, either by private initiative or as part of ALMP. Besides, information whether on–the–job training was financed or co–financed by means of the Federal Employment Service cannot be obtained in the data. Therefore, most authors evaluate only treatment accompanied by the receipt of individual income maintenance during the time in training (PSFT–IM). Individuals who did not receive income maintenance while being treated because of being in contributory employment or in other ALMP schemes at the time of training are not subject to evaluation. PSFT is likely to be underestimated by applying the narrow definition of PSFT–IM as public sector sponsored training. For West Germany, evaluations often combine assessments of the outcomes from training in general, thus for both PSFT and training initiated by the individuals without public sponsoring, which clearly overstates PSFT. We assume that the heterogeneity of different time periods for evaluations together with the different definitions of treatment as broad further training, PSFT–IM, or non specified on–the–job or off–the–job training to have considerable influence on the results found in the studies.

1.4.1.2.1 East Germany

Data and evaluation period

Data for the evaluation of training in East Germany mainly refers to the GSOEP East, which started in 90 (Lechner 1998, 1999, Pannenberg 1995, 1996, Staat 1997, Hujer, Wellner 2000) and the LMM East, available for 90–94 (Hübler 1994, 1997, Fitzenberger, Prey 1998, 2000, Prey 1999, Kraus, Puhani, Steiner 1999).

The data of the GSEOP East provide broad socioeconomic information, e.g. individual labour market variables but also socioeconomic variables of the household. The other data, the LMM, offer less information about the socioeconomic background of its population. A very important limitation of this data is the lack of time variable information on the training programmes. Nevertheless, there are clear advantages as these data were with 10,751 observations at the starting point twice as big as the GSOEP (N = 4,453 in 90). Even in the presence of serious panel mortality, the sample is still quite large at the end of the observation period in 94 (N = 3,500).

The evaluations analyse both data sets for different periods: One part of the evaluations focuses on the implementation of training until 92 (Hübler 1994, Kraus, Puhani, Steiner 1999, Pannenberg 1995). Most studies however evaluate training over period 90–94 (Lechner 1998, 1999, Fitzenberger, Prey, 2000, Pannenberg 1996, Hübler 1997, 1998, Hujer, Wellner 2000). Staat (1997) evaluates treatments starting 92–4. After 94, the income maintenance information used for the identification of public sector sponsored further training was dropped out of the questionnaire for the GSOEP, so that PSFT was no longer identifiable.

The selection of different periods for the assessment of outcomes is assumed to have influence on the results of the studies because of two reasons: First, PSFT in the early period of 90–2 is hardly comparable to the later years, because building the institutions for the implementation of programmes in East Germany was still in progress by then. Secondly, the differences in the observation period also determine to which extent the evaluation studies can consider the differences in the pre–training labour market history or the differences in the employment prospects after the programmes.

Programme variables

As indicated in the description of the data under section 4.1, the evaluation studies define treatment not consistent as PSFT, but with respect to the different data sources as: evaluations of further training in general, covering both PSFT and privately initiated training (Hübler 1994, Pannenberg 1995). The reason why we included these studies in this survey is twofold, first it is plausible that the overall share of PSFT was high especially in the beginning of training in East Germany, and secondly it is worth comparing the outcomes with studies that only evaluate a subgroup of the PSFT–IM (with income maintenance). Many studies focus on the narrow distinction of further training as PSFT–IM and underestimate PSFT in East Germany. Further training (FT) can be subdivided into two different subgroups: (i) There are evaluations of training within firms (on–the–job–training, OJT) or courses in training centres providing further training outside firms (especially for the unemployed, off–the–job training, OFT), which again cover both PSFT or non–public sector sponsored training (Pannenberg 1996, Lechner 1998). Especially in East Germany, OFT was often explicitly targeted to unemployed persons and should be (mainly) regarded as PSFT. (ii) We find evaluations that assess explicitly the subgroup of PSFT–IM (Lechner 1998, Fitzenberger, Prey 1998, 2000, Prey 1999, Hübler 1997, 1998, Staat 1997, Hujer, Wellner 2000, Kraus, Puhani, Steiner 1999).

Outcome variables

As shown in Table 1.5, the studies evaluate training on various outcomes: Five evaluations are based on hazard rate models (Pannenberg 1995, 1996, Hujer, Wellner 2000, Kraus, Puhani, Steiner 1999), estimating the outcomes of PSFT on the transition from unemployment to employment after a programme or with respect to the search duration for new employment (Staat 1997). The studies by Fitzenberger, Prey (1998, 2000), Hübler 1994 and Hübler 1997 estimate the effects on dummy variables indicating at a certain point in time whether an individual was in employment or not. Four evaluation studies focus on differences in average unemployment rates of participants and non–participants (Fitzenberger, Prey 1998, Lechner 1998, 1999, Bergemann et al. 2000) with non-parametric approaches. In most studies, there are estimates on the effects of training programmes on individual earnings, too.

Methodology

The evaluation studies differ widely with respect to the methodological design and the solution of the evaluation problem as well as the measurement of ALMP outcomes. All in all, there are two main groups of evaluations, parametric panel data estimates and nonparametric evaluations based on matched samples.

The first group of evaluations are parametric estimations that usually apply linear panel data estimates for the treatment effects on wages (Hübler 1997, Pannenberg 1995). For the evaluations of employment outcomes, either random effects probit and tobit maximum likelihood estimates (Hübler 1997, 1998, Fitzenberger, Prey 1998, 2000, Prey 1999) or hazard rates models (Pannenberg 1995, 1996, Hujer, Wellner 2000) are used.

The evaluation problem is solved with methodologies either controlling on observables within the parametric outcome equation (Pannenberg 1995), by instrumental variables (such as the propensity score to participate in further training estimated with a probit model, cf. Staat 1997), or by a simultaneous estimation of programme participation and outcomes (Fitzenberger, Prey 2000, Prey 1999). Furthermore, some approaches include the pre–programme test in order to sufficiently encounter the problem of unobservables (Hübler 1997, 1998, Fitzenberger, Prey 1998, 2000, Bergemann et al. 2000). Fitzenberger, Prey (1998, 2000) and Bergemann et al. (2000) additionally implement difference–in–differences estimators (DID), by including preprogramme and postprogramme dummies in the outcome estimate. The estimated coefficients then provide the programme effects by subtracting the preprogramme from the postprogramme coefficient.

The studies by Hujer, Wellner (2000) and Hübler (1997, 1998) apply matching techniques either by removing observations from the control group and the creation of (reduced) "most similar samples" (Hübler 1997) or by nearest neighbour matching on the basis of the propensity score and further conditioning variables in order to obtain matched samples for the application of parametric models (e.g. hazard rates, Hujer, Wellner 2000).[19]

The idea of matching participants and non–participants is also central in the application of nonparametric evaluation approaches. The studies by Lechner (1998, 1999) and Bergemann et al. (2000) apply matching on the estimated propensity score (and additional variables) and identify the non–treatment outcome by the nearest neighbour in the sample of non–treated individuals. Within the matched samples, the outcome is just the average difference between the treatment sample and the matched non–treatment observations or a difference–in–differences that also controls for time constant selection bias on unobservable characteristics.

[19] Methodology of matching approaches, see section 1.3

Results

The results of the different evaluations do not show clear outcomes of PSFT on employment in East Germany. Due to the dissimilar designs of the studies, a direct comparison of the obtained estimators for the effect of PSFT is hardly possible. Therefore, we rather concentrate on positive, negative or insignificant effects as the results of the different studies.

For the broad category of further training (FT), a clear statement on the effectiveness of the programmes cannot be found in the evaluation studies: Pannenberg (1995) finds positive effects for OFT, although these effects are lowered depending on the duration of programmes and previous unemployment experience. Lechner (1999) and Bergemann et al. (2000) do not find any significant effect in evaluations on the average unemployment difference between the matched pairs of participants and non–participants in the long–run. Bergemann et al. (2000) also evaluate the effects of a second treatment, which however also does not lead to an increased employment of the treatment group compared to the non–treatment outcome either.

For the evaluations of the narrower concept of PSFT, too, there are mainly negative or insignificant outcomes: Fitzenberger, Prey (1998) find positive employment effects in the DID estimates in simultaneous random effects probit estimates for the period 93–4 and 90–4 if the employment history of the individuals in the period before the measurement of outcomes is taken into consideration (dynamic specification). Staat (1997) concludes that training had no significant effect on employment stability, except for participants aged above 45 years who seem to increase their probability to be employed due to treatment. In hazard rate estimations on the basis of matched samples, Hujer, Wellner (2000) find insignificant effects comparable to Lechner (1998) on the basis of the same data and the same period with a nonparametric analysis of average differences in employment and unemployment based on matched samples. Fitzenberger, Prey (1998) who additionally to parametric evaluation apply a matching approach in order to check the sensitivity of outcomes with respect to the underlying methodology do not find that significant differences in employment any longer based on this approach.

Only once, the studies found significant effects on individual wages (Pannenberg 1996) for the participation in OFT between 90 and 94. All other studies surveyed do not estimate significant wage effects, neither in parametric nor in nonparametric approaches.

1.4.1.2.1 West Germany

Data and evaluation period

In the second part of Table 1.5 we summarise recent evaluation studies for further training in West Germany. The majority of these studies refers to the data of the GSOEP West. One study makes use of the data of the German Life History Study (GLS), a retrospective survey of several birth cohorts with approximately 2,200 individuals (Schömann, Becker 1998).

Generally, the GSOEP data are available for the period from 84–94. Information on the periods of training, employment or unemployment can be exactly reconstructed on a monthly basis surveyed by a retrospective question about the employment history of the previous year at each point of observation. Like for East Germany, the evaluations differ with respect to the observation period and only Hujer, Maurer, Wellner (1997b, 1998) use the whole period: Pannenberg (1995) reduces the data to the period of 84–91, Prey (1997) to 84–93, Hujer, Maurer, Wellner (1997a) use the data for the period 86–91, Prey (1999) from 85 to 94. Except for the evaluation of the German Life History study (GLS), in which data are sampled on the basis of birth cohorts, all evaluation studies cited here assess the impact of the policies of the 80's and 90's, when ALMP was already targeted towards the problem groups on the labour market. This however was less important in the 70's, so that the results based on the GLS can hardly be compared with the other studies.

The sample sizes are small: For the treatment group, most studies work with samples of around 100 participants (the treatment group sometimes consists of more observations, depending on the design of the data, either as a panel data or spell data). For the group of PSFT–IM, the sample size in most cases lies clearly below 100 observations.

The programme variables

For West Germany, too, the cited evaluations differ in terms of the definition of treatment as either further training in general (FT) or specified as further training with income maintenance (PSFT–IM). Four evaluations for West Germany are based on the broad category of treatment (Pannenberg 1995, Hujer, Maurer, Wellner 1997a, 1997b, 1998, Schömann, Becker 1998). Aside from Hujer, Maurer, Wellner 1997a, 1997b, who evaluate treatments separately for short–term and long–term courses, the evaluations do not distinguish the treatment within the broad category of FT. PSFT–IM is subject to three evaluation studies (Prey 1997, 1999, Staat 1997) for West Germany.

The outcome variables

In most cases, the outcome variable is specified as the transition rate from unemployment to employment (Pannenberg 1995, Hujer, Maurer, Wellner 1997a, 1997b, 1998). The outcomes are estimated by discrete hazard rate models either including an instrument instead of the treatment dummy (Hujer, Maurer, Wellner 1997a) (see section below) or different treatment dummies for short–term or long–term training (Hujer, Maurer, Wellner 1997b). Staat (1997) estimates the effects of training by ordered probit models for the duration of job search and employment stability. For the evaluation of PSFT–IM, Prey (1997, 1999) evaluates the effects on employment or unemployment in random effect probit models. In the evaluation based on the German Life Study (Schömann, Becker 1998), the outcomes are specified as individual wages.

Methodology

The evaluations by Hujer, Maurer and Wellner (1997a,b, 1998), develop different evaluation strategies the same data in order to solve selection bias. In their first paper, Hujer, Maurer and Wellner (1997a) replace the treatment dummy by the propensity score in order to control for selection on observables and use the propensity score as an instrument (IV), which is comparable to Staat (1997). In their second and third analysis for training in West Germany, Hujer, Maurer and Wellner (1997b,

1998) prefer matching on the propensity score and further conditioning variables either as an oversampling of treated and non–treated individuals (1997b) or as a matching of treated individuals to the nearest neighbour of the control sample (1998).

Analogously to the evaluation of training for East Germany, Prey (1997, 1999) estimates programme effects on the basis simultaneous random effect probit estimations of participation, employment and wages, including preprogramme variables. Schömann, Becker (1998) implement linear panel estimations on wages, which control for observables within the parametric model.

Results

Two evaluations find significantly negative effects for public sector sponsored further training (PSFT–IM): Pannenberg (1995) finds negative effects for the whole population and Prey (1997) for men (compared to women who do not show significant changes in the probability of employment due to the treatment). Staat (1997) finds positive effects of training on employment for the group of lower skilled workers as well as positive effects on women's employment. With slightly different data, Hujer, Maurer and Wellner find more positive than negative outcomes: Based on the first approach that models the estimated propensity score as an instrumental variable, the effects for short term courses are positive and the effects for long–term courses are insignificant with respect to the transition to employment (Hujer, Maurer, Wellner 1997a). In their second study (Hujer, Maurer, Wellner 1997b), the hazard rates to employment are positively increased by short–term and long–term courses. In their follow–up study on the outcomes of FT, Hujer, Maurer, Wellner (1998), the authors compare the short–term and long–term effects training on the basis of nearest neighbour matching and find a positive training effect on outcomes in the short run, which disappears over time (Hujer, Maurer, Wellner 1998).

Prey (1999) analyses the effects of PSFT–IM for the period 85–94, so that we rather have a subsample of the treated individuals in the evaluations by Hujer, Maurer and Wellner. Here, we find significantly negative long–term employment effects for men, and insignificant effects for women.

A different outcome is evaluated in Schömann, Becker (1998). This study focuses on the wage effects of training (FT) with split samples for men and women. Schömann, Becker (1998) find significantly positive income effects of training for men under the restriction that they do not change the employer or the occupational position within the firm over time. Men, who change the employer, do not benefit from further training. For women, the results are opposite, indicating that further training leads to a significant increase in individual earnings only if women change their employer, but has only insignificant effects if women stay with their firm.

Table 1.5 Overview of micro–evaluations, further training (East Germany, Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Hübler (1994)	LMM East, 90	N: 4,679 P: 1,276	FT	▪ Job search prob. ▪ Working time	Simultaneous probit estimates for participation in training and job search	Job search + Working time –
Pannenberg (1995)	GSOEP East 90– 1992	N: 2,017 P: 76	FT	▪ Transition rate from unemployment (OFT) (1) ▪ Monthly gross wages (OJT) (2)	▪ Discrete hazard rate model (1) ▪ Linear panel estimate with fixed effects (2)	(Displayed for PSFT–IM only): Transition rate from unemployment: FT * income maintenance (PSFT–IM):– Wages: FT * income maintenance (PSFT–IM): 0
Pannenberg (1996)	GSOEP East 90–94	Employment: 1. N: 1,075 P: 90 Wages: 2. N: 661 P: 55	OFT	▪ Transition from unemployment to employment (1) ▪ Individual wages (2)	Discrete hazard rate model (1) Linear panel model with fixed effects (2) ▪ Preprogramme test	1) Employment.: ▪ Training effect: + ▪ Duration of scheme: 0 ▪ Prev. Unemployment > 6 months: – 2) Wages: training effect: + Duration: – Years of training: – Previous unemployment: 0
Fitzenberger Prey (2000)	LMM East, 90–94	N: P: Women 2409 325 Men 2414 146	PSFT–IM	▪ Employment ▪ Real net hourly wages	▪ Simultaneous RE models for treatment, employment, and wages ▪ PPT and endogenous modeling of real wages ▪ Regression based difference–in–differences estimator ▪ Sensitivity tests for 1990–93	Employment: Wages: 0
Hübler (1997)					See Section job creation	
Staat (1997)	GSOEP East	N: 1,153 (Employment) P: 315 N: 916 (Wages) P: 172	PSFT–IM	▪ Job search duration ▪ Employment stability ▪ Wages	Age group specific control groups, ▪ Probit regression for participation ▪ Outcome variable: Search and employment duration, both ordered probit model ▪ Treatment dummy replaced by the estimated propensity score (IV approach) to control for selection	Search duration: 0 Employment stability: 0 Except age group 25–34: – age group 45–54: + wages (employed only): 0

Table 1.5 Overview of micro–evaluations, further training (East Germany) (cont., Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Hübler (1998)	LMM East 90–94	N: 2,886 P: PSFT–IM 206 Unemployed 223 Non–participants 922	PSFT–IM (among several others)	1) Employment 2) Job search probability 3) Working time 4) Income (3) and 4) employed only)	▪ RE Estimates with matched samples ▪ PPT: Significant differences between P and C group remain in unobservables ▪ OLS on wages and working time ▪ Logit Estimates on employment and job search effects	(displayed for PSFT–IM only) 1994 outcomes ▪ OLS (according to time of training 1990/1991/1992/1993) income / working t. PSFT–IM /0/–/0 /0/–/0 ▪ LOGIT (according to time of training 1990/1991/1992/1993) Employment / Job Search PSFT–IM /–/0/– /+/+/+ ▪ RE without reduction of the C–Group Emp. Inc. Job Search PSFT–IM – – + ▪ RE with reduction of the C–Group (5–35% of non–participants an PPT) Emp. Inc. Job Search PSFT–IM – – 0
Fitzenberger Prey (1998)	LMM East 90–94 93–94	Various sub samples ▪ 1990–94 ▪ Men: N: 3,862 PSFT–IM P: 27* ▪ Women: N: 3,637 PSFT–IM P:57 ▪ average total and participants per year	PSFT–IM (among several others)	▪ Employment ▪ Net hourly wages	▪ Simultaneous static and dynamic RE Probit Static = without lagged employment Dynamic = lagged employment included Specifications of the RE model: 1: Static (93–94) 2: Dynamic (93–94) 3: Dynamic, long–term effects of PSFT–IM (93–94) 4: Dynamic, long–term effects of PSFT–IM, preprogramme dummy (93–94) 5: Static (90–94), long–term effects 6: Static (90–94), long–term effects, preprogramme dummy 7: Dynamic with long–term effects of PSFT–IM (90–94) 8: Dynamic, long–term effects of PSFT–IM, preprogramme dummy (90–94) Difference–in–differences (4, 6, 8)	1. RE (displayed for PSFT–IM variables only) ▪ Employment Men / Women 1 short–term – – medium – – 2 short–term 0 0 medium 0 + 3 short–term 0 0 long–term 0 (+) 4 long–term + + 5 short–term – – long–term – – 6 long–term – – 7 long–term 0 0 8 long–term 0 + ▪ Wages: always 0

Table 1.5 Overview of micro–evaluations, further training (East Germany) (cont., Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Fitzenberger Prey (1998) (cont.)					▪ Differences in employment and wages with matching on propensity score and time varying covariates (nearest neighbour matching) ▪ Sensitivity analysis with reduced sample 93–94	2. Matching ▪ Employment: Men Women: Short–term – – Long–term 0 0 ▪ Wages Men Women: Short–term 0 0 Long–term 0 0
Lechner (1998)	GSOEP East 90– 94	N: 1,163 P 103	PSFT–IM	▪ Unemployment ▪ Full employment ▪ Real gross wages	▪ Nearest neighbour matching – propensity score (Probit estimate) and – additional time varying covariates ▪ Average differences in matched sample	Unemployment: Short–term: + Long term: 0 Full–employment: Short–term: – Long–term: 0 Wages: 0
Hujer, Wellner (2000)	GSOEP East 90–94 (Unbalanced panel)	N: 1632 P: 142 (231)	PSFT–IM	▪ Employment duration after programme (1) ▪ Reemployment probabilities after programme (2)	▪ Nearest neighbour matching with – propensity score (RE Probit with time varying covariates before begin of programme) – further matching variables ▪ Mixed proportional discrete time hazard rate model in matched sample	Short–term courses: Reemployment chances 0 Employment duration 0 Long–term courses: Reemployment chances 0 Employment 0
Kraus, Puhani, Steiner (1999)	LMM East 90–94 2 Periods: 90–92 92–94	Participation N: 3,503 Unemployment N: 3,095 Training N: 1,744 (spells) Estimate on % of total period 90–2 92–4 men 24,2 12,8 women 35,6 27,4	PSFT–IM	Transition rate from unemployment or training to (stable and unstable) employment	▪ Controlling for observables; ▪ Preprogramme test ▪ No differences in non–observables (after comparison) ▪ Discrete hazard rates	Women 1st: – Women 2nd + Men 1st: – Men 2nd +
Lechner (1999)	GSOEP East 90–94	N = 1105 P = 131	OFT	▪ Differences in unemployment rates (in %) ▪ Differences in gross monthly wages	▪ Nearest neighbour matching (2 alternatives) a) Matching on p–score (time invariant variables) and time varying covariates b) Matching on p–score estimate (invariant and varying covariates) ▪ Nonparametric differences in matched sample	Unemployment: 0 Wages 0
Bergemann, Fitzenberger, Schultz, Speckesser (2000)	LMM SA 91–98	N = 4656 P = 920 P (2nd) = 184	FT reiterated treatments	▪ Employment rate	▪ Nearest neighbour matching with propensity score ▪ Difference–in–Differences	Employment rate early period (1992) – late period (1994) – Reiterated treatment 0

Table 1.5 Overview of micro–evaluations, further training (West Germany) (Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Pannenberg (1995)	GSOEP West 84–91	OJT OFT N: 1965 715 P: 308 26	FT	Transition rate from unemployment (for OJT, among others)	▪ Discrete hazard rate (off–the–job) ▪ Probit and logit estimates ▪ Linear panel estimate with fixed effects (wages)	(Displayed for PSFT–IM only): OFT * income maintenance (=PSFT–IM): 0
Hujer, Maurer, Wellner (1997a)	GSOEP West, 86–94	N: 827 P: 100	FT	Transition rate from unemployment	▪ Random effects probit for propensity score ▪ Treatment dummy in Outcome equation replaced by propensity score (estimated from 1) to account for selection (IV approach) ▪ Discrete hazard rates with unobserved heterogeneity	Short–term courses: + Long–term courses: 0
Hujer, Maurer, Wellner (1997b)	GSOEP West 84–94	N: 1180 P: 113 (218 controls)	FT	Transition rate from unemployment	▪ Matched samples (oversampling) with 1) Propensity score (probit estimate with time invariant characteristics) 2) Time varying covariates (1– 12 months before the begin of the measure) ▪ Discrete hazard rates	Short–term courses: + Long–term courses: 0 → +
Prey (1997)	GSOEP West 84–93	N: 7522 P: 134 (1985) (FT)	PSFT–IM (among others)	▪ Employment probability	Simultaneous, dynamic RE probit model with preprogramme test	Long–term effect (for PSFT–IM only): Men: – Women: 0
Staat (1997)	GSOEP West 84– 94	Employment, Job Search N: 1702 P: 311 Wages: N: 1569 P: 247	PSFT–IM	▪ Job search duration ▪ Employment stability ▪ Wages	Age group specific control groups, ▪ Probit regression for participation ▪ Outcome variable: Search and employment duration, both ordered probit model ▪ Treatment dummy replaced by the estimated propensity score (IV approach) to control for selection	▪ Search duration: 0, except for Aged > 45: – Low skilled: – Women: – ▪ Employment stability: 0, except for low skilled: + ▪ wages: + only for women
Hujer, Maurer, Wellner (1998)	GSOEP West 84–94	N: 934 P: 219	FT	Transition rate from unemployment	▪ Nearest neighbour matching by Propensity score: RE probit on time constant variables and Time variable covariates of labour market status before start of scheme ▪ Discrete hazard rates w. unobserved heterogeneity	Short–term effects: + Long–term effects: 0

Table 1.5 Overview of micro–evaluations, further training (West Germany)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Schömann, Becker (1998)	German Life history study (59–83)	Men: N: 1089 Women: N: 1082	FT	Income for workers – without mobility – with internal mob. – or external mob.	Linear panel estimate, Treatment dummy replaced by participation probability in training estimated in a Cox–model	Men / Women: Internal mob. + / 0; External mob. 0 / +; No mobility + / 0
Prey (1999)	GSOEP West 85–94	N: 469 P: 42 (PSFT–IM)	PSFT–IM (among others)	▪ Employment probability ▪ Wages	Simultaneous, dynamic RE probit model with preprogramme test	Long–term effects (for PSFT–IM only): Employment: – Wages: –
Bender, Klose (2000)	IAB Employment Statistic	Specified subsample P: 1150 CTRL (pre–match): 9440 Matched pairs: 878	PSFT–IM	▪ Unemployment post training ▪ Employment duration after re–ntering	▪ Selection of control groups Matching on the basis of socioeconomic indicators	Long–term effects: +

Notes

FT	Further training	–	positive effect	GSOEP	German Socioeconomic Panel	OLS	Ordinary least squares
						LSDV	Least squares dummy variables
OFT	Off–the–job training	0	insignificant effect	LMM	Labour Market Monitor East		
OJT	On–the–job–training	+	negative effect	LMM–SA	Labour Market Monitor Saxony–Anhalt	2SLS	Two stage least squares
PSFT	Publicly sponsored further training		with respect to the outcome variable	N	Sample size	FGLS	Feasible Generalised Least Squares
JC	Job creation			P	Size of the treatment group	IV	Instrumental variables
WS	Wage subsidy			CRTL	Size of the control group	RE	Random effects probit estimates

1.4.1.3 Job creation

Data and period

Except for the most recent microeconomic evaluation for JC on the basis of the official employment register for East and West Germany (Hujer, Caliendo, Thomsen 2003), basically all previous empirical evidence for the effects of job creation refer to the East German situation at the beginning of the 90's. These evaluation studies use data provided by the Labour Market Monitor Sachsen–Anhalt (LMM–SA) or by the LMM for East Germany and evaluate the effectiveness of the policies with respect to employment. Because of the extensive implementation of JCs in the period after 1991, these data offer sufficiently big treatment group for a credible evaluation of JC. Evaluations on income effects of JC have neither been carried out in East nor in West Germany so far.

In the following, we discuss all available evaluation studies for JC. In most cases, there is no explicit distinction which programme is evaluated (either SAM or ABM) except for the recent study by Hujer, Caliendo, Thomsen (2003) who evaluate the effects of job creation in East and West Germany following the ABM regulation.

The evaluations by Steiner, Kraus (1995), Kraus, Puhani, Steiner (2000), Hübler (1997) evaluate the short–term and medium term outcomes on employment (up to 94). The more recent evaluations by Eichler, Lechner (1998) and Bergemann et al. (2000) with the Labour Market Monitor Sachsen–Anhalt cover a longer period of observation (up to 1998), but only for one of five East German regions. The most recent study by Hujer, Caliendo, Thomsen (2003) evaluates outcomes for an entry cohort into JC in the year 2000.

Outcome variables

Steiner, Kraus (1995) and Kraus, Puhani, Steiner (2000) focus on the transition rate from unemployment to employment and implement discrete hazard rate estimations of the transition probability to employment. Hübler (1997) evaluates the differences in the transition probability to employment, unemployment or inactivity at the end of the observation period (November 94) with the same data set and an almost identical subsample. The study estimates the differences in employment on the basis of multinominal logit models and probit estimates. Eichler, Lechner (1998) evaluate the outcomes on the basis of matched individuals of the treated and non–treated populations and compare the average differences in unemployment within matched samples. Bergemann et al. (2000) and Hujer, Caliendo, Thomsen (2003) estimate the impact on employment within matched samples applying propensity score matching and difference–in–differences estimators.

Methodology

In their first evaluation study on JC in East Germany, Steiner Kraus (1995) control for selectivity, by the specification of different reference groups on the basis of observable information and compare the different outcomes of the treated compared to the non–treated control group. They implement specific preprogramme tests to control for the individual differences in employment prospects before the start of the programme, but cannot find differences between the treated and non–treated individuals, so that they conclude that a "sufficient control on unobserved heterogeneity" is achieved.

In their second evaluation on JC in East Germany, Kraus, Puhani, Steiner (2000) implement an extended approach to control for selectivity based on observable characteristics. Unobserved heterogeneity is not considered. As the preprogramme differences in the outcome variable are again insignificant, they estimate the impact on the hazard rate to employment.

Hübler (1997) applies different procedures to control for selectivity: By that, the results clearly show the sensitivity to the method of correction for selectivity. In different specifications, the selection is corrected by either controlling on observables (1), simultaneous parametric random effects estimates with preprogramme test (2) and additional "restrictions" of the naive control group by the application of "matched samples" based on a treatment estimate (random effects probit) and further variables.

Eichler, Lechner (1998) correct for selection bias based on observable characteristics by nearest neighbour matching on the propensity score, which they estimate by a parametric probit model. Preprogramme differences in employment probabilities of treated and non–treated individuals are not taken into consideration, and unobserved heterogeneity across the treated and non–treated individuals is assumed to be stable. The most recent studies by Bergemann et al. (2000) and Hujer, Caliendo, Thomsen (2003) implement matching approaches on the propensity score, too.

Results

As in the case of evaluation studies of further training, the results suggest rather a failure of the effectiveness of JC with respect to the employment prospects of the treated compared to the non–treated individuals: Steiner and Kraus (1995) found significantly positive employment effects for men 12 months after the end of the treatment. However short–term effects for men were significantly negative as well as the short– and long–term effects for women. Due to the restricted period of observation (90–2), this evaluation has to be interpreted as preliminary compared to the other evaluations with a longer period (usually up to 94).

Hübler (1997) uses the same data, however evaluates the effects for several different employment outcomes. Long–term effects of JC on employment, unemployment and inactivity are estimated by multinomial logit estimates for the employment status at the end of the period of observation (November 94). This study finds positive treatment effects on employment in the short as well as in the long–run. However, there in is no significant effect on the labour market participation in general and a negative effect on activity in the long–run. When controlling on observables, the results are almost the same: The long–term positive effects of JC remain, i.e. treated individuals are more likely to be employed in November 94 than non–treated when controlling on observables. Within matched samples, Hübler (1997) finds that both short and long–term effects are no longer significant for women. For men, there are negative outcomes in both the short and the long run.

Eichler, Lechner (1998) find contrasting results in their nonparamentric evaluation of average differences of the unemployment within matched samples. According to their analysis, job creation increases employment rates for men and for women, both in the short and in the long–run. However, JC shows also negative effects on the participation in the labour force for women – a partially opposing effect.

Kraus, Steiner, Puhani (2000) conclude that the transition probability from unemployment to employment is significantly lower for men and women if being treated. The results hold true for both periods of measurement, 90–2 and 92–4.

Bergemann et al. (2000) also find negative effects in the short– as well as in the long–run, however the negative effects becomes insignificant for treatments starting in the later years. Hujer, Caliendo, Thomsen (2003) find negative employment effects for all participation groups in the short–run, however in the long–run, they do no longer find significantly different employment rates. Surprisingly, these results hold for both West and East Germany.

Table 1.6 Overview of micro–evaluations, job creation (East Germany) (Notes: end of table 1.5)

Authors	Data Period	No. Of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Steiner, Kraus (1995)	LMM East 90– 1992	N: 2179 (unemployed) P: 582 Split samples for men and women	JC	Transition rates to employment and unemployment	▪ Different groups (“reference groups) as control groups specified by socioeconomic and previous unemployment characteristics for men and women ▪ Discrete hazard rates with ordered Logit models	Outflows to employment: Men: ▪ short–term: 0 ▪ after 12 months + Women: ▪ short–term: – ▪ long–term: –
Hübler (1997)	LMM East 90–94	N: 2886	Simultaneous evaluation of JC and PSFT–IM (among others)	Employment (compared to unemployment and inactivity as specified in the last column)	▪ Multinominal logit: Employment status 1994 (without treatment determinants) ▪ Random Effects Probit Estimates Different evaluation approaches – controlling on observables – random effects estimates – preprogramme test and – matched sampling approaches	JC and PSFT–IM only ▪ Without determinants: 1. M–Logit Estimates: Unemployment: Short–term long–term PSFT–IM: + + JC: 0 + 2. M–Logit Estimates: Inactivity : Short–term long–term PSFT–IM: + + JC: 0 0 ▪ Controlling on observables: Long–term outcomes compared to ... Unemployment Inactivity PSFT–IM: + 0 JC: + – ▪ RE Probit on employment Men Short–term long–term PSFT–IM: – + JC: – – Women Short–term long–term PSFT–IM: – – JC: 0 0

Table 1.6 Overview of micro–evaluations, job creation (East and West Germany) (Notes: end of table 1.5)

Authors	Data Period	No. Of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Eichler, Lechner (1998)	LMM SA 91–97	Spells: No–P: 12,565 P: 1,123	JC	Unemployment	▪ Nearest neighbour matching on propensity score (Probit) and Time varying covariates immediately before the begin of a programme ▪ Intertemporal Stability of selection on observables assumed ▪ Nonparametric difference–in–differences	Men / Women Short–term: – / – Long–term – / –
Kraus, Puhani, Steiner (2000)	LMM East 90–94 2 periods: 90–92 92–94	Participation model N: 3503 JC model N: 718 Unemployment model N: 3095	JC	Transition rates to (stable or unstable) employment or inactivity	▪ Controlling for observables ▪ Error component specification for outcome and selection equations concerning unobserved heterogeneity: negligible ▪ Discrete hazard rates with unobserved heterogeneity	In both periods: Men– Women –
Bergemann, Fitzenberger, Schultz, Speckesser (2000)	LMM SA 91–98	N = 4656 P = 606 P (2nd) = 146	JC reiterated treatments	▪ Employment rate	▪ Nearest neighbour matching with propensity score ▪ Difference–in–Differences	Employment rate ▪ Early period (1992) – ▪ Late period (1994) 0 ▪ Reiterated treatment 0/+
Hujer, Caliendo, Thomsen (2003)	Programm and register data	P: 11,376 CRTL: 232,399	JC	▪ Registered unemployment ▪ Job seeking	▪ Nearest neighbour matching based on the propensity score ▪ Average differences in matched samples	Short–term effects Men / Women East: – / – West – / – Long–term effects Men / Women East: 0 / 0 West: 0 / 0

1.4.1.4 Other programmes

Some few microeconometric evaluations exist for programmes other than JC and PSFT. Table 1.7 summarises the findings of these studies. Basically, there are two evaluations focusing on the support of self–employment (Pfeiffer, Reize 1999, 2001), one on the effects of various types of wage subsidies on the employment performance of firms (Hujer, Caliendo, Radic 2001) and one on the temporary integration of unemployed in non–profit temporary work schemes (Almus et al. 1999). All evaluations are based on administrative data recorded in the implementation process of the programmes, which are not available from the survey data like the GSOEP and the LMM.

Pfeiffer, Reize (1999) evaluate the outcomes of the bridging allowance programme, the temporary income maintenance for unemployed who take up new self–employment. On the basis of the start–up panel of the Center for European Economic Research (ZEW), the authors evaluate the effect of the subsidy on the survival of business start–ups. The sample is restricted to firms with a maximum of 18 employees at the time of the start–up, which are located in selected local employment office districts in Germany. The treatment group comprises 124 start–ups in West and 196 in East Germany. Selection bias taken into consideration by simultaneously estimating the survival probability (modelled as a probit model) and the probability of being subsidised, so that the estimation controls for the correlation of the error terms between the two estimated forms. As a result, the authors do neither find significant gains in the survival probability nor negative effects.

In a follow–up study, Pfeiffer, Reize (2001) evaluate the outcomes of supported training on the probability of employment and self–employment with data taken from the Federal Institute of Occupational Training for the period 90–92 in West Germany. The outcomes are estimated as the likelihood to become self–employed in a probit model, which corrects for selectivity by including the inverse of the Mills' ratio into the outcome equation.

A most recent study by Hujer, Caliendo, Radic (2001) focuses on the effects of wages subsidies on the employment performance of firms and applies data from the German IAB establishment panel. This data set, a yearly survey of more than 4,000 establishments in West Germany, allows to estimate the employment effect for different skills groups. Unfortunately, the firm data are not rich enough to analyse the effects of different types of wage subsidies as described above under section 1.2.4.2 and pools either wage–subsidies, job–creation or structural adjustment programmes. The study uses the matching approach based on observable characteristics and implements either nearest (i) neighbour matching without replacement , (ii) nearest neighbour matching with additional covariates or (iii) kernel matching. Within the matched samples of – the unit of interest is the establishment – the authors implement difference–in–differences estimators (DiD), so that further time constant selection bias based on unobservable characteristics is controlled for by differencing. The DiD–approach estimates the effects of wages subsidies on the actual employment of the firm in different skills groups for a period up to three years after the implementation of wage subsidies (1997–9). The effects are in all cases and for all skill groups insignificant.

The last study cited under this section is an evaluation of the non–profit temporary work programme in the West German region Rhineland–Palatinate. Treated individuals of the programme are compared with controls from administrative data with respect to the average difference in unemployment after treatment.

In order to obtain adequate controls from the administrative data covering 144,000 individuals for the period of observation (96–8), this study applies nearest neighbour matching on the basis of the propensity score and further time varying covariates and estimates the outcomes nonparametrically: In the short–run, this study finds a positive programme effect, which however do not hold in the long–run.

Table 1.7 Overview of micro–evaluations, other programmes (Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Pfeiffer, Reize (1999)	▪ ZEW Panel ▪ official data East and West 15 districts 93 – 95 Firms < = 18 employees	Survival probability: West East N: 2461 2240 P: 124 196 Employment: West East N: 1879 1788 P: 105 179	Income maintenance for new self–employed (“Bridging Allowance”)	▪ Survival of start–up ▪ Employment	▪ Controlling for observables ▪ Probit for the existence of adequate information on employment Probit model on survival and subsidisation	West East Survival 0 0 Employment 0 0
Pfeiffer, Reize (2001)	QOC 91–92 W.–Germany	N: 3964 (new employees and self–employed)	FT	Income of ▪ new self employed ▪ new dependent employees	▪ Effects of training on business–start up (Probit) ▪ Effects of incomes with correction of the selection bias by the inverse of the Mills ratio ▪ No correction of unobserved heterogeneity	Influence on start–up: + Effects on income: 0
Almus, Egeln, Lechner, Pfeiffer, Spengler (1999)	Prg. data Jo seekers data 96–98	P: 134 CRTL: 144.002 (Potential controls)	Temporary work for reintegration	Re–Integration into regular employment	▪ Pre–match on the basis of a first probit (time constant variables) ▪ Nearest neighbour matching on propensity score (Probit estimate) and time varying covariates ▪ Differences in matched sample	Short–term: + Long–term: 0
Hujer, Caliendo, Radic (2001)	IAB Establishment Panel	N: 1700 (firms) P: 87	Wage subsidies	Actual (i) low, (ii) medium and (iii) high skilled employment of the firm one, two and three years after the programme	▪ Matching on propensity score (either nearest neighbours, nearest neighbours with additional covarites or kernel matching) ▪ Further differencing within matched samples	(i) 0 (ii) 0 (iii) 0 in short, medium and long–run

1.4.2 Evaluations of macroeconomic outcomes

Many estimations of macroeconomic outcomes of ALMPs are cross–national comparisons (OECD 1993, Kraft 1994, Bellmann, Jackman 1996b, Schmid 1995) on the basis of internationally comparable data of unemployment and vacancies. Although these studies also cover Germany, they bring about only imprecise evidence for the functioning of ALMP, which is usually modelled by the level of expenditure for ALMP as percentage of the national gross domestic product. A further differentiation of ALMP is not taken into consideration and only few policy recommendations can be gained from these studies. Besides, the problems concerning the international comparability of data and institutional frameworks of ALMP, these evaluations in most cases do not include information on the design of ALMP and the different programmes. Therefore, we focus on studies, which were explicitly estimated the macroeconomic outcome for Germany on the basis of regional data.

For aggregate outcomes, there are in principle two different approaches to estimate the impact of ALMP, either on a general equilibrium or reduced form approaches identifying effects on outcomes such as the Beveridge curve and the wage setting framework (see section 3 of this paper for an overview, Layard, Nickell, Jackman 1991).

Since the beginning of the 90's, eight macroeconomic evaluation studies have been published estimating the effectiveness German ALMP: Bellmann, Lehmann (1991) evaluate outcomes of ALMP on the aggregate regional outflows for specific groups of short–term and long–term unemployed men. Pannenberg, Schwarze (1996) estimate an extended wage curve approach and evaluate, to which extent the average regional wages are affected by the level of ALMP. Büttner, Prey (1998) and Prey (1999) evaluate the outcomes of ALMP on the regional mismatch. Schmid, Speckesser, Hilbert (2001) evaluate policy outcomes on regional structural unemployment, the effect of ALMP on the regional level, share and change of unemployment as well as on the aggregate regional outflows from unemployment. Hagen, Steiner (2000) evaluate the effects of ALMP on the regional level of unemployment for East Germany for the years 93–99 by estimating separately the effect on inflows and outflows. In the follow–up study by Hagen (2003), the author estimates the macroeconomic effects of ALMP in East Germany with three different approaches either for the regional matching efficiency (inflows into regular employment), the regional job seeker rates (Beveridge curve) or the dynamic labour demand on the same data basis, however with quarterly data for the years 1998–2001. Hujer, Blien, Caliendo and Zeiss (2002) estimate the macroeconomic employment outcome of ALMP on the regional job–seekers rate with regional data for West and East Germany for the same period

The control for endogeneity is crucial for the results of these evaluations, as indicated in section 3.4 of this paper. The authors cope with endogeneity by the application of different approaches: Bellmann (1991), Pannenberg, Schwarze (1996), Schmid, Speckesser, Hilbert (2001) and Hagen, Steiner (2000) implement fixed regional effects in the estimations to control for endogeneity. Büttner, Prey (1998) and Prey (1999) use additionally to fixed effects instrumental variables. Hagen (2003) and Hujer, Blien, Caliendo and Zeiss (2002) control for endogeneity by several instrumental variables estimators for dynamic panel models, including the estimator developed by Arellano, Bond (1991) and Blundell, Bond (1998).

ALMP is usually modelled in form of *accommodation ratios*. These ratios indicate the participation stocks in each ALMP programme as a percentage of the whole regional extended unemployment, i.e. the total programme participants and the total regional unemployment, and hence, they describe the regional programme level applied to the group of (potential) participants – the whole non–employment in the region. Pannenberg, Schwarze (1996) focus on further training, Prey (1999) and Büttner, Prey (1998), Hagen, Steiner (2000), Hagen (2003) and Hujer, Blien, Caliendo and Zeiss (2002) evaluate the outcomes of the two main programmes JC and PSFT. Bellmann, Lehmann (1991) and Schmid, Speckesser, Hilbert (2001) estimate the outcomes of JC, PSFT and targeted wage–subsidy programmes for the long–term unemployed.

Bellmann, Lehmann (1991) conclude that JCs significantly promotes outflows from short–term unemployment. The two other programmes, wage subsidies and further training have neither a positive nor a negative impact on the outcome variables, neither in the short–term nor in the long–term. Pannenberg, Schwarze (1996) focus on the outcomes of further training on wages in East Germany. The impact of ALMP in this context can be interpreted as successful, because the regional level of PSFT has a lowering effect on wages. In one of their estimations on regional unemployment outflows, Schmid, Speckesser, Hilbert conclude that JC did not significantly affect for the overall outflows from unemployment and that PSFT has a slightly decreasing impact on this outcome, which is comparable to the result by Büttner, Prey (1998) and Prey (1999) for the dynamic specification of the disequilibrium model, which indicates that further training had an increasing effect on regional mismatch. On the other hand, Schmid, Speckesser, Hilbert (2001) find significantly negative effects of PSFT on the regional level and structure of long–term unemployment, which is the main target group of ALMP and conclude that PSFT has a decreasing impact at least for the problem groups on the labour market in the short–run. Prey (1999) finds overall decreasing effects of JC on the regional mismatch. In separate specifications for men and women, the effect of JC can no longer be found for the subpopulation: significantly negative effects on regional mismatch can only be found for men due to the JC programme, while PSFT has no effects for men and has even an increasing effect for women. Hagen, Steiner (2000) find a positive effect of ALMP on the level of unemployment in the long run, i.e. the unemployment level increased. Hujer, Blien, Caliendo and Zeiss (2002) estimated separately the effects on mismatch for East and West Germany. They find a reducing effect of further vocational training and job creation in West Germany on the job–seekers rate, whereas for Eastern Germany no significant effect could be found. Hagen (2003) finds a negative effect of job creation on the regional matching function and no significant effects of further training and structural adjustment schemes as well as insignificant effects for all different programmes of ALMP in the long–run based on the estimation of the Beveridge curve. The estimation of the labour demand function shows that further training has no effects on employment, but job creation leads to significant displacement effects.

Table 1.8 Overview of macro evaluations (Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Bellmann, Lehmann (1991)	Employment offices districts 79–88 (quarterly data)	N: 142	PSFT, JC and WS	Unemployment duration specific outflow rates (five categories)	▪ Linear estimate with regional fixed effects ▪ Hausman Test to control for endogeneity	JC / PSFT / WS STU: – / 0 / 0 LTU: 0 / 0 / 0
Pannenberg, Schwarze (1996)	E.–German employment offices districts 92–94	N: 35	PSFT	Aggregate wages	▪ Influence of job searcher rate on unemployment ▪ Wage curve estimates controlling for endogeneity: OLS fixed regional effects (1) FGLS with random individual and fixed regional effects (2) IV(2SLS) with fixed regional effects (3)	Results for wage increases: (–) (–) (–)
Büttner, Prey (1998) (extended by Prey [1999])	W.–German Planning Regions 86– 93	N: 74	JC PSFT	Mismatch: labour market disequilibrium	▪ Estimation of "transacted labour " and determination of the regional jobless rate ▪ Estimate of matching efficiency with OLS, LSDV, 2SLS, Dynamic Panel (GMM) ▪ Controlling for endogeneity – fixed regional and time effects – IV and regional labour market structure	JC: – PSFT: 0 Extended by Prey (1999)
Prey (1999)	W.–German Planning Regions 86– 93	N: 74	See Büttner, Prey 1998	Mismatch: labour market disequilibrium	See Büttner, Prey 1998; Further specifications: ▪ controlling for regional age structure and recipients of social assistance ▪ separate accommodation of ALMP for men and women	▪ Extended model: (several specifications separated by /) JC / PSFT Static: 0/+ / 0/0 Dynamic: –/–/– / 0/+/0 ▪ ALMP accommodation for men/ women (specifications separated by /) 1. static: JC / PSFT Men: 0 / 0 Women: 0 / 0 2. dynamic: JC / PSFT Men: –/– / 0/0 Women: 0/0 / +/+

Table 1.8 Overview of macro evaluations (cont., Notes: end of table 1.5)

Authors	Data Period	No. of Observations	Programme	Outcome Variable	Evaluation Methodology	Results
Schmid, Speckesser, Hilbert (2001)	W.–German employment offices districts 94– 97	N: 142	PSFT, JC and WS	1) Long–term unemployment (groups with unemployment of > 6 months or > 24 months) 2) Unemployment outflows	▪ Linear estimate with regional fixed effects ▪ Linear estimate with first differences	1) Long–term unemployment (LTU) ▪ as % of total labour force JC PSFT WS > 6 0 – 0 > 24 0 – – ▪ as % of all unemployed JC PSFT WS > 6 0 – 0 > 24 0 (–) 0 ▪ Change of LTU JC PSFT WS > 6 0 0 – > 24 0 0 – 2) Aggregate Unemployment Outflows: PSFT: – JC: 0 WS: 0
Hagen, Steiner (2000)	E.–German employment districts 93–99	N = 35	PSFT and JC	regional job matching	Linear estimate with regional fixed effects	short–run 0, long run: + flows into of unemployment + flows out of unemployment +
Hujer, Blien, Caliendo and Zeiss (2002)	E.– and W.–German employment districts	N = 175 (N = 141 West. N = 34 East, quarterly data)	PSFT and JC	regional job–seekers rate	▪ Fixed effects ▪ 2SLS ▪ Dynamic panel models (System GMM)	West Germany ▪ further training – ▪ job creation – East Germany 0
Hagen (2003)	E.–German employment districts 98–01	N = 35 quarterly data	PSFT, structural adjustment and JC	1) matching efficiency 2) job seeker rate 3) Labour demand	▪ Fixed effects ▪ GMM ▪ 2 SLS	1) Matching: ▪ PSFT: 0 ▪ JC: – ▪ SAM: 0 2) Job seekers rate: ▪ PSFT: 0 ▪ JC: 0 ▪ SAM: 0) Labour demand: ▪ PSFT: 0 ▪ JC: –

1.5 Conclusion

This paper provides an overview of the evaluation of ALMP in Germany of the last years. The following results can be summarised:

- ALMP in Germany consists of a wide range of different programmes for different target groups. Some of these programmes serve the same target groups and work with the same or very similar incentive structures. Others programmes are complementary concerning the integration targets the seek to fulfil. After the reform of the legal basis of ALMP in Germany, the local offices of the German Employment Service gained a new flexibility in linking programmes better to the regional conditions. Therefore, we expect more heterogeneous treatments in the future as well as a narrower targeting towards the groups with severe problems of finding work.
- With respect to the methodology of ALMP evaluation, we conclude that the solution of the microeconomic evaluation problem in non–experimental evaluation implements different approaches either applying parametric correction on observables or – especially of late – the popular statistical matching approaches. Even though matching techniques based on the propensity score became more important over the last years, there is no “best practice” how one should cope with selection bias based on observable characteristics. Besides, there are wide controversies in the literature how selection bias based on unobservable characteristics can be taken into consideration in the design of evaluation studies and how the dynamic reduction of the employment rate before treatment should be considered in the evaluation of outcomes (“Ashenfelter’s Dip”).
- The recent methodological developments in the context of European ALMP, especially whether one can use a random variation in the starting dates of ALMP programmes as an alternative identifying assumption (“timing–of–events”) or how an adequate control group can be obtained if the timing of the non–treatment needs to be considered, have not yet been explicitly modelled in the evaluation studies for Germany. Timing–of–events could be an instrument to model both the participation decision and the outcome simultaneously, however assuming that anticipation of the programme does not occur (which is unlikely in the context of generous ALMP programmes). Furthermore, the recent methodological literature forces evaluation research to reconsider the problem of finding a sufficient control group *without conditioning on the future outcome* and without the implicit assumption that a control observation remains a control observation. With respect to this, the findings of the ALMP evaluations for Germany need to be interpreted as evaluations that analyse treatments for a *specific period,* for which the timing of treatment and non–treatment matches sufficiently and only if a current control group is interpreted adequately as the control group for a time–specific treatment. These methodological constraints need to be considered if one tries to draw any inference from the evaluation studies.

- When paying attention to the methodological constraints of the recent German literature of ALMP evaluation, we can summarise the following findings:

  1. For East Germany, only few studies found positive employment effects of ALMP for certain subpopulations, which however are not consistent across all different specifications. In most studies, only insignificant or negative employment effects could be found.
  2. For West Germany, the policy effects of ALMP are vague because of the fact that most studies evaluate the effects of both public sector and privately financed further training. Those studies exclusively evaluating the effects of public sector sponsored training (Prey 1997, 1999 and Staat 1997) show – with the exception of low skilled workers – on average negative employment effects.
  3. The most recent evaluations of job creation programmes shows negative effects on employment. In East Germany, only Eicher/Lechner(1999) find positive effects of job creation, all other evaluations indicated that job creation reduces the employment chances of the treated.
  4. The macroeconomic outcome of ALMP found in most studies is significantly negative. In some studies, further training seems to decrease the regional rate of long–term unemployment, whereas job creation programmes show in some case significantly positive effects on the matching.

The conclusion with respect to the effectiveness of ALMP, which can be drawn from this survey, however should be interpreted with care. Especially, we need to restrict the interpretation in the following way:

1. As shown in the first part, we see a wide range of ALMP in Germany. However, only job creation and further training have been subject to evaluation so far, and this mainly for the East. For a large number of ALMP programmes, there does not exist any empirical evidence, e.g. there is only one evaluation study dealing with the effects of wage–subsidies (Hujer, Caliendo, Radic 2001).
2. The overview of the different evaluation studies could clarify that a unique way of solving the problems of selection on observable and unobservable characteristics does not exist. The different approaches how selection bias based on observable characteristics can be supposed to have an important influence on the evaluation results, especially if the recent methodological debates are considered that disallow to interpret most evaluation results without further assumption than the CIA (Fredricksson, Johansson 2003).
3. As important as different methodological approaches how to evaluate ALMP are the big differences in the data mainly consisting of small sections of the survey data provided by the GSOEP and the LMM. With few exceptions, it is impossible to exactly mirror the institutional regulation of the treatments with these data: The category "further training" basically consists of a wide range of either privately or public sector financed training. And even if one could clearly distinguish publicly from privately initiated further training, the treatments are heterogeneous with respect to the duration of training, they can

offer short– or longer term treatments for more or less firm specific skills, they might consist of basic occupational knowledge or the provision of limited skills and techniques. To the authors' opinion, the policy conclusions of an evaluation of such broad policy categories remain unclear.

While considering these constraints, the implication one can conclude from the cited studies is weak. However, we believe that the average employment effect of both programmes (further training and job creation) at best are insignificant, because otherwise they should have been more obvious across all the cited studies – even if the underlying data are different and the methodology chosen to overcome the identification problem influences the results to a certain extent.

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2 Evaluation of further training in East Germany based on observational data

2.1 Introduction

In the absence of experimental data, any empirical evaluation of programmes of active labour market policy (ALMP) has to address difficult methodological issues in identifying the programme impact for the participants because the situation of non–treatment is not observable. Both selection bias based on observable and unobservable characteristics prevent from using the non–treatment population or the situation of the treated before treatment as the non–treatment outcome of the treated.

For the construction of an adequate comparison group for participants, the vast literature on evaluation offers quite a range of different procedures, and in recent years, the use of non–parametric matching approaches based on the propensity score became to some extent the standard procedure in non–experimental evaluation because of only few functional form assumptions of the problem of selection bias based on observable characteristics compared to e.g. regression based correction for selection on observables. However, critical parameters need to be set for implementing matching, too, and the purpose of this paper is to provide a sensitivity analysis about the changes of the outcomes if certain parameters in matching approaches are varied, especially the (i) matching estimator itself and the (ii) bandwidth. Furthermore, we show how the outcomes change if we draw inference based on (iii) a bootstrap procedure taking into account that the propensity score is itself estimated. We check the sensitivity of the results of a Difference–in–differences estimator that additionally to selection on observable can take account of remaining selection bias based on unobservable characteristics and which proved to be a very effective tool in controlling for both selection on observables and unobservables (Heckman et al. 1998 refer to various studies for the U.S. in which he compares the properties of DiD estimators in matched samples based on the propensity score with those found in experimental evaluation approaches).

The data we refer to in this paper are of the Labour Market Monitor Sachsen–Anhalt (*Arbeitsmarktmonitor Sachsen–Anhalt* LMM–SA) and we estimate the programme effect for a first or a second, reiterated participation in the further training programme in East Germany, which is effectively the most important ALMP programme in Germany. Although the specification of the outcome equation in form of a DiD–estimator considers to a certain extent selection on unobservables – and can also depict that shortly before the participation in a labour market program the employment situation of the future participants deteriorates disproportionately – the estimated model is not flexible enough to take into account the dynamic nature of the employment process. Therefore, we implement an outcome equation in the associated paper (Bergemann, Fitzenberger, Speckesser 2004), which estimates difference–in–differences for hazard rates and evaluates employment effects controlling for the path dependency of the employment history of both treated and non–treated individuals. With regard to the purpose of this paper, the specification however is acceptable because it focuses on learning with "real world data", how and to which extent the choice of critical parameters influences the estimated policy impact of the programme.

The remainder of the paper is structured as follows: Section 2.2 gives a short description of ALMP in Eastern Germany, which was carried out in accordance with the former Labour Promotion Act (*Arbeitsförderungsgesetz*, AFG) for the period of observation of the LMM–SA data. Section 2.3 discusses the microeconomic evaluation problem and describes, how matching approaches can help to overcome selection on observables. We describe nearest neighbour estimators – a special case of the category of non–parametric estimators – and kernel matching estimators. We briefly describe the parameter of interest of an evaluation of a reiterated participation in further training and how we estimate the outcomes in matched samples in the presence of further selection bias based on unobservables and a dynamic selection processes before the treatment. Section 2.4 implements the different matching approaches and compares the results, especially, how the precision of critical parameters influences the outcomes.

2.2 Institutional regulation

2.2.1 Further training

The public promotion of further vocational training as a scheme of ALMP was first regulated in the Labour Promotion Act (AFG) of 69. It was amended extensively between 69 and 97 mirroring the changing labour market conditions of the 70's. These amendments took into consideration structural change, persisting unemployment, increased female participation in the labour market and new financial constraints on the unemployment insurance. However, the three principal further training programmes remained until the complete revision of the labour promotion in 97/98: Further Training (*Fortbildung*), Retraining (*Umschulung*) and Integration Subsidies (*Einarbeitung*) were left unchanged (§§ 33 – 52). In the process of Economic and Social Intergration of the former GDR, the regulation of the AFG was implemented in the GDR as of July 90 (§ 249 AFG).

In 98, the AFG was replaced by the new Social Law Book III (SGB III). The redesign of ALMP programmes themselves led only to few new instruments; in general, the programmes of the former AFG remained in place (except the Integration Subsidy, which is now subsumed under "subsidised employment").

2.2.2 The Labour Promotion Act

Further vocational training (§ 41 AFG) includes programmes by which vocational knowledge and skills are assessed, maintained and extended or adapted to technical developments. These programmes offer opportunities for career advancement, provide a vocational qualification or enable the participants to work in other employment. Participants in full–time courses may be paid a subsistence allowance (*Unterhaltsgeld*) if the conditions of entitlement are satisfied. To qualify, the person must meet the requirement of being previously employed for a minimum duration during a set period of time, i.e. at least 1 year in contributory employment or receipt of unemployment benefit or subsequent unemployment assistance. This set period may be extended for persons returning to the labour market. The subsistence allowance amounts to 67% for participants with at least one dependent child, otherwise 60% of wages. A prerequisite condition in all cases is that participation in the training programme is "necessary". In cases where the person has not fulfilled the requirement of previous employment, but had received unemployment assistance until the start of the programme, a subsistence allowance equal to the rate of unemployment assistance may be paid. The BA may bear the costs of further training incurred directly through the training scheme, especially including course fees.

The target of the *retraining* (§ 47 AFG) is the promotion of a new basic vocational training (including a new certificate) for people who have already finished a first professional training. Such a promotion of training can be justified if industrial and/or occupational change lead to the obsolescence of skills. Additionally, personal reasons could justify retraining, if a person is no longer capable to work in his/her current position (e.g. health restrictions). The promotion of retraining is restricted to a maximum period of 24 months, for this period, the person may also be granted a subsistence allowance as for further training if the eligibility criteria apply.

The third instrument under the AFG are *integration subsidies* (§ 49 AFG). Under this regulation, employers receive wage subsidies to compensate for lower performance so that workers may be integrated into the labour market. This "familiarisation integration subsidy" corresponds to 30% of the total wage costs (i.e. the wages and the employers' share of the social insurance contributions) and can be granted up to a duration of six months, for the very difficult–to–place up to one years with a rate subsidy 50% of the wage costs. Participants under this scheme receive the standard negotiated salaries (usually based on collective agreements).

2.2.3 Participation

Participation in further training schemes started immediately after the unification: In the last three months of 90, already 98,500 persons participated. During the year 91 participation peaked and nearly 900,000 persons started a programme. After 91 participation entries continuously declined to a minimum of 166,000 in 97. In 99 there are still 183,000 persons entering a further training scheme (Table 2.1). The participants started any of the three subprogrammes, and the shares of these programmes seem to be relatively stable over the period of observation. After 98 the subtotals are no longer available due to the change in legislation (introduction of the SGB III).

Table 2.1 Participation stocks in further training in East Germany, 90 to 99

	90*	91	92	93	94	95	96	97	98	99
Total	98,561	892,145	887,555	294,153	286,928	257,463	269,227	166,031	235,959	183,317
Further Training	74,511	629,656	591,016	181,592	199,144	184,347	204,090	128,190	**	**
Retraining	19,408	129,862	183,089	81,460	68,569	52,756	48,102	27,258	**	**
Integration Subsidy	4,642	132,627	113,450	31,101	19,215	20,360	17,035	10,583	**	**

* Data for September–December 1990; ** not available

Source: Bundesanstalt für Arbeit (1993, 1997, 2000)

The participation stocks are comparable, although the annual stocks are of course lower than the entries for further training and integration subsidies (these programmes usually last less than one year). The figures of the stocks of participants in retraining are higher, indicating that retraining has an on average higher duration than the other programmes (Table 2.2).

Table 2.2 Participation entries in further training in East Germany, 90 to 99

	90	91	92	93	94	95	96	97	98	99
Total	**	279,800	491,200	380,608	258,945	255,796	238,904	183,570	151,034	143,356
Further Training	**	**	**	154,674	105,372	143,801	141,173	107,786	**	**
Retraining	**	**	**	221,540	150,780	107,198	92,886	72,092	**	**
Integration Subsidy	**	**	**	4,394	2,793	4,797	4,845	3,692	**	**

** Not available

Source: Bundesanstalt für Arbeit (1993, 1997, 2000)

If the participation figures of the first ten years after unification are added up, this would amount to 3.571 Million entries into these programmes. If the labour force of East Germany is assumed to be approximately 7.5 Million, this figure clearly shows the far reaching impact of further training in East Germany.

2.3 Non–experimental evaluation

2.3.1 The evaluation problem

Non–experimental evaluation is based on a "potential–outcome–approach" to causality (Rubin 1974). It states that the causal effect of treatment–on–the–treated can be identified by comparing the results of a programme (YT) for the participating individuals after the treatment $(D=1)$ with the hypothetical situation of the same individuals if they had not taken part in the programme $(YC|D=1)$. Thus, the average effect of treatment–on–the–treated is given by

(1) $E\{YT|D=1\}-E\{YC|D=1\}$.

The evaluation problem consists of estimating $E\{YC|D=1\}$ since the outcome in the non–treated situation cannot be observed for the treated individuals. In principle, two alternative approaches can be applied to estimate the average non–treatment outcome based on the situation of (i) programme participants before treatment (before–after–comparison) or (ii) a control group of persons which did not participate. The major drawback of the before–and–after comparison lies in the assumption of a constant average non–treatment outcome over time for the treated population. For instance, changes in the overall state of economy might lead to a violation of this assumption

(2) $E\{YC_{t0}|D=1\} \neq E\{YC_{t1}|D=1\}$,

where $t0$ denotes a point of time before treatment and $t1$ after treatment. Furthermore, the average value of the outcome of non–participants typically does not represent the correct average non–treatment outcome as participants and non–participants differ in characteristics which influences the outcome variable,

(3) $E\{YC|D=1\} \neq E\{YC|D=0\}$.

Thus, the participants differ from participants before treatment and from non–participants due to observable and unobservable characteristics giving rise to a selection bias.

2.3.2 Selection bias based on observable characteristics

To take account of the problem of selection on observables, the paper refers to the Conditional Independence Assumption (CIA) which implies that it does not make a difference whether one estimates the average results without treatment on the basis of persons of the participating or the non–participating group as long as they have the same characteristics X . Under the CIA, one gets

(4) $E\{YC|D=1,X\} = E\{YC|D=0,X\}$

indicating that treatment group and the non–treatment group which according to equation (3) are not comparable are now comparable on average when conditioning on X .

This approach allows us to estimate consistently the average effect of treatment–on–the–treated for the participants if they are compared to corresponding non–participants. Referring to the CIA, it is necessary to discuss the way how an appropriate non–treatment outcome can be estimated from the data. A very popular method of evaluating the effect of treatment–on–the–treated is the so called matching approach which "is based on the intuitively attractive idea of contrasting the outcomes of programme participants (denoted YT_i) with the outcomes of comparable non–participants (denoted YC_j). Differences in the outcomes between the two groups are attributed to the programme" (Heckman, Ichimura, Todd 1998, notation adjusted to the author's notation).

Under the Conditional Independence Assumption, the average effect of treatment–on–the–treated can be estimated by

(5) $$\frac{1}{N_1}\sum_{i\in\{D=1\}}\left(YT_i - \sum_{j\in\{D=0\}} w_{N_0,N_1}(i,j)YC_j\right)$$

where $j \in \{D=0\}$ is the group of non–treated individuals and the kernel weight $w(i,j)$ defines the "closeness" between the treated individuals *i* and *j* in terms of the relevant observable characteristics. Here, one simply estimates the non–treatment outcome of any treated individual i with observable characteristics X by taking an average outcome for non–participants with the same characteristics X – these are the fitted values of nonparametric regressions in the sample of non–participants at the local individual's characteristics X. The nonparametric regression basically can be interpreted as a weight function $w_{N_0,N_1}(i,j)$: *j* should have a higher weight for i if the two are more similar. For each treated individual i, the weights sum up to 1 over the whole sample of non–participants. The estimated effect of treatment–on–the–treated can then just be estimated by averaging this difference of the observed treatment outcome and the locally estimated non–treatment outcome over the whole sample of treated individuals N_1. Thus, the non–treatment outcome for participant $i \in \{D=1\}$ is constructed on the basis of the whole sample of non–participants N_0, so that

(6) $$\sum\nolimits_{j\in\{D=0\}} w_{N_0,N_1}(i,j) = 1$$

and N_0 and N_1 are the numbers of individuals for which $\{D=0\}$ and $\{D=1\}$, respectively (cf. Heckman et al. 1998).

Matching estimators differ only with respect to the weights attached to members of the comparison group (ibd.: 1023). Generally, the choice of the comparison group lies in between two extremes: On the one hand, the comparison group can consist of all non–treated observations and the individual non–participants are weighted. On the other hand, the matched control observation could consist only of the most similar non–participant with respect to X. The two options are shown in more details in the next two sections.

2.3.2.1 Nearest neighbour matching

The basic idea behind nearest neighbour matching is intuitively clear: We take one treated individual i from the sample of treated individuals and look for the most similar individual from the non–treated observations j, so that a one to one match leads to a very favourable structure of our sample. The respective treated individual is contrasted directly to one non–treated individual. This matching procedure works as follows: We define a neighbourhood $C(X_i)$ for each treated individual i. The persons matched to i are in A_i, where $A_i = \{j \in \{D=0\} | X_j \in C(X_i)\}$. In the case of nearest neighbour matching, the neighbourhood is defined as

(7) $$C(X_i) = \left\{X_j \middle| X_j = \arg\min_j \|X_i - X_j\|, j \in \{D=0\}\right\}$$

in a flexible way: $\| \ \|$ is a norm, $w_{N_0,N_1}(i,j)=1$ for $j \in A_i$ and $w_{N_0,N_1}(i,j)=0$ otherwise (ibd.: 1023).

The favourable structure of a strict matching of one treated to one control observation comes at the cost that nearest neighbours can nevertheless be quite different in terms of X. So nearest neighbours do not guarantee a priori that a good match is achieved. Secondly, a strict one–to–one matching might lead to a loss of appropriate comparison observations depending on the matching procedure itself (cf. section 2.4.2.2 below for the implementation of matching in the data): Consider a matching for all treated observations i, where we start with the first observation and match it in terms of nearest neighbours with an appropriate control observation. If the matched control observation is removed from the pool of all control observations, fewer and fewer potential control observations remain to reiterate this procedure for the entire sample of treated observations. Thus, the later treated observations might only find poor comparison observations in terms of nearest neighbours. Similarly to definition of calipers, a "golden rule" for the matching procedure does not exist even though sensitivity analyses show the properties of certain algorithms for certain samples (cf. Augurzky 2000). In small samples, it is supposed to be critical for the quality of the match whether identified comparison observations remain in the group of potential controls for further matching or not.

On the other hand, it is not guaranteed that a one–to–one matching of nearest neighbours exploits the information of the sample of control observations appropriately; further options could be to match to more than one control observation by applying a weight function, a variable number of control observations or by applying nearest neighbours only within predefined calipers. Some authors suggest, that especially in small samples, the properties of the matched samples highly depend on whether the pool of controls remains constant and/or control observations are applied more than once as the control observation for a treated individual i (Hübler 1998; Hujer, Wellner 1999).

2.3.2.2 Kernel matching

The intention of kernel matching is the application of a weight function for the identification of the appropriate weight for the whole sample of non–observations to construct the potential non–treatment outcome for the treated individual. Here, the weight function in equation (6) is specified as

(8) $$w_{N_0,N_1}(i,j) = \frac{K_{ij}}{\sum_{j \in \{D=0\}} K_{ij}}$$

where $K_{ij} = K\big((X_j - X_i)/h\big)$ is a weighting function that downweights distant observations X_j from X_i and h is a bandwidth parameter (Heckman et al. 1998: 1024). The potential outcome is estimated by local regressions at i on the basis of all non–treated individuals j, i.e. the expected outcome for treated individuals i in the hypothetical state of non–treatment is estimated on the basis of a weighted average of all non–treated individuals $j \in \{D=0\}$ following the idea of a local linear regression model. The weights depend on the deviation of observable characteristics $(X_j - X_i)$ with a sum of the weights equal to one.

Local regression

Without assuming a specific form of the regression function m, a datum point remote from X_i carries little information about the value of $m(X_i)$. Thus, an intuitive estimator for the conditional mean function $m(X_i)$ is the running local average.

An improved version of this is the locally weighted average which is illustrated for one participant's non–treatment outcome in the following: Let K be any real–valued function assigning weights to observations, i.e. a formula that gives each observation the same weight or weights with an underlying probability distribution. The function K is usually a symmetric probability function ('kernel'). Let h – the bandwidth – be a nonnegative number controlling the size of the local neighbourhood. Then, the Nadaraya–Watson kernel regression estimator (NW) $m(x) = m$ minimises

(9) $$\sum_{j\in\{D=0\}} \{YC_j - m\}^2 K\left(\frac{X_j - X_i}{h}\right)$$

with respect to m, giving the normal equation

$$\sum_{j\in\{D=0\}} YC_j K_j = \sum_{j\in\{D=0\}} K_j m .$$

The solution of m as $\hat{m}(X_i) = \sum (K_j)^{-1} \sum K_j YC_j = \overline{YC}_K$ where $K = K\left(\frac{X_j - X_i}{h}\right)$,

so that $\overline{YC}_K$ is a weighted average of $\overline{YC}_j$ values with respect to the characteristics X_i of the local treated individual i (Pagan, Ullah 1999: 93). This weighted regression formula is then repeated for the entire sample of the participants

An alternative estimator is a Local Linear Regression (LL), where the minimisation problem implements a local slope parameter:

(10) $$\sum_{j\in\{D=0\}} \{YC_j - m - b(X_j - X_i)\}^2 K\left(\frac{X_j - X_i}{h}\right)$$

and minimises with respect to m and b. This local regression estimator can be calculated by running a weighted least squares regression not only on a constant but including the deviation $(X_j - X_i)$ as a second variable in the estimated equation.

Thus, whereas the NW estimator fits a local constant with respect to the observable characteristics X_i, the local linear estimator fits a straight line (ibd.). With the LL estimator, we can consider the local curvature. By mean of this, we expect better local fit by applying the local linear model. In the empirical part, the NW as well as the LL model are applied to find out whether and to which extent we can see a difference between the two estimators with respect to the outcomes.

Concerning the asymptotic behaviour of NW and LL non–parametric estimators, Fan, Gijbels (1996: 20) emphasise that the local linear estimator is to be preferred to the NW estimator as it "adapts automatically to the random design by assigning an asymmetric weighting scheme, while maintaining the same kind of smooth weighting scheme as the NW estimator". Following their explanation, the bias should be smaller when applying the local linear regression than in the NW estimation procedure. However, such an a priori statement which estimator is superior with respect to the evaluation problem (which can also be found in Heckman, Ichimura, Smith, Todd 1998) seems not to be permitted: based on simulated data, Frölich (2003) could demonstrate that the properties of the LL estimator are especially sensitive to the choice of the bandwidth and the choice of the kernel (for a kernel with bounded support like the Epanechnikov kernel), so that the asymptotic superior behaviour when applying these estimators to small samples crucially depends on the selection of bandwidth and kernel function.

Kernel function

As mentioned above, the kernel function is a probability distribution (such as a standard Gaussian), a function defined to be zero outside of a certain range, or any other convenient form. The kernel function should be symmetric. The bandwidth h is a smoothing parameter and will be discussed in greater detail shortly. A closer examination of the numerator of equation (9) or (10) gives some insight into the weighting situation, that is, more weight is associated with the observations at locations close to X_i and less weight to observations more distant. The kernel function in this paper is always specified as a Gaussian kernel with

$$(11)\qquad K(j) = (2p)^{-1/2} \exp(-1/2 j^2) \text{ with } j = \left(\left(X_j - X_i\right)/h\right).$$

Other options what kind of unimodal distribution functions could be applied are the uniform, the Epanechnikov, the biweight and triweight kernel functions (Fan, Gijbels 1996: 15). Härdle (1990) and Härdle, Linton (1994) concluded that it is the choice of bandwidth, and not the choice of kernel function, that is critical to performance of the nonparametric fit. Therefore, the normal kernel function will be used in this application: The bandwidth determines how fast the weights decrease as the distance from X_i increases. The rate at which the weights decrease relative to the locations of the X_j controls the smoothness of the resulting estimate.

Bandwidth choice

Consider first the case where h is small (close to zero): The point of prediction itself possesses most of the weight with *only the closest observations* to this point receiving the remainder of the weight (recall the weights do sum to unity). Under such a scenario, the resulting fit would essentially "connect the dots" formed by the observed data points and possess high variance. In other words, instead of obtaining a robust underlying fit for the process, different samples would yield much different fits due to sampling variability and the over–dependence of the fits on the respective individual data sets.

Now consider the other case where h is very large (equal to or close to equal to the entire range in X_j). Instead of *concentrating the weights on a single point or handful of data*, the weight is fairly evenly distributed across all the observations. Such a fit is considered oversmoothed (with high bias) because it essentially fits the value YC_i at each data point (ibd.).

In principle, there is no "golden rule of bandwidth selection". Pagan, Ullah (1999: 19) discuss that if h is chosen high the variance of the estimated parameters is quite low as a large number of points are used for the estimation. On the other hand, a small h gives fragile density estimates and locally, only few points are included in the estimation, so that the variance increases, but less bias is produced. Thus, the trade–off between variance and bias is especially important in our application, where actually selection bias is to be minimised with respect to the central question of this paper. Frölich (2003) extensively discusses the sample properties of various non–parametric matching estimators, including the Nadaraya–Watson estimator, local linear and nearest neighbour matching. Based on simulated data, he could show that local linear estimators are especially sensitive with respect to the bandwidth choice in areas of sparse data. With respect to the performance of the different estimators assessed by the mean integrated squared error criterion, he concludes that the bandwidth selection for local linear estimators seems to be difficult and suggests to a rather small bandwidth value for this estimator compared to the Nadaraya–Watson estimator (ibd., 74). Thus, we should rather tend to a undersmoothing than to have a too high value of h.

As a first option, the selection of the bandwidth could follow a visual insight in the data. Therefore, the literature recommends for example to plot the relevant variables and to decide according to this impression (Pagan, Ullah 1999: 49). Another option quite extensively used in the vast amount of literature on bandwidth selection is the application of Silverman's Rule of Thumb (ROT). As an optimal bandwidth selection for a Gaussian kernel, Silverman (1986: 47f.) gives the recommendation of

$$h_{ROT} = 0.9A \cdot n^{-1/5} \tag{12}$$

where h is the selected bandwidth and $A = \min(std, iqr/1.34)$, in which std is the standard deviation and iqr the interquartile range of the sample (the sample size is n).

Note that Silverman's rule provides an optimal bandwidth choice for local density estimations. In this paper, we refer to this bandwidth and over or under smooth with respect to it, which is often applied also in nonparametric regression functions. However, an optimal bandwidth choice for a nonparametric regression function would imply that the error sum of squares are minimised for one individual observation. The literature suggested different strategies to achieve this (see Pagan, Ullah 1999: 118–122), e.g. a bandwidth choice based on cross–validation where the error sum of squares is minimised for an observation if this is omitted from the sample ("leave–one–out"). Our associated paper (Bergemann, Fitzenberger, Speckesser 2004) implements a bandwidth choice by a two step leave–one–out procedure which mimics the estimation of the average expected non–

participation outcome for each period: First, we identify the nearest neighbour of non–participants for each treated individual, i.e. the individual whose propensity score is closest to the non–participant. Secondly, a bandwidth is chosen that minimises the sum of the period–wise squared prediction errors for the nearest neighbour of non–participants applying a sample of non–participants, that omits the nearest neighbour of the treated individual over the whole time period (90–99). The resulting bandwidth is usually smaller than the bandwidth according to Silverman's rule.

In order to become sensitive for the selection of bandwidth with respect to the outcomes, also over and undersmoothing with respect to Silverman's rule of thumb $(h_{ROT} \approx 0.06)$ are considered, hence bandwidths of $h = 0.02$ and $h = 0.06$ are selected. Especially an undersmoothing should lead to more favourable features with respect to solving selection bias as the bias will be minimised with $h \to 0$ in non–parametric estimations.

Matching on the propensity score

The observable characteristics have not been specified yet. X should be considered as a vector of many different variables which determine the participation in a programme of further training. Therefore, a disadvantage of matching is the "curse–of–dimensionality", i.e. it might be difficult to match with respect to a high–dimensional vector of X. Therefore, this paper follows the result of Rosenbaum and Rubin (1983) that the CIA in equation (4) also holds with respect to the probability of treatment ("propensity score") $P(X)$ as a function of the observable characteristics X, i.e.

(13) $$E\{YC|D=1,P(X)\}=E\{YC|D=0,P(X)\}.$$

On the one hand, this result allows to match upon the one–dimensional probability effectively using the "closeness" in the propensity score as the weighting scheme in equation (4). This dimension–reduction feature reduces the problem of finding adequate matches. On the other hand, the difficult issue in this context is that the propensity score has to be estimated itself. In general, it is an open question which form of matching is more efficient, see Heckman, LaLonde, Smith (1999) and Rubin, Thomas (2000), and the recent German literature mostly follows a suggestion by Lechner (1998) who uses a hybrid approach combining matching nearest neighbours on the propensity score with matching on selective important observable characteristics which often are not time–invariant (e.g. the previous employment status).

2.3.3 Multiple treatments

To take into account multiple treatments, we expand the evaluation approach sketched above. According to our definition, multiple treatments are repeated ALMP experiences over the lifecycle meaning that an individual is assigned more than once to a programme. Our approach estimates the average effect of treatment–on–the–treated of the i^{th} participation in further training compared to the situation of not having participated in the programme at most (i–1) times.

(14) $$E\{YT|D_i=1,P(X)\}-E\{YC|D_i=0,P(X)\}$$

Evaluating the additional employment effect of the i^{th} participation occurs in two steps: First, we estimate the propensity score for the event that an individual participates at least i separate times in the programmes as opposed to participating at most (i–1) times. Matches are then formed between the two groups of individuals such that for each individual participating at least i times the best match is found among those individuals participating at most (i–1) times.

The additional treatment effect is estimated as the average DiD in employment in the matched sample. By estimating DiD and treating previous programme participation as non–employment, we "automatically" estimate the average additional effect of the i^{th} programme participation. The matching procedure implies that conditional on the propensity score, the matched controls are found among all non–participants based on the likelihood to participate zero, one, and up to (i–1) times, respectively.

Note that we are only evaluating the *incremental* effect of a second treatment in this application. An alternative evaluation could investigate multiple treatments with respect to the *combined* outcomes, which is probably interesting if the participants are unlikely to find a job after the first treatment and are treated by sequences of programme combinations in order to reach a specific treatment goal (this is estimated in the corresponding paper Bergemann, Fitzenberger, Speckesser 2004). If the policy design (and the related selection process of the participants) suggests that such programme sequences are already foreseen for participants at the beginning of the first treatment, there should be no problems of modelling appropriately the selection process.

However, to a certain extent, the evaluation of combined treatment sequences could condition on future outcomes: It is an open question whether the effect of the combined sequences can be identified with the whole group of non–participants or whether the selection process occurs to a later point in time. Probably, the effect of treatment–on–the–treated of a second treatment can only be identified based on the participants of the first treatment. However, we do not know whether the sequences are already planned when the individuals start the first treatment or whether the second treatment is only offered to individuals who passed the first treatment, so that the choice of an appropriate control group is probably difficult and the identification of the effect of treatment–on–the–treated requires further assumptions. Therefore, we only estimate the average incremental effect of treatment–on–the–treated.

2.3.4 Selection bias based on unobservable characteristics

Most econometric literature on the evaluation of ALMP makes use of the assumption that selection bias due to observable characteristics and selection bias due to unobservable characteristics can be considered separately (Heckman, Ichimura, Todd 1998). While matching estimators as well as other solutions on selection bias due to observable characteristics copes with the influence of measurable and measured variables on the participation decision, selection bias due to unobservable characteristics has to be dealt with differently. The following section suggests the solution of a difference–in–difference estimator.

In particular unobservable characteristics could be differences in the benefits which individuals expect from participation in a treatment which might influence their decision whether to participate or not, because these characteristics are hardly measurable. In addition, particular groups of individuals might exhibit bad labour market prospects which the employment agencies are targeting at and which cannot be identified by the researcher. Furthermore, differences in the motivation of participants are also unobservable to us but not necessarily so for the agents in the employment offices.

To account for selection on unobservables, the literature has pursued various strategies (econometric selection models and difference–in–difference estimators, see Heckman, LaLonde, Smith 1999). Here, we implement a "conditional difference–in–differences estimator" (cDiD), where conditional means that treatment and control group are already comparable conditional on X by applying e.g. matching approaches. The DiD–estimator is based on the assumption of time–invariant linear selection effects. This estimator extends simple before–after comparisons to determine the treatment effect based on the presumption that the outcome variable can also change over time due to reasons which are unrelated to the treatment. Thus, the change for the treated is contrasted to the change for comparable non–treated individuals. Assuming that the observed outcome $Y_{i,t}$ for individual i at time t can be described by the following equation:

(15) $$Y_{i,t} = a_i + D_{it}\left(g_T(X_{it}) + e_{T,it}\right) + \left(1 - D_{it}\right)\left(g_C(X_{it}) + e_{Ci,t}\right)$$

with $YT_{i,t} = Y_{i,t}$ for $D_{i,t} = 1$ and $YC_{i,t} = Y_{i,t}$ for $D_{i,t} = 0$, a general DiD estimator consists of matching individuals i and j with the same observable characteristics $X_{i,t1} = X_{j,t1}$ and $X_{i,t0} = X_{j,t0}$ where i receives treatment between period $t0$ and $t1$ and j is a non–treated individual. Further assumptions are that $g_{Ti,t}(X_{it})$ and $g_{Ci,t}(X_{it})$ are the individual specific outcomes in the treated and non–treated state, respectively, that the permanent unobservable individual effect a_i is correlated with programme participation and that $e_{T,it}$ and $e_{C,it}$ are additional error terms for the treatment and non–treatment state. Thus, a_i captures the effect of selection on unobservables and can be differenced out in order to obtain a constant treatment estimator by

(16) $$\left(YT_{i,t1} - YC_{i,t0}\right) - \left(YC_{j,t1} - YC_{j,t0}\right) = g_T(X_{it1}) - g_C(X_{it1}) + e_{T,it1} - e_{C,it0} - e_{C,jt1} + e_{C,jt0}$$

Heckman, Ichimura, Todd (1998) refer to various studies for the U.S. based on experimental evidence which indicate that conditional DiD combined with non–parametric matching has shown to be a very effective tool in controlling for both selection on observables and unobservables.

2.4 Empirical analysis

2.4.1 Data

The empirical evaluation of further training programmes is based on data from the Labour Market Monitor Sachsen–Anhalt (Arbeitsmarktmonitor Sachsen–Anhalt, LMM–SA) from the years 97, 98 and 99. The LMM–SA is a panel survey of the working–age population of this *Bundesland* with 7,100 participants in 97 and 5,800 in 98. 99, the sample size is around 4,760. From the 97 survey onwards, information became available that allows the researcher to reconstruct the complete labour market history of the individuals on a monthly basis since the beginning of 90. Those who first participated in the panel survey after 97, e.g. in latter two waves, are recorded retrospectively until 90.

The monthly data cover all labour market positions, i.e. whether individuals were in employment, unemployed or participated in a programme of ALMP, as well as information on periods in the education system, inactivity or in military service up to December 99. Individuals who did not participate in the 98 survey are recorded until at least September 97, those who dropped out in 99 at least until October 98.

In general, the unbalanced panel comprises all individuals with complete information about their labour market history between January 90 and at least until September 97 (i.e. individuals who completed the retrospective question of the 97 survey and later). The individuals are at least 25 years old in January 90 and are in dependent employment, self–employment, or on maternal leave before the start of the Economic and Social Union in June 90, so that only persons who were part of the active labour force of the former GDR are included in the sample. Persons, whose labour market status is any other than employed, unemployed, on maternal leave, inactive or in ALMP programmes at any time (i.e. civil servants, persons in the military service or in the education system) are excluded completely from the analysis. Excluding these groups from the sample can be justified in order to build the analysis upon a consistent data base: Civil servants a priori show a lower risk of being laid–off than employees of the non–public sector (essentially, there is no risk at all for civil servants, and civil servants are exempted from contributing to the unemployment insurance, too) and persons in military service and the education system were not part of the active population in East Germany subject to this evaluation.

The resulting sample consists of 5,224 individuals which are representative for the former GDR labour force. Figure 2.1 summarises the basic figures about participation in ALMP programmes in East Germany. In the first ten years after the unification, 37% of the total labour force participated at least once in ALMP. All programmes were extensively implemented (13% of the sample started at least once a Job Creation Scheme, 4% worked under a wage–subsidy programme in the private sector), but further training of course was the most important programme[1]: one fifth of all observations had a least one spell of training[2] during the 90ies, in the sample to be analysed here, these are 1,021 treated individuals.

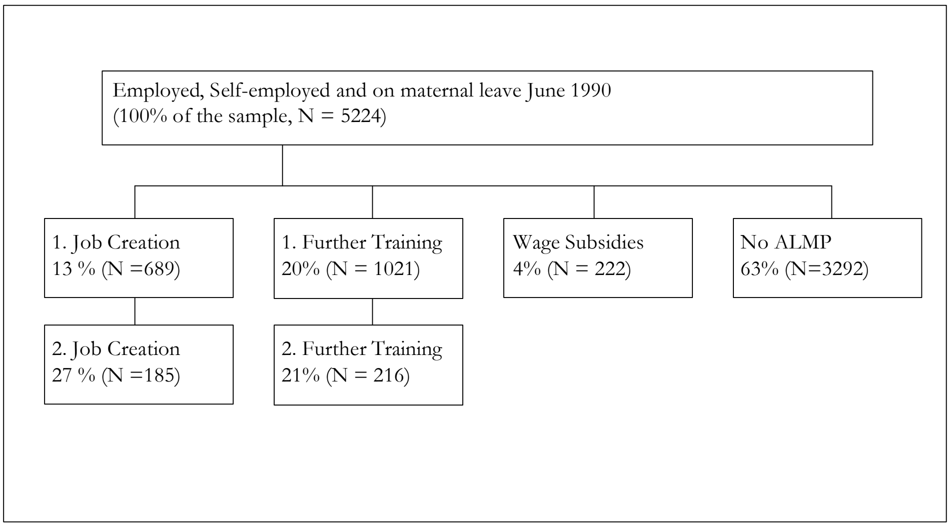
Reiterated treatment in the further training programme occurred in 216 cases, i.e. more than one fifth of the population with a first treatment in this programme participated at least a second time.

[1] The data base does not allow to differentiate whether a treatment in further training specifically was a further vocational training or a retraining scheme (integration subsidies are excluded in the survey design of the LMM-SA). Therefore, the broad category of further training evaluated in this analysis can only give an average treatment effect across these programmes, which nevertheless covers possible heterogeneous treatment effects of the subprogrammes.

[2] The question in the LMM-SA on further training does also include privately financed training. However, calculations with the aid of the GSOEP–East shows that a high coverage of public sector sponsored training is achieved (for 1993 more than 88%).

In the point of view of the author, it is therefore rational to differentiate the effects of a first and a second treatment (as suggested in Bergemann et al. 2000).[3]

Figure 2.1 Multiple treatments of active labour market policy in East Germany



2.4.2 Matching

2.4.2.1 Estimation of the propensity score

The propensity score is estimated by a parametric Probit model. As the data do not provide time–varying information, the model includes only static observable characteristics as occupational degree, gender, age, residence and interaction terms of sex and occupation degree (estimation results can be found in Table 2.A3 in the appendix). The estimation is conducted individually for the first and the second treatments in a further training programme so that we obtain values for the propensity individually. The potential control group of 'non–participants' from which to match are – as indicated under section 2.3.4 – individuals who are not treated in the specific programme and specific iteration but who could have been or could be participant in any other programme. This implies that the pool of control observations is always relatively large and a sufficient number of observations is available for matching.

2.4.2.2 Nearest neighbours

For nearest neighbour matching, the matching procedure works as follows: As the first step, the propensity score is estimated for both the first and the second further training scheme. Thus, two specific matched samples are constructed on the basis of the participants in programme m and all other persons who have not been assigned either to a first or to a second programme of further training.

3 The descriptive statistics for individuals participating in a first and a second further training program com pared to non–participants and the total sample can be seen in Tables A1 – A2 (Appendix).

Step 1: The sample is divided into two separated subsamples. The first group consists of the treated individuals i of programme m (T–group) and the naive control group of non–participants j (C–group). The propensity score is estimated.

Step 2: One individual i_m is randomly selected from the T–group.

Step 3: Find the person $j_{i,m}$ in the C–group which has the most similar propensity score among all control observations, so that the difference of the propensity scores between i_m and $j_{i,m}$ is minimised.

Step 4: Save the matched nearest neighbours for all available months, so that i_m is removed from the T–group and $j_{i,m}$ is removed from the C–group[4].

Step 5: Restart the matching for the next observation of the T–group until no more observations are left in the T–Group.

As assumed in the associated paper (Bergemann et al. 2004), time variable selection bias on the basis of unobservable characteristics may still exist in the data. Since the additional effect of the respective participation should be estimated, the DiD–estimator is included as a second step. Other approaches are suggested and implemented in Lechner (1998) and Hujer, Maurer, and Wellner (1998) who in addition to the propensity score match on the time varying information about the employment status before individuals enter a programme.

Based on several short and long–term preprogramme tests, it can be shown that the anticipation of a programme seems to involve a dynamic and disproportionate decline of the participants employment chances relative to those of non–participants before the programme starts. It is likely that this decline is related to the subsequent participation and the DiD estimator should be aligned with respect to the preprogramme level of employment early enough not to be affected by Ashenfelter's Dip (Ashenfelter 1978).

2.4.2.3 Kernel matching

As in the case of nearest neighbour matching, kernel matching also uses the propensity score and no further additional time variable covariates in the matching algorithm. In total, four matched samples are constructed for each the first and second further training scheme, because in the case of kernel matching, more options have to be specified (bandwidth, kernel, estimator). Therefore the construction of four specific matched samples should allow to control for a part of the properties associated with possible specifications of the nonparametric form. Again, the first step is a parametric estimation of the propensity score. After that, the procedure works as follows:

[4] Removing the matched individuals from the pool of possible control observations ensures that one observation can be matched exactly once to a treated individual. If individuals were matched more than once, the variance of the matched sample would be underestimated. In principle, the multiple use potential control observations could increase the quality of matching and reduce bias, but the variance should be weighted accordingly.

Step 1: The sample is again divided into two subsamples. The first group consists of the treated individuals of the respective sequence of the programme (i.e. first or second further training, T–group), the second group is the group of non– participants in (C–group). Note that for the reiterated treatment, the control group consists of either participants in the first further training or individuals who did not participate at all on order to identify the average incremental effect of treatment–on–the–treated for the specific iteration (not conditional on the first participation). The propensity scores are estimated for both the first and the second participation, so that the following steps are separately implemented for both first and second further training participants.

Step 2: We select the bandwidth within the sample of non–participants for the estimated propensity score. In this step, three different bandwidths are selected, so that all following steps multiply. The chosen bandwidths are:

$h = h_{ROT} \qquad h = 0.02 \qquad h = 0.06$

Step 3: We select the first treated individual for his first available point in time corresponding to the 18th month before the start of the treatment and for which a non–treatment outcome shall be estimated based on the matching. The basis for the estimation of the non–treatment outcome are all available non–treated individuals at the same calendar time, i.e. the kernel matching controls for the panel mortality and uses only individuals who are available to the same calendar time.

Step 4: A weighting function is created as

$$K\left(\left(\hat{b}\,X_j - \hat{b}\,X_i\right)/h\right)$$

with a Gaussian Kernel weighting the predicted propensity scores $\hat{b}\,X_j$ of the non–treatment sample with respect to the predicted propensity score $\hat{b}\,X_i$ of the (local) treated individual i_m.

Step 5: The expected employment outcome of non–participation

$$E\left(YC_{i,t-18} \middle| P(X_i), D=1\right) = \sum_{j \in \{D=0\}} w_{N_0,N_1}(i,j)\, YC_{j,t-18}$$

for the first treated individual in the hypothetical state of non–treatment for the month –18 is estimated by a nonparametric regression with the weighting function of step 4 in the sample of all non–treated observations. Two alternative estimators are applied, so that the following steps multiply:

a) Nadaraya–Watson b) Local Linear

Step 6: Step 3, 4 and 5 are also implemented to estimate the expected values of the socioeconomic characteristics of the non–treatment outcome (age, gender, whether an individual was treated in another ALMP programme before. The weights are defined as in step 4, but of course relative to the value of the socioeconomic variables of the treated individual.

Step 7: The observed information on employment, age, gender and programme participation of the first treated individual and the nonparametrically estimated non–treatment outcomes for employment, age, gender and programme participation are written to a new data set. This observation is the first "matched sample" for the first participant at the first point of time.

Step 8: Repeat step 3 to step 7 for all months from 17 before to 36 months after the participation of the individual for data available.

Step 9: Repeat step 3 to step 8 for all other treated individuals.

2.4.3 Specification of outcome equation

While matching is supposed to take account of selection bias on observable characteristics, selection bias on the basis of unobservable characteristics might still exist. Therefore a conditional DiD–estimator[5] is implemented to cope with selection on unobservable characteristics at least if these are time constant. This DiD–estimator is applied for the outcome variable of employment as a dummy variable indicating either employment or non–employment. The state of non–employment includes the participation in ALMP programmes such that previous and subsequent participation in a programme are both accounted for in the evaluation.

The DiD estimator evaluates an average employment effect of a programme relative to all possible non–employment states for the treated individuals. The outcome variable measures the effect of treatment–on–the–treated based on a DiD approach. Here we focus on differences in employment between treatment and non–treatment outcome:

$$\Delta Y_{i,t} = YT_{i,t} - \overline{YC}_{Ki,t} \quad (19)$$

with $YT_{i,t}$, taking the value of 1(dependent employment, self–employment) or 0 (unemployment, motherhood, housewife, retirement, ALMP programme). $\overline{YC}_{Ki,t}$ is the estimated non–treatment outcome of the respective person based on nearest neighbour or kernel matching. Note that in case of kernel matching, the predicted non–treatment outcome can show any value between 0 and 1.

If the potential outcome is estimated with nonparametric regressions, the values can be different than 0 or 1 for the non–treatment outcome. As non–employment for the treated and controls comprises both unemployment and other forms of inactivity as well as programme participation, it is implicitly controlled for repeated participation after the end of a programme in the outcome variable. If an individual restarts treatment within the time window, this treatment is seen as a

[5] The specification of the outcomes within matched samples of nearest neighbours follows the associated paper by Bergemann et al. (2000, 2004). The test for the specification estimated here are available upon request.

failure of the programme (i.e. it is coded as non–employment in the outcome variable). The same argument applies to the out–of–labour–force states (motherhood, housewife, retirement).

The outcome equation considers a time window of 18 months before treatment and 36 months after treatment. The DiD–estimator is implemented by including the employment situation before treatment in the panel regression for the time after treatment where the estimated parameters to any point in time after treatment show the DiD estimator of the programme effect (Bergemann, Fitzenberger, Speckesser 2004). Note that the effects are estimated for the period after the end of the programme, i.e. the duration of the programme itself is not included. There might be reasons why one should include the programme duration itself into the outcome equation, especially if the programme is short term and aims at the integration of the participant by stimulating the search process. In such cases it would be worth evaluating the programme effect immediately after the start of the treatment. However, as most programmes are longer term and intend to increase the employability of the participants by additional skills and formalised occupational degrees, individuals tend to interrupt the search process and to resume searching after the end of the programme – complying with the design of the programme (full–time training). In our associate paper (Bergemann, Fitzenberger, Speckesser 2004), we evaluate the programme effects for both cases – either by including the programme duration in the period of outcomes or by starting the evaluation after the end of the programme. Except for the very early months of the post–treatment period, the outcomes hardly differ. As the effect of the programme may vary upon socioeconomic characteristics of the participants and the timing of the programme, these characteristics are included in the outcome regression.

The preprogramme employment level for the alignment of the DiD–estimator proves critical for the outcome as pointed out above. Therefore dummy variables are included in the regression which control for the preprogramme effects on the outcome variable. This effect together with the long–run preprogramme employment differences are allowed to depend upon various socioeconomic characteristics and upon the timing of the programme τ. Put together, the outcome equation based on monthly data for $t = -18,...36 \quad (t \neq 0)$ is specified as

(20)

$$\Delta Y_{i,t} = a_0 + a_1 t + a_2 t^2 D[t < AD(t)] + a_X{}'\tilde{X} + \sum_{\substack{k=AD(t)\\ k\neq 0}}^{36} [b_{k,0} + b_{k,1} t] D(t = k)$$

$$+ (g_{X,0}^{AD} + g_{X,1}^{AD} t)'\tilde{X}\, D(AD(t) \leq t \leq -1) + (g_{X,0}^{PO} + g_{X,1}^{PO} t)'\tilde{X}\, D(t \geq 1)$$

where

t	month before $(t < 0)$ and after treatment $(t > 0)$
t	calendar time of treatment beginning (month)
$\tilde{X}$	vector of pair wise socioeconomic characteristics of treated and matched control (age dummy for individuals aged 40 and above, sex, dummy participation in another ALMP of a different type before) defined as deviations form their average in the treatment sample

a_0, a_1, a_2, a_x	coefficients measuring the long–run preprogramme differences depending upon the month when the programme starts (t) and the characteristics $\tilde{X}$
$AD(t)$	month before the begin of the programme when Ashenfelter's Dip starts depending upon t
$b_{k,0}, b_{k,1}$	coefficients modelling the differences in $\Delta Y_{i,j}$ for $k \geq AD(t)$ as a function of t relative to the long–run preprogramme differences for individuals with average characteristics
$g_{x,0}^{AD}, g_{x,1}^{AD}, g_{x,0}^{PO}, g_{x,1}^{PO}$	coefficients modelling the impact of socioeconomic characteristics $\tilde{X}$ during Ashenfelter's Dip (AD) and after the programme (PO) (with [$g_{x,1}$] and without [$g_{x,0}$] an interaction of the calendar time τ).
D	dummy variable taking the value of one when the event in parentheses occurs

Equation (20) is estimated as a linear regression and inference is based upon heteroskedasticity–consistent standard error estimates[6].

By implementing the DiD estimator this way, $b_{t,0} + b_{t,1} + (g_{X,0}^{PO} + g_{X,1}^{PO} t)'\tilde{X}$ shows the postprogramme effect relative to long–term preprogramme situation – the average employment outcome for the period from 18 months before the treatment up to the beginning of Ashenfelter's dip (depending on the timing of the treatment covers the first two to six months before the treatment). In this form, long–term preprogramme are reported in the intercept term, the time trends and long–term differences caused by observable characteristics a_0, a_1, a_2, a_x. Inserting further preprogramme dummies for the period of Ashenfelter's dip in the DiD outcome estimation pays attention to the dynamic selection process immediately before treatment (i.e. increasing differences between treated individuals and the matched control), so that the post treatment effects – the DiD estimates – are related only to the long–run preprogramme differences[7].

The specification allows the employment differences before and after the programme to depend in a very flexible way upon the month when the programme begins and other socioeconomic characteristics. Therefore, possible heterogeneity in the preprogramme period is taken into account by selection level and in the impact of the programme. In particular, the analysis depends critically on the time t when the programme starts. While in 90 and 91 the preprogramme selection level

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6 In order to account also for the estimation error of the propensity score estimation, we implement a bootstrap procedure in section 2.4.4.3

7 In our corresponding paper (Bergemann, Fitzenberger, Speckesser 2004), the specification is slightly different, estimating the long-term preprogramme differences separately. The outcome is then modelled explicitly as difference-in-differences by subtracting the outcome from these estimated long-term preprogramme differences. This procedure – contrarily to the estimation of DiD as modelled in (20) – accounts for the fact that only individuals that are available for the long-term preprogramme period are used in order to estimate a_0, a_1, a_2, a_x.

is likely to be small since unemployment had been rare in the past, it quickly grows with the rise of unemployment during the next years. In addition to the heterogeneity discussed so far, also the length of a possible preprogramme decline in the outcome variable is allowed to depend upon the time when the programme starts. During the period shortly after unification, it is likely that the dip is fairly short since programme participation could not have been anticipated long before. However, the situation changes with the occurrence of high unemployment when people realised that labour market problems were quite severe and that ALMP at a large scale was likely to be a permanent feature of the labour market. To capture the transitory employment declines before the programme starts as Ashenfelter's Dip, the following heuristic approach is chosen. For the first programme participation, a visual inspection of the average employment differences between treated and matched controls before and after the programme as a function of the time when the programme starts indicates that the dip occurs during one to two months in 90/91 and increases over time to something of at most six months. In order to obtain a lower bound of the DiD estimate of the employment effects of a programme (the employment of the future participants decreases during the dip), we are conservative in constructing $AD(t)$ as follows

$$AD(t) = \begin{cases} 0 & \textit{until } 90/7 \\ -1 & 90/8 \\ -2 & 90/9-90/10 \\ w_t(-2)+(1-w_t)\overline{AD} & \textit{between } 90/11 \textit{ and } 94/7 \\ \overline{AD} & \textit{after } 94/7 \end{cases}$$

where the eventual starting point $\overline{AD}$ is –6 months. Between November 90 and July 94, $AD(t)$ increases in absolute value from 2 months to $|\overline{AD}|$. The weights w_t are constructed to provide a linear interpolation and $AD(t)$ is rounded to the nearest integer.

The average DiD employment estimate of the programme based on the specification in (16) for month $t=1,\ldots,36$ after the programme is given by

(17) $DiD_t = \hat{b}_{t,0} + \hat{b}_{t,1} + (\hat{g}^{PO}_{X,0} + \hat{g}^{PO}_{X,1}t)'\tilde{X}\, D(t \geq 1)$

where DiD_t depends upon the time t when the programme starts and it is evaluated at average socioeconomic characteristics. The effects for individuals with different characteristics can be evaluated by adding $(g^{PO}_{X,0} + g^{PO}_{X,1}t)'X$ for specific participation groups.

Within the context of multiple treatments, one can identify the average effect of either the first or the second treatment with respect to employment. Thus, concerning the effect of a second treatment, the DiD–estimation provides the average additional employment effect of the second treatment based on the construction of our DiD–estimator and the definition of the employment variable. The effect of a first treatment for a person in a second treatment is included in the permanent preprogramme effect.

2.4.4 Evaluation results

The following sections summarise important results of the evaluation of a first or second further training programme for the different matching approaches suggested. The results are presented in figures 2.2–2.3. In general, these figures should be interpreted as follows: The thick line shows the DiD_t –estimator for the time period until 36 months after participation as defined in equation (17). The preprogramme period depends on the time of treatment. The surrounding lines indicate the 90% confidence interval. Participation takes place at time 0. This is shown as an interruption of the curves. The postprogramme period here shows the average treatment effect on the treated; if individuals with specific socioeconomic characteristics X are considered, the respective terms $g'X$ of the outcome equation can be added. The results of the estimations can be found in tables 2.A4 and 2.A5 (Appendix).

The zero–line is the reference line in relation to which a success or failure of the programme is measured: A programme can be considered as economically successful in terms of employment if the confidence interval lies in the positive region. Additionally, the alpha–coefficient $a_0 + a_1 t + a_2 t^2$ in the figures indicates the remaining level of long–term unobserved heterogeneity within the matched pairs or between the treated individual and the nonparametrically estimated non–treatment outcome in percentage points of the employment rate. Again the surrounding thin lines are the associated confidence intervals. The alpha–coefficient gives the percentage points by which the employment rate of participants differs to the comparable non–treatment outcome because of remaining unobservable characteristics. Again, if individuals with specific characteristics are considered, the terms of the outcome equation have to be added. A negative coefficient could be interpreted as a successful targeting of ALMP on persons with a very bad labour market prospect.

2.4.4.1 Nearest neighbour matching

As the focus in this paper is laid on average employment effects of first treatments in further training programmes starting in December 92 and a second participation in these schemes starting in December 94, the results shown here should only be understood as exemplary.

Concerning a first treatment in the further training, the estimated results show a sharp decline in employment for the participants compared to the non–treatment outcome in the period immediately before treatment. Within the matched samples, the employment rates of treated are to 33 percentage points (ppoints) lower. After the end of the programme the employment rate is still much below the level of comparable control observations, but shows a positive dynamic. After more than two years, the average effect of the programme on the participants is insignificant which can be interpreted as an overall effect of treatment on the treated of zero in terms of employment. Although the initial employment level after treatment is negative, the employment dynamic is higher due to a programme participation and the overall effect is at least nonnegative.

The remaining level of unobserved heterogeneity indicates that participants have in general reduced employment chances than the matched controls: The long–term preprogramme differences in employment are –25 ppoints. Thus the targeting towards groups which are particularly affected by labour market problems can be seen as successful (figures 2.2–2.3).

Individual programme effects are also estimated for individuals who participate in a second programme of further training by the end of 94. Here, the evaluation indicates that the phenomenon of Ashenfelter's Dip is less important than for participants in a first further training programme: The employment level decreases only to a level of –10 ppoints compared to non–participants in anticipation of the programme. After the end of the programme, the participants on average exhibit insignificant employment effects, i.e. they have no benefit or disadvantage due to the participation in the programme (figures 2.2–2.3). Although the employment effect seems to decrease after 19 months relative to the end of the treatment, this result is not supposed to be very robust because the number of matched pairs decreases over time.

The level of unobserved heterogeneity for participants in second further training is much lower than for the treatment in a first further training which indicates that the participants in such a programme have much worse employment chances than the matched controls. These programmes can also be understood as successful in terms of targeting: The participants have on average 65 ppoints reduced employment chances.

2.4.4.2 Kernel matching

One central question of this analysis is the identification of differences in employment effects resulting from the method used for solving selection bias due to observable characteristics. Therefore, the evaluation of the employment effects of a first or second further training programme is replicated on the basis of alternative matching approaches. Interesting differences between the different matching approaches with respect to the employment outcomes of a first treatment of further training are:

a) NW: Compared to the evaluation based on nearest neighbour matching, the first participation in further training has quite different employment outcomes. On the one hand, the decline of employment rate of the participants is more severe if NW matching is applied: Participants have an on average by 39 ppoints reduced employment compared to 34 ppoints in the case of nearest neighbour matching. On the other hand, the level of remaining unobserved heterogeneity is higher: –28 ppoints compared to –25 ppoints in the NN matching. Besides this, the most important difference is that the average treatment effect remains consistently negative over time: For a long period the individuals significantly decreased their employment chances by participating in the programme. Comparing this to the results of NN matching, the employment effects of the treated are 10 ppoints lower if matched samples are constructed by nonparametric regressions estimates (figure 2.2).

b) In the case of applying local linear estimations, employment effects for the first treatment look similar to those of the NW regression matching. Employment immediately before treatment varies depending on the specification of the bandwidth between –35 ppoints for $h = 0.02$ and –39 ppoints $h = 0.06$. The average employment effect due to participation in the programme 36 months after treatment varies between –13.7 ppoints for $h = 0.02$ and –12.7 ppoints for a bandwidth selection of $h = h_{ROT}$. Although these differences are not very high, the specification of the bandwidth clearly matters (figures 2.2–2.3).

Similarly to the estimations based on NN matching, the estimators indicate a positive employment dynamic caused by the programme participation. The matched samples here however show that employment is only increasing up to 18 months after treatment, whereas the estimator in NN matched samples has a consistently upward slope until the end of the observation period. The evaluation based on kernel matching has obviously better properties with respect to the declining number of observations at the end of the period than a strict one–to–one matching based on nearest neighbours – the confidence intervals are smaller.

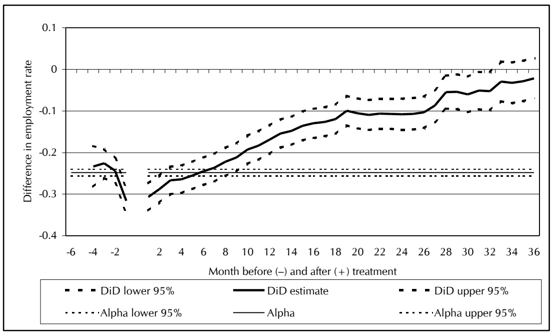
Estimations of the outcome of a second further training scheme based on kernel matching also show quite different outcomes than those using nearest neighbours. The results can be summarised as follows:

a) NW: While matched samples on the basis of nearest neighbours suggest a 10 ppoints reduced employment of treated individuals immediately before the begin of a programme, in matched samples based on NW–estimations, employment rates of treated are only 14 ppoints lower than those of non–treated. Furthermore estimations for nearest neighbours suggest increasing employment dynamics up to 18 months after treatment, although the average employment effect remains insignificant over the whole period of observation. This positive dynamic cannot be found for the DiD_t estimator here: The effect immediately after participation is zero and does not show any dynamic over time. The remaining level of unobserved heterogeneity finally suggests that participants have much lower long–term pre-programme employment rates if samples are matched with the aid of the NW regression than in the case of NN matching: The alpha coefficient lies ten percentage points below the level of the NN matching (figure 2.3).

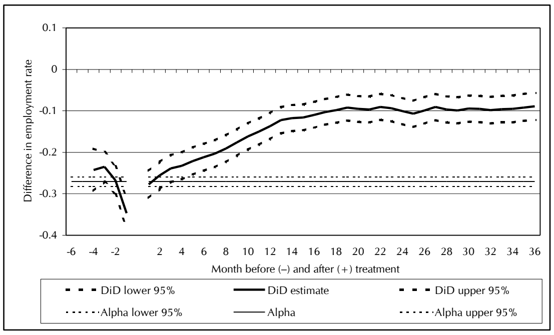
b) LL: The results of the estimations with matched samples based on LL regressions differ only to a minimal quantity from those estimated in NW regression matched samples (figures 2.2–2.3). Nevertheless, according to the specification of the bandwidth parameter, the employment effects of treatment on the treated decrease as $h \to 0$.

Figure 2.2 Employment effects of 1st participation in a further training scheme, December 1992

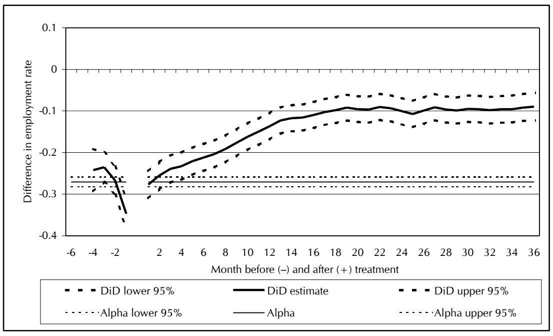
– Estimates based on Nearest Neighbour Matching –



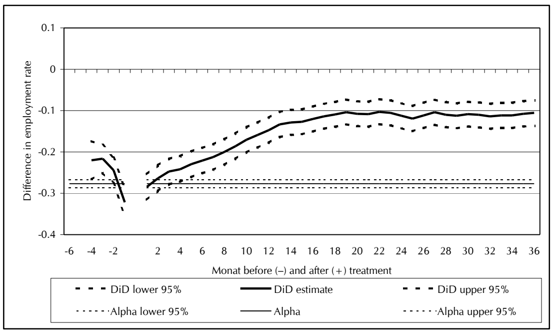
– Estimates based on Kernel Matching with N/W Estimator, $h = h_{ROT}$–



– Estimates based on Kernel Matching with Local Linear Estimator, $h = h_{ROT}$ –



– Estimates based on Kernel Matching with Local Linear Estimator, h = 0.02–



– Estimates based on Kernel Matching with Local Linear Estimator, h = 0.06–

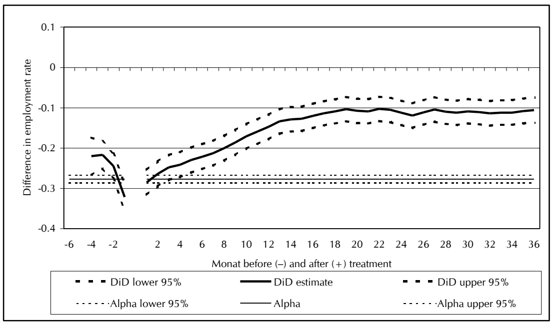
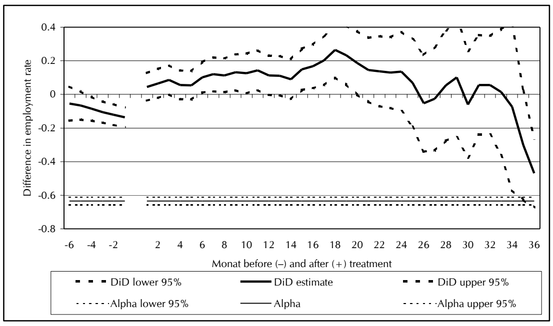
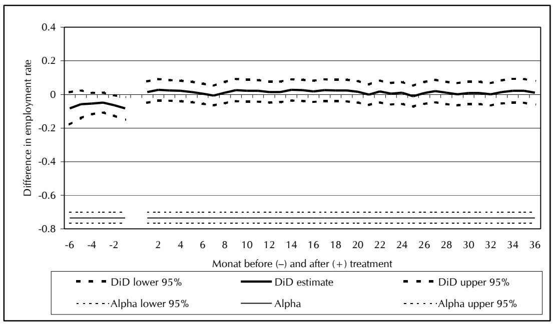


Figure 2.3 Employment effects of 2nd participation in a further training scheme, December 1994

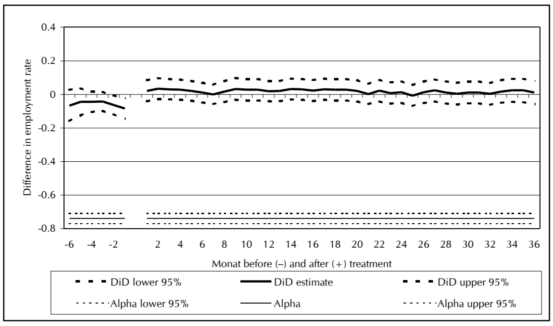
– Estimates based on Nearest Neighbour Matching –



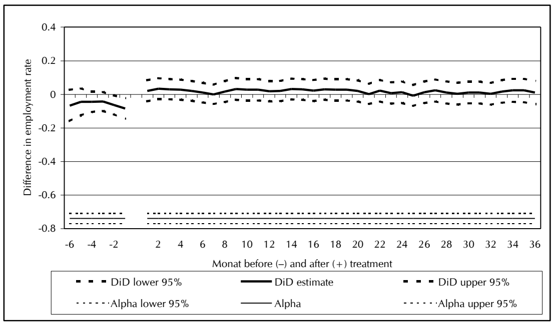
– Estimates based on Kernel Matching with N/W Estimator, h = h_{ROT}–



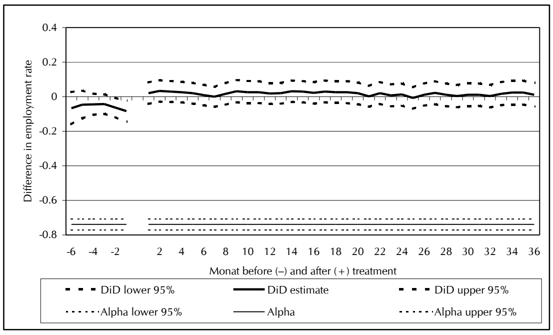
– Estimates based on Kernel Matching with Local Linear Estimator, h = h_{ROT} –



– Estimates based on Kernel Matching with Local Linear Estimator, h = 0.02–



– Estimates based on Kernel Matching with Local Linear Estimator, h = 0.06–



2.4.4.3 Bootstrap confidence intervals

The dimension reduction feature of matching on the propensity score comes at the costs that the propensity score itself is estimated by a parametric probit model. Therefore, the standard errors of the estimated treatment effects are likely to be underestimated. To take account of the sampling variability of the propensity score estimate, we implement a bootstrap procedure for construction of confidence intervals.[8]

The basic principle of the bootstrap involves reiterated estimation of the parameter of interest by drawing randomly new samples with replacement from the original data. The sampling procedure with replacement implies that one can select certain observations two or more times and others not at all. Each sample then is slightly different from the original sample. Repeating this procedure for a large number, one gets pseudo samples similar to the underlying distribution of the data. We resample the data before estimating the propensity score and before fixing the bandwidth in the sample of the non–treatment observations, so that the estimated outcomes within the matched samples estimates from the pseudo samples take into account of the sampling error of the propensity score. We repeatedly estimate the coefficients of the outcome equation from the random samples and calculate the empirical variance of the estimated coefficients in order to obtain bootstrap standard errors of our estimates. These standard errors then do not rely on any distributional assumptions (like normality).

More formally, we want to estimate the standard error of an estimator $\hat{q}$ in a dataset D_N. The sample values are the outcomes of random variables z_i with a probability density of p_z. We want to make inference about the true parameter θ, the outcome of applying the statistical functional *t(.)* to F, so that θ=*t(f)*. The simplest example of such a functional is just the average. The estimate of θ is $t = t(\hat{F})$ where $\hat{F}$ is an estimate of the empirical distribution function F based on the data z_i. The bootstrap now enables the estimation of the variance of the functional *t*, i.e. by repeatedly estimate $\hat{q}$ by using a simulated data set (Efron 1979, Efron, Tibshirani 1993) and we consider the dataset

$$D_{(b)} = \{z_1^*, z_2^*, \dots z_N^*\}$$

obtained by randomly sampling the known distribution. $D_{(b)}$ is called the bootstrap sample.

---

8 The bootstrap standard errors are calculated for a simplified version of the outcome equation, omitting the terms for the socioeconomic characteristics, which mainly replicates the extended model including the socioeconomic covariates. The exact form of the model underlying the bootstrap standard errors is

$$\Delta Y_{i,t} = a_0 + a_1 t + a_2 t^2 D[t < AD(t)] + \sum_{\substack{k=AD(t)\\ k\neq 0}}^{36} b_k D(t=k) + (g_1^{AD} t + g_2^{AD} t^2) D(AD(t) \le t \le -1)$$
$$+ (g_1^{PO} t + g_2^{PO} t^2) D(t \ge 1)$$

where estimation of the influence of socioeconomic characteristics is omitted (see Bergemann, Fitzenberger, Speckesser 2004).

Consider B repetitions of $D_{(b)}$ sampling and define

$$\hat{q}_b = t\left(D_{(b)}\right) \qquad b = 1,\ldots,B$$

the statistic computed by $D_{(b)}, b = 1,\ldots,B$. $\hat{q}_b$ is called the bootstrap replication of $\hat{q}$. The statistical properties of $\hat{q}$ can then be calculated on the basis of the distribution of $\hat{q}_b$. The bootstrap estimator of the variance of the estimator is the variance of the set $\hat{q}_b$, $b = 1,\ldots,B$:

$$\text{(21)} \qquad Var_{bs}\left(\hat{q}\right) = \frac{\sum_{b=1}^{B}\left(\hat{q}_{(b)} - \hat{q}_{(.)}\right)^2}{(B-1)}$$

where

$$\hat{q}_{(.)} = \frac{\sum_{b=1}^{B}\hat{q}_{(b)}}{B}$$

Under the assumption that the estimator $\hat{q}$ is normally distributed with mean q and variance s^2, one can easily calculate the confidence intervals by plugging the bootstrap estimate of the variance into the well known parametric confidence interval formula, using the critical values of the normal distribution. Then, the 100(1–α)%–confidence interval is given by

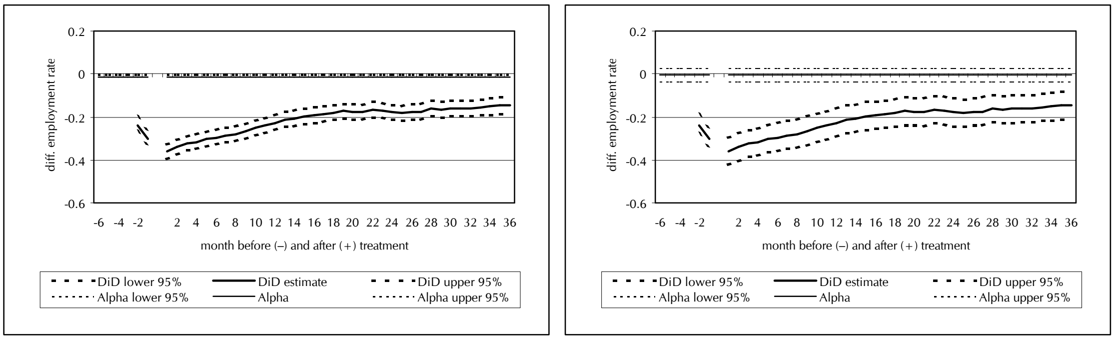
$$\text{(22)} \qquad \hat{q} \pm z_{a/2}\sqrt{Var_{bs}\left(\hat{q}\right)} = \hat{q} \pm z_{a/2}\sqrt{\frac{\sum_{b=1}^{B}\left(\hat{q}_{(b)} - \hat{q}_{(.)}\right)^2}{(B-1)}}$$

where z_a is the α–quantile from the standard normal distribution.

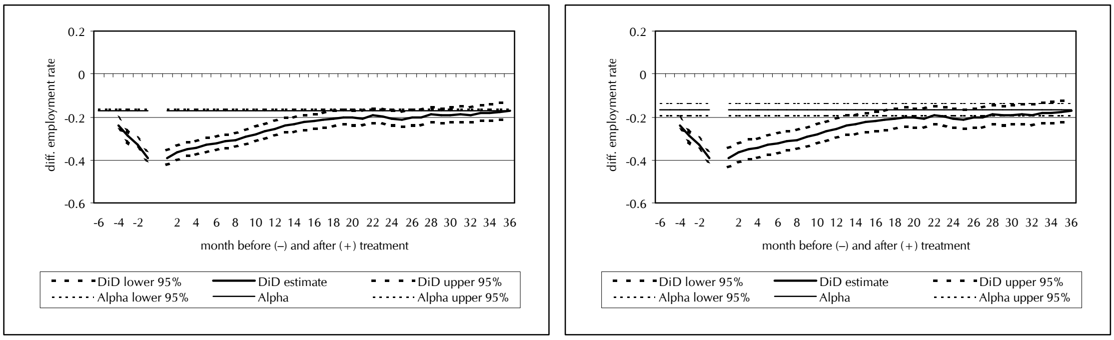
As exemplary, we report the confidence intervals of the estimated CDiD estimators for matching based on the propensity score with local linear regressions when the bandwidth is chosen according to Silverman. The following figure 2.4 reports the results for the estimated effects of participation in a first further training at different points in time. The left part of this figure describes the estimated confidence intervals without bootstrapping, the right side shows the confidence intervals for the same effects when the bootstrap estimated standard errors are based on 200 resamples. As expected, the standard errors increase for the CDID–estimator if we do not ignore the estimation error of the propensity score. It is however surprising that this does mainly affect the long–term preprogramme differences, for which the confidence intervals become much bigger. The confidence intervals of the CDID estimates increase, too, but mainly for the outcomes of very early (1990) and very late (1996) participation. The estimated effects of treatment on the treated are still significantly negative.

Figure 2.4 Employment effects of 1st participation in a further training scheme– LL kernel matching, h = hROT – Confidence intervals & bootstrap confidence intervals –

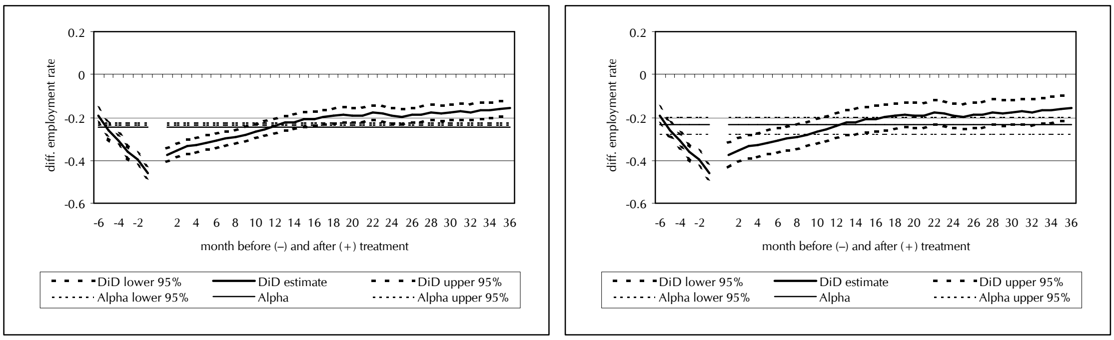
December 1990



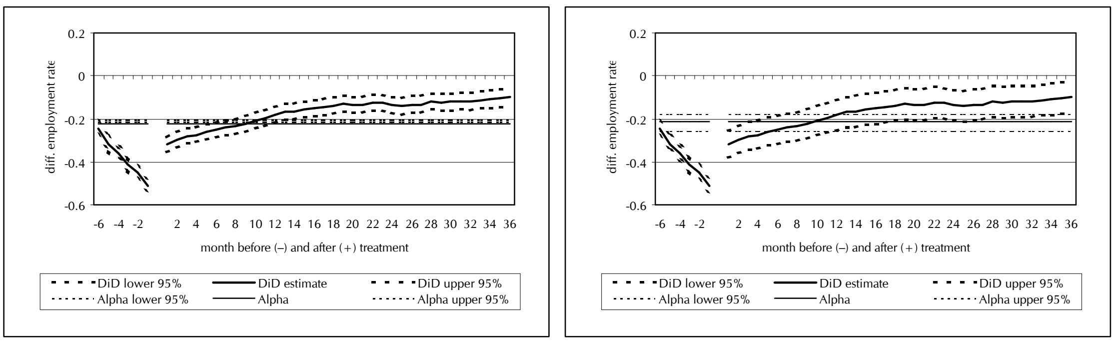
December 1992



December 1994



December 1996



2.5 Conclusion

The wide use of non–parametric matching approaches for the evaluation of ALMP in recent years originates from the awareness that one should be less restrictive to the data in non–experimental studies with respect to the functional form when creating an adequate comparison level one wants to contrast the estimated treatment effect to. However, non–parametric matching, too, offers options, among which the researcher has to choose with respect to critical parameters when applying these procedures. In this paper, we show the sensitivity of the estimated effects of treatment–on–the–treated if we vary (i) the local estimator (nearest neighbour, Nadaraya–Watson or local linear estimators), (ii) the bandwidth and (iii) take into account the sampling variability of the probit estimate for drawing inference on the estimated employment effect. Contrary to simulation studies, this paper should bring about evidence for the robustness of the evaluation results of real–world data if we critically consider the changes of certain parameters. First and foremost, this was done in order to decide upon the most appropriate specification applied in our associated paper (Bergemann, Fitzenberger, Speckesser 2004). The study conducted here should increase the sensitivity towards the results of the effects of further training reported in our associated paper, where we estimate the effects of further training based on the Labour Market Monitor Sachsen–Anhalt.

Following the suggestion of the previous paper (Bergemann et al. 2000), this evaluation distinguishes the effects of a first from those of a second treatment in further training and identifies average treatment effects on the treated of a first and the average incremental effect of a second participation in further training. The treatment effect is allowed to vary over time, so that we restrict the sensitivity analysis to exemplary evidence for the evaluation of a first participation in a further training programme starting by the end of 92 and a second further training programme starting by the end of 94. The robustness of the treatment effects when applying non–parametric standard errors resulting from a bootstrap is shown exemplarily for participation in a first further training programme at the end of 90, 92, 93 and 94 and matching on with local linear regressions and selecting the bandwidth according to Silverman's rule for the density of the standard normal distribution.

The results in section 2.4.4 indicate that neither the participation in a first nor in a second further training have positive effects for the participants in any specification. The participation in a first further training significantly decreases employment compared to non–participation. Participants in a second programme have an effect of treatment on the treated concerning employment, which is zero.

However this paper shows that the evaluation approach influences to a certain extent the evaluation results. In particular, the comparison of different matching approaches point out:

- The estimated effects are lower for evaluations of matched samples based on kernel regressions compared to the estimators in the case of nearest neighbour matching. We believe the results of kernel matching – and thus the more negative treatment outcome when applying the respective outcome equation – to be more credible because of two reasons:

1. Panel mortality: If we match participants to nearest neighbour non–participants, we do not benefit as much as we can from the information provided by the whole group of non–participants: As it is not warranted to observe nearest neighbour for the same period, the insignificant effect at the end of the period of observation when applying nearest neighbour matching could reflect composition effects of the non–treatment sample. We might e.g. observe mainly unemployed in the control group in the long run, because other individuals left the local labour market via intra German moving activity to the West. Then, the effects based on nearest neighbour matching overstate the true effect of treatment–on–the–treated. Kernel matching makes use of all available control observations at different points in time and allows to create a locally weighted average of all available non–treatment observations. It then provides a more credible non–treatment outcome if compositional effects of the naïve control group occur.
2. Practice of nearest neighbours: It is often stated that nearest neighbours prove better with respect to the solution of selection bias based on observables than kernel matching estimators, following the simple argument that nearest neighbour estimators correspond to kernel estimators with the smallest possible bandwidth – and thus the most similar non–treatment outcome and the smallest possible remaining bias based on observables. However, in practice, the advantages of nearest neighbour matching are less obvious, especially if implemented without replacement of the non–treatment observations. In this case, nearest neighbours can be quite dissimilar and the estimator is probably not optimal with respect to the balancing properties of differences in observable characteristics between treated and non–treated individuals.
3. Besides the problem of finding adequate matches in general (support problem), there is no consensus for the nearest neighbours estimators on how to take into consideration the substantive variability of the probit estimation to which the whole matching procedure refers. It could be shown how important the sampling error of the probit estimation is when reporting bootstrap standard errors.

- The selection of bandwidth in non–parametric regressions also affects the results. However, this is – contrary to what the vast literature on the correct choice of the bandwidth in non–parametric estimators suggests – of minor importance in our application.

To summarise, a clear statement about the efficiency of further training remains difficult. Although all results are negative, there is only little robust evidence about the true effectiveness of further training: Sensitivity to critical issues of matching show how far the econometric specification *does* influence the evaluation results – even if applying approaches that impute less structure on the models than traditional econometric estimators.

In our corresponding paper (Bergemann, Fitzenberger, Speckesser 2004), we explicitly model the employment dynamics and specify separate outcome equations for either the probability of staying in employment or of leaving non–employment. Considering the transition from non–employment to employment to be less likely than the probability of staying in employment, modeling the employment outcome as transition rates is more appropriate than using unconditional employment rates. In the case of such an employment outcome, the training effects become in some cases positive, but mostly insignificant. Training as a first treatment shows mostly insignificant but for some cases positive effects on the employment probabilities conditional upon the employment state in the previous month. The effect depends on the time the programs took place corresponding to the institutional changes taking place during the 90s. Combined sequences of two programs with a first training program do not prove successful. The incremental effects of the second treatment however appear to have slightly positive effects on the probability to remain employed.

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2.7 Appendix

Table 2.A1 Descriptive statistics for participants in 1st further training and non–participants

	Treated		Non–treated		Total	
	Mean	Std–Dev	Mean	Std–Dev	Mean	Std–Dev
Age 1990	36.5509	7.0363	37.8967	7.3796	37.6311	7.3321
Gender (1 = female)	0.5858	0.4928	0.4500	0.4976	0.4768	0.4995
Region						
Dessau	0.1309	0.3375	0.1142	0.3181	0.1175	0.3221
Halberstadt	0.0902	0.2866	0.0944	0.2925	0.0936	0.2913
Halle	0.1620	0.3686	0.1951	0.3963	0.1886	0.3912
Magdeburg	0.2338	0.4234	0.2411	0.4278	0.2397	0.4269
Merseburg	0.1445	0.3518	0.1290	0.3353	0.1321	0.3386
Sangerhausen	0.1232	0.3288	0.0923	0.2895	0.0984	0.2979
Stendal	0.0660	0.2483	0.0837	0.2770	0.0802	0.2716
Wittenberg	0.0495	0.2170	0.0501	0.2181	0.0500	0.2179
Professional education						
Semi–skilled worker	0.0107	0.1028	0.0143	0.1188	0.0136	0.1158
Skilled worker	0.4772	0.4997	0.4140	0.4926	0.4265	0.4946
Craftsman	0.0640	0.2449	0.0808	0.2726	0.0775	0.2675
Technical college	0.1872	0.3903	0.1913	0.3934	0.1905	0.3927
University education	0.2444	0.4300	0.2757	0.4469	0.2695	0.4438
Participation						
Begin of treatment	50.8700	28.8891				
End of treatment	66.3395	28.8847				
Number of observations	1031		4193		5224	

Table 2.A2 Descriptive statistics for participants in 2nd further training and non–participants

	Treated		Non–treated		Total	
	Mean	Std–Dev	Mean	Std–Dev	Mean	Std–Dev
Age 1990	36.3871	6.8838	37.6850	7.3468	37.6311	7.3321
Gender (1 = female)	0.7419	0.4386	0.4654	0.4989	0.4768	0.4995
Region						
Dessau	0.1567	0.3643	0.1158	0.3201	0.1175	0.3221
Halberstadt	0.0876	0.2833	0.0939	0.2917	0.0936	0.2913
Halle	0.1475	0.3554	0.1903	0.3926	0.1886	0.3912
Magdeburg	0.2258	0.4191	0.2403	0.4273	0.2397	0.4269
Merseburg	0.1429	0.3507	0.1316	0.3381	0.1321	0.3386
Sangerhausen	0.1336	0.3411	0.0969	0.2958	0.0984	0.2979
Stendal	0.0461	0.2102	0.0817	0.2739	0.0802	0.2716
Wittenberg	0.0599	0.2379	0.0495	0.2170	0.0500	0.2179
Professional education						
Semi–skilled worker	0.0046	0.0679	0.0140	0.1174	0.0136	0.1158
Skilled worker	0.5207	0.5007	0.4224	0.4940	0.4265	0.4946
Craftsman	0.0277	0.1644	0.0797	0.2708	0.0775	0.2675
Technical college	0.1843	0.3887	0.1907	0.3929	0.1905	0.3927
University education	0.2581	0.4386	0.2700	0.4440	0.2695	0.4438
Participation						
Begin of treatment	77.4793	22.5928				
End of treatment	87.0230	20.9206				
Number of observations	217		5007		5224	

Table 2.A3 Propensity score estimations for 1st and 2nd further training programmes

	1st Further Training		2nd Further Training	
Parameter	Estimate	t–statistic	Estimate	t–statistic
Constant	-0.95499	-5.55459	-2.23460	-5.81302
Age 35 - 44	-0.09174	-2.00778	-0.07353	-1.02142
Age 45 and older	-0.30528	-5.44670	-0.27571	-2.95357
Region				
Halberstadt	-0.08435	-0.95830	-0.14740	-1.06757
Halle	-0.17174	-2.29109	-0.24357	-2.07289
Magdeburg	-0.10208	-1.44008	-0.18402	-1.68509
Merseburg	-0.01614	-0.20355	-0.09536	-0.78347
Sangerhausen	0.08255	0.97595	0.01502	0.11780
Stendal	-0.21131	-2.23869	-0.36481	-2.26036
Wittenberg	-0.09081	-0.84968	-0.07440	-0.46031
Professional education (all)				
Skilled worker	0.08384	0.50110	0.40526	1.05797
Craftsman	-0.00739	-0.04022	-0.05233	-0.11637
Technical college	0.30257	1.66604	0.34878	0.84045
University education	0.18180	1.07158	0.56095	1.45740
Professional education (women)				
Semi-skilled worker	0.14164	0.44635		
Skilled worker	0.50664	8.32502	0.66004	6.55025
Craftsman	0.77006	4.32762	0.95078	2.71710
Technical college	-0.03407	-0.34066	0.47237	2.39654
University education	0.12014	1.51021	0.33052	2.66986
Number of Observations	5165		5165	
Number of pos. Observations	1021		216	
LR (zero slopes)	149.588		107.587	
Log likelihood	-2493.06		-843.292	

Table 2.A4 Estimation results of the effects of 1. further training on differences in employment

	NEAREST NEIGHBOURS		NADARAYA-WATSON		LOCAL LIN. h=ROT		LOCAL LIN. h=0.06		LOCAL LIN. h=0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
α_0	0.1266	9.4509	-0.1180	-3.8384	-0.1117	-3.8218	-0.0643	-1.9405	-0.1382	-5.7269
α_1	-0.0143	-22.8885	-0.0054	-5.6725	-0.0057	-6.2094	-0.0070	-7.0837	-0.0050	-6.1978
α_2	0.0001	19.2843	0.0000	5.2796	0.0000	5.7068	0.0000	6.5977	0.0000	5.6538
$\alpha_{\text{age controls}}$	-0.0195	-1.9280	0.0393	3.1854	0.0475	3.9300	0.0632	5.2389	0.0418	3.7839
$\alpha_{\text{sex controls}}$	0.2163	2.4345	-0.1031	-0.5545	-0.2712	-1.7915	-0.4032	-1.9227	-0.1713	-1.7407
$\alpha_{\text{another ALMP before controls}}$	0.5457	21.2612	1.4912	3.6341	1.4886	3.6794	1.9996	4.1505	1.1314	3.9086
$\alpha_{\text{age treated}}$	-0.0024	-0.2433	-0.0477	-6.3903	-0.0477	-6.3875	-0.0476	-6.3829	-0.0477	-6.4177
$\alpha_{\text{sex treated}}$	-0.2486	-2.7955	-0.0672	-9.1897	-0.0667	-9.1341	-0.0669	-9.1509	-0.0663	-9.0695
$\alpha_{\text{another ALMP before treated}}$	-0.5292	-42.7166	-0.5476	-52.0288	-0.5479	-52.0362	-0.5480	-52.0360	-0.5482	-51.8882
$\beta_{-6,0}$	-0.7516	-6.3773	-0.5878	-5.3511	-0.5640	-5.1612	-0.6700	-5.9039	-0.4830	-4.7534
$\beta_{-5,0}$	-0.6258	-6.8274	-0.4413	-4.8846	-0.4245	-4.7378	-0.5287	-5.6293	-0.3449	-4.1582
$\beta_{-4,0}$	-0.5034	-8.4243	-0.3524	-5.3109	-0.3442	-5.2676	-0.4441	-6.3323	-0.2840	-4.9525
$\beta_{-3,0}$	-0.4424	-10.2938	-0.3129	-5.6407	-0.3091	-5.6985	-0.3993	-6.7128	-0.2472	-5.2250
$\beta_{-2,0}$	-0.4178	-11.3945	-0.3469	-6.1567	-0.3381	-6.1326	-0.4264	-6.9279	-0.2683	-5.7436
$\beta_{-1,0}$	-0.5285	-13.8837	-0.4840	-7.3886	-0.4710	-7.3173	-0.5570	-7.6663	-0.3944	-7.4626
$\beta_{1,0}$	-0.5932	-14.0871	-0.3255	-6.4622	-0.3496	-7.1583	-0.3568	-6.7021	-0.3485	-8.1196
$\beta_{2,0}$	-0.5582	-13.3336	-0.2910	-5.8398	-0.3148	-6.5146	-0.3225	-6.1312	-0.3145	-7.3843
$\beta_{3,0}$	-0.5236	-12.2935	-0.2601	-5.2753	-0.2838	-5.9351	-0.2920	-5.6137	-0.2844	-6.7280
$\beta_{4,0}$	-0.5371	-12.4872	-0.2546	-5.1960	-0.2783	-5.8539	-0.2869	-5.5623	-0.2798	-6.6330
$\beta_{5,0}$	-0.5343	-12.5007	-0.2442	-5.0277	-0.2679	-5.6823	-0.2769	-5.4206	-0.2702	-6.4371
$\beta_{6,0}$	-0.5086	-11.9074	-0.2315	-4.8113	-0.2552	-5.4614	-0.2645	-5.2319	-0.2584	-6.1989
$\beta_{7,0}$	-0.4927	-11.2618	-0.2188	-4.5786	-0.2424	-5.2185	-0.2523	-5.0281	-0.2466	-5.9358
$\beta_{8,0}$	-0.4915	-11.0077	-0.2081	-4.3817	-0.2314	-5.0113	-0.2419	-4.8560	-0.2365	-5.7105
$\beta_{9,0}$	-0.4600	-10.2434	-0.1807	-3.8495	-0.2040	-4.4698	-0.2148	-4.3630	-0.2101	-5.1278
$\beta_{10,0}$	-0.4328	-9.4541	-0.1492	-3.2152	-0.1722	-3.8163	-0.1835	-3.7726	-0.1784	-4.4003
$\beta_{11,0}$	-0.4213	-9.1192	-0.1317	-2.8603	-0.1548	-3.4563	-0.1665	-3.4500	-0.1620	-4.0203
$\beta_{12,0}$	-0.4155	-8.9978	-0.1080	-2.3686	-0.1312	-2.9595	-0.1432	-2.9969	-0.1404	-3.5161
$\beta_{13,0}$	-0.4073	-8.8577	-0.0828	-1.8277	-0.1059	-2.4038	-0.1183	-2.4930	-0.1163	-2.9310
$\beta_{14,0}$	-0.3963	-8.4863	-0.0672	-1.4943	-0.0902	-2.0638	-0.1030	-2.1857	-0.1014	-2.5739
$\beta_{15,0}$	-0.3630	-7.5012	-0.0516	-1.1632	-0.0749	-1.7371	-0.0881	-1.8947	-0.0872	-2.2477
$\beta_{16,0}$	-0.3853	-7.8562	-0.0391	-0.8854	-0.0621	-1.4462	-0.0760	-1.6420	-0.0744	-1.9219
$\beta_{17,0}$	-0.3540	-7.4480	-0.0192	-0.4369	-0.0422	-0.9883	-0.0565	-1.2259	-0.0561	-1.4584
$\beta_{18,0}$	-0.3401	-7.0237	-0.0084	-0.1921	-0.0310	-0.7287	-0.0460	-1.0004	-0.0460	-1.2006
$\beta_{19,0}$	-0.3254	-6.5487	0.0052	0.1196	-0.0175	-0.4114	-0.0329	-0.7161	-0.0327	-0.8531
$\beta_{20,0}$	-0.3068	-6.1257	0.0040	0.0918	-0.0188	-0.4413	-0.0346	-0.7528	-0.0355	-0.9256
$\beta_{21,0}$	-0.3128	-6.3476	0.0075	0.1704	-0.0152	-0.3556	-0.0316	-0.6855	-0.0325	-0.8454
$\beta_{22,0}$	-0.2877	-5.7891	0.0303	0.6912	0.0081	0.1895	-0.0089	-0.1940	-0.0105	-0.2742
$\beta_{23,0}$	-0.2947	-5.8678	0.0270	0.6141	0.0046	0.1087	-0.0127	-0.2745	-0.0140	-0.3635
$\beta_{24,0}$	-0.2949	-5.6969	0.0291	0.6612	0.0069	0.1618	-0.0108	-0.2335	-0.0127	-0.3304
$\beta_{25,0}$	-0.3031	-5.8570	0.0290	0.6558	0.0069	0.1603	-0.0112	-0.2405	-0.0136	-0.3514
$\beta_{26,0}$	-0.3125	-6.0922	0.0430	0.9665	0.0208	0.4827	0.0022	0.0478	-0.0004	-0.0116
$\beta_{27,0}$	-0.2856	-5.5179	0.0615	1.3781	0.0393	0.9077	0.0203	0.4303	0.0168	0.4336
$\beta_{28,0}$	-0.2734	-5.2518	0.0679	1.5197	0.0457	1.0544	0.0261	0.5534	0.0225	0.5829
$\beta_{29,0}$	-0.2640	-4.8911	0.0710	1.5776	0.0491	1.1231	0.0290	0.6088	0.0250	0.6417
$\beta_{30,0}$	-0.2532	-4.6684	0.0803	1.7702	0.0583	1.3268	0.0379	0.7907	0.0333	0.8526
$\beta_{31,0}$	-0.2531	-4.5041	0.0818	1.7894	0.0597	1.3471	0.0391	0.8083	0.0331	0.8426
$\beta_{32,0}$	-0.2472	-4.3755	0.0827	1.7931	0.0608	1.3590	0.0398	0.8123	0.0332	0.8393
$\beta_{33,0}$	-0.2486	-4.2236	0.0909	1.9501	0.0692	1.5328	0.0476	0.9618	0.0403	1.0119

Table 2.A4 (cont.) Estimation results of the effects of 1. further training on differences in employment

	NEAREST NEIGH-BOURS		NADARAYA-WATSON		LOCAL LIN. h=ROT		LOCAL LIN. h=0.06		LOCAL LIN. h=0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
$\beta_{34,0}$	-0.2441	-4.1745	0.1009	2.1426	0.0791	1.7347	0.0572	1.1431	0.0490	1.2222
$\beta_{35,0}$	-0.2346	-3.9837	0.1104	2.3185	0.0884	1.9180	0.0662	1.3069	0.0582	1.4372
$\beta_{36,0}$	-0.2316	-3.9414	0.1188	2.4642	0.0969	2.0756	0.0741	1.4440	0.0657	1.6071
$\beta_{-6,1}$	0.0158	9.2680	0.0079	4.3194	0.0078	4.3248	0.0102	5.2622	0.0063	3.9756
$\beta_{-5,1}$	0.0136	9.5220	0.0057	3.5803	0.0056	3.6115	0.0080	4.7680	0.0041	2.9811
$\beta_{-4,1}$	0.0114	10.2306	0.0043	3.1641	0.0043	3.2612	0.0066	4.6267	0.0029	2.5748
$\beta_{-3,1}$	0.0099	10.3561	0.0034	2.6598	0.0034	2.7799	0.0056	4.1791	0.0020	1.8949
$\beta_{-2,1}$	0.0087	9.8345	0.0034	2.6303	0.0034	2.6702	0.0055	3.9619	0.0018	1.6996
$\beta_{-1,1}$	0.0098	10.6895	0.0050	3.4288	0.0049	3.4247	0.0070	4.3892	0.0032	2.7025
$\beta_{1,1}$	0.0118	11.3687	0.0025	2.1296	0.0031	2.7225	0.0037	2.9533	0.0030	2.8991
$\beta_{2,1}$	0.0114	10.9123	0.0022	1.8467	0.0028	2.4256	0.0033	2.6951	0.0026	2.5731
$\beta_{3,1}$	0.0110	10.3014	0.0018	1.5164	0.0024	2.0827	0.0029	2.3915	0.0022	2.1953
$\beta_{4,1}$	0.0114	10.5806	0.0018	1.5406	0.0024	2.1104	0.0029	2.4303	0.0022	2.2367
$\beta_{5,1}$	0.0116	10.7344	0.0018	1.5831	0.0024	2.1554	0.0030	2.4832	0.0023	2.2949
$\beta_{6,1}$	0.0112	10.3308	0.0017	1.4979	0.0023	2.0687	0.0029	2.4140	0.0022	2.2117
$\beta_{7,1}$	0.0110	9.8547	0.0016	1.4082	0.0022	1.9739	0.0028	2.3390	0.0021	2.1222
$\beta_{8,1}$	0.0114	9.7968	0.0017	1.4483	0.0022	2.0122	0.0028	2.3862	0.0022	2.1824
$\beta_{9,1}$	0.0108	9.1574	0.0013	1.1493	0.0019	1.7057	0.0025	2.1108	0.0018	1.8587
$\beta_{10,1}$	0.0105	8.7617	0.0009	0.7508	0.0014	1.2932	0.0020	1.7373	0.0014	1.4191
$\beta_{11,1}$	0.0105	8.6447	0.0007	0.6101	0.0013	1.1514	0.0019	1.6143	0.0013	1.2783
$\beta_{12,1}$	0.0107	8.7349	0.0004	0.3550	0.0010	0.8914	0.0016	1.3821	0.0010	1.0137
$\beta_{13,1}$	0.0109	8.9146	0.0001	0.0811	0.0007	0.6084	0.0013	1.1287	0.0007	0.7252
$\beta_{14,1}$	0.0108	8.5591	-0.0002	-0.1795	0.0004	0.3394	0.0010	0.8874	0.0004	0.4439
$\beta_{15,1}$	0.0102	7.8135	-0.0006	-0.5408	0.0000	-0.0176	0.0006	0.5713	0.0001	0.0713
$\beta_{16,1}$	0.0110	8.1163	-0.0008	-0.7039	-0.0002	-0.1877	0.0005	0.4251	-0.0001	-0.1028
$\beta_{17,1}$	0.0102	7.8424	-0.0011	-1.0393	-0.0006	-0.5325	0.0001	0.1148	-0.0004	-0.4537
$\beta_{18,1}$	0.0100	7.3993	-0.0013	-1.1973	-0.0007	-0.7005	0.0000	-0.0292	-0.0006	-0.6172
$\beta_{19,1}$	0.0102	7.4025	-0.0015	-1.4052	-0.0009	-0.9136	-0.0002	-0.2196	-0.0008	-0.8426
$\beta_{20,1}$	0.0095	6.8282	-0.0016	-1.4602	-0.0010	-0.9725	-0.0003	-0.2656	-0.0008	-0.8871
$\beta_{21,1}$	0.0095	6.8238	-0.0017	-1.5829	-0.0011	-1.1023	-0.0004	-0.3754	-0.0009	-1.0146
$\beta_{22,1}$	0.0089	6.2470	-0.0022	-2.0196	-0.0016	-1.5603	-0.0009	-0.7888	-0.0014	-1.5014
$\beta_{23,1}$	0.0091	6.3160	-0.0022	-2.0077	-0.0016	-1.5463	-0.0009	-0.7701	-0.0014	-1.4696
$\beta_{24,1}$	0.0091	6.0550	-0.0024	-2.2453	-0.0018	-1.7956	-0.0011	-0.9930	-0.0016	-1.7270
$\beta_{25,1}$	0.0093	6.1282	-0.0026	-2.4140	-0.0020	-1.9750	-0.0013	-1.1515	-0.0018	-1.9119
$\beta_{26,1}$	0.0097	6.2731	-0.0027	-2.5563	-0.0022	-2.1255	-0.0014	-1.2840	-0.0019	-2.0626
$\beta_{27,1}$	0.0094	5.9549	-0.0030	-2.8220	-0.0025	-2.4033	-0.0017	-1.5309	-0.0022	-2.3559
$\beta_{28,1}$	0.0099	6.1354	-0.0034	-3.1308	-0.0028	-2.7246	-0.0020	-1.8151	-0.0025	-2.7136
$\beta_{29,1}$	0.0097	5.6778	-0.0035	-3.2644	-0.0030	-2.8690	-0.0022	-1.9380	-0.0026	-2.8669
$\beta_{30,1}$	0.0092	5.3058	-0.0037	-3.3804	-0.0031	-2.9936	-0.0023	-2.0472	-0.0028	-2.9927
$\beta_{31,1}$	0.0095	5.1597	-0.0037	-3.4181	-0.0032	-3.0362	-0.0024	-2.0808	-0.0028	-3.0300
$\beta_{32,1}$	0.0093	5.0117	-0.0038	-3.4990	-0.0033	-3.1265	-0.0025	-2.1576	-0.0029	-3.1193
$\beta_{33,1}$	0.0100	5.1292	-0.0040	-3.6294	-0.0035	-3.2690	-0.0026	-2.2798	-0.0030	-3.2676
$\beta_{34,1}$	0.0098	4.9230	-0.0043	-3.8455	-0.0037	-3.4954	-0.0029	-2.4847	-0.0033	-3.5098
$\beta_{35,1}$	0.0096	4.8534	-0.0044	-3.9607	-0.0039	-3.6177	-0.0030	-2.5924	-0.0034	-3.6508
$\beta_{36,1}$	0.0097	4.9259	-0.0046	-4.0811	-0.0040	-3.7474	-0.0032	-2.7051	-0.0036	-3.7886

Table 2.A4 (cont.) Estimation results of the effects of 1. further training on differences in employment

	NEAREST NEIGH-BOURS		NADARAYA-WATSON		LOCAL LIN. h = ROT		LOCAL LIN. h = 0.06		LOCAL LIN. h = 0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
$\gamma^{AD}_{\text{age controls},0}$	0.0033	0.0638	-0.3495	-5.3961	-0.2924	-4.4689	-0.3266	-4.9735	-0.2304	-3.8428
$\gamma^{AD}_{\text{age controls},1}$	0.0003	0.0170	-0.0621	-2.1549	-0.0412	-1.4107	-0.0514	-1.7612	-0.0180	-0.7133
$\gamma^{PO}_{\text{age controls},0}$	-0.0377	-1.7835	-0.0995	-4.9446	-0.0832	-4.1631	-0.0926	-4.6154	-0.1013	-5.3734
$\gamma^{PO}_{\text{age controls },1}$	0.0028	3.0381	0.0015	2.0149	0.0015	1.9570	0.0013	1.6886	0.0019	2.6185
$\gamma^{AD}_{\text{age treated},0}$	0.0170	0.3321	0.0534	1.7855	0.0521	1.7363	0.0490	1.6396	0.0533	1.7747
$\gamma^{AD}_{\text{age treated},1}$	0.0120	0.7420	0.0215	2.4931	0.0210	2.4285	0.0206	2.3948	0.0206	2.3906
$\gamma^{PO}_{\text{age treated},0}$	-0.0453	-2.1783	-0.0574	-4.2976	-0.0584	-4.3734	-0.0600	-4.4962	-0.0555	-4.1663
$\gamma^{PO}_{\text{age treated},1}$	-0.0008	-0.9422	0.0005	0.9221	0.0005	0.9031	0.0005	0.9696	0.0004	0.7943
$\gamma^{AD}_{\text{sex controls},0}$	-0.0672	-0.1444	-2.4324	-3.0259	-1.5844	-2.1431	-1.7894	-1.8483	-0.8934	-1.8160
$\gamma^{AD}_{\text{sex controls},1}$	-0.0190	-0.1276	-0.6192	-2.8531	-0.4293	-2.1863	-0.6040	-2.3863	-0.2110	-1.6961
$\gamma^{PO}_{\text{sex controls},0}$	-0.6278	-5.1170	-1.5294	-4.4950	-1.0604	-3.6618	-1.5267	-3.8120	-0.6190	-3.2255
$\gamma^{PO}_{\text{sex controls},1}$	0.0113	2.5283	0.0378	2.9447	0.0314	2.8961	0.0468	3.1226	0.0043	0.5804
$\gamma^{AD}_{\text{sex treated},0}$	-0.0042	-0.0091	-0.0518	-1.8226	-0.0546	-1.9176	-0.0528	-1.8551	-0.0577	-2.0270
$\gamma^{AD}_{\text{sex treated},1}$	0.0067	0.0450	-0.0042	-0.5212	-0.0047	-0.5763	-0.0046	-0.5725	-0.0047	-0.5849
$\gamma^{PO}_{\text{sex treated},0}$	0.4439	3.6138	-0.1645	-12.8935	-0.1643	-12.8840	-0.1647	-12.9051	-0.1633	-12.8055
$\gamma^{PO}_{\text{sex treated},1}$	-0.0092	-2.0517	0.0033	7.0234	0.0033	7.0376	0.0033	7.0146	0.0034	7.0795
$\gamma^{AD}_{\text{another ALMP before controls},0}$	-0.1452	-1.5123	-2.7769	-2.0226	-2.9127	-2.1247	-3.9735	-2.4438	-1.8270	-1.8597
$\gamma^{AD}_{\text{another ALMP LMP before controls},1}$	-0.0037	-0.1414	-0.2064	-0.5529	-0.3099	-0.8365	-0.1738	-0.3945	-0.3627	-1.3823
$\gamma^{PO}_{\text{another ALMP LMP before controls},0}$	-0.1050	-1.8189	-0.2864	-0.4185	-0.8246	-1.2393	-0.5720	-0.7313	-0.7151	-1.4935
$\gamma^{PO}_{\text{another ALMP before controls},1}$	0.0017	0.5519	0.0482	1.9538	0.0503	2.1141	0.0429	1.5301	0.0286	1.6593
$\gamma^{AD}_{\text{another ALMP before treated},0}$	0.3925	9.1539	0.4102	13.6398	0.4097	13.5801	0.4065	13.4947	0.4092	13.6641
$\gamma^{AD}_{\text{another ALMP before treated},1}$	0.0315	2.8605	0.0326	4.0752	0.0323	4.0355	0.0319	3.9951	0.0318	3.9852
$\gamma^{PO}_{\text{another ALMP before treated},0}$	0.4604	18.0888	0.4881	27.6976	0.4866	27.6247	0.4849	27.5063	0.4893	27.8076
$\gamma^{PO}_{\text{another ALMP before treated},0}$	0.0011	0.8389	0.0010	1.5709	0.0010	1.5517	0.0010	1.6218	0.0009	1.4765
No. of observations	5509		11504		11504		11504		11504	

Table 2.A5 Estimation results of the effects of 2. further training on differences in employment

	NEAREST NEIGH-BOURS		NADARAYA-WATSON		LOCAL LIN. h=ROT		LOCAL LIN. h=0.06		LOCAL LIN. h=0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
α_o	-0.2186	-2.7270	-0.3397	-3.0999	-0.3614	-3.3105	-0.3619	-3.3198	-0.3032	-3.4990
α_1	-0.0118	-4.8443	-0.0121	-5.0234	-0.0119	-4.8804	-0.0118	-4.8808	-0.0139	-6.2748
α_2	0.0001	4.5871	0.0001	6.3002	0.0001	6.2841	0.0001	6.2830	0.0001	7.4816
$\alpha_{\text{age controls}}$	0.1212	3.9927	-0.1682	-2.7047	-0.1707	-2.7348	-0.1708	-2.7359	-0.0438	-0.6778
$\alpha_{\text{sex controls}}$	-0.1892	-7.5467	0.2046	0.3782	0.4168	0.8756	0.4088	0.8631	-0.2313	-1.1843
$\alpha_{\text{another ALMP before controls}}$	0.6330	19.1325	0.7701	1.1911	0.5709	0.9172	0.5682	0.9159	0.1220	0.3804
$\alpha_{\text{age treated}}$	-0.0814	-2.5082	0.0286	1.8706	0.0287	1.8804	0.0287	1.8805	0.0256	1.6905
$\alpha_{\text{another ALMP before treated}}$	-0.1524	-6.7059	-0.1448	-9.2263	-0.1441	-9.1406	-0.1441	-9.1395	-0.1405	-8.9652
$\beta_{-6,0}$	-0.4094	-1.6448	-0.7096	-2.8716	-0.6365	-2.6512	-0.6368	-2.6570	-0.7450	-4.2448
$\beta_{-5,0}$	-0.4514	-2.2161	-0.6152	-2.9572	-0.5564	-2.7406	-0.5566	-2.7459	-0.6573	-4.2069
$\beta_{-4,0}$	-0.6009	-3.6849	-0.5459	-3.2523	-0.5023	-3.0956	-0.5024	-3.1008	-0.5893	-4.6620
$\beta_{-3,0}$	-0.5935	-4.2151	-0.5021	-3.2861	-0.4751	-3.2424	-0.4751	-3.2473	-0.5300	-4.4480
$\beta_{-2,0}$	-0.6075	-4.3760	-0.5525	-3.4413	-0.5409	-3.5964	-0.5408	-3.6011	-0.5786	-4.9850
$\beta_{-1,0}$	-0.6624	-4.6467	-0.6039	-3.2669	-0.6077	-3.5804	-0.6075	-3.5853	-0.6282	-4.9950
$\beta_{1,0}$	-0.4091	-1.9958	-0.3510	-2.1658	-0.3233	-2.0371	-0.3245	-2.0478	-0.5681	-4.4025
$\beta_{2,0}$	-0.4057	-1.9572	-0.3598	-2.2488	-0.3326	-2.1206	-0.3338	-2.1309	-0.5721	-4.4600
$\beta_{3,0}$	-0.3470	-1.6713	-0.3770	-2.4357	-0.3506	-2.3046	-0.3517	-2.3149	-0.5826	-4.6791
$\beta_{4,0}$	-0.3866	-1.9062	-0.3972	-2.5976	-0.3710	-2.4662	-0.3720	-2.4762	-0.5953	-4.8035
$\beta_{5,0}$	-0.2448	-1.2319	-0.4299	-2.8157	-0.4047	-2.6905	-0.4057	-2.7002	-0.6224	-4.9803
$\beta_{6,0}$	-0.2230	-1.0957	-0.4629	-3.0209	-0.4382	-2.8989	-0.4390	-2.9082	-0.6492	-5.1166
$\beta_{7,0}$	-0.1453	-0.6771	-0.4846	-3.2148	-0.4607	-3.0964	-0.4615	-3.1055	-0.6657	-5.3248
$\beta_{8,0}$	0.0222	0.0985	-0.4429	-2.8308	-0.4191	-2.7094	-0.4198	-2.7168	-0.6123	-4.6244
$\beta_{9,0}$	0.0397	0.1633	-0.4100	-2.5782	-0.3871	-2.4607	-0.3877	-2.4670	-0.5739	-4.2214
$\beta_{10,0}$	-0.1292	-0.5299	-0.4341	-2.7517	-0.4118	-2.6377	-0.4123	-2.6437	-0.5898	-4.3570
$\beta_{11,0}$	-0.2233	-0.8631	-0.4372	-2.8071	-0.4152	-2.6927	-0.4156	-2.6982	-0.5855	-4.3708
$\beta_{12,0}$	-0.1742	-0.6800	-0.5218	-3.4756	-0.5002	-3.3649	-0.5006	-3.3707	-0.6630	-5.2173
$\beta_{13,0}$	-0.2848	-1.2208	-0.5366	-3.6383	-0.5160	-3.5314	-0.5162	-3.5369	-0.6724	-5.3850
$\beta_{14,0}$	-0.2570	-1.0973	-0.5067	-3.3881	-0.4867	-3.2840	-0.4869	-3.2886	-0.6368	-4.9856
$\beta_{15,0}$	-0.2744	-1.1499	-0.5027	-3.3783	-0.4834	-3.2769	-0.4835	-3.2808	-0.6245	-4.8974
$\beta_{16,0}$	-0.2321	-1.0062	-0.5200	-3.4358	-0.5014	-3.3352	-0.5014	-3.3384	-0.6361	-4.8456
$\beta_{17,0}$	-0.3722	-1.6093	-0.5374	-3.5435	-0.5194	-3.4455	-0.5193	-3.4483	-0.6480	-4.9119
$\beta_{18,0}$	-0.3325	-1.1035	-0.5565	-3.6872	-0.5393	-3.5929	-0.5391	-3.5953	-0.6585	-5.0143
$\beta_{19,0}$	-0.2960	-0.9775	-0.5815	-3.8308	-0.5645	-3.7364	-0.5642	-3.7382	-0.6750	-5.0893
$\beta_{20,0}$	-0.2412	-0.8015	-0.5971	-3.9443	-0.5813	-3.8612	-0.5809	-3.8627	-0.6842	-5.1948
$\beta_{21,0}$	-0.2456	-0.8183	-0.6167	-4.2246	-0.6012	-4.1476	-0.6008	-4.1491	-0.6955	-5.5299
$\beta_{22,0}$	-0.3072	-0.9133	-0.5979	-3.9032	-0.5827	-3.8344	-0.5822	-3.8348	-0.6693	-4.9903
$\beta_{23,0}$	-0.3771	-1.0886	-0.6375	-4.1537	-0.6230	-4.0934	-0.6225	-4.0936	-0.7020	-5.2317
$\beta_{24,0}$	-0.4091	-1.1321	-0.6184	-4.0269	-0.6044	-3.9713	-0.6037	-3.9709	-0.6752	-5.0508
$\beta_{25,0}$	-0.3480	-0.9184	-0.6640	-4.3844	-0.6506	-4.3383	-0.6499	-4.3378	-0.7147	-5.4587
$\beta_{26,0}$	-0.2387	-0.6314	-0.5969	-3.8762	-0.5841	-3.8332	-0.5833	-3.8317	-0.6392	-4.8058
$\beta_{27,0}$	-0.3488	-0.8992	-0.5682	-3.5951	-0.5562	-3.5612	-0.5552	-3.5587	-0.6022	-4.3897
$\beta_{28,0}$	-0.4243	-1.0616	-0.6029	-3.8251	-0.5918	-3.7982	-0.5908	-3.7955	-0.6306	-4.6038
$\beta_{29,0}$	-0.4570	-1.0747	-0.6175	-3.9315	-0.6066	-3.9141	-0.6055	-3.9109	-0.6361	-4.6746
$\beta_{30,0}$	-0.2914	-0.7737	-0.6418	-4.0364	-0.6310	-4.0273	-0.6298	-4.0239	-0.6510	-4.7420
$\beta_{31,0}$	-0.3145	-0.8358	-0.6214	-3.8930	-0.6110	-3.8903	-0.6097	-3.8864	-0.6241	-4.5624
$\beta_{32,0}$	-0.3318	-0.8849	-0.6435	-4.0243	-0.6337	-4.0327	-0.6323	-4.0284	-0.6387	-4.6856
$\beta_{33,0}$	-0.3163	-0.7885	-0.5992	-3.6823	-0.5900	-3.6903	-0.5885	-3.6853	-0.5877	-4.2212

Table 2.A5 (cont.) Estimation results of the effects of 2. further training on differences in employment

	NEAREST NEIGH-BOURS		NADARAYA-WATSON		LOCAL LIN. h=ROT		LOCAL LIN. h=0.06		LOCAL LIN. h=0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
$\beta_{34,0}$	-0.2616	-0.6171	-0.5585	-3.3778	-0.5497	-3.3811	-0.5482	-3.3754	-0.5376	-3.8035
$\beta_{35,0}$	-0.1062	-0.2830	-0.5621	-3.3773	-0.5540	-3.3894	-0.5524	-3.3834	-0.5336	-3.7686
$\beta_{36,0}$	0.0475	0.1249	-0.5796	-3.5072	-0.5718	-3.5273	-0.5701	-3.5211	-0.5424	-3.9072
$\beta_{-6,1}$	0.0108	2.7453	0.0160	4.2988	0.0151	4.1202	0.0151	4.1262	0.0178	6.0465
$\beta_{-5,1}$	0.0113	3.2456	0.0149	4.4955	0.0141	4.3196	0.0141	4.3248	0.0167	6.0644
$\beta_{-4,1}$	0.0135	4.3035	0.0138	4.7205	0.0132	4.5880	0.0132	4.5921	0.0157	6.2391
$\beta_{-3,1}$	0.0131	4.4211	0.0131	4.7515	0.0128	4.6964	0.0128	4.6999	0.0149	6.0954
$\beta_{-2,1}$	0.0131	4.4329	0.0137	4.8132	0.0135	4.8958	0.0135	4.8990	0.0154	6.3542
$\beta_{-1,1}$	0.0137	4.5451	0.0143	4.6156	0.0143	4.8396	0.0143	4.8431	0.0159	6.3557
$\beta_{1,1}$	0.0125	3.2519	0.0117	4.0683	0.0113	3.9669	0.0113	3.9753	0.0158	6.2127
$\beta_{2,1}$	0.0128	3.2657	0.0120	4.2242	0.0117	4.1224	0.0117	4.1305	0.0161	6.3477
$\beta_{3,1}$	0.0121	3.0895	0.0123	4.3799	0.0119	4.2737	0.0119	4.2816	0.0162	6.4853
$\beta_{4,1}$	0.0123	3.1720	0.0125	4.5072	0.0122	4.3988	0.0122	4.4063	0.0164	6.5721
$\beta_{5,1}$	0.0099	2.5799	0.0130	4.6657	0.0127	4.5596	0.0127	4.5668	0.0168	6.6955
$\beta_{6,1}$	0.0104	2.5760	0.0134	4.7804	0.0130	4.6746	0.0131	4.6815	0.0171	6.7513
$\beta_{7,1}$	0.0093	2.1947	0.0136	4.8859	0.0132	4.7805	0.0132	4.7870	0.0172	6.8385
$\beta_{8,1}$	0.0064	1.4871	0.0131	4.6764	0.0128	4.5709	0.0128	4.5766	0.0167	6.4980
$\beta_{9,1}$	0.0065	1.4104	0.0129	4.5468	0.0126	4.4454	0.0126	4.4505	0.0163	6.3029
$\beta_{10,1}$	0.0092	1.9113	0.0132	4.6776	0.0129	4.5769	0.0129	4.5816	0.0165	6.3948
$\beta_{11,1}$	0.0110	2.2414	0.0132	4.7310	0.0129	4.6299	0.0129	4.6342	0.0165	6.4161
$\beta_{12,1}$	0.0097	1.9974	0.0145	5.2640	0.0142	5.1612	0.0142	5.1656	0.0177	7.0048
$\beta_{13,1}$	0.0115	2.4681	0.0148	5.4047	0.0145	5.3033	0.0145	5.3074	0.0179	7.1320
$\beta_{14,1}$	0.0107	2.2760	0.0145	5.2754	0.0142	5.1768	0.0142	5.1803	0.0175	6.9430
$\beta_{15,1}$	0.0120	2.4793	0.0144	5.2538	0.0141	5.1569	0.0141	5.1600	0.0173	6.8688
$\beta_{16,1}$	0.0116	2.4039	0.0146	5.2749	0.0143	5.1776	0.0143	5.1801	0.0174	6.8244
$\beta_{17,1}$	0.0145	2.8483	0.0150	5.4181	0.0147	5.3218	0.0147	5.3241	0.0177	6.9406
$\beta_{18,1}$	0.0149	2.4142	0.0152	5.5256	0.0150	5.4312	0.0150	5.4331	0.0179	7.0124
$\beta_{19,1}$	0.0138	2.1647	0.0157	5.6590	0.0154	5.5637	0.0154	5.5653	0.0182	7.0945
$\beta_{20,1}$	0.0121	1.8828	0.0158	5.7221	0.0156	5.6334	0.0156	5.6347	0.0183	7.1403
$\beta_{21,1}$	0.0115	1.7885	0.0158	5.8279	0.0156	5.7419	0.0156	5.7429	0.0182	7.2437
$\beta_{22,1}$	0.0123	1.7520	0.0158	5.7055	0.0156	5.6279	0.0156	5.6284	0.0181	7.0349
$\beta_{23,1}$	0.0134	1.8499	0.0163	5.8478	0.0161	5.7750	0.0161	5.7753	0.0184	7.1568
$\beta_{24,1}$	0.0140	1.8067	0.0160	5.7679	0.0159	5.6999	0.0158	5.6998	0.0181	7.0485
$\beta_{25,1}$	0.0119	1.4485	0.0165	5.9473	0.0163	5.8844	0.0163	5.8841	0.0185	7.2303
$\beta_{26,1}$	0.0080	0.9617	0.0157	5.6374	0.0155	5.5801	0.0155	5.5793	0.0176	6.8657
$\beta_{27,1}$	0.0103	1.1827	0.0154	5.4681	0.0152	5.4197	0.0152	5.4184	0.0172	6.6337
$\beta_{28,1}$	0.0129	1.4108	0.0158	5.6184	0.0156	5.5710	0.0156	5.5694	0.0175	6.7545
$\beta_{29,1}$	0.0142	1.4444	0.0159	5.6631	0.0157	5.6197	0.0157	5.6177	0.0175	6.7675
$\beta_{30,1}$	0.0088	0.9898	0.0164	5.8072	0.0163	5.7698	0.0163	5.7676	0.0179	6.8853
$\beta_{31,1}$	0.0111	1.2800	0.0161	5.6783	0.0159	5.6487	0.0159	5.6462	0.0175	6.7419
$\beta_{32,1}$	0.0114	1.3180	0.0163	5.7543	0.0162	5.7312	0.0162	5.7284	0.0176	6.8012
$\beta_{33,1}$	0.0104	1.0584	0.0158	5.5407	0.0157	5.5211	0.0157	5.5179	0.0170	6.5317
$\beta_{34,1}$	0.0081	0.7089	0.0153	5.3142	0.0151	5.2951	0.0151	5.2915	0.0163	6.2464
$\beta_{35,1}$	0.0017	0.1849	0.0153	5.3139	0.0152	5.3015	0.0152	5.2976	0.0163	6.2254
$\beta_{36,1}$	-0.0037	-0.4498	0.0154	5.3651	0.0153	5.3581	0.0153	5.3541	0.0162	6.2806

Table 2.A5 (cont.) Estimation results of the effects of 2. further training on differences in employment

	NEAREST NEIGH-BOURS		NADARAYA-WATSON		LOCAL LIN. h=ROT		LOCAL LIN. h=0.06		LOCAL LIN. h=0.02	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
$\gamma^{AD}_{\text{age controls},0}$	-0.1663	-1.6304	-0.0225	-0.1211	-0.0160	-0.0860	-0.0161	-0.0867	0.0387	0.3942
$\gamma^{AD}_{\text{age controls},1}$	-0.0195	-0.8069	-0.0504	-0.7706	-0.0479	-0.7299	-0.0479	-0.7299	-0.0229	-0.7734
$\gamma^{PO}_{\text{age controls},0}$	-0.2422	-2.8807	0.0963	1.2041	0.1010	1.2567	0.1011	1.2575	0.1808	2.3848
$\gamma^{PO}_{\text{age controls },1}$	0.0144	3.8364	0.0002	0.0844	0.0000	-0.0107	0.0000	-0.0108	-0.0080	-2.9862
$\gamma^{AD}_{\text{ age treated},0}$	0.1214	1.1241	-0.0534	-1.3842	-0.0531	-1.3742	-0.0530	-1.3738	-0.0513	-1.3375
$\gamma^{AD}_{\text{ age treated},1}$	0.0226	0.8755	0.0017	0.1711	0.0018	0.1788	0.0018	0.1792	0.0016	0.1602
$\gamma^{PO}_{\text{age treated},0}$	-0.1259	-1.4437	0.0218	0.9467	0.0215	0.9370	0.0215	0.9376	0.0175	0.7649
$\gamma^{PO}_{\text{age treated},1}$	-0.0198	-4.9271	-0.0041	-4.9966	-0.0041	-4.9945	-0.0041	-4.9948	-0.0038	-4.6584
$\gamma^{AD}_{\text{sex controls},0}$	0.1084	1.2987	1.9883	1.2309	0.9523	0.7041	0.9591	0.7125	1.0475	2.0355
$\gamma^{AD}_{\text{sex controls},1}$	0.0059	0.2786	0.4420	1.0636	0.1849	0.5319	0.1865	0.5392	0.1757	1.3262
$\gamma^{PO}_{\text{sex controls},0}$	0.0167	0.2704	0.0204	0.0265	-0.0490	-0.0715	-0.0543	-0.0797	-0.4355	-1.4575
$\gamma^{PO}_{\text{sex controls},1}$	0.0017	0.4717	0.0302	1.0726	0.0274	1.0939	0.0274	1.1000	0.0149	1.3626
$\gamma^{AD}_{\text{ another ALMP before controls},0}$	-0.1604	-1.3169	-0.5969	-0.3615	-0.7743	-0.5265	-0.7722	-0.5267	-0.2013	-0.2656
$\gamma^{AD}_{\text{ another ALMP LMP before controls},1}$	-0.0128	-0.4286	-0.1087	-0.2415	-0.2495	-0.5978	-0.2485	-0.5974	-0.1560	-0.7467
$\gamma^{PO}_{\text{another ALMP LMP before controls},0}$	-0.0034	-0.0490	2.0769	2.1183	2.3533	2.5407	2.3398	2.5342	0.6360	1.3175
$\gamma^{PO}_{\text{ another ALMP before controls},1}$	-0.0250	-5.3561	-0.1249	-3.7869	-0.1316	-4.3246	-0.1308	-4.3123	-0.0418	-2.5395
$\gamma^{AD}_{\text{ another ALMP before treated},0}$	0.1133	1.4972	0.0565	1.4574	0.0577	1.4724	0.0577	1.4722	0.0511	1.3162
$\gamma^{AD}_{\text{ another ALMP before treated},1}$	0.0054	0.2856	-0.0023	-0.2181	-0.0016	-0.1559	-0.0016	-0.1562	-0.0026	-0.2507
$\gamma^{PO}_{\text{ another ALMP before treated},0}$	0.2733	4.1594	-0.0497	-2.1403	-0.0511	-2.1980	-0.0511	-2.1972	-0.0394	-1.7044
$\gamma^{PO}_{\text{ another ALMP before treated},0}$	-0.0191	-5.0073	0.0008	0.9522	0.0008	1.0072	0.0008	1.0054	0.0003	0.4262
No. of observations	40095		50917		50917		50917		50917	

3 Using social insurance data for the evaluation of active labour market policy: Employment effects of further training for the unemployed in Germany

3.1 Introduction[1]

In 2002 the German Federal Employment Service (*Bundesanstalt für Arbeit*, BA) spent around 20 Bill. € for Active Labour Market Policies (ALMP)[2] – mainly for further training and temporary employment aiming at the (re–) integration of unemployed. The main question for social scientists as well as for policy makers is whether these policies actually increased the employment chances of the people they seek to help.

Over the last years, a number of surveys have been published about the effectiveness of the programmes summarising empirical evaluation studies of further training and job creation schemes in Germany (Fitzenberger, Speckesser 2000, 2002; Hagen, Steiner 2000; Rabe 2000; Fertig et al. 2001; Hujer, Caliendo 2001). Practically all the studies make use of the micro data of panel surveys (such as the German Socio–Economic Panel [GSOEP] and the Labour Market Monitors for East Germany [LMM] or for the Federal State of Saxony–Anhalt [LMM–SA]). Although the GSOEP and the LMM–SA are both rich with respect to informative covariates, which can be of help to overcome the microeconomic evaluation problem, the evaluation studies summarised in these surveys suffer from severe shortcomings with respect to the quality of the treatment information and to the precision of the employment history before and after treatment. Besides, most evaluation studies only assess the effects of such policies in East Germany and only for an early period in the beginning 90's (up to 94, because the income maintenance information used for the identification of public sector sponsored further training was then dropped out of the questionnaire for the GSOEP). Finally the samples sizes of these studies are on average small and do neither allow the researcher to evaluate the effects of any heterogeneous treatment nor of treatments which are explicitly offered to specific target groups (e.g. short–term or long–term unemployed). As a conclusion of all these shortcomings of the available data, the use of alternative data is necessary in order to estimate the effects of heterogeneous treatments. To draw such kind of inference the researcher needs more information about the employment history of the individual, and especially a larger sample size than provided by the panel surveys of the 90's.

1 This study is a part of the project "On the effectiveness of further training programmes. An evaluation based on register data provided by the Institute of Employment Research, IAB (Über die Wirksamkeit von Fortbildungs– und Umschulungsmaßnahmen. Ein Evaluationsversuch mit prozessproduzierten Daten aus dem IAB)". The data were generated within a joint project of the Chair of Econometrics at the University of Mannheim and the Swiss Institute for International Economics and Applied Economic Research at the University of St. Gallen (SIAW) (more information about the data and the construction of an integrated evaluation sample can be obtained in Bender et al. 2004). Financial and technical support by the IAB under contract number 6-531 A of the Federal Employment Service is gratefully acknowledged. The project group is grateful to the Federal Ministry of Economics and Labour (Bundesministerium für Wirtschaft und Arbeit) for the provision of non anonymous data under the regulation of § 75 SGB X.
Of course, neither institution bears any responsibility for the uses made of the data, nor the inferences drawn by the author.

2 http://www.arbeitsamt.de/hst/services/geschaeftsbericht/gb2002.pdf

This evaluation study is the first attempt to take advantage of the official social insurance data which are generated by the mandatory social insurance bodies and which are practically available for total dependent employment and registered unemployment. These data – the employment register – consist of the employment history for approximately 50,000,000 individuals over the years 75 to 97. As the main source for this paper, we use a 1% non–anonymous subsample of these data. Register data were then merged in a multiple–step procedure with benefit data for the unemployed and participants in training programmes and with data surveyed in the local offices of the Federal Employment Service for the *participants* in further training programmes for the period 80–97. Compared to the data of the GSOEP and other surveys, the data used in this paper offer rich information about quite heterogeneous courses: further training consists of a) the provision of specific occupational skills, b) complete retraining of the employed in order to obtain a formal degree for a different profession, c) short–term courses which increase the search effectiveness of the individuals or d) German language courses for the immigrants. The data used in this study allow to distinguish these treatments and to examine the effects of informative categories of treatments for important target groups.

This paper suggests to focus rather on types of training than on programmes and discusses how further training can influence the employment prospects if a specific treatment is given to target groups experiencing a specific unemployment duration. In this paper we evaluate the most important type.

From the technical point of view, the paper makes use of a conditional independence assumption which claims that the employment outcome for the treated and the non–treated populations in case of non–treatment are similar conditional on a set of covariates which cover socio–economic characteristics as well as the previous employment history of the individuals, so that we can estimate the programme impact based on non–experimental data. Due to a consistent stratification of treatment and control groups based on the same lay off time and the same duration of unemployment before treatment, no further differencing with respect to unobservable characteristics is taken into consideration. The analysis makes use of the popular matching approach. We implement kernel matching on the propensity score (Rosenbaum, Rubin 1983). Within matched samples we compare the employment levels of treated and matched controls in a nonparametric way.

The remainder of the paper is structured as follows: Section 3.2 gives a short description of the institutional regulation and the basic participation figures of ALMP. The further training programmes considered in this study were carried out in accordance to the Labour Promotion Act (Arbeitsförderungsgesetz, AFG). However, the institutional regulation of further training does not provide information about specific policies implemented according to this regulation. Therefore, section 3.3 provides an understandable plot of the different options of further training, their target groups and course contents. Section 3.3 also describes how data were merged from different sources and how we identify different treatments.

Section 3.4 discusses the microeconomic evaluation problem and describes the the approach taken to control for selection on observables with a matching approach based on a non–parametric kernel regression. It implements this methodology based on panel data and estimates the employment effects. Section 3.5 offers a conclusion.

As in the case for the old evaluation studies based on survey data, the results indicate again that the estimated effects are in most cases negative, at best insignificant.

3.2 Regulation of further training

3.2.1 Programmes

For the period of observation, further training in Germany is regulated on the basis of the Labour Promotion Act (*Arbeitsförderungsgesetz*, AFG) and is offered and co–ordinated by the German Federal Employment Service. It aims at ensuring or improving occupational flexibility, career advancement and the prevention of skill shortages. However, following the persistent unemployment after the 70's, the programmes of further training change their character from a preventive ALMP rather towards an intervention policy offered to unemployed and those who are at severe risk of becoming unemployed. However, the new target groups are offered the same type of programmes as formerly to the employed, either further training, retraining programmes or integration subsidies ("*Fortbildung und Umschulung*", FuU).

The increasing number of unemployed entering these programmes changed the aims of the programmes from the skill–upgrading programmes that were focused to the employed to short–term programmes in which individuals are taught new technologies and partial enhancement of existing skills for occupational re-integration. Although many changes concerning benefit level and eligibility groups were implemented the traditional policies further training, retraining and integration subsidy – remained unchanged until 97 and are still in practice today. In the following, we give a short description of the programmes:

- **Further training** includes the assessment, maintenance and extension of skills, including technical development and career advancement (*Weiterbildung*). The duration of the courses depends on individual pre–dispositions, other co–financing institutions and adequate courses provided by the training suppliers.

- **Retraining** enables vocational re–orientation if a completed vocational training does not lead to adequate employment (*Umschulung*). Re–training is supported for a period up to 2 years and aims a providing a new certified occupational skill.

- As third programme of further training, the **integration subsidy** (*Einarbeitungszuschuss*) offers financial aid to employers providing employment to workers who have been unemployed or directly threatened by unemployment. The BA offers the grant for an adjustment period until the supported persons reach full proficiency in their job. The amount and the duration of the payment depend on the difference between the current productivity level of the employee and that required by the job (up to 50% of the standard wage in the respective occupation).

- In 79, **short-term training** was introduced under §41a AFG aiming to "increase prospects of integration". With this programme, skill assessment, orientation and guidance should be offered to unemployed. The curricula under this programme are usually short–term lasting from two weeks up to two months and are intended to increase the placement rate of the unemployed. Most of these programmes did not supply additional qualifications to the participants, but aim at keeping their search behaviour upright and increasing the integration.

Except for the integration subsidy in which participants are paid the standard salaries, the schemes work under the same conditions for the participants: Participants in full–time courses are granted an income maintenance (*Unterhaltsgeld*) if the conditions of entitlement are satisfied. Under certain conditions a proportional income maintenance may also be granted to persons who are participating in a part–time course. To qualify, persons must meet the requirement of being previously employed for a minimum duration during a set period of time, i.e. at least 1 year in contributory employment or receipt of unemployment benefit or subsequent unemployment assistance. The set period may be extended for persons returning to the labour market.

The income maintenance amounts to 67% of wages for participants with at least one dependent child, otherwise 60% which is equivalent to the unemployment benefit. However, benefits used to be much higher for the 80's and early 90's with up to 80% of previous net earnings granted under specific conditions. If a person does not fulfil the requirement of previous employment, but had received unemployment assistance until the start of the measure, an income maintenance may be paid as well. While participating on the FuU schemes, participants re–qualify for unemployment insurance payments providing additional incentives to them to participate in programmes. The BA bears all the costs of further training incurred directly through the training scheme, especially including course fees.

3.2.2 Changes in the regulation

Programme changes

Over the 80's and 90's policy changes are implemented in the AFG regulation on further training. The major change is the termination of the "programme to increase the prospects of integration" (*Programme zur Verbesserung der Vermittlungsaussichten*, § 41a) in 92 when another programme substitutes it, which is no longer considered as part of further training but of the general placement activity: participants starting this programme after 92 are then recorded as unemployed while being treated.

Changes of the benefit level

Other important changes concern the level of the income maintenance. Starting from a level of 80% for participation in a programme, it is reduced to 75% for individuals with and 68% for those without children in 82. In 84, a further reduction leads to a level of 68%/ 63% of the previous net earnings, which then is revised when the level increased back to 73%/70% in 86. After 91, migrants do no longer receive income maintenance because a special income maintenance scheme is implemented for this target group. Up to 92, participants in language courses are a substantial quantity of the ALMP participants – not only in language courses, but also in occupational skill adjustment programmes. In 94 finally, the level of benefits for participants in any programme of further training was reduced to 65%/60% of their previous earnings which corresponds also to the current benefit level.

The grant of income maintenance always depends on the type of promotion, whether it is a "necessary" participation because of individual unemployment or severe risks of becoming unemployed or whether participation is considered as "advisable" in terms of future employment and earnings effects for the participants. A formal entitlement for the payment of income maintenance is only given to participants with "necessary" participation, however, the judgement about whether the programme is advisable or necessary lies in the responsibility of the individual employment office.

Further training for the employed ended

Further training offered to the employed or offered to unemployed without satisfying the condition of "necessary" training often consists of training provided to the employed for upper professional training and career advancement ("advisable training"). In the 80's, individuals participating under advisable training are also granted income maintenance payments and reimbursed for any course fees by the BA. A major change takes place when income maintenance for advisable promotion of further training is granted as a loan after 82 with more restrictive criteria for the employed. Individuals entering after 82 will therefore have a different incentive structure then before. In 86, the criteria for adjustment and skill increase are weakened again, so that more employed persons are supposed to start these programmes.

The "advisable" promotion of further training terminates in 94. After 94, mainly unemployed participants start a programme of further training, although especially in East Germany the participation under the weak criterion of "threatened by unemployment" still allows employed participants directly to start an ALMP programme even if there was no previous period of unemployment. Given these differences in the participation structure of FuU and the changes in the legislation decreasing the incentives for employed individuals to participate in either one of these schemes, FuU can be considered to be a programme mainly focusing on the unemployed from the mid 80's onwards.

3.2.3 Participation

Among the three FuU programmes, the general further training scheme (*Berufliche Weiterbildung*) is the by far most important in both East and West Germany. Starting with a total of 232,500 participants in 80, about 70% of all participants started a further training scheme, whereas only 14% (32,600) begin a programme under the Integration subsidy (*Eingliederungszuschüsse*) scheme. New participants in the retraining programme summed up to 37,900 (*Berufliche Umschulung*, about 16% of total). On average, participant stock is about 89,300 in 80. In 85, participant entries are already 60% higher in total, with a specific gain in the further training programmes then amounting to 80% of all participant entries. Between 80 to 90, participation in public sector sponsored further training more than doubles to 514,600, 74% of these are entries in further training programme. The retraining programme has on average participation increases to 63,300 in 90 from 37,900 in 80.

When labour market policy is extended to East Germany, participation peaks at 887,600 entries in East Germany in 92 and 574,700 in West Germany and then declines to 378,400 in West Germany and 269,200 in East Germany in 96. Over the years further training increases its share to 77% in West and 76% in East Germany. The share of participants in retraining amounts to 20% in West and 18% in East Germany.

Table 3.1 Participation in further training until 1997

Year	Annual entries				Annual average stocks
	Total	Further training	Retraining	Integration Subsidy	
1980	232,500	162,400	37,900	32,600	89,300
1985	371,000	298,200	45,100	27,700	114,900
1990	514,600	383,400	63,300	67,900	167,600
1991					
West:	540,600	421,200	70,500	48,900	189,000
East:	705,300	442,800	129,900	132,600	76,700
1992					
West:	574,700	464,500	81,500	28,700	180,600
East:	887,600	591,000	183,100	113,500	292,600
1993					
West:	348,100	266,000	72,200	9,900	176,800
East:	294,200	181,600	81,500	31,100	309,100
1994					
West:	306,800	224,900	73,100	8,800	177,900
East:	286,900	199,100	68,600	19,200	217,400
1995					
West:	401,600	309,700	81,800	10,000	193,300
East:	257,500	184,300	52,800	26,400	216,100
1996					
West:	378,400	291,600	77,300	9,500	203,600
East:	269,200	204,100	48,100	17,000	205,000

Source: Amtliche Nachrichten der Bundesanstalt für Arbeit, several volumes

3.3 Social insurance data

3.3.1 Merging insurance account data with participation data

The subsequent evaluation study is based on social insurance data and on data for training participants: On the one hand, the IAB Employment Subsample (IABS) consists of insurance register data for each employee recorded by the German social insurance system. Individuals in dependent employment are usually subject to the mandatory social insurance system. The IABS additionally reports episodes, which individuals spent in unemployment related to benefit payments (see Bender, Haas, Klose 2000). On the other hand, the German Employment Service used to report the structure, contents, duration and benefit payment for participants in further training schemes in a monthly survey as a result of internal and external monitoring objectives (FuU–data, see Bender et al. 2004). The following section describes these basic data, the problem of creating an integrated evaluation data base, how data are prepared for the subsequent analysis and how the information provided by the IABSLED– and the FuU–data were used in order to identify fairly homogeneous treatments in data.

3.3.1.1 Employment subsample and the benefit recipients data

The IABS consists of two major statistics: the core data are drawn from the Employment Subsample (*Beschäftigtenstichprobe* BST) of the Institute for Employment Research (IAB). The BST is a 1% random sample drawn from the mandatory employment register data for all employees who are covered by the social security system over the period 75–97. Data are based on an integrated reporting system for all social insurance bodies (health, retirement and unemployment insurance) that are mandatory for all employees. The procedure requires employers to report the beginning or ending of any employment subject to social insurance contributions. Additionally, employers are obliged to provide information on every ongoing employment subject to social insurance payments on December 31 of every year. Information reported by the employer at every observation includes individual characteristics, such as sex, nationality, educational attainment, as well as gross earnings over the last employment spell. The accounted earnings of these data serve as the basis for social insurance payments (especially unemployment benefit and pension payments). Social insurance contributions are compulsory for dependent employees earning above a minimum wage that is free of social insurance contributions for the period we observe here. However, among the dependent employees specific groups working on a part–time basis with few weekly hours and civil servants are excluded. Although these groups are not sampled, the IABS covers approximately 80% of the German labour force.

The second important source apart from the information of the BST is the benefit payment register (*Leistungsempfängerdatei* [LED]) of the Federal Employment Service. LED data consist of spells for individuals who receive certain benefit payments from the BA. Besides unemployment benefit or assistance, these data also record very detailed information about income maintenance payments related to the participation in further training schemes.

The integrated data of the IABS comprising BST and LED samples rich and longitudinal information about the whole employment history and the participation in public sector sponsored further training for the majority of the German labour force. However, the "standard" IABS known from other empirical studies (e.g. Fitzenberger 1999) does not provide an adequate source for the evaluation of further training because it does not report the receipt of benefit *if the BST reports employment at the same time*. These parallel spells are likely to occur in the case of further training: Participants may be recorded as employed while being on an on–the–job–training within a firm, even if they are not compensated by the employer and continue the reception of income maintenance payments related to training. Due to this structural underreporting of further training, the IABS needed to be merged a second time to the original LED–data, so that the evaluation data includes also benefit payments parallel to dependent employment (in the following denoted as IABSLED data).

However, as basic sampling of the IABS results from the employment register, only individuals who experience at least one spell of dependent employment between 75–97 can be sampled. Individuals are omitted if they are experiencing only unemployment between 75–97 or a part of this period as long as unemployment was their *only* status on the labour market. For the evaluation study this sampling implies that one should restrict the analysis to entrants into programmes from unemployment that were previously employed because the control group does not allow to construct a non–treatment outcome for treated individuals who did not experience registered unemployment before. The IABS samples roughly 1% of the overall dependent employment and benefit receipt, resulting to 591,627 individuals in the period 75–97 for both East and West Germany and to 8,293,879 spells. Contrary to survey data, the employment and benefit information of IABS is assumed to be highly reliable because the data is collected in order to document the time individuals spend in employment or unemployment for the mandatory pension system.

3.3.1.2 Data for training participation

The participation data for further training programmes refer to a survey implemented in the local offices of the Employment Service under the regulation of the Labour Promotion Act (§ 6 P. 3 AFG). Information is collected for all participants in further training, retraining, integration subsidies and language courses in Germany (FuU–data). These data report the entries into and exits from courses of further vocational training. Besides it reports information about the type of courses, the intended integration objectives and rough information about the contents of the courses with respect to the skills provided. The FuU-data originate from the BA's internal controlling and give an overview about the persons in FuU–programmes, the type of programme, the aim of the courses, the type of training (whether the training takes place in classrooms or "on the job"), the carrier of the programme and the beginning and ending of the treatment and again personal characteristics of the participants (information about sex, age, nationality, the region in which the programme takes place, the educational attainment, the employment status before treatment and other important characteristics). The FuU–data also indicates the type of income maintenance paid during the participation in a programme.

These data were merged to the IABSLED data by the social insurance number as an identifier for all individuals sampled in the IABSLED–data. Through the merging procedure, one can add the complete employment history to the treatment information. In the end, the merged data supplies an integrated evaluation data base consisting of comparable, longitudinal information for treatment *and* control group that covers all participants in further training, retraining, integration subsidies and short–term training courses as well as language training. The sample size of the FuU–data amounts 54,767 individuals corresponding to 72,983 spells of treatment in the period 80–97 (for West Germany, and 91–7 for the new federal states). Basically, all individuals receiving training related benefits that are sampled in the IABS and IABSLED–data should be part of the FuU–data[3].

However, some individuals are not accounted in the participation data because of a lack of reliability of the survey. These persons participating in a programme without being reported in the FuU–data can be identified as participants from the income maintenance data of the IABSLED–data given

[3] However, as it did not incur any sanctions for the local offices of the Employment Service not to report the data, the participation is not fully reported in the data.

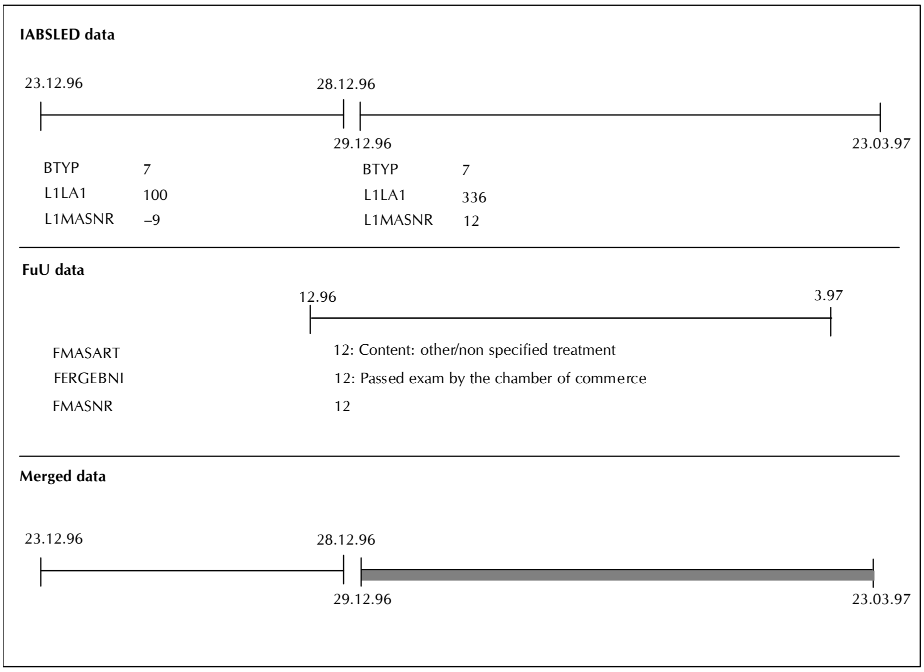
they receive specific benefits related to training participation. The coding information of the income maintenance variable from the LED–data allows for the identification of treatment.

3.3.1.3 Evaluation data

The IABSLED data and the FuU–data are merged to create an integrated data for evaluation because the participation data does neither cover any control group observation nor does it provide the longitudinal information for the treatment group and the IABSLED does not offer structural information for participants in further training schemes which is necessary to identify understandable, homogeneous treatments. Besides, some of the treatments do not incur the payment of income maintenance, so that participation would again be structurally underreported if the evaluation study was based only on the IABSLED data.

The merging is complex because of the size of data, but the main problems arise from the different frequency of both files: While the IABSLED–data offer information with daily frequencies, the FuU–data only provides monthly information. The merging procedure therefore does only allow an approximate merge of both files. It is based on the social insurance number, the timing correspondence of information of both files and further important covariates. As both data offer partly the same context information, one can achieve a good approximation if a complex merging procedure takes care of these contextual information: For example, both sources include the "reference number of the treatment" which codes an unique number to all treatments taking place in one of the 181 subdivisions of the Federal Employment Service. By using this information, one can merge the files even if they provide partly contradictory information concerning the income maintenance payments, the occupational status or the timing of the treatment. Furthermore, income maintenance payments related to the treatment and the expected end of either the benefit (from the IABSLED–data) or the course (from the FuU–data) allow an additional check whether data can be matched sufficiently. An illustrative example of the merging procedure can be found in Figure 3.1.

Figure 3.1 Merging insurance account data with participation data



The figure describes the merging procedure for one hypothetical individual in a very simple case. The upper third of the figure shows the information resulting from the merged IABSLED–data. Two spells are recorded, which could be matched to the training spell from the FuU–data in the second third below. Due to the monthly frequency of the data, we do not know whether the training spell started December 23rd or 28th. However, the IABS provides additional information indicating that only one of the two spells of the IABS corresponds to a training participation of the individual: The variable BTYP (offering information about the employment or benefit status) indicates for both spells the receipt of benefit (code 7). However, the information on income maintenance from the LED–data indicates for the first spell the receipt of unemployment benefit (code 100) and only for the second spell a payment individuals receive only in case of further training (corresponding to code 336). Taking into consideration this additional information, we should match rather the second IABSLED spell than the first to the FuU–data. Furthermore, the IABSLED data offer the "reference number of the treatment" that codes a unique identifier for a specific treatment implemented by one of the local offices of the employment service. If the regional information from IABSLED is concordant with those of the FuU–data (FMASNR [the reference number of the treatment from the FuU–data] and L1MASRNR [the same information from the LED data] must be identical for the treatment spell). In the example, only one of the two spells reports a valid reference number of treatment again indicating that a match of only the second spell to the FuU–spell is justified. This example describes a very simple case how we achieve the merging of data with different frequency. For a complete description of the merging procedure, see Bender et al. 2004, chapter 3.2.

In addition to the merging itself, numerous corrections are implemented for increasing the quality of the data: Inconsistencies of both files occurred with respect to the reported level of education and occupational status, the year of birth and the family status. All these variables needed to be corrected for in order to prepare the empirical analysis. The correction of the variable providing information on the level of school and professional education is especially important for this evaluation study, because we assume the individual skills to be the decisive reason for an assigned into treatment. However, the schooling and vocational training information in the IABS can be considered to be biased because the employer might only provide information about the *actual* occupations fulfilled by the employees, although the variable in principle should report the formal level of education. The fulfilled occupation however does not necessarily correspond to the qualification individual obtained in schooling and vocational training: It is likely that individuals may work below their actual level of education if the labour market conditions justify this. This phenomenon is especially important for individuals who face a certain risk of loosing employment (e.g. due to structural change) or who are expected to bear higher occupational mobility (e.g. frequent job changes in service industries). As the information of the individual's vocational training is provided by the employers, we suppose it to be the level of education, which is necessary to fulfil the individual's precise job, but the individual's formal skill level may lie above this position. Contrary it is unlikely that individuals working in a position with high formal skill requirements (e.g. a university degree) can actually fulfil their position without an adequate vocational training. We therefore assume the information of the level of education in the IABS data to be downward biased.

The correction of the skill level is mostly an upgrade of the reported skills to the highest level of education reported for the individual, based on assumptions how frequent different levels of education need to appear before such an adjustment is justified (and at which individual age). A detailed description of the correction can be found in Bender et al. (2004, chapter 3). This correction of the level of education was implemented for both the treatment and the control group. Many other indicators have been subject to a comparable validation procedure, making both data sets consistent with respect to the information provided and the quality of conditioning on covariates for the evaluation study.

The treatment population resulting from the merged IABSLED–FuU–data amounts to 1,169,871 spells for the period 75–97, corresponding to 54,756 persons who participated at least once in further training. Overall, 72,897 treatments are recorded. The merged IABSLED–data comprise 3,603,265 spells for 208,928 persons who either belong to the treatment group (for individuals who are not reported by the FuU–data) or to the naïve control group. As the spells usually vary widely with respect to the duration, data is recoded into a monthly panel for the analysis.

3.3.2 Varieties of further training

As mentioned in section 3.2, the basic regulation of further training only provides a framework, but does not define specific treatments with respect to integration targets or specific target groups. In practice, very different treatments (e.g. training for career advancement or short–term courses for very long–term unemployed) can be implemented under the same AFG regulation. Before turning to an empirical analysis, it is indispensable to reconsider specific type of further training and how such treatments can be identified in the data. Social insurance records do not offer individual labour market information which can be interpreted for evaluation in a straightforward way. The same is true for the participation figures provided by the FuU–data. Thus, before analysing the data it is crucial to understand the coding of the variables and to integrate the fragmented information of the benefit variables from the IABSLED and the course descriptions from the FuU–data into an interpretable concept of further training with homogeneous treatments. The following section provides seven different types of further training (referred to as type [a]– [g]).

3.3.2.1 Target specific types of further training

(a) Preparation, social skills and short–term training

This type of training provides non–vocational skills in educational institutions or participants are taking part in programmes evaluating their problems in finding regular employment (*Feststellungsmaßnahmen*, § 41a AFG). The training provides skills on a general level and focuses on an improvement of the job search process. In other cases short–term training is implemented as a first stage for continued training, so that the programmes prepare the participants for another further training (*Vorschaltmaßnahmen*). In short–term training, the provision of profession specific skills is supposed to be of minor importance and individuals who enter this type of treatment are supposed to lack fundamental general skills and social skills for job search. Therefore, we assume these courses to offer the promotion of social, individual and methodical skills, especially for the difficult–to–place unemployed in need of advice and guidance after long periods of unemployment, motivation for qualification and employment, strategies of behaving in conflict situations, techniques to learn more efficiently and training for application procedures. We assume these treatments not to provide formal certificates or degrees.

(b) Provision of specific professional skills and techniques

The objective of this type of further education is the improvement of the starting position in finding a new job by providing additional skills and specific professional knowledge in short–term and medium–term courses. These programmes serve to learn or freshen up of single skills, e.g. computer skills or the new operational practises. They are is intended for unemployed or persons at risk of becoming unemployed in order to facilitate integration into full employment.

This type of treatment makes up the majority of public sector sponsored further training programmes and is usually carried out by external educational institutions. Courses provide classroom training and the acquisition of professional knowledge by working practise. In most cases, participants are provided certificates about the courses, signalling refreshed or newly acquired skills and the amount of theory and work–experience achieved. The treatment is specific to the skills of the first vocational training degree and aims at increasing the individual chances of finding new employment within their profession. Compared to the short–term courses above, this type of training is supposed to have an influence on the matching probability of the unemployed with jobs offered because the qualification provided is formalised and individuals are given certificates after training.

(c) Qualification via the educational system/retraining

This type of training consists of the provision of a new and comprehensive training according to the regulation of the German dual system of vocational training. It is offered to individuals who completed already a first vocational training and face severe difficulties in finding a new employment within their profession. Retraining is formal vocational training into a certified occupation after the end of a first vocational training. It might however also be offered to individuals without a first formal training, as long as participants are supposed to be integrated through the educational system. Up to 94, this type of treatment is also accessible to individuals without the formal criterion of "necessity" for career advancement. Participants are then granted an income maintenance as a loan.

Qualification via the educational system/retraining provides a formal certificate allowing individuals to increase their chances to find employment by enduing additional signals in the search process. As the retraining programme offers an in–depth training in the occupational skills following the regulation of the German system of apprenticeship, the formal certificates are widely accepted. Federal law regulates the curricular contents for such certificates. Contents and lessons are scheduled with respect to the legal requirements and to the practical part in companies. Most participants of the programme are interested in getting either a formal job qualification (§25 Federal Occupational Training Law [*Berufsbildungsgesetz*]; e.g. bricklayer, painter, carpenter) or a job certificate recognised by the chambers of industry and commerce. Participants who have already finished a first apprenticeship are supposed to join the programme only due to severe personal reasons (e.g. health problems) or because of regional mismatch in their field of qualification due to structural change.

Vocational training according to the German dual system consists of both, theoretical training and work experience. The theoretical part of the training takes place in the educational institutions. The practical part of the programme is often carried out in firms which provide participants work experience in their field, but sometimes also in training establishments of the institutions providing this type of training. This type of treatment includes "encouragement of formal job qualifications" (*Förderung beruflicher Abschlussprüfungen*) and the official retraining programme (*Umschulung*). The goal of both is the achievement of a formal job qualification in order to improve the job match.

(d) Training for specific job offers

The main objective of this type of training is the provision of specific occupational and social skills to individuals who intend to accept a job offer and to fulfil the formal requirements for the specific job. Training of this type provides specific skills and qualification as described under (b). Generally individuals pass through short–term courses with specific professional skills in order to meet the requirements for a job offer. The contents such courses are closely linked to the employment, in which individuals are employed afterwards. Usually courses take place in the training division of companies. Contents of the courses also consist of social, personal and methodological knowledge. Most of these courses are not supposed to result in a certified job qualification, but to provide practical education and qualification below the level of formal qualification. Compared to training which offers a certification after the end of a programme, this type of training has only little impact on future employment prospects, once the job match with the precise employer is achieved. During the time of training participants are employed in the firm in which they intend to work afterwards in regular employment or in firms of the same sector of activity preparing the work for regular employment.

(e) Direct integration in the first labour market

This type of training aims at integration through wage subsidies according to § 49 AFG. Wage subsidies are paid for the employment of formerly long–term unemployed and are intended to decrease the competitive disadvantage of these recruits for the period of familiarisation with the skill requirement of the job. Individuals receive only practical guidance for the employment according to the requirements of the firm and are not provided certifiable qualifications. The training consists basically of working in the future profession under the supervision of the staff of the firm. Sometimes training companies and suppliers of further training programmes prepare the type of training by providing special qualifications to the participants. Mainly companies of the private sector implement this type of training. Firms are obliged to pay the agreed wage of the sector to the participant. Furthermore, a minimum time of employment after the end of the scheme is required.

(f) Career advancement subsidy

This type of treatment provides training for individuals who are not unemployed or threatened by unemployment, either as a retraining or as a career advancement in a practised profession. This type of training terminates 94. "Qualification for career advancement" works by providing loans to participants. Although not strictly active labour market policy, career advancement was an important part of public sector sponsored further training in the early 90's (and before). In this treatment, participants are enabled to obtain an advanced formal degree in their profession, e.g. a

- "Foreman" degree: According to the regulations of the chamber of handicraft this formal degree allows the holder to start an own business and to teach apprentices. An examination board at the chamber of handicraft administers the exam, which is a substitute for a state controlled examination. The examinee has to proof that he can perform his handicraft remarkably well and that he has expert knowledge and a sufficient knowledge in business administration and law as well as skills in supervising apprentice.
- "Technician" degree: According to the general agreement of the conference of the ministers of education of the German federal states, the formal technical training lasts two years and is implemented mainly in public technical colleges. A state examination board gives the degree of a state audited technician with reference to the specialisation if an exam is passed ("*Staatlich geprüfter Techniker*").
- "B.A. business administration": This continued education takes place in applied business schools and lasts two years. Structure, degree and organisation are regulated by general agreement of the conference of the ministers of education of the German federal states. Participants are qualified for a job at the lower management level in business or in the admini-

stration. The graduation allows the participant to call himself a *"Staatlich geprüfter Betriebswirt"* (state audited master of business administration) with reference to his specialisation (e.g. state audited gastronome).

(g) Language training

Besides further vocational training, language training is also part of the provision of further training in Germany as regulated by the AFG. The encouragement in participation in courses in German is intended to integrate asylum seekers, displaced persons, ethnic Germans and refugees into the labour market. Participants are provided support for an adequate education in language skills to fulfil regular employment.

3.3.2.2 Identifying further training in merged data

This section describes how we identify the aforementioned types of training in the merged. The coding plan can be found in the appendix.

Using the benefit information from the LED–data

In the merged data set, we combine the IABS data a second time with the benefit data (LED). As mentioned above under section 3.1, this merging procedure corrects for the fact that the IABS always records employment if the employer reports it – even if the individual receives benefits. As the merged LED–information provides often a number of parallel spells for one IABS spell, it was necessary to match up to three benefit spells to one IABS spell reporting employment or benefit receipt (see Bender et al. 2004, Chap. 3.1).

Therefore, the merged data consists of the benefit information from the IABS (the variable "original benefit information" [*Leistungsart im Original*] LA1) and three additional variables indicating parallel benefit reception from the original LED data ("parallel original benefit information 1–3" [*Leistungsart im Original* 1–3] L1LA1, L2LA1, L3LA1). These four benefit variables offer valuable information about the type of benefit paid by the employment service in case of training which facilitates the identification of the type of treatment: The benefit information does not only indicate whether a treatment is carried out under the further training or the retraining regulation, but also whether the transfer was given for full–time or part–time courses, to participants in language training or as a loan for career advancement training. The exact coding plan of these variables changed over the years, so that the benefit codes provided by this variable have to be considered on a year–by–year basis. The types of training, which are discussed above, can be identified using these benefit variables, but also by combining this information with other variables of the IABS (especially the variable of the occupational status) and the merged FuU–participation data (see below).

Type of training from FuU–data

In this evaluation study one of the most important advantages compared to survey data is the information about the precise type of training. It allows us to identify homogeneous treatments for the evaluation. In the merging process, up to two parallel FuU–spells are merged to one spell of the

IABS data because in many cases the FuU–data provided more than one parallel spell. These two parallel spells provide two variables indicating the type of course (Maßnahmeart [FMASART1, FMASART2]).

Combining the information in merged data

Participation in training can be identified by either the LED–data or the FuU–data. In the best case, both sources deliver the same information about the treatment. Then, one can easily identify the type of treatment from both data sources. However, due to the quality deficiencies in the participation data, in many cases participation is not documented in the FuU–data. In this case, the LED–data helps to identify the treatment on the basis of the benefit variable. In other cases, we observe individual records showing employment in the IABS information and at the same time training in the FuU–data. This is for example the case if the treatment takes place in a firm and individuals are paid a normal salary (e.g. integration subsidy) or if individuals are prepared for precise job offers. In such cases, we are able to identify the treatment group only on the basis of the occupational status information from the IABS ("*Beschäftigungstyp*", BTYP) combined with the information provided by FMASART1, FMASART2.

We take advantage of the information form all three parallel benefit spells, the original benefit information as shown in the IABS and the type of treatment as recorded in the two parallel FuU–spells in order to generate the most precise information available with respect to the type of treatment of either the first, the second or the third spell of the LED data compared to the FuU–data. We also identify treatments if one of the sources does not record an interpretable information about treatment: Often it seems as if individuals were granted unemployment benefit while being in a training programme although the legal regulation would imply a receipt of special benefits related to the treatment: At this point again, we use the FuU–data for the identification of the treatment and assume them to be more credible.

Improving the precision of treatment information

The following approach was chosen in order to ensure that both the information coming from FuU and LED–data are taken into consideration in order to obtain the most precise information of the type of training:

- Since trainings spells are often coded as "other, non–specified programmes" (FMASART1 = 12 [*Sonstige Anpassungen*]) in the FuU–data, we increase the precision of information about the type of treatment by relying on the second parallel information about the type of training: The second FuU–spell is used if the first FuU–spell is coded as "other adjustment" ("*Sonstige Anpassungen*"). If the second spell includes a code different from 12, which leads to a more precise information about the type of training, this information will be used instead of FMASART1. This combined information of FMASART1 and FMASART2 is referred to as FMASART* in the following.
- If we observe parallel spells from the LED–data that provide contradictory information about the type of benefit paid to the claimant, we identify a treated person when ever one of the three

spells of benefit payments provide the information that *an income maintenance payment related to training* occurred. To put it differently: if the L1LA1–variable indicates unemployment benefit and the second variable (L2LA1) indicates any payment of a training related benefit, then the latter is used for the identification of the treatment status. The aggregated information from the benefit data is referred to as L*LA1.

- If the benefit variables L*LA1 show information opposing to a related FuU–spell we use the FuU–information in these cases (e.g. benefits for retraining in the LED data in combination with information about "provision of specific professional skills" in the FuU–data). Another example: The benefit information is coded as 310 corresponding to "further education for resettlers or ethnic German" (EGGUF *Notwendige Fortbildung bei Aus– und Übersiedlern*) and the FMASART* variables specify the treatment as "vocational exam", FMASART* is supposed to be more precise with respect to the type of treatment.

3.3.3. Descriptive Statistics

Type of training and related benefit payments

Table 3.2 describes the relationship between type of treatment (a) – (g) as defined above and the benefit payment related to treatment for the period 90–7 based on spell data of the merged IAB-SLED–FuU–data: The types of training are displayed in columns and the benefit information coming from the IABSLED–data in rows. The benefit information is subdivided into several target specific benefit payments. First, we observe quite a substantive number of participants receive unemployment benefit or unemployment assistance while being in further training (indicated by the FuU–data): especially participants in *career advancement, short–term training* and *specific skills*–training are receiving unemployment benefit at the time of treatment. Without a merge of the IABSLED to the FuU–data, these individuals would not have been identified in the data as participants according to the benefit information. These individuals would have been regarded as unemployed implying a structural underestimation of the participation in training: the IABS without additional participation data would not have been appropriate for the evaluation of further training.

The next part of table 3.2 shows in which type of training individuals participate if the benefit information refers to payments for resettlers, German ethnics and refugees. In most cases, these benefits are granted to participants in language courses as expected. If the benefit granted is not explicitly related to the participation in language courses, we also find a substantive number of participants in either the career advancement or the specific skills training.

In case of benefit payments related to short–term training, individuals mainly participate in this type of training, but also to a substantive fraction in retraining and career advancement schemes. If individuals receive income maintenance related to retraining or further vocational training, we observe that many of these individuals also participate in other types of training, e.g. career advancement. Again, we see that it is necessary also to exploit the information from the FuU–data in order to identify the most similar participants for the latter analysis.

Table 3.2 Type of treatment and benefit** payment

Information of income maintenance payment	Type of training								
	Missing*	Preparation, social skills and short–term training (a)	Specific job know–ledge (b)	First labour market education system (c)	Precise jobs (d)	Direct integration (e)	Career adv-ancement (f)	Language training (g)	Total
Match of FuU–data and benefit information was not achieved***	1430			1764	7102	2172	8209	232	20909
Benefit information: Unemployment benefit or unemployment assistance									
Regular unemployment benefits	9	254	551	135		49	345	7	1350
Unemployment assistance for temporary soldiers			1	1					2
Unemployment assistance which follows unemployment benefits	2	318	202	65		8	146	2	743
Original unemployment assis-tance, no claim for unemploy-ment benefits		42	36	4		3	13		98
Benefit information: Resettlers, German Ethnics and Refugees									
Benefits for language education			1						1
Benefits for further education for resettlers or German Ethnics			2041	152		14	125		2332
Income maintenance for language courses for asylum seekers and refugees								79	79
Income maintenance for language courses for German Ethnic or recipients of welcome benefits								728	728
Benefits for necessary further education for resettlers or German Ethnics				213			65		278
Benefits for full–time language courses for resettlers or German Ethnics								426	426
Benefits for part–time language courses for resettlers or German Ethnics								2258	2258
Benefits for full time language courses for asylum seekers and refugees								51	51
Other benefit for resettlers								405	405
Benefits for full time language for asylum seekers and refugees								1692	1692

* Missing values originate from codes which were obsolete in the 90's, but which occur nevertheless for unknown reasons (e.g. benefit information L*LA1 = 315), from an illogical combination of short–term training according to §41a *and* employment at the same time which could not be interpreted as further training or from codes in the participation data which were not supposed to occur in the 90's (e.g. FMASART* = 22, 23).

** Coding referring the 90's (see Appendix for details)

*** In most cases, the training information refers to the participation information from FMASART*, which however does not match to a related benefit information from the IABSLED–data (mismatch). In these cases, the training is carried out while individuals were in contributory employment. This usually happens if individuals are granted a career advancement subsidy (39% of all cases). See Bender et al. (2004) for further sources of failure in matching

Table 3.2 Type of treatment and benefit** payment (continued)

Information of income maintenance payment	Type of training								
	Missing*	Preparation, social skills and short–term training (a)	Specific job know–ledge (b)	First labour market education system (c)	Precise jobs (d)	Direct integration (e)	Career advancement (f)	Language training (g)	Total
Benefit information: Income maintenance related to short–term training									
Income maintenance amounting to unemployment benefits for necessary short–term training in § 41a		5							5
Full income maintenance because of unemployment or in danger of loosing the job for necessary short–term training in § 41a		514		4		1	255		774
Income maintenance amounting to un–employment assistance for necessary short–term training in § 41a		595	6	478		2	9		1090
Short–term training for resettlers or German Ethnics		451		3			1		455
Benefit information: Income maintenance related to further vocational training									
Income maintenance for further education, unemployment and conditions for income maintenance not met, income maintenance amounting to unemployment benefits is paid			62	3			14		79
Income maintenance for necessary further education for unemployed persons or persons whose jobs are in danger			3963	195		2	744	3	4907
Income maintenance amounting to unemployment assistance because of necessary further education due to unemployment or danger of loosing the job as of 1.1.94			369	22			83	27	501
Income maintenance for part time further education 44 IIb			221			2	9		232
Benefit information: Income maintenance related to retraining									
Income maintenance for retraining of unemployed persons or persons whose jobs are in danger				1913			91	4	2008
Income maintenance amounting to unemployment benefits because of retraining of former unemployed persons				27			2		29
Income maintenance amounting to unemployment assistance because of retraining of former unemployed persons				161			15		176
Income maintenance for part time jobs and retraining				927		1	51	1	980

* Missing values originate from codes which were obsolete in the 90's, but which occur nevertheless for unknown reasons (e.g. benefit information L*LA1 = 315), from an illogical combination of short–term training according to §41a *and* employment at the same time which could not be interpreted as further training or from codes in the participation data which were not supposed to occur in the 90's (e.g. FMASART* = 22, 23).

** Coding referring the 90's (see Appendix for details)

*** In most cases, the training information refers to the participation information from FMASART*, which however does not match to a related benefit information from the IABSLED–data (mismatch). In these cases, the training is carried out while individuals were in contributory employment. This usually happens if individuals are granted a career advancement subsidy (39% of all cases). See Bender et al. (2004) for further sources of failure in matching

Table 3.2 Type of treatment and benefit** payment (continued)

Information of income maintenance payment	Type of training								
	Missing*	Preparation, social skills and short–term training (a)	Specific job know–ledge (b)	First labour market education system (c)	Precise jobs (d)	Direct integration (e)	Career advancement (f)	Language training (g)	Total
Benefit information: Income maintenance as a loan for "advisable" training									
Income maintenance paid as loan for advisable further education							2050	3	2053
Income maintenance paid as loan for advisable retraining							19		19
Benefit information not valid (due to coding errors or employment)									
No valid code*	920	218	10	170			351	5	1674
Benefit information not valid (e.g. employment)	377			2			2		381
Total	2738	2397	7463	6239	7102	2254	12599	5923	46715

* Missing values originate from codes which were obsolete in the 90's, but which occur nevertheless for unknown reasons (e.g. benefit information L*LA1 = 315), from an illogical combination of short–term training according to §41a *and* employment at the same time which could not be interpreted as further training or from codes in the participation data which were not supposed to occur in the 90's (e.g. FMASART* = 22, 23).

** Coding referring the 90's (see Appendix 3.7.2 for details)

*** In most cases, the training information refers to the participation information from FMASART*, which however does not match to a related benefit information from the IABSLED–data (mismatch). In these cases, the training is carried out while individuals were in contributory employment. This usually happens if individuals are granted a career advancement subsidy (39% of all cases). See Bender et al. (2004) for further sources of failure in matching

Descriptive Statistics

Participation figures in the different types of further training for the years 90–97 are shown in table 3.3 based on spell data. These treatment figures result from the aforementioned assumptions of sections 3.2.1 and the identification of the treatments in the data set as defined under 3.2.2: The most important group consists of the participants in career advancement training amounting to one quarter of all treatment spells. Usually, these persons are employed while participating. For the 90's, we expected already a focus of the further training programmes on the problem groups on the labour market, so that the high share of participants in these trainings is quite surprising.

For the types of training besides career advancement as defined above, the most important category is the "provision of specific professional skills"–training on which we will concentrate in the following with 7,463 spells for the 90's. Almost equally important as this programme is participation in the retraining programme with 13.4% of all spells and "training for specific jobs" with 15.2% of all spells. Language training courses are also an essential part of further training, with 12.7% of all spells. Direct integration and the short–term training programmes are less important with around 5% of all training spells.

Table 3.3 Participation in further training by type of treatment

	Frequency	Percentage	Cumulated percentage
Missing*	2738	5.9	5.9
Preparation, social skills and short–term training	2397	5.1	11.0
Provision of specific professional skills	7463	16.0	27.0
Integration via education system	6239	13.4	40.3
Training for specific jobs	7102	15.2	55.5
Direct integration (wage subsidy)	2254	4.8	60.4
Career advancement	12599	27.0	87.3
Language training	5923	12.7	100.0
Total	46715	100.0	

* Missing values originate from codes which were obsolete in the 90's, but which occur nevertheless for unknown reasons (e.g. benefit information L*LA1 = 315), from an illogical combination of short–term training according to §41a *and* employment at the same time which could not be interpreted as further training or from codes in the participation data which were not supposed to occur in the 90's (e.g. FMASART* = 22, 23).

3.4 Empirical evaluation

3.4.1 Methodology

3.4.1.1 Conditional independence assumption

Like in most non–experimental evaluation studies following Roy (1951) or Rubin (1974) we rely on the assumption that the causal effect of treatment–on–the–treated can be identified by comparing the results of a programme (YT) for the participating individuals after the treatment $(D=1)$ with the hypothetical situation of the same individuals if they had not taken part in the programme $(YC|D=1)$. Of course the non–treatment outcome is hypothetical and cannot be observed with non–experimental data. Besides, the outcomes of any person do not depend on the overall level of participation in the programme, so that general equilibrium effects are assumed to be inexistent in the evaluation design[4]. The average parameter of interest is the effect of treatment–on–the–treated given by

(1) $$E\{YT|D=1\}-E\{YC|D=1\}.$$

The main problem of all evaluation studies based on non–experimental data consists of estimating $E\{YC|D=1\}$. In principle, two alternative approaches can be applied to estimate the average non–treatment outcome: the situation of programme participants before treatment (before–after–comparison) or a control group of persons, which did not participate.

[4] This is referred to as the stable unit value assumption, see Lechner (1998: 19) for details.

- The major drawback of the before–and–after comparison lies in the assumption of a constant average non–treatment outcome over time for the treated population. For instance, changes in the overall state of economy might lead to a violation of this assumption

  (2) $$E\{YC_{t0}|D=1\} \neq E\{YC_{t1}|D=1\},$$

  where $t0$ denotes a point of time before treatment and $t1$ after treatment.

- The average value of the outcome of non–participants typically does not represent the correct average non–treatment outcome as participants and non–participants differ in characteristics which influences the outcome variable,

  (3) $$E\{YC|D=1\} \neq E\{YC|D=0\}.$$

Thus, the participants differ from participants before treatment and from non–participants due to observable and unobservable characteristics giving rise to a selection bias. To correct for selection on observables, the paper refers to the Conditional Independence Assumption (CIA) which implies that it does not make a difference whether one estimates the average results without treatment on the basis of persons of the participating or the non–participating group as long as they have the same characteristics X. Under the CIA, one gets

(4) $$E\{YC|D=1,X\} = E\{YC|D=0,X\}$$

indicating that treatment group and the non–treatment group are comparable conditional on X.

In most evaluation studies the treatment assignment is a static problem and the information contained in the timing of treatment is typically ignored. With the merged IABSLED–FuU–data we can benefit from panel data which does not only offer information on the exact timing of the programme, but also for a very long–time span before and after treatment documenting the employment history of the individuals as well as re–iterated and multiple treatments. It allows us to be more precise with respect to the covariates X than most of the evaluation studies that rely on a static representation of conditional independence.

The conditional independence assumed in the following takes into consideration that treatments can start at different points in an individual unemployment experience and that the timing of treatments is related to the expected outcomes with respect to both calendar time and time relative to treatment supplementary to a conditioning on X. Precisely, the conditional independence assumption is restricted to

(5) $$\begin{aligned} &E\{YC_{k,t}|D_{k,t}=1,(S_t=t_{1,k,t}-t_{0,t}),X_{t1,t}\} = \\ &E\{YC_{k,t}|D_{k,t}=0,(S_t=t_{1,k,t}-t_{0,t}),X_{t1,t}\} \end{aligned}$$

where the potential outcomes $YC_{k,t}$ for either participation D=1 or non–participation D=0 in any of $k=3$ treatments (for the groups starting the treatment after short–term, medium–term or long–term unemployment) for a specific calendar time t are conditionally independent given an equal unemployment duration S_t to the same calendar time lasting up to the time of treatment t_1 from the start of unemployment after being employed in t_0 and further observable characteristics $X_{t,t}$ at the time of the treatment assignment (i.e. the first month of treatment t_1).

The conditioning on unemployment duration for treatment and non–treatment populations is reached by sample stratification of the treatment and non–treatment populations with respect to the unemployment experience before treatment (3.4.1.2). Further conditioning covariates X are then taken into consideration within each of the stratified samples (3.4.1.3). The conditional independence assumption should also be sufficient to overcome differences in the outcome variables before treatment that could indicate remaining unobservable differences between the treatment and the non–treatment group. This needs to be considered in the empirical implementation by testing for the significance of the difference in the outcome variables to various points in time before treatment (3.4.1.4).

If sample stratification and further conditioning on observables are sufficient to overcome any differences in the outcome variable before treatment as well as in all other observable characteristics, then the average effect of treatment–on–the–treated is just the difference in the sample means of treated individuals and non–treated control observations at any point in time after the start of the programme in *t1*.

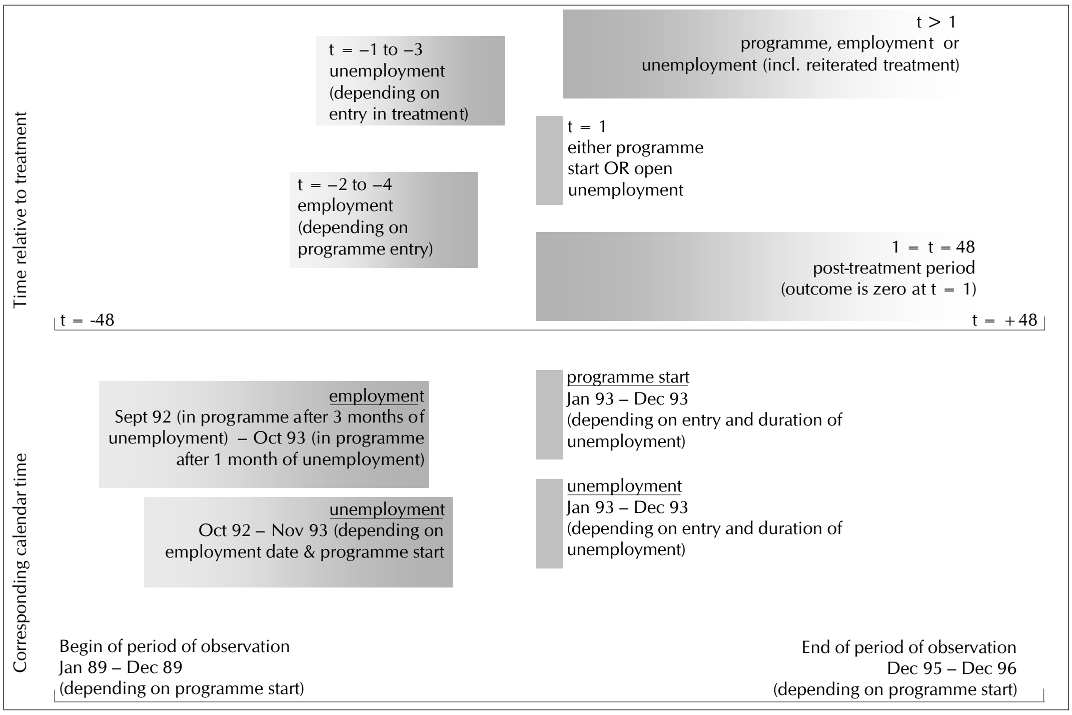
3.4.1.2 Sample stratification

Referring to the conditional independence assumption, we condition on the previous employment history before treatment by stratifying the samples. The employment and unemployment experience previous to treatment is assumed to be the most important explanation for the assignment to treatment because of the institutional regulations of the eligibility to treatment. Since treatment is particularly intended for groups with specific unemployment duration, the criterion of necessity of the treatment is usually related to previous unemployment of a specific duration. On the other hand, the calendar time of treatment also affects the assignment process because of changing budget conditions within each calendar year or changes in the focus of the policies from one year to another. Therefore, we restrict our analysis to groups who start unemployment the same time relative to the following treatment or non–treatment and who start this treatment to the same calendar time so that treatment and control groups become comparable with respect to the timing of the treatment within an individual unemployment spell at τ. However, even when applying a stratification on the same unemployment duration, it is virtually impossible to control for *all* possible remaining differences in the outcome variable before treatment: The control group still might consist of individuals who start employment at the beginning of the next month – who already anticipate the beginning of a new employment spell in the month we apply for stratifying our samples! In such a cases, the effect of treatment–on–the–treated would be slightly underestimated because the employment outcome for the non–treated individuals in the post–treatment period would be slightly higher than in the absence of such anticipation effects. However, these effects are supposed to be very weak and controlled for to a certain extent by the matching procedure.

A rigorous implementation of this stratification however fails due to practical limitations of the data: A stratification with respect to the calendar time of the programme *and* the time relative to the unemployment the individuals cannot be achieved due to the small sample sizes of treatment cohorts in specific calendar months τ after a specific duration of unemployment. Therefore, the treatment groups are aggregated for a specific calendar year (τ=93,94) and previous unemployment experiences lying in intervals between 1–3, 4–6 and 7–9 months of unemployment before treatment.

This aggregation to broader groups relaxes from an exact link between the calendar time *t* and the time relative to treatment *t* suggested in the conditional independence assumption. Figure 3.2 gives a graphical illustration of the relation between treatment time and calendar time for entries into training after short–term unemployment in the calendar year 93, which is implemented in this analysis. We aggregate all individuals who start treatment in 93 after 1–3 months of unemployment after employment ended – implying that treatments can start in any of the calendar months of the year 93 and that the individuals started unemployment between October 92 and November 93.

Figure 3.2 Calendar time relative to the time of treatment (exemplary for treatment in 93 starting after one to three months of unemployment)



All other treatment and non–treatment samples (for the programmes starting 4–6 and 7–9 months of unemployment and programmes starting in 94) follow the same motivation. In more details, the stratification works as follows:

Treatment samples $i \in \{D_{k,t} = 1\}$ are drawn from a monthly panel of the IABSLED–FuU– and the IABSLED–data: There are six different treatment groups for either East or West Germany consisting of the participants in the specific occupational skills training who start either after three different durations of unemployment *k*=1–3 (for the groups starting the treatment after 1–3, after 4–6 and after 7–9 months of unemployment) in either the calendar year *t (with t* =93, 94*).* Treatment samples for East and West Germany are created based on the regional information at the time t=1 of treatment.

Control groups are chosen based on the same stratification like for the respective treatment groups and consist of individuals who do not participate in the specific skills programme in these years (but who might participate in other programmes such as retraining). For each treatment group, we draw a naïve control group of individuals who experienced the same duration of unemployment after being laid off from employment like the treated individuals before treatment at the same calendar time. Of course, individual lay–off time and the individual duration of unemployment up to the time of the treatment might vary within the intervals as described above. We select the control group observations on the basis of the time spent in unemployment up to the month when the treatment starts for the treated. This selection procedure implies that observations might appear in the control groups of different treatment samples, e.g.

- The treatment group with individuals of 1–3 months of unemployment before treatment is juxtaposed to a naïve control group, i.e. the control group prior to matching, with the same unemployment experience before the calendar month of treatment.
- Individuals from the naïve control group who remain unemployed for a longer duration might also serve as a control groups for participants in a treatment after 4–6 months of unemployment in the same calendar year.

Finally, the stratification of treatment and control sample requires that members of the naïve control group need to be unemployed also in t=1. Consequently, both groups are similar with respect not only to the time spent in unemployment after being laid–off from employment, but also with respect to the outcome in the first month of treatment. Individuals who re–enter employment before t=1 of the treatment time as described under 3.4.2.2 *are excluded from the naïve control group*. Again, the control groups for East and West Germany are constructed based on the regional information at t=1 of the treatment time.

As aforementioned, the aggregation of the treatment and control samples to groups with short–term, medium–term and long–term unemployment before the treatment in the years 93–4 disallows (a) to relate exactly the timing of the unemployment duration before treatment and (b) the exact time of entry into the treatment between the treatment and the control observations. Therefore, we include the calendar time of unemployment entry and the months of unemployment before treatment as further conditioning variables for the matching approach implemented in the next section with respect to the remaining differences in observable characteristics X.

3.4.1.3 Kernel matching

In order to correct for remaining selection bias based on observable characteristics we implement a statistical matching approach. Matching is widely used in the empirical social sciences in order to keep boundary conditions constant in causal analysis (Winship, Morgan 1999). In the context of evaluation studies matching approaches produce a comparison group that resembles the treatment group with respect to the observable characteristics, which in this context might be understood as the boundary conditions.

Under the Conditional Independence Assumption, the average effect of treatment–on–the–treated can be estimated by

$$\frac{1}{N}\sum_{i\in\{D=1\}}\left(YT_{i,k,t}-\sum_{j\in\{D_{k,t}=0\}}w(i,j)YC_{j,k,t}\right) \tag{4}$$

where $YT_{i,k,t}$ is the outcome of a treated person $i\in\{D_{k,t}=1\}$, $YC_{j,k,t}$ the outcome for non–treatment of all non–participants $j\in\{D_{k,t}=0\}$ in the programme *k* (after a specific unemployment duration) in calendar year t. For reasons of simplicity, we omit the indices *k* and *t* in the following. We construct the non–treatment outcome of a treated individual by implementing a weight function $w(i,j)$ in the sample of the control observations with respect to X of each participant i. The weight function gives a higher weight to untreated persons with high similarity with respect to the X of the participant and a lower weight to persons with only low similarity with respect to X. According to this weight function, the non–treatment outcome for each participant $i\in\{D=1\}$ is constructed on the basis of the whole sample of non–participants, so that

(7) $\quad \sum_{j\in\{D=0\}} w(i,j)=1$

Different matching estimators vary with respect to the weights attached to members of the comparison group: On the one hand, the comparison group can consist of all non–treated observations and the individual non–participants are weighted with respect to the characteristics of the local individual (e.g. kernel matching). On the other hand, the matched control observation could consist only of the most similar non–participant with respect to X (nearest neighbour matching). Heckman, Ichimura, Todd (1998) discuss the properties of different matching estimators in more details. In this paper, we apply kernel matching estimators with local linear regression which turned out to be as powerful as nearest neighbour estimators with respect to selection bias based on observable characteristics based on experimental evidence (Heckman, Ichimura, Todd 1998: 27). Kernel matching implements weight functions for the sample of non–observations in order to construct the potential non–treatment outcome for a treated individual. The weight function for this estimator is specified as

(8) $\quad w(i,j)=\dfrac{K_{ij}}{\sum_{j\in\{D=0\}}K_{ij}}$

where $K_{ij}=K\left(\left(X_j-X_i\right)/h\right)$ is a kernel that down weights distant observations from the characteristics of an individual participant X_i. h is a bandwidth parameter. The potential outcome is estimated in a local linear regression at i on the basis of a weighted average of *all* non–treated individuals $j\in\{D=0\}$. The weights depend on the deviation of observable characteristics $\left(X_j-X_i\right)$ with a sum of the weights equal to one. We apply local linear regressions for the estimation of the local treatment outcomes: The non–treatment outcome of a treated individual is estimated in the non–treatment sample depending on the observable characteristics X_j of the non–treated relative to the local treated individual's characteristics X_i weighted by the kernel function. The minimisation problem is analogous to a weighted least squares estimation:

$$(9) \qquad \sum_{j \in \{D=0\}} \{YC_j - m - b(X_j - X_i)\}^2 K\left(\frac{X_j - X_i}{h}\right)$$

where OLS minimizes with respect to m and b. The estimated parameter $\hat{m}$ then just represents the non–treatment outcome predicted by the local regression – a locally weighted average. However the local linear model allows for a local slope parameter. A closer examination of the numerator of equation (9) lends some insight into the weighting situation: more weight is associated with the observations at locations close to X_i (the fit location) and less weight to observations farther apart. The kernel function throughout this paper is specified as a Gaussian kernel with

$$(10) \qquad K(j) = \frac{1}{\sqrt{2p}} \exp\left(-\frac{1}{2} j^2\right) \text{ with } j = \left((X_j - X_i)/h\right).$$

Härdle (1990) concluded that the choice of the bandwidth – and not the choice of kernel function – is essential to the performance of the nonparametric fit. The bandwidth determines how fast the weights decrease as the distance from X_i increases. The rate at which the weights decrease relative to the locations of the X_j controls the smoothness of the resulting estimate. There is no "golden rule of bandwidth selection". Pagan, Ullah (1999: 19) discuss that if h is chosen high, the variance of the estimated parameters is quite low as a large number of points are used for the estimation. A small h gives fragile density estimates and locally, only few points are included in the estimation, so that the variance increases, but less bias is produced. The trade–off between variance and bias is especially important in our application, where actually selection bias is to intended to be minimized. With respect to selection bias based on observable characteristics, we should rather tend to an under smoothing than to have a too high value of h. An option quite often used is the application of Silverman's Rule of Thumb (ROT). As an optimal bandwidth selection for a Gaussian kernel, Silverman (1986: 47f.) gives the recommendation of

$$(11) \qquad h_{ROT} = 0.9A \cdot n^{-1/5}$$

where h is the selected bandwidth and $A = \min(std, iqr/1.34)$. std stands for the standard deviation, iqr for the interquartile range of the sample, n for the sample size. In this paper, this recommendation is followed.[5] Note that Silverman's rule – although often applied to local linear regressions in the literature – basically provides an optimal bandwidth choice for local density estima-

5 Additionally, we tried also bandwidths, which undersmooth relative to Silverman's rule (e.g. $h_{ROT}/2$). However, as the matching based on the bandwidth h_{ROT} already corrects sufficiently for remaining selection bias based on observables (see below for the test statistics) in all specifications, an explicit under smoothing in order to obtain more favourable features with respect to solving selection bias was not implemented in this paper (even though it is the case that the bias will be minimized with $h \to 0$).

tions. The choice of the optimal bandwidth for a nonparametric regression function is more complex and usually consists of reiterative procedures where the bandwidth is chosen many times in a reiterated procedure (Pagan, Ullah 1999: 199), so that the estimated prediction error of the local estimates is minimised. A bandwidth choice by reiterated procedures would certainly have been preferable, but could not be implemented so far due to the size of the data.

For the follow-up study, a bandwidth choice based on cross–validation is foreseen for any of the treatments: This procedure should lead to an optimal bandwidth choice by reproducing the estimation of the average expected non–participation outcome in the sample of non–participants for each period in two steps: First, we identify the nearest neighbour *nn(i)* for each participant in the sample of non–participants with respect to the estimated propensity score. Then, the optimal bandwidth should minimise the sum of point–wise squared prediction errors over the points of time after treatment

$$\sum_{t=1}^{T}\left[\frac{1}{N_t}\sum_{i=1}^{N_t}\left(YC_{nn(i),t}-\sum_{j\in\{D=0\}nn(i)} w(i,j)YC_{j,t}\right)\right]^2$$

where the prediction of the employment status for *nn(i)* is not based on the nearest neighbour *nn(i)* and $t=1,\ldots T$ denotes the month for which the non-treatment outcome is estimated (i.e. 36 months after treatment, see Bergemann, Fitzenberger, Speckesser 2004: 9).

Propensity score

Consider X to consist of a vector of many observable characteristics. Then a disadvantage of matching is the "curse–of–dimensionality" with respect to all dimensions of X. Therefore, this paper follows the result of Rosenbaum and Rubin (1983) that the CIA in equation (4) also holds with respect to the probability of treatment (= propensity score) $P(X)$ as a function of the observable characteristics X, i.e.

(12) $$E\{YC|D=1,P(X)\}=E\{YC|D=0,P(X)\}.$$

On the one hand, this result allows matching upon the one–dimensional probability. Effectively we use the "closeness" of the propensity score of control observations with respect to the treated individuals as an estimator for the non–treatment outcome. This dimension–reduction diminishes the problem of finding adequate matches and the problem of empty cells. However, propensity matching comes at the costs that the propensity score has to be estimated itself.

3.4.1.4 Outcomes and preprogramme test

The correction for selection bias should make the employment history of participants and non–participants comparable before treatment. If the adjustment for selection–bias does not align the preprogramme outcomes for the future participants and the control–group, and "if it is plausible to assume that the source of preprogramme differences in earnings between the two types of individuals is the same as for the postprogramme differences, (...) the correction procedure is rejected."

(Heckman, Hotz 1989: 866). The outcome of treatment–on–the–treated with respect to employment is

(13) $$\Delta Y_{i,k,t,t} = YT_{i,k,t,t} - \overline{YC}_{K\,i,k,t,t}$$

for any participant of the programme *k* after a specific period of unemployment duration in calendar year *t* (these subscripts will be dropped in the following) with $YT_{i,k,t,t}$ taking the value 1 in case of dependent employment and 0 in the case of recorded benefit reception in the data. $\overline{YC}_{K\,i,k,t,t}$ is the estimated non–treatment outcome of the respective person based on kernel matching. *t* indicates the time relative to treatment.

A preprogramme test consists of estimating the effect of treatment on the outcome variable *before* treatment. By definition the employment outcomes are equal for the first month of treatment for both groups Furthermore, the stratification takes into consideration that individuals start unemployment after an ending employment spell to the same time relative to treatment (i.e. for individuals who start treatment after up to three months of open unemployment, the employment outcome is zero at least at *t*=–1). With the propensity score matching employment and unemployment should be balanced out for the different groups. We therefore decided to test the preprogramme effects of treatment as

(PPT) $$PPT_t = \frac{1}{N_t}\sum_{i=1}^{N_t}\Delta Y_{i,t}$$

for any of the months before the beginning of the unemployment spells resulting in either a treatment *k* in calendar time τ (indices dropped for simplicity) for the treatment group or non–treatment. The dummy coefficients for the points of time (*t*<–4) before treatment are modelling the average differences in $\Delta Y_{i,t}$ as a function of *t*. The average difference for the treatment sample N_t of the respective period *t* should not be different from zero if the sample is suitable for the evaluation of differences in employment related to treatment. The preprogramme test is estimated by evaluating (PPT) as a linear regression (inference is based upon heteroskedasticity–consistent standard error estimates).

3.4.2 Implementation

3.4.2.1 Case selection and sample stratification

Monthly data

We decided to create monthly data from the spell data of the merged evaluation database, so that we obtain a comparable panel data set. As the spell data show a daily frequency, generating the monthly data could not be achieved without further assumptions. The most important implication of the creation of monthly panel data was on the one hand that spells needed to be split to monthly labour market status information; on the other hand, we had to decide how spells with duration of less than one month were recoded into the monthly structure. In these cases, the spell with the longest duration was chosen, and the information of this spell was coded for the entire month. If there were spells of equal length, which could serve as the "main" employment status of the respective month, we selected the first of any of these spells.

Restriction to individuals starting treatment from unemployment

The basic case selection restricts the sample to individuals who begin unemployment after being employed in the years 92–4. It should be noted that unemployment here corresponds to the reception of unemployment benefit, because the social insurance data only record the receipt of unemployment benefit, i.e. a smaller fraction then total unemployment. Therefore, the results should be interpreted with care, especially for the post–treatment period.

However, by focusing on individuals who terminated dependent employment before becoming unemployed, most unemployed individuals are recorded in our data because of having an unemployment benefit claim resulting from the employment and because of important institutional incentives to apply for unemployment benefit. Even if individuals do not receive unemployment benefits in the first or second month after ending employment (because voluntary quits), they can claim unemployment benefit from the third month onwards up to a period of one year. In such cases, individuals are coded in our sample as being unemployed for three months. It is therefore credible to assume that virtually all unemployed who leave dependent employment claim unemployment benefit within the first three months[6] – and that data of benefit reception can be used for evaluation if the target group are short– and medium–term unemployed because these data covers the vast majority of all unemployed coming from dependent employment.

In order to identify a homogeneous treatment group, we decided to focus on the employment effects of participants starting a training of the type "specific professional skills" after having experienced unemployment for several durations. This restriction allows us to take only individuals into consideration, which are either recorded once in the LED data. Employed persons who never receive any payment of income maintenance are excluded from the analysis. If we considered also individuals who start treatment directly from a previous employment spell or parallel to an employment spell, an appropriate comparison groups should also consist of these individuals because they are in principle also eligible to treatment and would be necessary in order to find a control group which is most similar with respect to the observable characteristics. Another reason why we restricted the analysis to this target group is that it is more informative for policy makers to evaluate treatments targeted to the unemployed because this corresponds to the policy which is still in practice in Germany.

Restriction to treatments starting 92–4

We decided to restrict the sample on individuals who start at least one spell of open unemployment in the years 92–94 for both the treatment and the comparison group and focus on entries into pro-

[6] Of course, there are some exceptions to this: Individuals whose previous work experience is of less than one year are only entitled to unemployment assistance – and the reception of this benefit depends on the

grammes following a spell of unemployment in these years resulting in a huge reduction of the complexity of the data. However, the employment information for all the individuals starting unemployment in these and the previous and subsequent years still results in an overall sample size of 4,280,557 person–month–observations for the period 88–97 for both East and West Germany (1,071,979 resulting from the merged IABSLED–FuU–data and 3,208,578 from the merged IAB-SLED–data).

Conditioning on employment/unemployment and stratification of samples

Finally we concentrate on individuals who start unemployment after leaving employment in any of the months for this period of observation and to evaluate the effects of such policies for them. An illustrative example of a monthly unemployment entry cohort can be found in table 3.4 where we show the labour market status for individuals starting an unemployment spell in the month October 92 either from unemployment or from a status outside the labour force. We see an overall sample of 2224 individuals starting unemployment in this month. In the month before, these individuals are either in employment or outside the labour market.

We observe this unemployment entry cohort over a period of four years and show the basic frequencies of the cohort at any point in time after the unemployment experience. The first and most important impression the data provides is that – besides the group re–entering employment – the most important alternative status to unemployment for this cohort is the participation in the "specific professional skills programme", followed by the participation in retraining and direct integration. In the following we will restrict the analysis to individuals who start *a first treatment in the specific professional skills* training. Participants who start the participation in training after experiencing unemployment are not very numerous, so that in a later step of the analysis the sample should be extended to the participants starting the treatment after employment, too. This however would also imply an extension of the control group: The naïve control group then should also consist of individuals who are never recipients of any benefit.

Note that we restrict the control group to individuals who do not start treatment at all, i.e. individuals starting treatment after a longer unemployment experience are excluded as well as individuals who start treatment in any other year than 93 or 94. We are aware that this restriction to non–participants could be understood as a conditioning on future outcomes if we follow Frederiksson, Johansson (2003) and that further assumptions are required: Frederiksson, Johansson (2003) show that the conditional independence assumption does not hold if one accounts for the fact that individuals who are not treated up to the end of the time window might be participants in a programme after this time. Their study could demonstrate that the effect of treatment–on–the–treated is positively biased if this is taken into account.

As it is virtually impossible in non–experimental evaluation to create a control group of which we know for sure that its individuals will never receive treatment, the estimated effect of treatment–on–the–treated of this analysis needs a further assumption (along the lines of Frederiksson, Johansson

whole household income and may not be granted in cases, in which the partner's income exceed a certain range.

2003): Taking the timing of treatment seriously, one can understand the estimated effect as treatment–on–the–treated *for a specific period*. Assuming this, a comparison group whose members do not start treatment in this specific period could provide a valid non–treatment outcome.

Table 3.4 Exemplary entry cohort (October 92)

			Participation in Further Training						
	Unemployment	Employment	Prep.	Specific skills	Re-training	Precise jobs	Direct integration	Promotion	Language
Aug 92	42	1843	0	1	0	0	0	0	0
Sep 92	0	1836	0	0	0	0	0	0	0
Oct 92	2224	0	4	5	5	0	0	5	0
Nov 92	1998	175	4	20	6	0	3	7	0
Dec 92	1832	284	4	34	6	0	3	9	0
Jan 93	1692	372	3	48	6	0	3	9	0
Jul 93	1143	719	1	53	7	0	13	6	1
Jan 94	924	806	0	30	11	0	1	4	0
Jul 94	715	904	0	27	10	0	3	2	0
Jan 95	655	881	2	27	8	0	0	0	0
Jul 95	503	945	1	23	7	1	0	0	0
Jan 96	518	877	1	19	8	0	1	0	0
Jul 96	457	931	2	22	6	1	1	0	0

Table 3.A1 in the appendix shows how many participants start a first specific professional skills type of treatment after one, two or three months of unemployment. For individuals who start treatment in January 93 and who started prior unemployment either in October, November or December 92, the overall sample size is N = 7 and the control group resulting from individuals who started unemployment at the same time and who experience unemployment as long as the treatment population before month 1 of the treatment is N = 1210. As defined under section 3.4.1.2 we decided not to evaluate any treatment at any point in time after any specific duration of unemployment, but to generate aggregates of individuals starting either one of the treatments after either short–term, medium–term or long–term unemployment in either 93 or 94.

The basic descriptive figures for these aggregates are reported in table 3.5 as well as descriptive statistics for the treatment and the control group prior to matching. For the year 93, the treatment group in specific professional skills training amounts to 121 in West and 170 in East Germany starting treatment in any of the months in 93 after 1–3 months of unemployment, for individuals starting treatment after 4–6 months of unemployment, these figures amount to N = 115 (West) and N = 138 (East), for entrants after 7–9 months of unemployment N = 60 (West) and N = 94 (East). These are the basic sample sizes of the treatment group. For 94, the overall sample sizes of the treatment group are slightly lower (table 3.5).

Table 3.5 Descriptive Statistics for cohorts starting treatment after short–term unemployment in 93 and naïve control group (prior to matching)

Programme entry year	93		93		93		94		94		94	
Programme entry time after unemplyment duration	1-3 months		4-6 months		7-9 months		1-3 months		4-6 months		7-9 months	
Federal territory	West		West		West		West		West		West	
Groups	T	NT	T	NT	T	NT	T	NT	T	NT	T	NT
Number of cases	121	17098	115	10898	60	7835	57	6616	95	5915	88	5816
Unemployment duration at programme start	2.909	3.462	5.93	6.568	8.95	9.614	3.123	3.507	6.084	6.662	9.148	9.751
Sector												
Agriculture	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00
Ground industry	0.09	0.12	0.13	0.13	0.10	0.14	0.07	0.13	0.06	0.12	0.10	0.13
Metal, automotive, electronics	0.21	0.20	0.17	0.23	0.20	0.24	0.12	0.13	0.23	0.19	0.22	0.21
Other industry	0.06	0.11	0.04	0.12	0.08	0.12	0.09	0.10	0.07	0.11	0.13	0.12
Building and civil engineering	0.08	0.10	0.06	0.07	0.07	0.07	0.12	0.15	0.06	0.09	0.08	0.07
Production related services, trade, and banking	0.39	0.26	0.40	0.26	0.37	0.24	0.44	0.26	0.38	0.27	0.39	0.26
Consumption related and social services, and the state	0.17	0.21	0.20	0.20	0.18	0.18	0.16	0.24	0.19	0.22	0.08	0.20
Occupational status of last employment												
Trainee	0.06	0.05	0.02	0.04	0.03	0.03	0.04	0.02	0.02	0.04	0.05	0.03
Blue collar	0.37	0.62	0.48	0.60	0.55	0.61	0.49	0.69	0.41	0.62	0.47	0.61
White collar	0.50	0.24	0.42	0.25	0.27	0.26	0.40	0.20	0.52	0.24	0.38	0.25
Working at home or missing	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Part-time worker	0.07	0.09	0.09	0.10	0.15	0.11	0.07	0.08	0.05	0.10	0.11	0.11
Regional information at programme entry												
Schleswig-Holstein/Hamburg	0.16	0.07	0.10	0.06	0.12	0.06	0.11	0.07	0.11	0.06	0.07	0.06
Lower Saxony/Bremen	0.07	0.14	0.20	0.14	0.10	0.14	0.11	0.15	0.07	0.15	0.18	0.14
Northrhine-Westphalia	0.31	0.27	0.28	0.29	0.28	0.30	0.32	0.25	0.31	0.29	0.21	0.30
Hesse	0.03	0.09	0.03	0.09	0.07	0.09	0.07	0.08	0.03	0.09	0.05	0.09
Rhineland-Palatinate/Saar	0.17	0.08	0.10	0.08	0.10	0.08	0.12	0.07	0.15	0.08	0.07	0.08
Baden-Wuerttemberg	0.11	0.15	0.12	0.16	0.13	0.16	0.09	0.14	0.15	0.16	0.14	0.17
Bavaria	0.16	0.21	0.17	0.18	0.20	0.17	0.19	0.24	0.19	0.17	0.30	0.17
Firm size of last employment												
less than 11	0.31	0.27	0.29	0.24	0.23	0.22	0.28	0.33	0.25	0.27	0.26	0.24
11 to 200	0.36	0.42	0.45	0.40	0.48	0.39	0.56	0.46	0.52	0.43	0.46	0.42
200 to 500	0.11	0.11	0.10	0.12	0.07	0.12	0.11	0.08	0.06	0.10	0.10	0.11
more than 500	0.23	0.20	0.16	0.25	0.22	0.27	0.05	0.13	0.17	0.20	0.18	0.23
Level of education												
No vocational training	0.15	0.28	0.18	0.29	0.18	0.30	0.18	0.25	0.10	0.27	0.13	0.29
Dual System	0.77	0.66	0.65	0.65	0.75	0.64	0.75	0.70	0.79	0.66	0.73	0.64
A-level	0.03	0.01	0.01	0.01	0.03	0.01	0.00	0.01	0.00	0.01	0.02	0.01
Technical college or university	0.05	0.04	0.15	0.04	0.03	0.04	0.07	0.03	0.12	0.05	0.13	0.05
no information	0.00	0.01	0.01	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01
Nationality												
German	0.89	0.83	0.86	0.82	0.90	0.83	0.90	0.82	0.92	0.82	0.90	0.81
EU citizen (1995 enlargement)	0.04	0.05	0.00	0.05	0.02	0.05	0.02	0.04	0.01	0.05	0.00	0.05
Former Yugoslavia	0.02	0.03	0.01	0.02	0.00	0.03	0.00	0.03	0.00	0.03	0.03	0.03
Turkey	0.03	0.06	0.05	0.07	0.05	0.07	0.07	0.06	0.04	0.07	0.03	0.07
Others	0.02	0.04	0.08	0.04	0.03	0.04	0.02	0.04	0.03	0.04	0.03	0.04
Region type												
Rural area	0.21	0.21	0.11	0.18	0.17	0.17	0.12	0.26	0.15	0.18	0.23	0.16
Medium populated area	0.34	0.36	0.34	0.36	0.40	0.35	0.37	0.36	0.31	0.36	0.38	0.36
Densely populated area	0.26	0.28	0.39	0.31	0.28	0.31	0.39	0.26	0.36	0.30	0.31	0.32
Metropolitain areas	0.20	0.15	0.16	0.16	0.15	0.17	0.12	0.13	0.19	0.16	0.09	0.17
Gross salary of former employment (€)	54.97	50.88	55.85	50.76	50.67	50.92	53.60	51.68	65.02	52.34	54.36	52.52
Sex	0.47	0.39	0.39	0.42	0.32	0.42	0.26	0.35	0.40	0.39	0.38	0.40

Table 3.5 Descriptive Statistics for cohorts starting treatment after short–term unemployment in 93 and naïve control group (prior to matching, cont.)

Programme entry year	93		93		93		94		94		94	
Programme entry time after unemplyment duration	1-3 months		4-6 months		7-9 months		1-3 months		4-6 months		7-9 months	
Federal territory	East		East		East		East		East		East	
Groups	T	NT	T	NT	T	NT	T	NT	T	NT	T	NT
Number of cases	170	8503	138	6919	94	5737	71	3287	96	2948	109	2658
Unemployment duration at programme start	2.918	3.554	6.051	6.672	8.904	9.73	3.099	3.551	6	6.642	9.046	9.727
Sector												
Agriculture	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Ground industry	0.16	0.14	0.13	0.14	0.09	0.15	0.09	0.12	0.12	0.11	0.06	0.11
Metal, automotive, electronics	0.17	0.10	0.20	0.12	0.13	0.13	0.07	0.07	0.19	0.09	0.12	0.10
Other industry	0.07	0.07	0.09	0.08	0.10	0.09	0.11	0.06	0.07	0.07	0.06	0.07
Building and civil engineering	0.05	0.11	0.04	0.09	0.07	0.08	0.10	0.16	0.10	0.12	0.05	0.10
Production related services, trade, and banking	0.27	0.24	0.25	0.24	0.28	0.24	0.27	0.24	0.29	0.23	0.33	0.24
Consumption related and social services, and the state	0.28	0.34	0.29	0.34	0.34	0.32	0.37	0.35	0.23	0.39	0.39	0.39
Occupational status of last employment												
Trainee	0.02	0.03	0.01	0.02	0.00	0.01	0.01	0.01	0.01	0.02	0.00	0.02
Blue collar	0.44	0.59	0.45	0.56	0.34	0.54	0.56	0.66	0.47	0.60	0.40	0.58
White collar	0.47	0.28	0.46	0.31	0.54	0.31	0.31	0.21	0.42	0.25	0.45	0.28
Working at home or missing	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Part-time worker	0.08	0.10	0.09	0.11	0.12	0.14	0.11	0.13	0.10	0.12	0.15	0.11
Regional information at programme entry												
Mecklenburg / West Pommerania	0.14	0.11	0.23	0.11	0.27	0.11	0.25	0.13	0.15	0.12	0.16	0.10
Berlin/ Brandenburg	0.22	0.29	0.18	0.29	0.18	0.29	0.28	0.28	0.19	0.29	0.22	0.30
Saxony-Anhalt	0.11	0.18	0.11	0.18	0.11	0.18	0.14	0.18	0.18	0.20	0.22	0.19
Saxony Free State	0.37	0.26	0.33	0.27	0.30	0.28	0.21	0.25	0.34	0.25	0.28	0.25
Thuringia	0.15	0.15	0.16	0.15	0.15	0.14	0.11	0.17	0.15	0.15	0.12	0.16
Firm size of last employment												
less than 11	0.23	0.24	0.26	0.20	0.25	0.19	0.25	0.28	0.22	0.25	0.32	0.25
11 to 200	0.37	0.46	0.45	0.43	0.39	0.42	0.59	0.49	0.51	0.48	0.39	0.46
200 to 500	0.18	0.12	0.13	0.14	0.20	0.14	0.10	0.11	0.12	0.12	0.09	0.12
more than 500	0.22	0.18	0.16	0.23	0.16	0.25	0.06	0.12	0.16	0.15	0.19	0.17
Level of education												
No vocational training	0.04	0.14	0.06	0.15	0.10	0.14	0.06	0.11	0.07	0.15	0.04	0.15
Dual System	0.84	0.76	0.85	0.75	0.72	0.74	0.78	0.82	0.83	0.77	0.85	0.76
A-level	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
Technical college or university	0.12	0.07	0.09	0.07	0.16	0.07	0.14	0.05	0.09	0.05	0.10	0.06
no information	0.01	0.03	0.01	0.03	0.02	0.05	0.01	0.02	0.00	0.03	0.01	0.03
Nationality												
German	0.99	0.97	1.00	0.98	1.00	0.98	1.00	0.97	1.00	0.97	1.00	0.97
EU citizen (1995 enlargement)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Former Yugoslavia	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Turkey	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01
Others	0.01	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01
Region type												
Rural area	0.45	0.46	0.45	0.45	0.49	0.44	0.55	0.48	0.44	0.48	0.47	0.45
Medium populated area	0.35	0.34	0.37	0.35	0.39	0.36	0.27	0.32	0.42	0.32	0.39	0.33
Densely populated area	0.05	0.02	0.02	0.03	0.02	0.03	0.03	0.02	0.04	0.02	0.02	0.02
Metropolitain areas	0.14	0.18	0.16	0.18	0.10	0.17	0.16	0.17	0.10	0.17	0.12	0.19
Gross salary of former employment (€)	40.11	37.45	39.35	36.89	35.53	36.05	41.52	38.08	39.07	36.83	37.11	36.88
Sex	0.50	0.46	0.52	0.49	0.71	0.50	0.42	0.42	0.46	0.48	0.66	0.53

3.4.2.2 Matching

The stratification on the previous unemployment duration and the calendar time of the treatment is the first part of the correction for selection bias due to observable characteristics. Other observable covariates as shown in table 3.5 are very different for the treatment and the control groups: With respect to these covariates, we implement the aforementioned matching approach.

(a) Probit Model

We implement matching is based on the propensity score as shown in (12). The standard approach for the estimation of the score function is a parametric model, which estimates the probability for both the treatment and the control group to participate in further training depending on observable covariates $X_{i,t1}$. for any of the treatments k in calendar year τ (indices dropped for simplicity). The latent model behind the probit model can be represented as follows:

(14) $$D^{*}_{i,t1} = X^{'}_{i,t1} b + e_i$$

where $e_i \sim N(0,1)$. The observed model however is a model of binary choice with

$$D_{i,t1,k} = \begin{cases} 1, & if \quad D^{*}_{i,t1,k} > 0 \\ 0, & if \quad D^{*}_{i,t1,k} \leq 0 \end{cases}$$

where we observe that the event $D_{i,t,k} = 1$ occurs with probability p and fails to occur with probability $1-p$. Since p_i is a probability, we can take any probability distribution function in order to parameterise p_i. As long as $f(X,b)$ is a probability distribution function, it will necessarily follow the restriction that p_i stays in the [0,1]–interval. We parameterise the probability of treatment p_i as the cumulative distribution function of the standard normal distribution and estimate the binary choice model as a probit model. The matching parameter – the propensity score – is predicted as

(15) $$X'_i \hat{b}.$$

(b) Results of the propensity score estimation

The results of the propensity score estimations can be found in table 3.A2 where we report all covariates included in the score estimations. The probit estimations model the influence of age (i), sector (ii), occupational status (iii), region (iv), firm size (v), level of education (vi), nationality (vii), regional type (viii), gender (ix), unemployment experience previous before treatment (x), the calendar time of unemployment entry (xi), number of months before programme (xii) on the participation in training for specific professional skills.

In most cases, age significantly affects the participation probability in training, indicating that the probability to take part in the courses decreases with age. The sector of the last employment prior to the unemployment experience does only affect the training participation in East Germany, indicating that short–term and medium–term unemployed from the building and the consumption related service sector have a lower probability to participate in the training in 93–4 compared to the base category of unemployed coming from manufacturing. The occupational status affects the participation decision: white collar workers in most cases are more likely to participate in the training compared to blue collar worker in East and West Germany for both training after short–term and after medium–term unemployment. For long–term unemployed entering the programme this selectivity does not exist.

The variables indicating the region in which the training is carried out also show a significant influence on the training decision. Compared to the base category (unemployed in the state North Rhine–Westphalia in West and unemployed in Berlin/Brandenburg in East Germany) participants in the northern states in the West are less likely to participate after short–term unemployment and more likely in the Free state of Saxony in 93. In 94, it seems as if these differences do not exist: Except for some few coefficients, the regional variables do not affect the participation decision. Concerning firm size of the last employment, we find a significant influence of the small–firm variable in some propensity score estimations, indicating that the individuals are less likely to participate in training if they were laid off by small firms compared to individuals laid off by medium size firms. However, these variables are significant only in few estimations.

The propensity score estimation includes the level of education. In many of the estimations we observe that a higher formal level of education corresponds to a higher propensity to participate in training whereas individuals without a formal occupational training participate relatively seldom in such treatments. However, the coefficients are not in all specifications significant.

The selectivity with respect to nationality indicates that non–German citizens have in some cases a significantly lower probability to participate in training. A selectivity of the treatment with respect to gender was only found in some specifications: In West Germany, women are less likely to take part in treatment after a duration of unemployment of 7–9 months, whereas in East Germany, the selectivity is just opposite in the same year (93). In 94, women are more likely to participate in East Germany after unemployment duration of 7–9 months whereas in the West, they tend more to take part in courses after unemployment duration of 1–3 months.

A surprisingly low influence results from variables which pay attention to the previous employment history of treated and control observations: In West Germany, employment six months before the start of treatment state seems to increase the probability to participate, whereas employment to an earlier point in time decreases it or is of insignificant influence in 93. This influence cannot be found in 94 for West Germany. In East Germany, it seems that especially employment long time before the participation is important for the assignment to treatment. The calendar month of treatment as well as the months of experienced unemployment before treatment (within the interval as defined above) show in most cases significant parameters.

(c) Overlap

Propensity score matching can only be successful concerning the conditioning on observable characteristics if the estimated propensity scores of treated and non–treated individuals overlap sufficiently. The estimated propensity scores of the treated individuals are without exception covered by the values of the control observations (figures 3.A1–3.A4 in the appendix). The figures show the frequency of the different values of the propensity scores for treated and non–treated individuals. We therefore can assume that a matching based on the propensity score controls sufficiently for the observables considered.

(d) Implementation of matching

As stated in the conditional independence assumption, we estimate the probit only for the time when the treatment starts (at t=1) according to the estimation described above. Based on this estimation, the employment outcome is predicted at any point in time relative to treatment. The matching algorithm works as follows:

Step 1: We select the month $t=1$ of the panel data for treatment and sampled control group. The propensity score is estimated as a static probit as indicated above based on time specific covariates of this month. The propensity score for each individual of the treatment and the control group is predicted based on (15).

Step 2: The individual propensity score is duplicated *at any of the panel month*s of the same treatment or control observation.

Step 3: A bandwidth is chosen with $h = h_{ROT}$ on the basis of $j \in \{D = 0\}$ of the specific treatment at t=1.

Step 4: The first observation of the first treated individual for the month t=–18 and all available control observations at the calendar time corresponding to t=–18 of the treated individual are selected. A weighting function is created as

$$K\left(\left(\hat{b}\,X_j - \hat{b}\,X_i\right)/\,h\right)$$

with a Gaussian Kernel weighting the predicted propensity scores $\hat{b}\,X_j$ of the non–treatment sample with respect to the predicted propensity score $\hat{b}\,X_i$ of the (local) treated individual.

Step 5: The expected employment outcome of non–participation

$$E\left(YC_{i,t-18} \middle| P(X_i), D = 1\right) = \sum_{j \in \{D=0\}} w(i,\, j)\ YC_{j,t-18}$$

for the first treated individual in the hypothetical state of non–treatment for the month –18 is estimated by a nonparametric regression with the weighting function of step 4 in the sample of all non–treated observations. The same procedure is applied to estimate the expected values of all socio–economic characteristics which are subject to the propensity score estimation (age [i], sector [ii], occupational status [iii], region [iv], firm size [v], level of education [vi], nationality [vii], regional type [viii], gender [ix], unemployment experience previous before treatment [x], the calendar time of unemployment entry [xi], number of months before programme [xii] on the participation in training for specific professional skills at t=1).

Step 6: Step 4 and step 5 are repeated for all months available to the treated individual for the time t=–17 to t=36 relative to the start of treatment based on the available naïve control group at the same calendar time.

Step 7 The socio–economic characteristics and the employment outcome for the treated individual and the estimated non–treatment outcome for employment and the predicted socio–economic characteristics are stored into a new data file.

Step 8 The steps 4–7 are repeated for all other treated individuals until no more treatment observations are available.

(e) Matching quality

A simple test for the quality of matching is the standard t–test that assesses whether the means of two groups are statistically different from each other with respect to the observable X. We construct the observable characteristics of the matched controls based on a local linear model applying the same weighting formula as for the dependent variable and predict the covariates for the matched sample. These "non–treatment characteristics of the treated" are then subject to a simple t–test that is a ratio of the difference between the two means of the treatment and the matched control group (numerator) and the dispersion of the scores (denominator). By means of this, it is an example of the signal–to–noise metaphor: the difference between the means is the signal, the bottom part of the formula is a measure of variability that is essentially noise that may make it harder to see the group difference. The complete formula of the test is:

$$t = \frac{\overline{X_i} - \overline{X_{j(i)}}}{\sqrt{\frac{\operatorname{var} X_i}{N} + \frac{\operatorname{var} X_{j(i)}}{N}}}$$

where $\overline{X_i}$ is the mean of the observable characteristics X of the treatment sample, $\overline{X_{j(i)}}$ is the sample means of the observable characteristics predicted as the control outcome for the treatment sample analogously to (9) for all observable characteristics, $\operatorname{var} X_i$ is the sample variance of the treatment, $\operatorname{var} X_{j(i)}$ the variance of the predicted control observations, N is the sample size of the treatment sample or the matched controls.

The results of the tests are shown in table 3.A3 in the appendix: With the exception of only very few dimension of the covariate vector X (namely the occupational status of trainees predicted for the participants in training after 7–9 months of unemployment in East Germany and the gross salary of the former employment predicted for participants after 4–6 months of unemployment in the West), there is never a significant difference with respect to the observable characteristics between the treated and the matched control. The matching procedure was successful in creating a suitable control group with respect to the observable covariates. The second test implemented here, the preprogramme test on the outcome variable as discussed under section 3.4.1.4 will be discussed in the following.

3.4.2.3 Preprogramme test

As a last test for the suitability of the matched sample – especially with respect to the application of a static comparison of the employment levels with and without treatment – one has to check whether unobservable characteristics are still existing in the data that could affect the outcome variable before treatment. We implement this test as motivated under section 3.4.1.4 for all different matched samples.

The results can be found in table 3.A4 of the appendix. Differences in unobservable characteristics seem to be overcome sufficiently by stratification and matching: the preprogramme test suggested here gives no evidence that employment rates are different at any point in time before treatment (with the exception of long–term unemployed eleven months before the beginning of treatment, where we observe significantly different outcomes however referring to a very small sample of treated individuals, see table 3.A4 in the Appendix).. To our opinion, it is then permitted to interpret the differences in the employment rate of the treatment sample compared to the matched controls' outcome as the causal effect of treatment–on–the–treated.

3.4.2.4 Employment outcomes

As defined above, the outcome of treatment–on–the–treated with respect to employment is represented by

$$\Delta Y_{i,k,t,t} = YT_{i,k,t,t} - \overline{YC}_{Ki,k,t,t} \tag{16}$$

for any participant of the programme *k* after a specific period of unemployment duration in calendar year *t* (these subscripts will be dropped in the following) with $YT_{i,k,t,t}$ taking the value 1 in case of dependent employment and 0 in the case of recorded benefit reception in the data. $\overline{YC}_{Ki,k,t,t}$ is the estimated non–treatment outcome of the respective person based on kernel matching. *t* indicates the time relative to treatment, i.e. $t>0$ for the months following the treatment.

Note that the time individuals spend in treatment is recorded as unemployment in the outcome variable and is considered in the period of observation – contrarily to section two of this thesis – in order to become more sensible for the very short–term effects of the treatment. In case of reiterated treatments or participation in another type of treatment after treatment in "specific professional skills" the employment outcome is also zero. We specify this outcome for any month after the beginning of treatment $t = 1,\ldots 36, (t \neq 0)$ for any of the treatments *k* after specific unemployment experience in calendar year *t* (subscripts are dropped for simplicity) as

$$TT_t = \frac{1}{N_t} \sum_{i=1}^{N_t} \Delta Y_{i,t} \tag{17}$$

Again the outcomes are estimated as linear regression models. Inference is based upon heteroskedasticity–consistent standard error estimates. The results for the monthly average differences in employment rates as percentages are summarized graphically in Figures 3.2–3.7. In general, the interpretation of the figures works as follows: The thick line shows the estimated differences in the employment rate of the treated individuals compared to the non–treatment outcome for the time period from the beginning of the treatment until 36 months after the beginning. We plot the employment effect TT_t as defined in equation (13) as a function of t. Figures 3.2 and 3.3 give the average employment effects, whereas figures 3.4–3.5 and 3.6–3.7 display the results for the subgroups below 36 years of age and above 35. The two surrounding dashed lines indicate the 95% confidence interval. Participation begins at $t = 0$, so that the duration of treatment is included in the period of observation. It would be plausible to expect that employment levels are slightly lower for the time while the treatment population takes part in the programme and should increase with the individuals re-entering the labour market from training. The zero horizontal line should be understood as the benchmark for measuring success or failure of the programme: According to our definition, a programme should be considered as successful in terms of employment if the confidence interval lies in the positive region, so that we have a significant difference in employment rate between the treatment sample and the matched control observations.

3.4.3 Results[7]

Differences for all participants

We estimate employment effects separately for participants in the specific skills training for the years 93 and 94 and for different target groups according to the previous time spent in unemployment. Figure 3.3 shows the results for all participants in the year 1993 in West and East Germany. With the exception of participants in training after short–term unemployment in East Germany 36 months after the beginning of the treatment, we never find a significantly positive coefficient of the outcome estimation, indicating that the programmes never had a positive outcome with respect to employment.

In general, the employment rates lie around to 40 percentage points (ppoints) lower due to treatment for the first month of treatment in West Germany and only slightly higher in East Germany for participants in training after a short–term unemployment experience in 93. As the time of the treatment is included in the period of observation, this result is clearly not surprising. Over the post–treatment period the differences in the employment rate between treatment and matched control decline from the first month onwards, except for the very long–term unemployed in East Germany whose employment remains 40 ppoints reduced compared to non–treatment for a period up to six months after the treatment.

Both in West and in East Germany, significant differences in the employment disappear after 20 months for the participants starting the treatment after experiencing short–term unemployment. For the individuals starting treatment after a medium–duration of unemployment, the percentage difference in employment rate immediately after the beginning of the treatment are lower than for individuals starting after short–term unemployment. They increase up to 12 months after the beginning of treatment to an insignificant difference for West Germany. In East Germany, there are significant differences for a period up to 18 months after the treatment begun which points out that either the duration of training is longer in East Germany or the negative employment effect due to treatment is more persistent in the post–treatment period.

For individuals who start treatment after a long–term unemployment, i.e. 7–9 months of unemployment before treatment, the differences in employment rates are comparable: In West Germany,

[7] As in the second section of this thesis, the standard errors of the estimated treatment effects are likely to be underestimated because we cannot account for the sampling variability of the propensity score estimate. However, the complexity of the data – especially the size of the naïve control group – did not allow to bootstrap the estimated coefficients. However, as most effects for the medium– and long–term employment outcome are already insignificant *without* trustworthy confidence intervals and standard errors are

the employment rates lie around 30 ppoints lower for participants compared to the matched control outcome, which is slightly higher than for participants after short–term and medium–term unemployment. The differences remains significantly negative the period up to 20 months after the beginning of the treatment. In East Germany, the employment level after the beginning of treatment lies more than 40 ppoints below the non–treatment outcome for the treated, and again, the difference remains significantly lower for a period up to 20 months of out period of observation. Contrary to West Germany, the employment level is significantly reduced by around 40 ppoints for a period 12 months after the beginning of the treatment, showing at the duration of the treatment in East Germany is possible higher than in the West.

The employment effects of treatment for the populations entering into treatment in the year 94 can be found in figure 3.4. Compared to the participation in the treatment one year earlier, we see the same pattern of treatment effects, but there are differences in the employment levels after treatment: While participants starting treatment after short–term unemployment in West Germany have significantly lower employment for a 20 months after the beginning of the treatment, this negative employment effect due to treatment is significant only for a shorter period. It could be caused by either an increase of the effectiveness of the treatment (better: a reduction of the negative effect following treatment) or by reducing of the time individuals spend in treatment. For the population starting treatment after short–term unemployment in East Germany, the same picture is found: The significantly negative employment rates for the treated can only be observed for a period up to one year after treatment. For the medium and long–term unemployed who start provision of specific skills, the employment rates are lower in the first month compared to 93, especially in East Germany, the employment rate for the treated individuals is 50 ppoints lower compared to non–treatment. The employment effect in general remains significantly negative for a longer time in East Germany than in the West, again indicating to a longer duration of the treatment in the East.

assumed to increase due to the variance of the propensity score estimation, it is not likely that the results will change qualitatively.

Figure 3.3 Employment effects for participants in 1993 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany

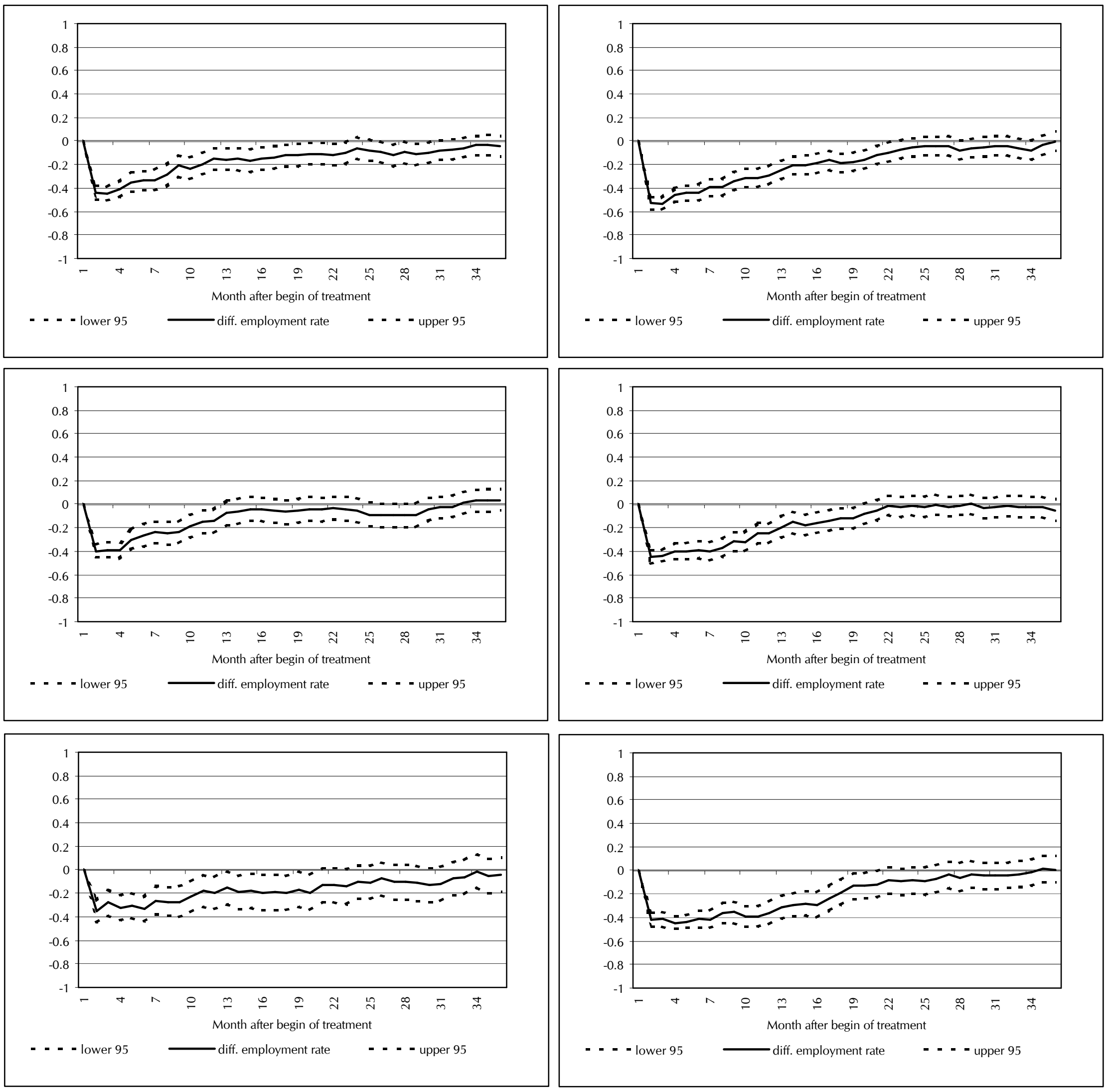
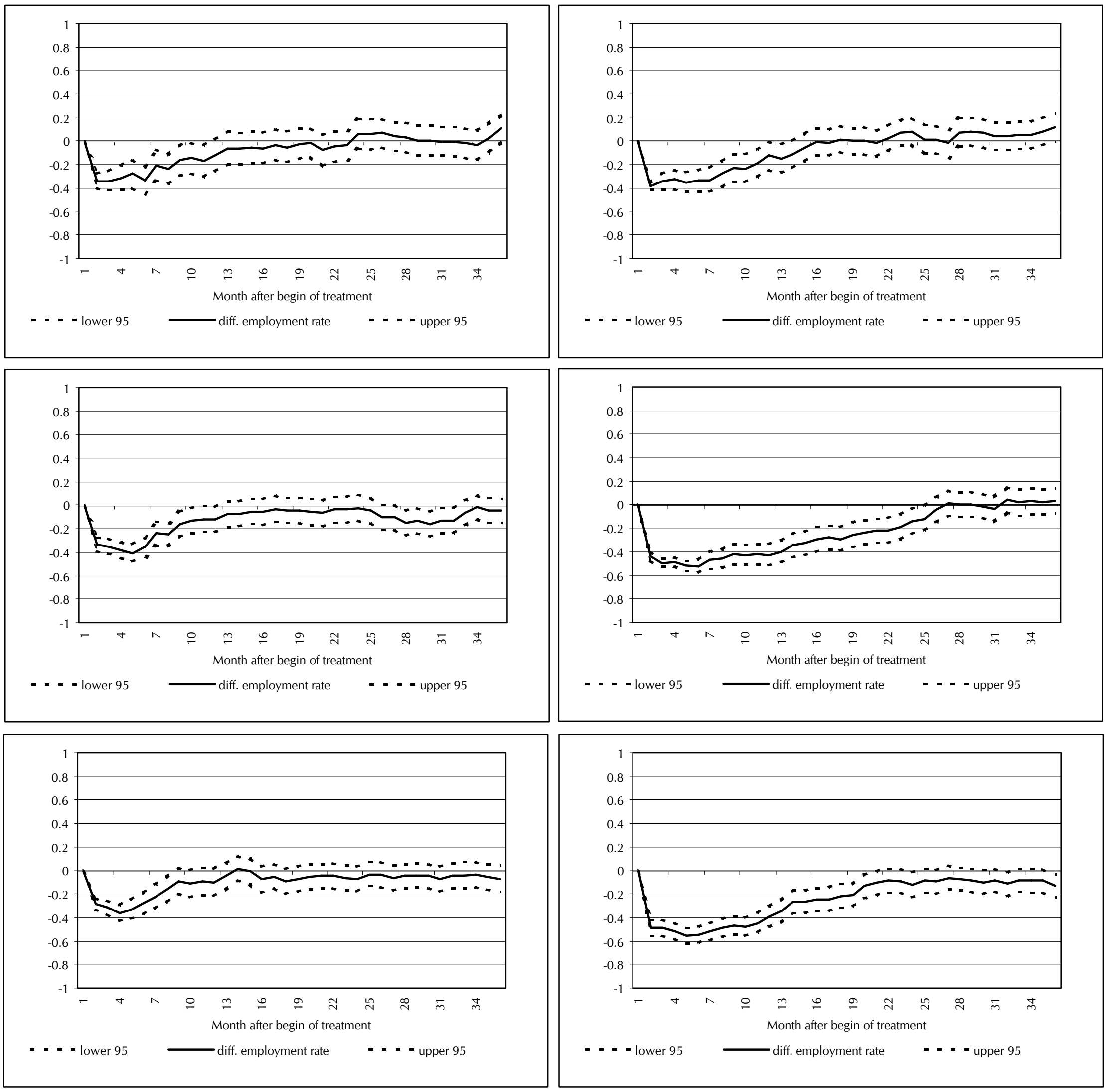


Figure 3.4 Employment effects for participants in 1994 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany



Differences across age groups

Figures 3.5 and 3.6 summarise the findings for the target groups below 36 years of age for treatments starting in the years 93 or 94. One should expect that policies for individuals starting treatment earlier in their life cycle on average exhibit better employment outcomes compared to the aggregate and to an older entry cohort. The results obviously resemble the results for the aggregate: Again, we find a significantly negative employment effect immediately after the beginning of the treatment. The differences in employment rates are similar to the aggregate figures. Compared to all participants, the employment effects of the young population become insignificant earlier after treatment, which might sign on a shorter duration of the programmes or a less negative employment effect. The differences between East Germany and West Germany again indicate that the programme generates poorer results in the East than in the West: The reduced employment for the participants compared to the non–treatment outcome remains significant for a longer period after the beginning of the treatment in the East: While the employment effect becomes insignificant in West Germany after 8 (for entrants coming form short–term unemployment) to 12 months (for those from long–term unemployment), the effect is negative for short–term unemployed in East Germany (up to 20 months after the beginning of the treatment).

We essentially never estimate any positive employment effects. However, there are some few exceptions to this when we compare the employment outcomes for the young cohorts between 93 and 94: For participants starting treatment in West Germany after short–term unemployment in the year 94, we find a positive employment effect 23–26 months after the beginning of the treatment, for participants in East Germany, there seems to be a positive employment outcome 35 months after treatment for young cohorts participating either after short– or after long–term unemployment 93. All other employment outcomes are insignificant for this as well as for the other populations starting the treatment after medium–term and long–term unemployment for both East and West Germany after a period following the beginning of the treatment in which the employment effect is significantly negative for all these populations. Again, the policies seem to more negatively affect the employment prospect for individuals in the East than in the West: Especially groups starting treatment after medium and long–term unemployment in East Germany show a longer and compared to the West a more severe reduction of the employment outcomes following the beginning of the treatment.

Figures 3.7 and 3.8 show the findings for the age group above 35 years. As in the case for the whole participation group, individuals start with a 40 ppoints reduced employment rate for the first months of treatment in West Germany after a short–term unemployment experience in 93. Over the post–treatment period, the differences in the employment rate between treatment and matched control disappear to an insignificant employment effect. The negative employment effect for the group above 35 lasts longer than for the total participation group, and an insignificant employment effect takes place later after the start of the programme than for all participants. These effects vary between the years for West Germany: For participants starting treatment after short–term unemployment in 93, we find a negative treatment effect up to 32 months after treatment, whereas this negative effect disappears after seven months for the participation group starting the treatment one year later. For East Germany, the effects are essentially the same for both years. Again, the estimations suggest that the negative employment effect is more persistent in East Germany compared to West Germany. At the end of the evaluation period, we never find any significant difference in the employment rate caused by the treatment.

Figure 3.5 Employment effects for participants in 1993 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany age group below 36

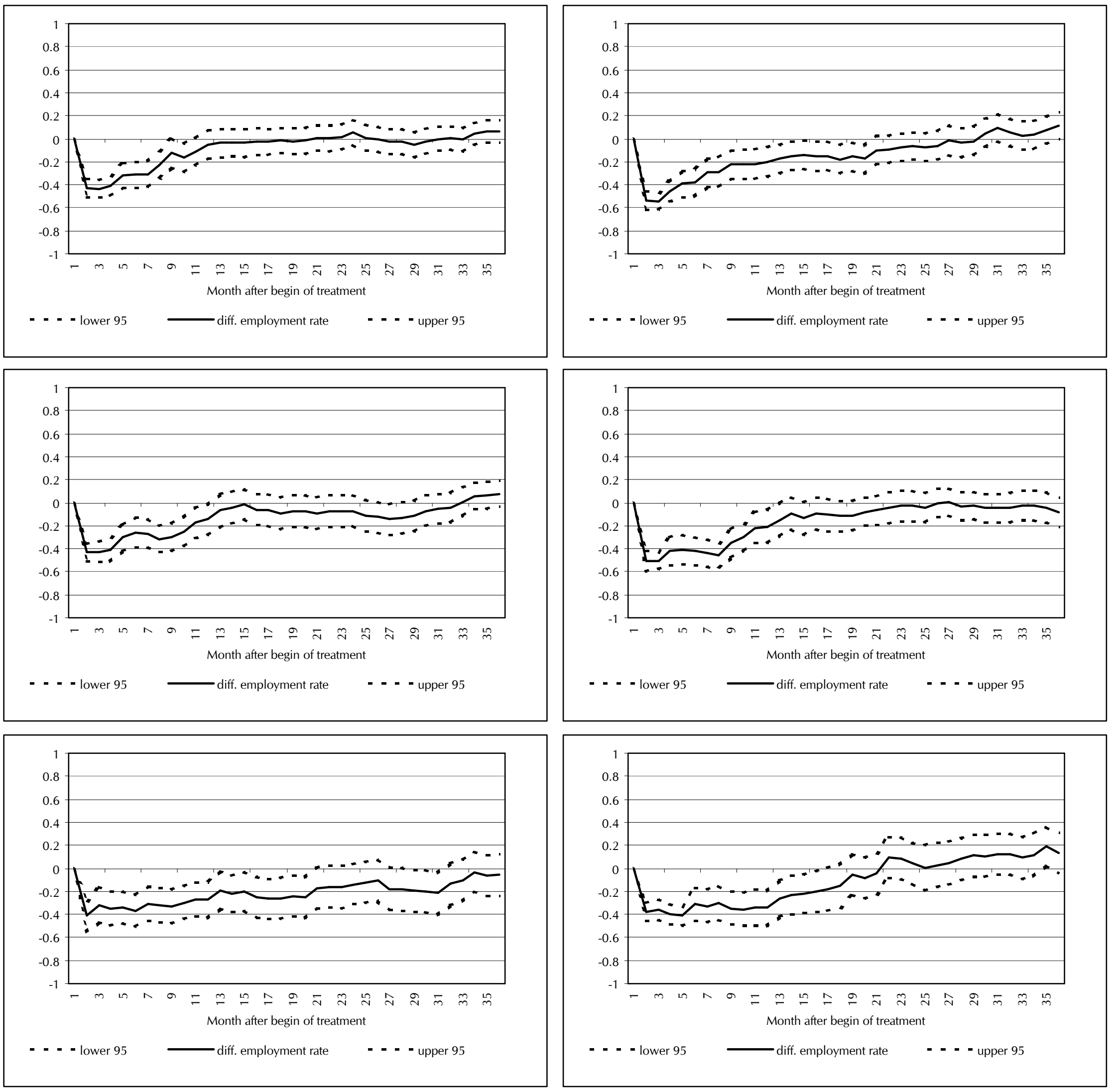


Figure 3.6 Employment effects for participants in 1994 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany age group below 36

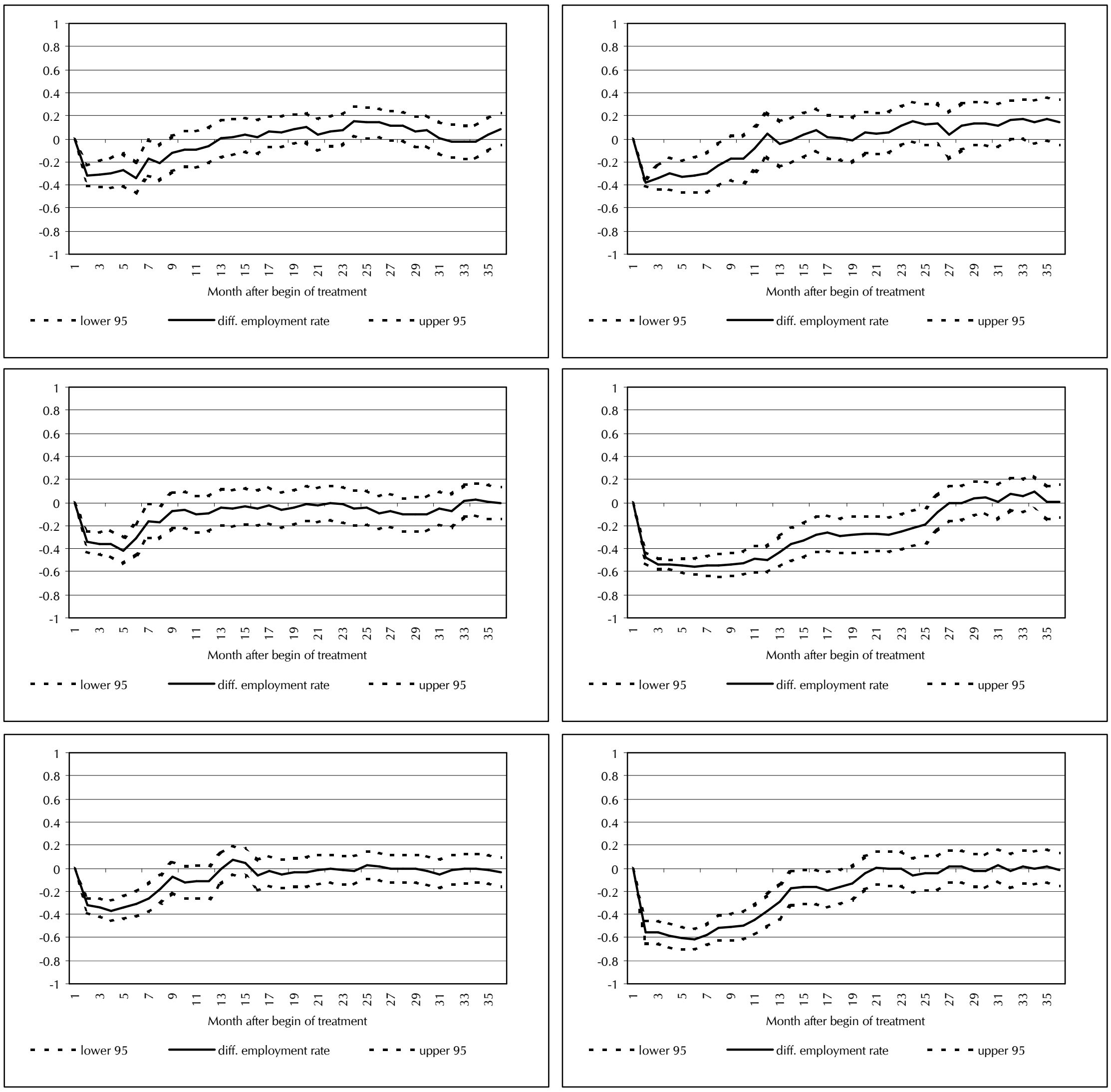


Figure 3.7 Employment effects for participants in 1993 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany age group above 35

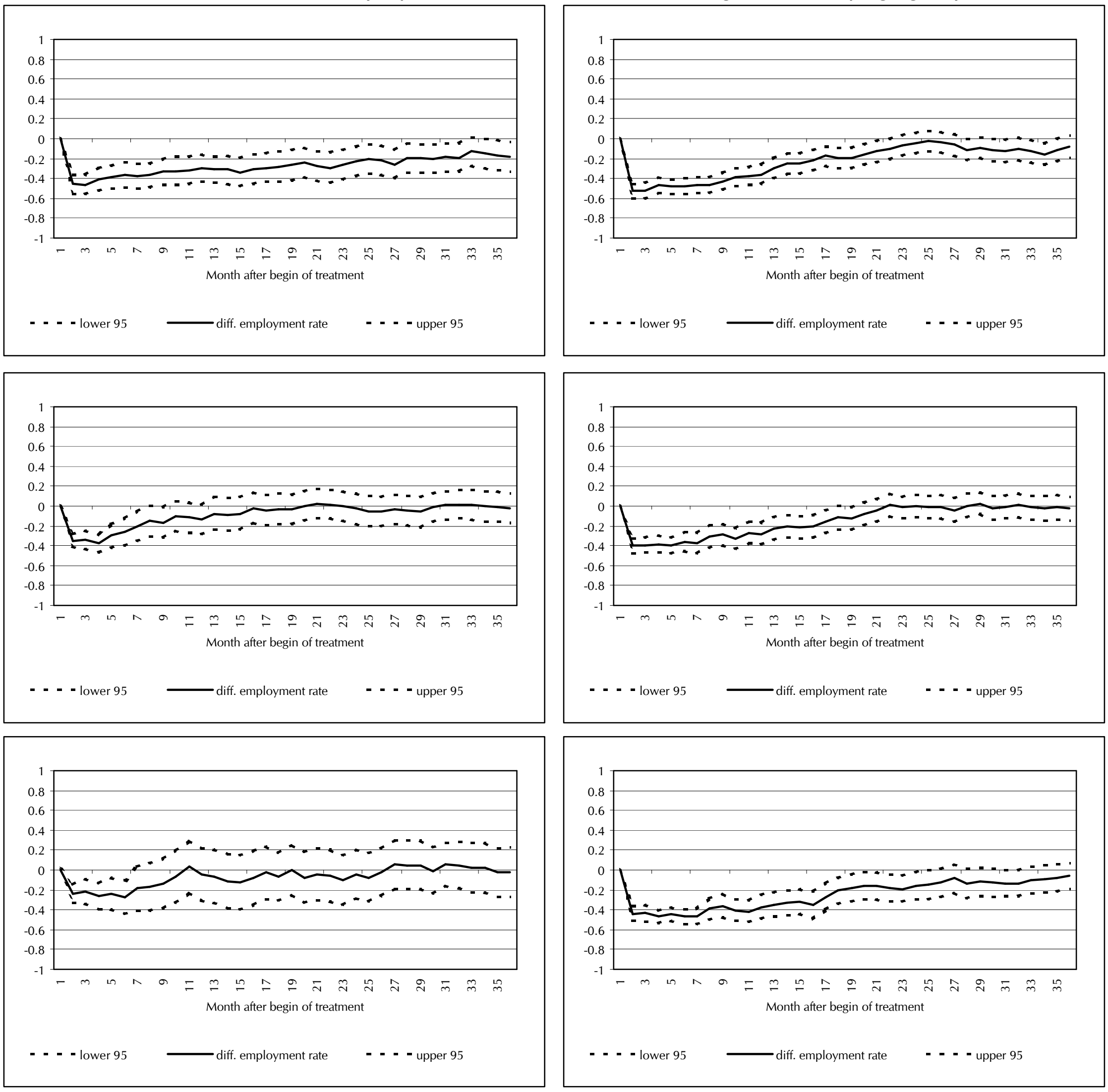
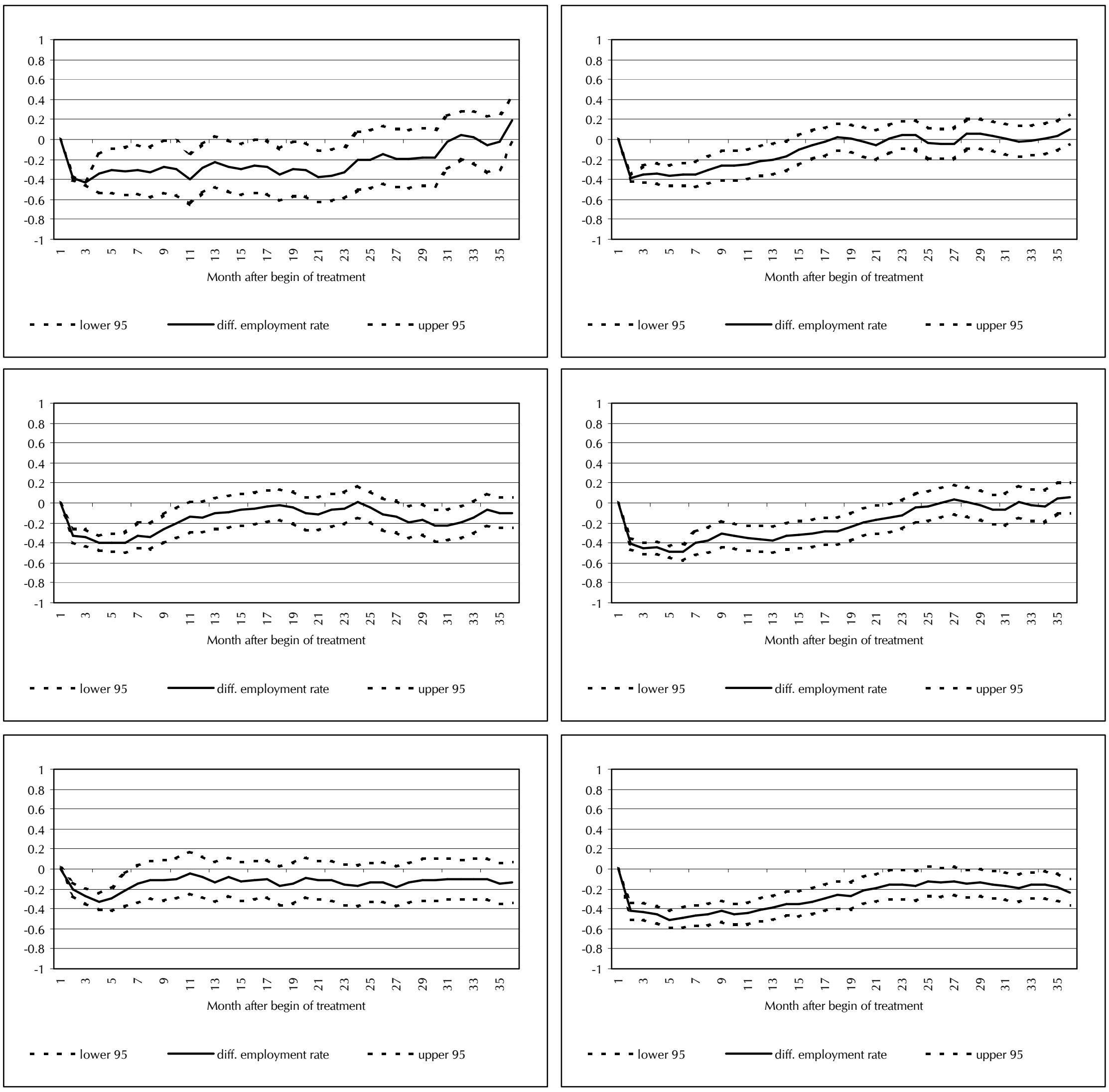


Figure 3.8 Employment effects for participants in 1994 after short–term (top), medium–term (mid) long–term (bottom) unemployment, in West (left) and East (right) Germany age group above 35



3.5 Conclusion

Over the last years, a number of analyses about the effects of public sector sponsored further training were published indicating that only poor employment effects for the participants resulted from these policies. As many of these evaluation studies made use of the available survey data from the GSOEP and the Labour Market Monitor, neither a direct link to the specific policies nor inference for specific targets groups could be drawn. It is vital for the progress of the empirical evidence to make use of alternative data providing new evidence for evaluation. This paper should be regarded as a first attempt to provide such evidence based on the social insurance data, which is regularly recorded for the biggest part of the active labour force in Germany.

It became apparent that using these data for evaluation study requires detailed knowledge both of the institutional regulations of further training and the available data. In preparing the analysis, a complex merge procedure needs to be implemented in order to create an integrated database for evaluation as well as an understanding how and to which particular groups the Employment Service offer these trainings. After sketching the basic regulation of further training in section 3.2, we suggested in section 3.3 to evaluate these policies rather as interventions for specific target groups (short, medium and long–term unemployed) and restricted to evaluation to the most important type of treatment, the provision of specific occupational skills to the unemployed.

The data offer extensive and most detailed information about the legal regulation under which further training is implemented in an individual case. Clear–cut treatments could only be identified by an integration of the information provided and by multiple validation processes. A direct application of evaluation approaches to social insurance data is not adequate because the information provided do not correspond to any socio–economic concept of work, treatment or unemployment. Consequently, only an extensive recoding could clarify the policy interventions in the data, so that we could identify most homogeneous treatment groups in the sample.

The evaluation study then implements different procedures to overcome the microeconomic evaluation problem in non–experimental data. We rely on conditional independence assumption claiming that the non–treatment outcome for treated and non–treated persons are comparable if we condition on observable characteristics. We restrict this identifying assumption to specific observable characteristics and especially to the employment history prior to treatment and condition on these covariates by stratifying the samples based on the same unemployment experience of treated and non–treated individuals to the same calendar time and further covariates subject to kernel matching on the propensity score. We extensively test whether the non–treatment outcome before treatment and the matching on covariates achieve appropriate evaluation data. As there are no more observable differences before treatment, we can evaluate the effect of treatment–on–the–treated non–parametrically within the matched samples.

The results indicate that the employment effect is significantly negative immediately after the beginning of the treatment. This is of course expected because we include the duration of the treatment into the outcome variables in our evaluation design. Some months after the beginning of the training, it seems to initiate a positive employment dynamic, but the effect of the treatment does not show a positive impact on employment.

The effect remains insignificant for long–time after the beginning of the treatment when the training itself is supposed to have ended for the treated. These insignificant employment effects were found for both years 93 and 94, for all three different target groups after short–term, medium–term and long–term unemployment and they are surprisingly similar for East and West Germany.

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3.7 Appendix

3.7.1 Tables and figures

Table 3.A1 Descriptive Statistics for cohorts starting treatment after short–term unemployment between January and December 93 and naïve control group

Unmployment entry time	Oct 92 - Dec 92		Nov 92 - Jan 93		Dec 92 - Feb 93		Jan 93 - Mar 93		Feb 93 - Apr 93		Mar 93 - May 93	
Programm entry time	Jan 93		Feb 93		Mar 93		Apr 93		May 93		Jun 93	
Groups	T	NT	T	NT	T	NT	T	NT	T	NT	T	NT
Number of cases	7	1210	15	1527	20	1650	10	1882	12	1012	5	1129
Unemployment duration at programme start	3.571	3.683	3.067	3.24	2.75	3.432	3.2	3.801	2.25	3.589	2.8	3.669
Sector												
Agriculture	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Ground industry	0.29	0.12	0.07	0.16	0.05	0.16	0.10	0.13	0.00	0.11	0.00	0.11
Metal, automotive, electronics	0.00	0.21	0.20	0.12	0.20	0.14	0.20	0.21	0.17	0.21	0.00	0.27
Other industry	0.00	0.10	0.00	0.10	0.15	0.08	0.00	0.12	0.08	0.14	0.40	0.13
Building and civil engineering	0.14	0.08	0.07	0.17	0.00	0.24	0.00	0.11	0.00	0.09	0.20	0.05
Production related services, trade, and banking	0.29	0.25	0.40	0.23	0.40	0.21	0.60	0.25	0.42	0.28	0.00	0.25
Consumption related/social services/state	0.29	0.24	0.27	0.23	0.20	0.17	0.10	0.18	0.33	0.18	0.40	0.18
Occupational status of last employment												
Trainee	0.14	0.03	0.00	0.03	0.05	0.05	0.10	0.03	0.00	0.08	0.20	0.05
Blue collar	0.71	0.61	0.53	0.73	0.35	0.76	0.20	0.63	0.17	0.61	0.00	0.63
White collar	0.14	0.27	0.40	0.17	0.45	0.14	0.50	0.24	0.75	0.22	0.80	0.21
Working at home or missing	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Part-time worker	0.00	0.09	0.07	0.07	0.15	0.06	0.20	0.10	0.08	0.09	0.00	0.10
Regional information at programme entry												
Schleswig-Holstein/Hamburg	0.14	0.06	0.13	0.07	0.20	0.06	0.10	0.06	0.25	0.06	0.40	0.06
Lower Saxony/Bremen	0.14	0.14	0.20	0.16	0.05	0.13	0.10	0.14	0.08	0.12	0.20	0.14
Northrhine-Westphalia	0.43	0.29	0.20	0.21	0.35	0.20	0.30	0.27	0.08	0.28	0.00	0.30
Hesse	0.00	0.09	0.07	0.08	0.00	0.07	0.00	0.09	0.08	0.09	0.20	0.10
Rhineland-Palatinate/Saar	0.14	0.08	0.13	0.09	0.15	0.07	0.10	0.08	0.17	0.07	0.00	0.07
Baden-Wuerttemberg	0.00	0.15	0.07	0.11	0.20	0.13	0.00	0.14	0.25	0.19	0.20	0.16
Bavaria	0.14	0.19	0.20	0.28	0.05	0.35	0.40	0.22	0.08	0.18	0.00	0.17
Firm size of last employment												
less than 11	0.57	0.27	0.40	0.35	0.30	0.30	0.50	0.23	0.08	0.26	0.20	0.25
11 to 200	0.00	0.40	0.27	0.45	0.40	0.47	0.40	0.42	0.42	0.40	0.20	0.37
200 to 500	0.00	0.12	0.13	0.07	0.15	0.08	0.00	0.12	0.00	0.10	0.20	0.10
more than 500	0.43	0.22	0.20	0.12	0.15	0.15	0.10	0.24	0.50	0.24	0.40	0.29
Level of education												
No vocational training	0.00	0.26	0.20	0.24	0.35	0.28	0.20	0.28	0.25	0.29	0.00	0.31
Dual System	1.00	0.68	0.67	0.72	0.60	0.68	0.70	0.66	0.58	0.67	0.80	0.63
A-level	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.08	0.01	0.20	0.01
Technical college or university	0.00	0.05	0.13	0.02	0.05	0.02	0.10	0.04	0.08	0.03	0.00	0.03
no information	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.02
Nationality												
German	0.86	0.83	0.73	0.85	0.85	0.86	1.00	0.85	1.00	0.81	0.80	0.81
EU citizen (1995 enlargement)	0.00	0.06	0.20	0.04	0.00	0.04	0.00	0.05	0.00	0.04	0.00	0.06
Former Yugoslavia	0.00	0.02	0.00	0.02	0.10	0.03	0.00	0.03	0.00	0.03	0.00	0.02
Turkey	0.14	0.05	0.07	0.05	0.05	0.05	0.00	0.04	0.00	0.07	0.00	0.07
Others	0.00	0.04	0.00	0.04	0.00	0.03	0.00	0.04	0.00	0.04	0.20	0.04
Region type												
Rural area	0.29	0.20	0.33	0.33	0.10	0.32	0.20	0.21	0.00	0.18	0.20	0.19
Medium populated area	0.14	0.36	0.40	0.35	0.45	0.35	0.30	0.34	0.33	0.35	0.20	0.35
Densely populated area	0.43	0.30	0.13	0.20	0.20	0.22	0.40	0.30	0.42	0.29	0.40	0.31
Metropolitain areas	0.14	0.14	0.13	0.12	0.25	0.12	0.10	0.15	0.25	0.18	0.20	0.15
Gross salary of former employment (€)	40.35	50.65	54.95	49.67	50.81	52.03	47.06	55.60	63.80	46.05	61.18	50.92
Sex	0.29	0.43	0.47	0.35	0.45	0.28	0.80	0.36	0.50	0.38	0.80	0.38

Table 3.A1 Descriptive Statistics for cohorts starting treatment after short–term unemployment between January and December 93 and naïve control group (cont.)

Unmployment entry time	Apr 93 - Jun 93		May 93 - Jul 93		Jun 93 - Aug 93		Jul 93 - Sep 93		Aug 93 - Oct 93		Sep 93 - Nov 93	
Programm entry time	Jul 93		Aug 93		34213		Oct 93		Nov 93		Dec 93	
Groups	T	NT	T	NT	T	NT	T	NT	T	NT	T	NT
Number of cases	5	1237	5	948	8	1020	11	1366	14	860	9	3257
Unemployment duration at programme start	3.6	3.79	2.6	3.396	3	3.539	3.455	3.78	2.429	3.671	2.889	2.874
Sector												
Agriculture	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00
Ground industry	0.00	0.12	0.20	0.10	0.13	0.08	0.00	0.11	0.07	0.13	0.33	0.12
Metal, automotive, electronics	0.60	0.18	0.00	0.21	0.00	0.22	0.36	0.22	0.36	0.21	0.22	0.19
Other industry	0.00	0.13	0.00	0.11	0.00	0.10	0.00	0.12	0.00	0.11	0.11	0.10
Building and civil engineering	0.20	0.06	0.20	0.06	0.00	0.08	0.09	0.06	0.21	0.08	0.11	0.08
Production related services, trade, and banking	0.00	0.29	0.40	0.29	0.88	0.29	0.36	0.28	0.36	0.26	0.22	0.25
Consumption related/social services/state	0.20	0.20	0.20	0.23	0.00	0.22	0.18	0.21	0.00	0.22	0.00	0.26
Occupational status of last employment												
Trainee	0.00	0.03	0.00	0.09	0.13	0.11	0.09	0.09	0.07	0.06	0.00	0.04
Blue collar	0.40	0.52	0.00	0.55	0.38	0.56	0.46	0.48	0.43	0.58	0.56	0.63
White collar	0.60	0.32	1.00	0.25	0.50	0.22	0.27	0.31	0.50	0.26	0.44	0.25
Working at home or missing	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Part-time worker	0.00	0.12	0.00	0.11	0.00	0.11	0.18	0.11	0.00	0.10	0.00	0.09
Regional information at programme entry												
Schleswig-Holstein/Hamburg	0.00	0.07	0.20	0.06	0.25	0.07	0.00	0.06	0.14	0.07	0.11	0.07
Lower Saxony/Bremen	0.00	0.14	0.00	0.13	0.00	0.15	0.00	0.13	0.07	0.14	0.00	0.15
Northrhine-Westphalia	0.20	0.27	0.40	0.30	0.75	0.29	0.27	0.29	0.36	0.27	0.33	0.29
Hesse	0.00	0.09	0.00	0.08	0.00	0.10	0.00	0.09	0.00	0.09	0.00	0.09
Rhineland-Palatinate/Saar	0.40	0.09	0.20	0.08	0.00	0.07	0.46	0.09	0.14	0.09	0.22	0.07
Baden-Wuerttemberg	0.40	0.17	0.20	0.18	0.00	0.15	0.00	0.17	0.00	0.16	0.11	0.15
Bavaria	0.00	0.17	0.00	0.17	0.00	0.18	0.27	0.18	0.29	0.19	0.22	0.18
Firm size of last employment												
less than 11	0.00	0.28	0.20	0.29	0.38	0.25	0.27	0.22	0.36	0.26	0.22	0.29
11 to 200	0.80	0.40	0.60	0.43	0.63	0.43	0.27	0.42	0.29	0.45	0.22	0.42
200 to 500	0.00	0.13	0.00	0.11	0.00	0.12	0.09	0.12	0.14	0.10	0.44	0.10
more than 500	0.20	0.19	0.20	0.18	0.00	0.20	0.36	0.24	0.21	0.19	0.11	0.19
Level of education												
No vocational training	0.00	0.24	0.00	0.31	0.13	0.32	0.09	0.29	0.00	0.26	0.11	0.27
Dual System	1.00	0.67	1.00	0.63	0.88	0.61	0.91	0.62	0.86	0.67	0.78	0.67
A-level	0.00	0.01	0.00	0.01	0.00	0.02	0.00	0.02	0.14	0.02	0.00	0.01
Technical college or university	0.00	0.07	0.00	0.04	0.00	0.04	0.00	0.06	0.00	0.05	0.11	0.04
no information	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	0.01
Nationality												
German	1.00	0.83	1.00	0.82	0.75	0.83	0.91	0.84	0.93	0.86	1.00	0.82
EU citizen (1995 enlargement)	0.00	0.04	0.00	0.05	0.00	0.05	0.09	0.04	0.07	0.03	0.00	0.05
Former Yugoslavia	0.00	0.03	0.00	0.03	0.00	0.02	0.00	0.02	0.00	0.01	0.00	0.03
Turkey	0.00	0.06	0.00	0.06	0.13	0.06	0.00	0.06	0.00	0.06	0.00	0.06
Others	0.00	0.04	0.00	0.04	0.13	0.04	0.00	0.04	0.00	0.04	0.00	0.04
Region type												
Rural area	0.20	0.14	0.20	0.18	0.00	0.19	0.27	0.17	0.43	0.19	0.22	0.20
Medium populated area	0.60	0.38	0.00	0.36	0.38	0.38	0.46	0.36	0.14	0.35	0.44	0.37
Densely populated area	0.20	0.33	0.40	0.31	0.25	0.28	0.18	0.32	0.14	0.32	0.22	0.28
Metropolitain areas	0.00	0.15	0.40	0.15	0.38	0.14	0.09	0.15	0.29	0.14	0.11	0.16
Gross salary of former employment (€)	71.83	52.49	42.06	46.21	48.52	44.68	48.66	53.40	63.14	49.42	67.71	51.71
Sex	0.40	0.46	0.80	0.44	0.38	0.42	0.46	0.42	0.29	0.40	0.33	0.41

Table 3.A2 Probit estimation of the propensity scores, treatment in 1993 after short-term (left), medium-term (centre) and long-term (right) unemployment in West Germany

N = 17219	Sc. Pseudo R-squared = .010747		
Positive obs. = 121	LR (zero slopes) = 174.540 [.000]		
Mean of dep. var. = .702712E-02	Schwarz B.I.C. = 784.403		
Sum of squared residuals = 117.945	Log likelihood = -633.219		
Pseudo R-squared = .018368			
Fraction of Correct Predictions =	0.99		
	Estimate	t-statistic	P-value
Constant	6.53	3.03	0.00
Age			
Years	-0.01	-2.95	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.15	-1.12	0.27
Other industry	-0.28	-1.74	0.08
Building and civil engineering	-0.10	-0.65	0.52
Production related services, trade, and banking	-0.04	-0.34	0.73
Consumption related and social services, and the state	-0.29	-2.30	0.02
Occupational status (left out category: blue collar)			
Trainee	0.11	0.66	0.51
White collar	0.44	4.77	0.00
At home, part-time or missing	0.15	1.01	0.31
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.23	1.82	0.07
Lower Saxony/Bremen	-0.30	-2.08	0.04
Hesse	-0.53	-2.48	0.01
Rhineland-Palatinate/Saar	0.25	2.09	0.04
Baden-Wuerttemberg	-0.15	-1.22	0.22
Bavaria	-0.23	-1.83	0.07
Firm size (left out: 11 to 200 employees)			
Less than 11	0.05	0.61	0.54
200 to 500	0.15	1.16	0.25
More than 500	0.24	2.31	0.02
Level of education (left out: dual system)			
Without formal education	-0.18	-1.73	0.08
Technical college/university	-0.06	-0.34	0.74
Nationality (left out: German)			
Non-German	0.03	0.25	0.81
Regional type (left out: densely populated)			
Rural area	0.08	0.64	0.52
Medium populated area	-0.01	-0.06	0.95
Inner city area	0.04	0.31	0.76
Sex (left out: male)			
Women	0.06	0.77	0.44
Employment experience previous to unemployment			
Employment six months before t = 1	0.20	1.56	0.12
Employment nine months before t = 1	-0.23	-1.83	0.07
Employment 12 months before t = 1	0.19	1.60	0.11
Calendar time of unemployment entry			
Month	-0.04	-3.72	0.00
Months before programme			
Months	-0.34	-7.74	0.00

N = 11013	Sc. Pseudo R-squared = .017995		
Positive obs. = 115	LR (zero slopes) = 185.118 [.000]		
Mean of dep. var. = .010442	Schwarz B.I.C. = 690.713		
Sum of squared residuals = 112.026	Log likelihood = -546.457		
Pseudo R-squared = .019344			
Fraction of Correct Predictions =	0.99		
	Estimate	t-statistic	P-value
Constant	13.16	5.44	0.00
Age			
Years	-0.01	-3.94	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	0.08	0.55	0.58
Other industry	-0.29	-1.50	0.13
Building and civil engineering	-0.15	-0.79	0.43
Production related services, trade, and banking	0.11	0.85	0.39
Consumption related and social services, and the state	-0.08	-0.62	0.54
Occupational status (left out category: blue collar)			
Trainee	-0.41	-1.58	0.11
White collar	0.22	2.11	0.04
At home, part-time or missing	0.12	0.79	0.43
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.22	1.44	0.15
Lower Saxony/Bremen	0.21	1.66	0.10
Hesse	-0.54	-2.24	0.03
Rhineland-Palatinate/Saar	0.12	0.77	0.44
Baden-Wuerttemberg	-0.04	-0.33	0.74
Bavaria	0.15	1.11	0.27
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.02	-0.26	0.79
200 to 500	-0.03	-0.19	0.85
More than 500	-0.15	-1.24	0.22
Level of education (left out: dual system)			
Without formal education	-0.03	-0.31	0.76
Technical college/university	0.42	3.08	0.00
Nationality (left out: German)			
Non-German	-0.07	-0.58	0.56
Regional type (left out: densely populated)			
Rural area	-0.40	-2.93	0.00
Medium populated area	-0.11	-1.09	0.28
Inner city area	-0.11	-0.86	0.39
Sex (left out: male)			
Women	-0.10	-1.13	0.26
Employment experience previous to unemployment			
Employment nine months before t = 1	0.36	2.46	0.01
Employment 12 months before t = 1	-0.16	-1.19	0.23
Employment 15 months before t = 1	0.13	1.11	0.27
Calendar time of unemployment entry			
Month	-0.06	-5.52	0.00
Months before programme			
Months	-0.40	-9.11	0.00

N = 7895	Sc. Pseudo R-squared = .015493		
Positive obs. = 60	LR (zero slopes) = 113.102 [.000]		
Mean of dep. var. = .759975E-02	Schwarz B.I.C. = 435.096		
Sum of squared residuals = 59.4587	Log likelihood = -295.999		
Pseudo R-squared = .011590			
Fraction of Correct Predictions =	0.99		
	Estimate	t-statistic	P-value
Constant	16.69	5.09	0.00
Age			
Years	-0.02	-5.05	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.03	-0.13	0.89
Other industry	-0.08	-0.38	0.70
Building and civil engineering	-0.15	-0.64	0.52
Production related services, trade, and banking	0.11	0.70	0.49
Consumption related and social services, and the state	0.02	0.10	0.92
Occupational status (left out category: blue collar)			
Trainee	-0.28	-0.92	0.36
White collar	0.06	0.43	0.67
At home, part-time or missing	0.01	0.02	0.98
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.21	1.05	0.29
Lower Saxony/Bremen	-0.08	-0.44	0.66
Hesse	-0.13	-0.55	0.59
Rhineland-Palatinate/Saar	0.06	0.29	0.78
Baden-Wuerttemberg	-0.03	-0.17	0.86
Bavaria	0.12	0.66	0.51
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.11	-0.80	0.43
200 to 500	-0.23	-1.09	0.28
More than 500	0.02	0.12	0.91
Level of education (left out: dual system)			
Without formal education	-0.23	-1.62	0.11
Technical college/university	-0.21	-0.70	0.48
Nationality (left out: German)			
Non-German	-0.30	-1.11	0.27
Regional type (left out: densely populated)			
Rural area	-0.07	-0.39	0.69
Medium populated area	0.07	0.55	0.58
Inner city area	-0.10	-0.54	0.59
Sex (left out: male)			
Women	-0.31	-2.58	0.01
Employment experience previous to unemployment			
Employment 12 months before t = 1	0.00	0.01	0.99
Employment 15 months before t = 1	0.21	1.19	0.24
Employment 18 months before t = 1	-0.06	-0.42	0.68
Calendar time of unemployment entry			
Month	-0.07	-4.86	0.00
Months before programme			
Months	-0.35	-6.55	0.00

Table 3.A2 Probit estimation of the propensity scores, treatment in 1993 after short-term (left), medium-term (centre) and long-term (right) unemployment in East Germany (cont.)

N = 8673	Sc. Pseudo R-squared = .032721		
Positive obs. = 170	LR (zero slopes) = 265.032 [.000]		
Mean of dep. var. = .019601	Schwarz B.I.C. = 835.762		
Sum of squared residuals = 155.794	Log likelihood = -704.276		
Pseudo R-squared = .066525			
Fraction of Correct Predictions =			0.98
	Estimate	t-statistic	P-value
Constant	16.90	7.47	0.00
Age			
Years	-0.01	-2.33	0.02
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.13	-0.98	0.33
Other industry	-0.11	-0.69	0.49
Building and civil engineering	-0.51	-3.08	0.00
Production related services, trade, and banking	-0.15	-1.25	0.21
Consumption related and social services, and the state	-0.27	-2.34	0.02
Occupational status (left out category: blue collar)			
Trainee	-0.18	-0.64	0.52
White collar	0.29	3.25	0.00
At home, part-time or missing	0.02	0.11	0.91
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.12	0.90	0.37
Saxony-Anhalt	-0.09	-0.72	0.47
Saxony Free State	0.25	1.92	0.06
Thuringia	0.04	0.29	0.77
Firm size (left out: 11 to 200 employees)			
Less than 11	0.07	0.74	0.46
200 to 500	0.30	2.88	0.00
More than 500	0.24	2.43	0.02
Level of education (left out: dual system)			
Without formal education	-0.49	-2.74	0.01
Technical college/university	0.14	1.09	0.28
Nationality (left out: German)			
Non-German	-0.32	-0.83	0.40
Regional type (left out: densely populated)			
Rural area	-0.24	-1.19	0.24
Medium populated area	-0.36	-1.98	0.05
Inner city area	-0.44	-2.08	0.04
Sex (left out: male)			
Women	0.07	0.93	0.35
Employment experience previous to unemployment			
Employment six months before t = 1	0.12	0.86	0.39
Employment nine months before t = 1	0.01	0.10	0.92
Employment 12 months before t = 1	-0.04	-0.39	0.69
Calendar time of unemployment entry			
Month	-0.08	-7.75	0.00
Months before programme			
Months	-0.56	-12.18	0.00

N = 7057	Sc. Pseudo R-squared = .032688		
Positive obs. = 138	LR (zero slopes) = 215.416 [.000]		
Mean of dep. var. = .019555	Schwarz B.I.C. = 695.963		
Sum of squared residuals = 128.066	Log likelihood = -571.898		
Pseudo R-squared = .053480			
Fraction of Correct Predictions =			0.98
	Estimate	t-statistic	P-value
Constant	15.89	5.67	0.00
Age			
Years	-0.02	-4.58	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.26	-1.83	0.07
Other industry	-0.26	-1.60	0.11
Building and civil engineering	-0.60	-3.06	0.00
Production related services, trade, and banking	-0.36	-2.81	0.01
Consumption related and social services, and the state	-0.44	-3.67	0.00
Occupational status (left out category: blue collar)			
Trainee	-0.52	-1.32	0.19
White collar	0.32	3.41	0.00
At home, part-time or missing	-0.28	-0.76	0.45
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.55	3.87	0.00
Saxony-Anhalt	0.05	0.29	0.77
Saxony Free State	0.30	2.07	0.04
Thuringia	0.19	1.24	0.22
Firm size (left out: 11 to 200 employees)			
Less than 11	0.04	0.45	0.65
200 to 500	-0.06	-0.52	0.60
More than 500	-0.21	-1.79	0.07
Level of education (left out: dual system)			
Without formal education	-0.27	-1.77	0.08
Technical college/university	0.02	0.16	0.87
Nationality (left out: German)			
Non-German			
Regional type (left out: densely populated)			
Rural area	0.15	0.50	0.62
Medium populated area	0.16	0.56	0.57
Inner city area	0.24	0.77	0.44
Sex (left out: male)			
Women	0.00	0.02	0.98
Employment experience previous to unemployment			
Employment nine months before t = 1	0.07	0.49	0.63
Employment 12 months before t = 1	0.05	0.37	0.71
Employment 15 months before t = 1	0.10	1.03	0.30
Calendar time of unemployment entry			
Month	-0.07	-5.38	0.00
Months before programme			
Months	-0.55	-10.31	0.00

N = 5831	Sc. Pseudo R-squared = .042846		
Positive obs. = 94	LR (zero slopes) = 224.281 [.000]		
Mean of dep. var. = .016121	Schwarz B.I.C. = 486.154		
Sum of squared residuals = 85.8468	Log likelihood = -369.097		
Pseudo R-squared = .072927			
Fraction of Correct Predictions =			0.98
	Estimate	t-statistic	P-value
Constant	21.69	6.15	0.00
Age			
Years	-0.02	-4.73	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.33	-1.45	0.15
Other industry	-0.08	-0.40	0.69
Building and civil engineering	0.08	0.33	0.74
Production related services, trade, and banking	-0.04	-0.22	0.82
Consumption related and social services, and the state	0.00	0.01	0.99
Occupational status (left out category: blue collar)			
Trainee	0.35	3.01	0.00
White collar	-0.19	-0.47	0.64
At home, part-time or missing			
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.62	3.55	0.00
Saxony-Anhalt	-0.07	-0.33	0.74
Saxony Free State	0.22	1.06	0.29
Thuringia	0.14	0.68	0.50
Firm size (left out: 11 to 200 employees)			
Less than 11	0.09	0.73	0.47
200 to 500	0.10	0.76	0.45
More than 500	-0.22	-1.53	0.13
Level of education (left out: dual system)			
Without formal education	-0.09	-0.55	0.59
Technical college/university	0.47	2.81	0.01
Nationality (left out: German)			
Non-German			
Regional type (left out: densely populated)			
Rural area	0.02	0.06	0.96
Medium populated area	0.12	0.38	0.71
Inner city area	-0.17	-0.45	0.65
Sex (left out: male)			
Women	0.30	2.71	0.01
Employment experience previous to unemployment			
Employment 12 months before t = 1	-0.01	-0.06	0.96
Employment 15 months before t = 1	0.15	1.21	0.23
Employment 18 months before t = 1	-0.09	-0.80	0.43
Calendar time of unemployment entry			
Month	-0.09	-5.75	0.00
Months before programme			
Months	-0.48	-9.87	0.00

Table 3.A2 Probit estimation of the propensity scores, treatment in 1994 after short-term (left), medium-term (centre) and long-term (right) unemployment in West Germany (cont.)

N = 6673	Sc. Pseudo R-squared = .012234		
Positive obs. = 57	LR (zero slopes) = 77.2079 [.000]		
Mean of dep. var. = .854189E-02	Schwarz B.I.C. = 426.120		
Sum of squared residuals = 55.8966	Log likelihood = -289.630		
Pseudo R-squared = .011744			
Fraction of Correct Predictions =	0.99		
	Estimate	t-statistic	P-value
Constant	-5.78	-1.70	0.09
Age			
Years	-0.02	-2.98	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.17	-0.72	0.47
Other industry	-0.07	-0.30	0.77
Building and civil engineering	-0.14	-0.64	0.52
Production related services, trade, and banking	0.01	0.03	0.98
Consumption related and social services, and the state	-0.25	-1.25	0.21
Occupational status (left out category: blue collar)			
Trainee	0.26	0.82	0.41
White collar	0.49	3.51	0.00
At home, part-time or missing	0.29	1.27	0.21
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.18	0.87	0.39
Lower Saxony/Bremen	-0.16	-0.79	0.43
Hesse	-0.13	-0.57	0.57
Rhineland-Palatinate/Saar	0.26	1.36	0.18
Baden-Wuerttemberg	-0.24	-1.18	0.24
Bavaria	0.04	0.21	0.84
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.22	-1.76	0.08
200 to 500	0.12	0.65	0.52
More than 500	-0.35	-1.55	0.12
Level of education (left out: dual system)			
Without formal education	-0.06	-0.38	0.70
Technical college/university	0.15	0.64	0.52
Nationality (left out: German)			
Non-German	-0.21	-1.18	0.24
Regional type (left out: densely populated)			
Rural area	-0.43	-2.33	0.02
Medium populated area	-0.14	-1.04	0.30
Inner city area	-0.25	-1.29	0.20
Sex (left out: male)			
Women	-0.31	-2.35	0.02
Employment experience previous to unemployment			
Employment six months before t = 1	0.03	0.21	0.84
Employment nine months before t = 1	-0.18	-1.25	0.21
Employment 12 months before t = 1	0.04	0.30	0.76
Calendar time of unemployment entry			
Month	0.02	1.53	0.13
Months before programme			
Months	-0.19	-2.64	0.01

N = 6010	Sc. Pseudo R-squared = .022606		
Positive obs. = 95	LR (zero slopes) = 128.190 [.000]		
Mean of dep. var. = .015807	Schwarz B.I.C. = 559.012		
Sum of squared residuals = 91.0305	Log likelihood = -424.144		
Pseudo R-squared = .027438			
Fraction of Correct Predictions =	0.98		
	Estimate	t-statistic	P-value
Constant	-5.87	-1.74	0.08
Age			
Years	-0.01	-1.94	0.05
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.35	-1.78	0.08
Other industry	-0.17	-0.91	0.36
Building and civil engineering	-0.34	-1.65	0.10
Production related services, trade, and banking	-0.08	-0.58	0.56
Consumption related and social services, and the state	-0.27	-1.81	0.07
Occupational status (left out category: blue collar)			
Trainee	-0.07	-0.24	0.81
White collar	0.39	3.42	0.00
At home, part-time or missing	-0.06	-0.30	0.77
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.15	0.87	0.39
Lower Saxony/Bremen	-0.25	-1.40	0.16
Hesse	-0.44	-1.81	0.07
Rhineland-Palatinate/Saar	0.35	2.18	0.03
Baden-Wuerttemberg	0.00	-0.01	0.99
Bavaria	0.06	0.41	0.68
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.15	-1.40	0.16
200 to 500	-0.16	-0.92	0.36
More than 500	-0.09	-0.64	0.52
Level of education (left out: dual system)			
Without formal education	-0.28	-1.98	0.05
Technical college/university	0.22	1.33	0.18
Nationality (left out: German)			
Non-German	-0.18	-1.22	0.22
Regional type (left out: densely populated)			
Rural area	-0.18	-1.22	0.22
Medium populated area	-0.13	-1.12	0.26
Inner city area	0.00	0.02	0.98
Sex (left out: male)			
Women	-0.05	-0.49	0.62
Employment experience previous to unemployment			
Employment nine months before t = 1	0.05	0.39	0.70
Employment 12 months before t = 1	0.01	0.09	0.93
Employment 15 months before t = 1	0.10	0.81	0.42
Calendar time of unemployment entry			
Month	0.03	1.89	0.06
Months before programme			
Months	-0.30	-5.69	0.00

N = 5904	Sc. Pseudo R-squared = .024659		
Positive obs. = 88	LR (zero slopes) = 136.150 [.000]		
Mean of dep. var. = .014905	Schwarz B.I.C. = 523.991		
Sum of squared residuals = 85.2095	Log likelihood = -389.398		
Pseudo R-squared = .021438			
Fraction of Correct Predictions =	0.98		
	Estimate	t-statistic	P-value
Constant	-3.87	-1.02	0.31
Age			
Years	-0.02	-5.32	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.02	-0.10	0.92
Other industry	0.01	0.05	0.96
Building and civil engineering	0.00	-0.01	0.99
Production related services, trade, and banking	0.02	0.14	0.89
Consumption related and social services, and the state	-0.52	-2.77	0.01
Occupational status (left out category: blue collar)			
Trainee	0.07	0.31	0.76
White collar	0.18	1.53	0.13
At home, part-time or missing	-0.34	-0.95	0.34
Region (left out: Northrhine-Westphalia)			
Schleswig-Holstein/Hamburg	0.28	1.34	0.18
Lower Saxony/Bremen	0.26	1.60	0.11
Hesse	-0.13	-0.59	0.56
Rhineland-Palatinate/Saar	0.06	0.29	0.78
Baden-Wuerttemberg	0.03	0.17	0.87
Bavaria	0.41	2.60	0.01
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.02	-0.19	0.85
200 to 500	0.02	0.11	0.91
More than 500	0.02	0.17	0.86
Level of education (left out: dual system)			
Without formal education	-0.35	-2.45	0.01
Technical college/university	0.42	2.46	0.01
Nationality (left out: German)			
Non-German	-0.70	-2.06	0.04
Regional type (left out: densely populated)			
Rural area	0.00	0.00	1.00
Medium populated area	0.05	0.38	0.70
Inner city area	-0.08	-0.43	0.67
Sex (left out: male)			
Women	-0.05	-0.43	0.67
Employment experience previous to unemployment			
Employment 12 months before t = 1	-0.02	-0.17	0.86
Employment 15 months before t = 1	-0.01	-0.04	0.97
Employment 18 months before t = 1	0.10	0.75	0.45
Calendar time of unemployment entry			
Month	0.02	1.26	0.21
Months before programme			
Months	-0.21	-4.68	0.00

Table 3.A2 Probit estimation of the propensity scores, treatment in 1994 after short-term (left), medium-term (centre) and long-term (right) unemployment in East Germany (cont.)

Short-term (left)

N = 3358	Sc. Pseudo R-squared = .025575
Positive obs. = 71	LR (zero slopes) = 81.7244 [.000]
Mean of dep. var. = .021144	Schwarz B.I.C. = 416.855
Sum of squared residuals = 67.6120	Log likelihood = -303.188
Pseudo R-squared = .027751	
Fraction of Correct Predictions =	0.98

	Estimate	t-statistic	P-value
Constant	-8.85	-2.64	0.01
Age			
Years	-0.01	-1.57	0.12
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.13	-0.46	0.65
Other industry	0.43	1.56	0.12
Building and civil engineering	-0.18	-0.68	0.50
Production related services, trade, and banking	0.08	0.35	0.73
Consumption related and social services, and the state	0.11	0.47	0.64
Occupational status (left out category: blue collar)			
Trainee	0.24	0.48	0.63
White collar	0.19	1.31	0.19
At home, part-time or missing	-0.09	-0.49	0.62
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.26	1.47	0.14
Saxony-Anhalt	-0.18	-0.91	0.36
Saxony Free State	-0.17	-0.76	0.45
Thuringia	-0.30	-1.37	0.17
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.19	-1.48	0.14
200 to 500	-0.15	-0.79	0.43
More than 500	-0.29	-1.36	0.18
Level of education (left out: dual system)			
Without formal education	-0.21	-0.98	0.33
Technical college/university	0.44	2.19	0.03
Regional type (left out: densely populated)			
Rural area	-0.15	-0.37	0.71
Medium populated area	-0.12	-0.32	0.75
Inner city area	-0.32	-0.77	0.44
Sex (left out: male)			
Women	-0.08	-0.65	0.52
Employment experience previous to unemployment			
Employment six months before t = 1	-0.13	-0.81	0.42
Employment nine months before t = 1	-0.38	-2.51	0.01
Employment 12 months before t = 1	0.52	3.67	0.00
Calendar time of unemployment entry			
Month	0.04	2.55	0.01
Months before programme			
Months	-0.22	-3.12	0.00

Medium-term (centre)

N = 2767	Sc. Pseudo R-squared = .059248
Positive obs. = 109	LR (zero slopes) = 154.363 [.000]
Mean of dep. var. = .039393	Schwarz B.I.C. = 489.162
Sum of squared residuals = 95.2518	Log likelihood = -382.167
Pseudo R-squared = .090462	
Fraction of Correct Predictions =	0.96

	Estimate	t-statistic	P-value
Constant	6.29	1.66	0.10
Age			
Years	-0.01	-3.10	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.46	-1.90	0.06
Other industry	-0.27	-1.06	0.29
Building and civil engineering	-0.36	-1.45	0.15
Production related services, trade, and banking	-0.04	-0.22	0.82
Consumption related and social services, and the state	-0.21	-1.19	0.24
Occupational status (left out category: blue collar)			
Trainee	0.17	1.44	0.15
White collar	0.17	0.73	0.47
At home, part-time or missing			
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.30	1.63	0.10
Saxony-Anhalt	0.12	0.68	0.50
Saxony Free State	0.08	0.42	0.68
Thuringia	-0.12	-0.61	0.54
Firm size (left out: 11 to 200 employees)			
Less than 11	0.11	0.89	0.37
200 to 500	-0.09	-0.54	0.59
More than 500	0.24	1.71	0.09
Level of education (left out: dual system)			
Without formal education	-0.60	-2.79	0.01
Technical college/university	0.24	1.26	0.21
Regional type (left out: densely populated)			
Rural area	0.32	0.79	0.43
Medium populated area	0.41	1.05	0.29
Inner city area	0.08	0.19	0.85
Sex (left out: male)			
Women	0.20	1.82	0.07
Employment experience previous to unemployment			
Employment 12 months before t = 1	-0.07	-0.42	0.68
Employment 15 months before t = 1	0.12	0.73	0.47
Employment 18 months before t = 1	-0.27	-2.04	0.04
Calendar time of unemployment entry			
Month	-0.01	-0.73	0.47
Months before programme			
Months	-0.55	-8.72	0.00

Long-term (right)

N = 3044	Sc. Pseudo R-squared = .049693
Positive obs. = 96	LR (zero slopes) = 141.855 [.000]
Mean of dep. var. = .031537	Schwarz B.I.C. = 467.667
Sum of squared residuals = 87.4202	Log likelihood = -355.374
Pseudo R-squared = .060545	
Fraction of Correct Predictions =	0.97

	Estimate	t-statistic	P-value
Constant	-1.80	-0.49	0.62
Age			
Years	-0.02	-3.82	0.00
Sector (left out category: metal and electronic)			
Agriculture and primary industry	-0.34	-1.69	0.09
Other industry	-0.32	-1.40	0.16
Building and civil engineering	-0.51	-2.49	0.01
Production related services, trade, and banking	-0.42	-2.51	0.01
Consumption related and social services, and the state	-0.70	-4.18	0.00
Occupational status (left out category: blue collar)			
Trainee	-0.38	-0.81	0.42
White collar	0.48	3.72	0.00
At home, part-time or missing	0.24	0.86	0.39
Region (left out: Berlin/Brandenburg)			
Mecklenburg / West Pommerania	0.23	1.16	0.25
Saxony-Anhalt	-0.01	-0.07	0.94
Saxony Free State	0.14	0.69	0.49
Thuringia	-0.04	-0.19	0.85
Firm size (left out: 11 to 200 employees)			
Less than 11	-0.18	-1.41	0.16
200 to 500	-0.06	-0.36	0.72
More than 500	0.07	0.47	0.64
Level of education (left out: dual system)			
Without formal education	-0.22	-1.17	0.24
Technical college/university	0.11	0.56	0.57
Regional type (left out: densely populated)			
Rural area	-0.27	-0.87	0.39
Medium populated area	-0.03	-0.12	0.90
Inner city area	-0.41	-1.21	0.23
Sex (left out: male)			
Women	-0.09	-0.85	0.40
Employment experience previous to unemployment			
Employment nine months before t = 1	0.28	1.66	0.10
Employment 12 months before t = 1	-0.46	-2.93	0.00
Employment 15 months before t = 1	0.52	3.52	0.00
Calendar time of unemployment entry			
Month	0.02	1.03	0.30
Months before programme			
Months	-0.42	-6.91	0.00

Table 3.A3 Differences in matched samples (selected co-variates) treatment in 1993 after short-term, medium-term and long-term unemployment

Participation in treatment 1993 after 1-3 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	34.03	33.52	-0.54
Months of unemployment before programme	2.91	2.90	-0.15
Agriculture and primary industry	0.09	0.09	0.10
Metal and electronic	0.21	0.20	-0.31
Other industry	0.06	0.06	0.13
Building and civil engineering	0.08	0.09	0.14
Production related services, trade, and banking	0.39	0.39	0.05
Consumption related, social services, state	0.17	0.17	-0.01
Trainee	0.06	0.06	0.14
Blue collar	0.37	0.38	0.06
White collar	0.50	0.49	-0.20
Working at home	0.07	0.08	0.12
part-time or missing	0.07	0.08	0.12
No vocational training	0.15	0.15	0.06
Dual System	0.77	0.78	0.17
A-level	0.03	0.02	-0.91
Technical college or university	0.05	0.05	0.01
German	0.89	0.89	0.00
Non-German	0.11	0.11	0.00
Gross salary of former employment (€)	54.97	51.48	-1.49
Sex	0.47	0.48	0.18

Participation in treatment 1993 after 1-3 months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	38.96	43.18	1.85
Months of unemployment before programme	2.92	2.89	-0.38
Agriculture and primary industry	0.16	0.15	-0.48
Metal and electronic	0.17	0.15	-0.64
Other industry	0.07	0.10	1.35
Building and civil engineering	0.05	0.06	0.43
Production related services, trade, and banking	0.27	0.26	-0.42
Consumption related, social services, state	0.28	0.28	0.22
Trainee	0.02	0.02	0.09
Blue collar	0.44	0.46	0.53
White collar	0.47	0.46	-0.33
Working at home	0.08	0.07	-0.41
part-time or missing	0.08	0.07	-0.41
No vocational training	0.04	0.04	0.13
Dual System	0.84	0.84	0.09
Technical college or university	0.12	0.11	-0.64
German	0.99	0.99	-0.34
Non-German	0.01	0.01	0.34
Gross salary of former employment (€)	40.11	40.48	0.25
Sex	0.50	0.48	-0.40

Participation in treatment 1993 after 4-6 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	35.09	34.95	-0.14
Months of unemployment before programme	5.93	5.61	-1.06
Agriculture and primary industry	0.13	0.12	-0.38
Metal and electronic	0.17	0.15	-0.44
Other industry	0.04	0.04	-0.27
Building and civil engineering	0.06	0.06	-0.23
Production related services, trade, and banking	0.40	0.43	0.57
Consumption related, social services, state	0.20	0.21	0.28
Trainee	0.02	0.02	0.26
Blue collar	0.48	0.47	-0.11
White collar	0.42	0.42	0.02
Working at home	0.09	0.09	0.02
part-time or missing	0.09	0.09	0.02
No vocational training	0.18	0.18	0.01
Dual System	0.65	0.66	0.22
Technical college or university	0.15	0.14	-0.20
German	0.86	0.87	0.18
Non-German	0.14	0.13	-0.18
Gross salary of former employment (€)	55.85	49.78	-1.93
Sex	0.39	0.40	0.09

Participation in treatment 1993 after 4-6 months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	38.57	37.75	-0.86
Months of unemployment before programme	6.05	6.01	-0.42
Agriculture and primary industry	0.13	0.13	0.07
Metal and electronic	0.20	0.19	-0.48
Other industry	0.09	0.08	-0.38
Building and civil engineering	0.04	0.05	0.37
Production related services, trade, and banking	0.25	0.25	-0.03
Consumption related, social services, state	0.29	0.31	0.45
Trainee	0.01	0.01	0.14
Blue collar	0.45	0.48	0.76
White collar	0.46	0.45	-0.22
Working at home	0.01	0.01	0.13
part-time or missing	0.09	0.06	-0.98
No vocational training	0.06	0.07	0.37
Dual System	0.85	0.83	-0.50
Technical college or university	0.09	0.08	-0.18
Gross salary of former employment (€)	39.35	36.86	-1.73
Sex	0.52	0.51	-0.26

Participation in treatment 1993 after 7-9 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	32.80	31.36	-1.20
Months of unemployment before programme	8.95	9.10	1.22
Agriculture and primary industry	0.10	0.09	-0.21
Metal and electronic	0.20	0.19	-0.16
Other industry	0.08	0.08	-0.07
Building and civil engineering	0.07	0.07	-0.03
Production related services, trade, and banking	0.37	0.39	0.30
Consumption related, social services, state	0.18	0.18	0.02
Trainee	0.03	0.03	-0.10
Blue collar	0.55	0.61	0.88
White collar	0.27	0.29	0.34
Working at home	0.02	0.02	0.07
part-time or missing	0.15	0.07	-1.64
No vocational training	0.18	0.18	-0.08
Dual System	0.75	0.77	0.30
Technical college or university	0.03	0.03	-0.15
German	0.90	0.86	-1.03
Non-German	0.03	0.03	-0.19
Gross salary of former employment (€)	50.67	46.19	-1.42
Sex	0.32	0.32	0.12

Participation in treatment 1993 after 7-9 months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	38.90	36.91	-1.83
Months of unemployment before programme	8.90	9.11	2.04
Agriculture and primary industry	0.09	0.05	-1.33
Metal and electronic	0.13	0.11	-0.48
Other industry	0.10	0.08	-0.42
Building and civil engineering	0.07	0.07	-0.29
Production related services, trade, and banking	0.28	0.32	0.93
Consumption related, social services, state	0.34	0.37	0.63
Trainee	0.00	0.05	6.43
Blue collar	0.34	0.33	-0.24
White collar	0.54	0.53	-0.18
Working at home	0.01	0.01	-0.23
part-time or missing	0.12	0.09	-0.74
No vocational training	0.10	0.10	-0.04
Dual System	0.72	0.75	0.50
Technical college or university	0.16	0.14	-0.60
Gross salary of former employment (€)	35.53	33.76	-0.85
Sex	0.71	0.72	0.14

Table 3.A3 Differences in matched samples (selected co-variates) treatment in 1994 after short-term, medium-term and long-term unemployment (cont.)

Participation in treatment 1994 after 1-3 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	31.67	31.44	-0.17
Months of unemployment before programme	3.12	3.15	0.27
Agriculture and primary industry	0.07	0.07	0.04
Metal and electronic	0.12	0.12	-0.08
Other industry	0.09	0.09	0.08
Building and civil engineering	0.12	0.12	-0.03
Production related services, trade, and banking	0.44	0.43	-0.17
Consumption related, social services, state	0.16	0.17	0.24
Trainee	0.04	0.03	-0.02
Blue collar	0.49	0.49	0.04
White collar	0.40	0.40	-0.05
Working at home	0.07	0.07	0.04
part-time or missing	0.07	0.07	0.04
No vocational training	0.18	0.18	0.03
Dual System	0.75	0.74	-0.32
Technical college or university	0.07	0.07	0.09
German	0.90	0.90	0.17
Non-German	0.11	0.10	-0.17
Gross salary of former employment (€)	53.60	53.83	0.07
Sex	0.26	0.27	0.17

Participation in treatment 1994 after 1-3 months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	37.87	37.47	-0.32
Months of unemployment before programme	3.10	3.08	-0.17
Agriculture and primary industry	0.09	0.07	-0.47
Metal and electronic	0.07	0.08	0.22
Other industry	0.11	0.12	0.09
Building and civil engineering	0.10	0.11	0.25
Production related services, trade, and banking	0.27	0.26	-0.10
Consumption related, social services, state	0.37	0.37	0.03
Trainee	0.01	0.01	-0.06
Blue collar	0.56	0.58	0.22
White collar	0.31	0.30	-0.25
Working at home	0.11	0.12	0.06
part-time or missing	0.11	0.12	0.06
No vocational training	0.06	0.07	0.31
Dual System	0.78	0.80	0.51
Technical college or university	0.14	0.12	-0.42
Gross salary of former employment (€)	41.52	40.05	-0.89
Sex	0.42	0.40	-0.37

Participation in treatment 1994 after 4-6 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	35.78	35.66	-0.13
Months of unemployment before programme	6.08	6.12	0.36
Agriculture and primary industry	0.06	0.05	-0.38
Metal and electronic	0.23	0.23	-0.15
Other industry	0.07	0.08	0.05
Building and civil engineering	0.06	0.06	-0.10
Production related services, trade, and banking	0.38	0.40	0.49
Consumption related, social services, state	0.19	0.18	-0.19
Trainee	0.02	0.02	0.19
Blue collar	0.41	0.41	-0.05
White collar	0.52	0.52	0.02
Working at home	0.05	0.05	-0.06
part-time or missing	0.05	0.05	-0.06
No vocational training	0.10	0.09	-0.10
Dual System	0.79	0.79	-0.05
Technical college or university	0.12	0.11	-0.22
German	0.92	0.91	-0.21
Non-German	0.08	0.09	0.21
Gross salary of former employment (€)	65.02	57.08	-2.83
Sex	0.40	0.40	0.02

Participation in treatment 1994 after 4-6months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	36.79	36.32	-0.45
Months of unemployment before programme	6.00	6.04	0.39
Agriculture and primary industry	0.12	0.11	-0.25
Metal and electronic	0.19	0.18	-0.13
Other industry	0.07	0.07	-0.03
Building and civil engineering	0.10	0.11	0.24
Production related services, trade, and banking	0.29	0.30	0.15
Consumption related, social services, state	0.23	0.23	-0.03
Trainee	0.01	0.01	-0.17
Blue collar	0.47	0.49	0.40
White collar	0.42	0.41	-0.07
Working at home	0.03	0.03	-0.32
part-time or missing	0.10	0.09	-0.49
No vocational training	0.07	0.07	-0.17
Dual System	0.83	0.83	-0.07
Technical college or university	0.09	0.09	-0.27
Gross salary of former employment (€)	39.07	39.54	0.25
Sex	0.46	0.45	-0.18

Participation in treatment 1994 after 7-9 months of unemployment, West Germany

	Sample mean participants	Mean matched controls	t-value
Age	33.21	31.98	-1.22
Months of unemployment before programme	9.15	9.33	1.84
Agriculture and primary industry	0.10	0.10	-0.19
Metal and electronic	0.22	0.21	-0.18
Other industry	0.13	0.12	-0.14
Building and civil engineering	0.08	0.08	0.17
Production related services, trade, and banking	0.39	0.41	0.49
Consumption related, social services, state	0.08	0.08	-0.07
Trainee	0.05	0.05	0.03
Blue collar	0.47	0.51	0.76
White collar	0.38	0.39	0.24
Working at home	0.01	0.01	-0.06
part-time or missing	0.11	0.06	-1.63
No vocational training	0.13	0.12	-0.16
Dual System	0.73	0.75	0.40
Technical college or university	0.13	0.12	-0.26
German	0.90	0.91	0.46
Non-German	0.01	0.01	-0.10
Gross salary of former employment (€)	54.36	52.23	-0.72
Sex	0.38	0.39	0.22

Participation in treatment 1994 after 7-9 months of unemployment, East Germany

	Sample mean participants	Mean matched controls	t-value
Age	37.67	37.43	-0.24
Months of unemployment before programme	9.05	8.98	-0.59
Agriculture and primary industry	0.06	0.06	0.23
Metal and electronic	0.12	0.10	-0.70
Other industry	0.06	0.05	-0.47
Building and civil engineering	0.05	0.06	0.90
Production related services, trade, and banking	0.33	0.34	0.11
Consumption related, social services, state	0.39	0.40	0.07
Trainee	0.00	0.02	14.16
Blue collar	0.40	0.44	0.80
White collar	0.45	0.43	-0.40
Working at home	0.05	0.04	-0.16
part-time or missing	0.15	0.11	-1.00
No vocational training	0.04	0.05	0.62
Dual System	0.85	0.84	-0.36
Technical college or university	0.10	0.09	-0.48
Gross salary of former employment (€)	37.11	36.23	-0.51
Sex	0.66	0.62	-0.79

Table 3.A4 Differences in matched samples with respect to outcome variable before treatment (PPT)

Participation in west Germany 1993 after ...

	short-term unemployment			medium-term unemployment			long-term unemployment		
	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value
t=-18	0.03	1.44	0.15	0.00	-0.04	0.97	-0.06	-1.18	0.24
t=-17	0.01	0.40	0.69	0.03	1.01	0.31	-0.04	-0.95	0.34
t=-16	0.02	0.92	0.36	0.00	-0.09	0.93	-0.09	-1.69	0.09
t=-15	0.05	1.92	0.06	-0.03	-0.91	0.36	-0.04	-0.91	0.36
t=-14	0.05	2.13	0.03	-0.05	-1.38	0.17	0.00	0.05	0.96
t=-13	0.02	1.03	0.30	-0.04	-1.11	0.27	0.00	-0.10	0.92
t=-12	-0.02	-0.74	0.46	-0.04	-1.18	0.24	0.02	0.87	0.39
t=-11	-0.04	-1.26	0.21	-0.06	-1.90	0.06	0.02	15.08	0.00
t=-10	0.00	0.13	0.90	-0.02	-0.70	0.48			
t=-9	0.00	-0.13	0.90	0.01	0.55	0.58			
t=-8	-0.02	-0.77	0.44	0.00	-0.26	0.80			
t=-7	-0.01	-0.52	0.60						
t=-6	-0.03	-1.26	0.21						
t=-5	-0.03	-1.41	0.16						

Participation in east Germany 1993 after ...

	short-term unemployment			medium-term unemployment			long-term unemployment		
	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value
t=-18	0.01	0.33	0.74	0.05	1.37	0.17	0.01	0.13	0.89
t=-17	0.01	0.13	0.89	0.03	0.89	0.37	-0.03	-0.52	0.60
t=-16	0.00	0.09	0.93	0.02	0.63	0.53	0.01	0.30	0.76
t=-15	0.04	1.34	0.18	0.01	0.33	0.74	-0.03	-0.74	0.46
t=-14	0.03	1.14	0.25	0.02	0.58	0.56	0.03	1.09	0.28
t=-13	0.03	1.12	0.26	0.02	0.71	0.48	0.04	2.39	0.02
t=-12	-0.01	-0.33	0.74	0.02	0.72	0.47	0.02	1.66	0.10
t=-11	0.00	0.01	0.99	0.03	1.71	0.09	0.00	0.22	0.82
t=-10	0.00	-0.11	0.91	0.03	1.74	0.08			
t=-9	0.00	0.19	0.85	0.02	1.78	0.08			
t=-8	-0.01	-0.41	0.69	0.02	1.78	0.00			
t=-7	-0.01	-0.52	0.60						
t=-6	0.00	0.14	0.89						
t=-5	-0.02	-1.52	0.13						

Participation in west Germany 1994 after ...

	short-term unemployment			medium-term unemployment			long-term unemployment		
	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value
t=-18	-0.05	-0.76	0.45	-0.02	-0.47	0.64	-0.10	-1.51	0.12
t=-17	-0.09	-1.41	0.16	0.01	0.36	0.72	-0.06	-1.28	0.20
t=-16	-0.07	-1.10	0.27	0.06	1.45	0.15	0.01	0.20	0.84
t=-15	-0.02	-0.28	0.78	-0.02	-0.46	0.64	0.00	-0.01	0.99
t=-14	0.02	0.34	0.74	-0.05	-1.07	0.29	-0.01	-0.29	0.77
t=-13	0.00	-0.07	0.94	-0.03	-0.60	0.55	-0.03	-0.67	0.50
t=-12	-0.03	-0.39	0.70	0.00	-0.04	0.97	-0.04	-1.10	0.27
t=-11	0.07	1.19	0.23	0.00	0.11	0.91	-0.04	-1.10	0.27
t=-10	0.05	0.91	0.36	-0.03	-0.68	0.50			
t=-9	-0.04	-0.56	0.58	-0.01	-0.40	0.69			
t=-8	-0.07	-1.09	0.28	0.01	0.24	0.81			
t=-7	-0.06	-1.04	0.30						
t=-6	-0.04	-0.91	0.36						
t=-5	0.01	0.24	0.81						

Participation in east Germany 1994 after ...

	short-term unemployment			medium-term unemployment			long-term unemployment		
	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value	Δ Y	t-statistic	P-value
t=-18	-0.01	-0.14	0.89	0.03	0.67	0.50	0.02	0.56	0.58
t=-17	-0.05	-0.87	0.38	0.02	0.64	0.52	0.02	0.50	0.62
t=-16	-0.06	-0.98	0.33	0.04	1.10	0.27	-0.01	-0.24	0.81
t=-15	0.05	1.05	0.29	0.01	0.36	0.72	0.00	-0.02	0.99
t=-14	-0.01	-0.20	0.84	-0.01	-0.19	0.85	-0.03	-0.71	0.48
t=-13	-0.03	-0.59	0.55	-0.03	-0.75	0.45	0.03	1.18	0.24
t=-12	-0.04	-0.79	0.43	0.00	-0.04	0.97	0.00	0.16	0.87
t=-11	-0.07	-1.31	0.19	0.01	0.38	0.71	0.02	1.51	0.13
t=-10	-0.11	-1.91	0.06	0.02	0.71	0.48			
t=-9	-0.05	-0.89	0.37	0.04	2.51	0.01			
t=-8	-0.04	-0.82	0.41	0.01	0.86	0.39			
t=-7	-0.06	-1.13	0.26						
t=-6	-0.06	-1.27	0.21						
t=-5	-0.05	-1.22	0.22						

Figure 3.A1 Predicted propensity scores, treatment after short-term(i), medium-term (ii) and long-term (iii) unemployment in West Germany 1993 (left: N, right: density estimates)

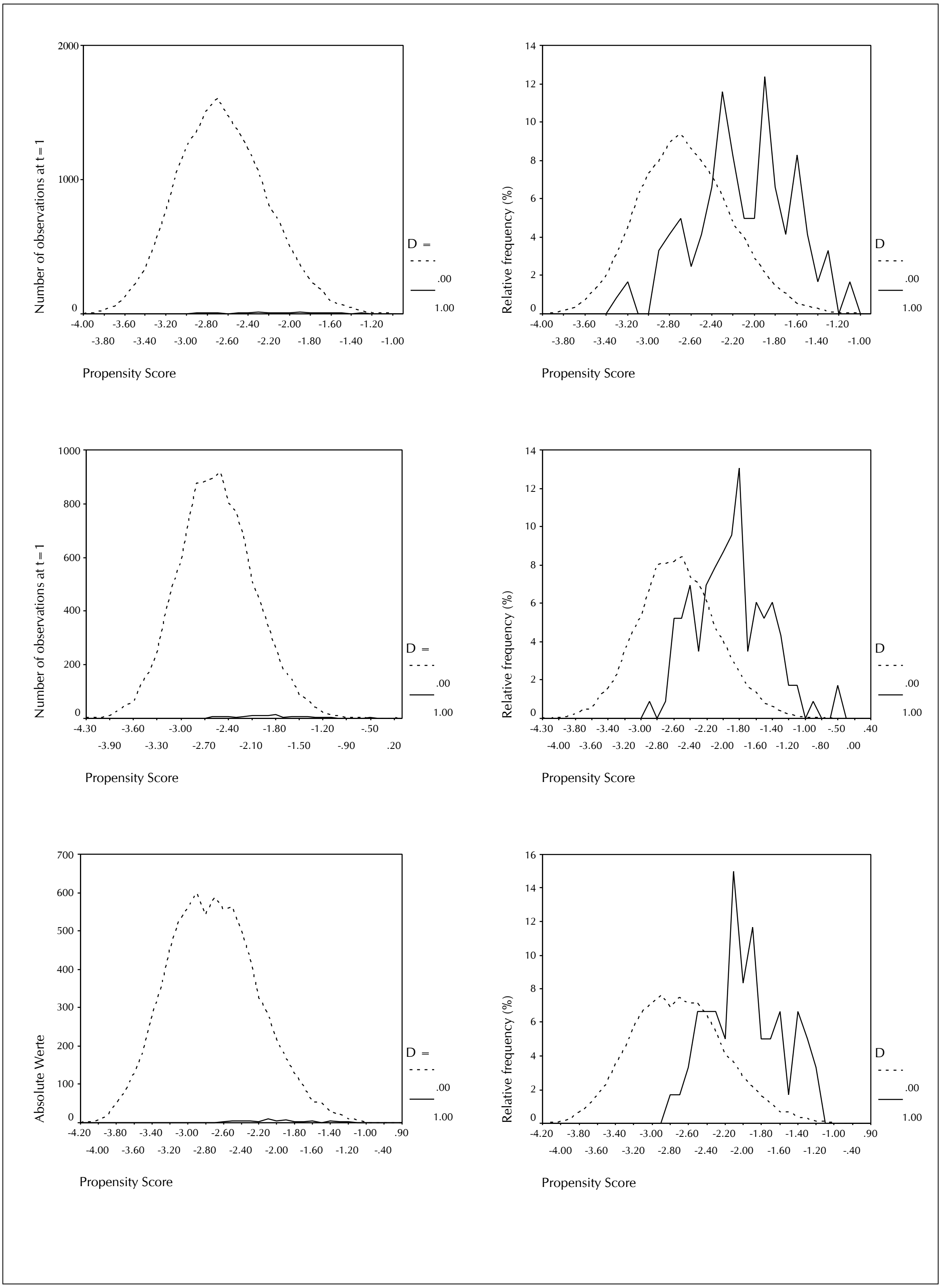


Figure 3.A2 Predicted propensity scores, treatment after short-term(i), medium-term (ii) and long-term (iii) unemployment in West Germany 1994 (left: N, right: density estimates)

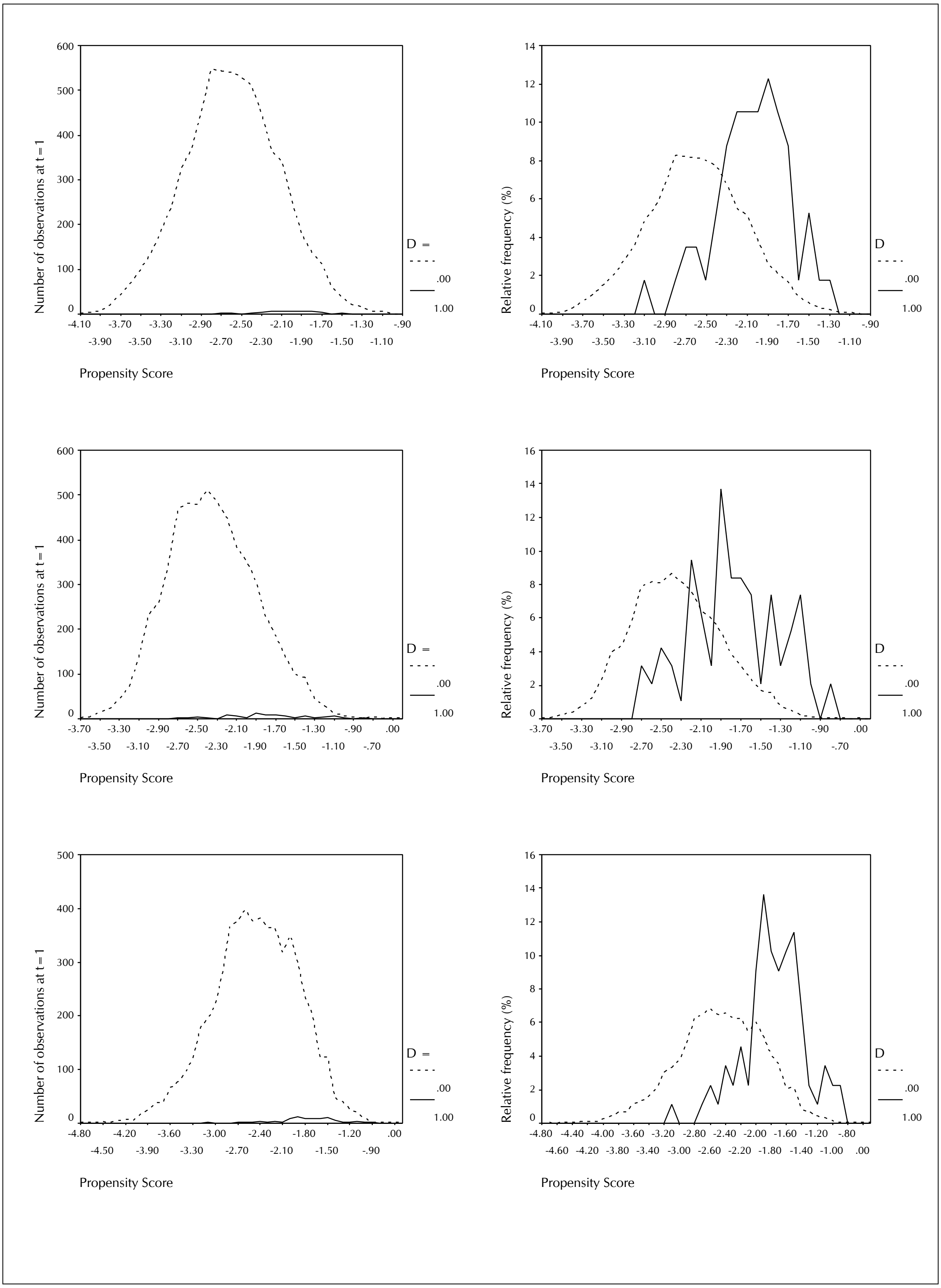


Figure 3.A3 Predicted propensity scores, treatment after short-term(i), medium-term (ii) and long-term (iii) unemployment in East Germany 1993 (left: N, right: density estimates)

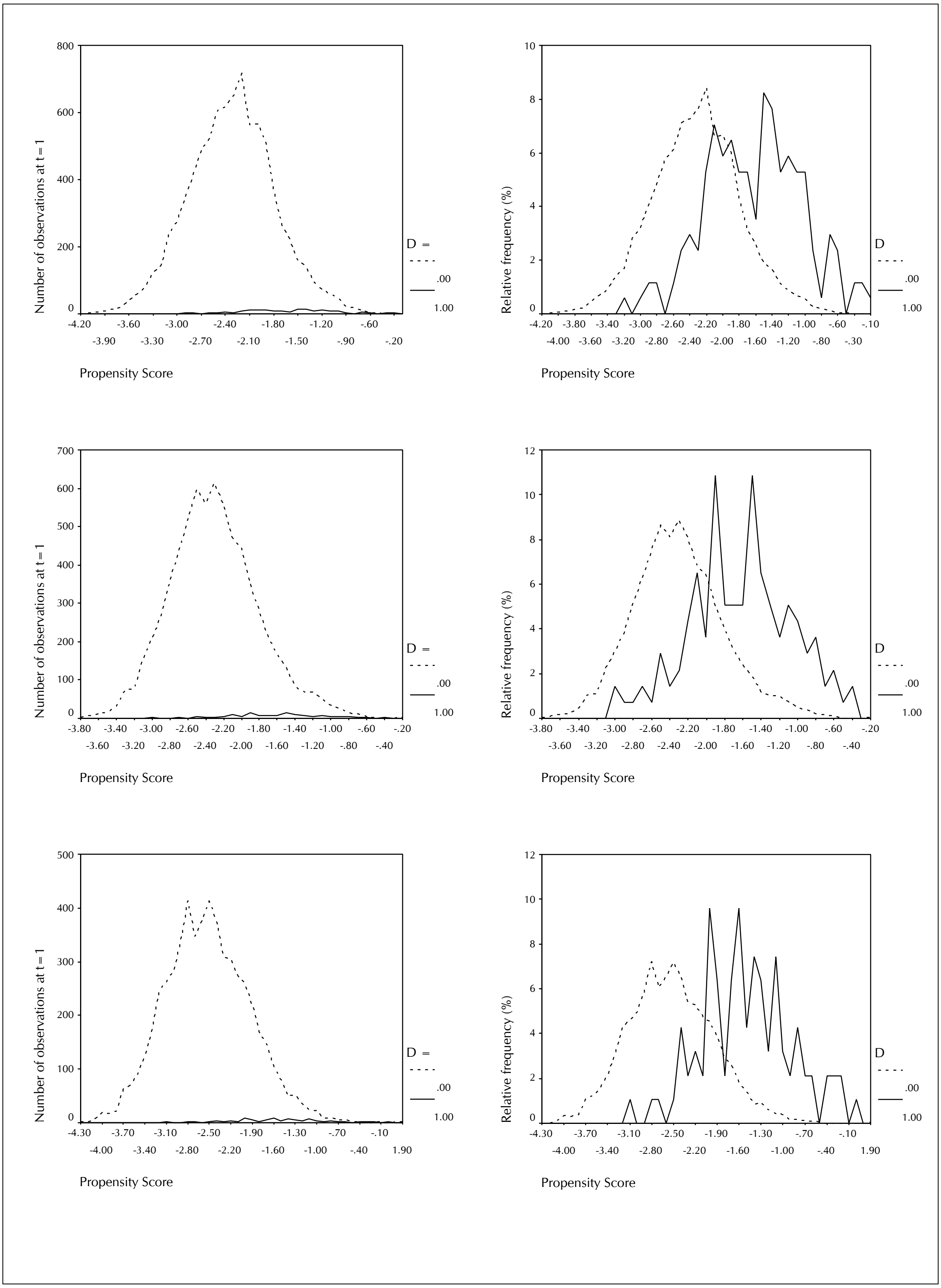
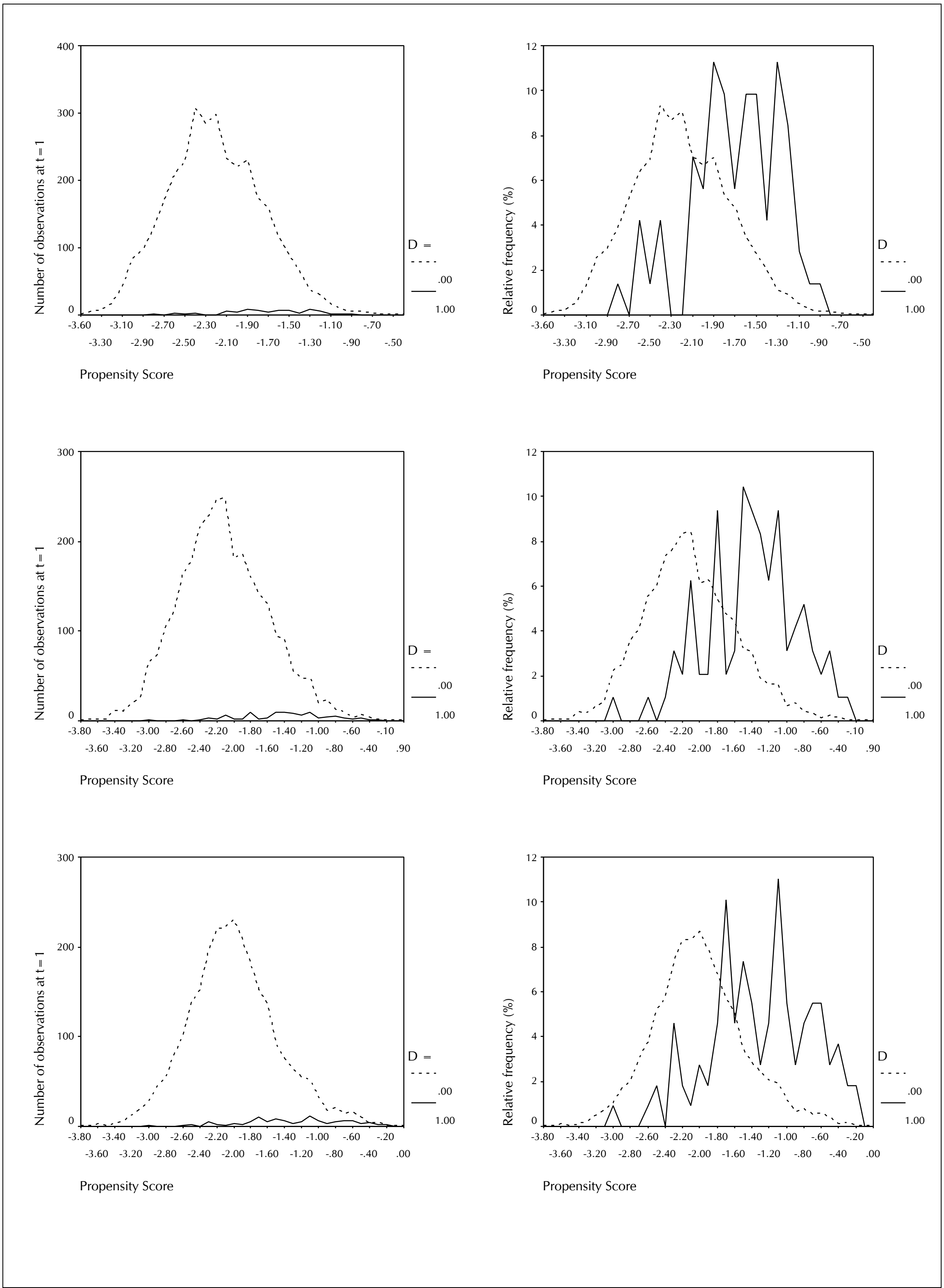


Figure 3.A4 Predicted propensity scores, treatment after short-term(i), medium-term (ii) and long-term (iii) unemployment in East Germany 1994 (left: N, right: density estimates)



3.7.2 Coding plan for the treatment information

Preparation, social skills and short–term training

(a) If the income maintenance information shows valid codes, "preparation" corresponds to a consolidated type of measure FMASART* if the following programme codes in the training data (FUU) appear:

Programme code	Label	Label in German
10	Training enterprise	Übungsfirma
11	Training studio	Übungswerkstatt
13	Short term training	§41a
20	Assess–, and preparation courses	Feststell–, Vorschalt– und Vorbereitmaßnahme

(b) If the values of an income maintenance payment according to § 41a AFG appear in the income maintenance variables, a participation in "Preparation, social skills and short–term training" is identified if the participation data (FUU) indicate either "missing" or "12 sonst. Anpassung der berufl. Kenntnisse" (other adjustment of working skills) in the same record:

Benefit code	Label	Label in German
UHG41A	Full income maintenance necessary short–term training	Unterhaltsgeld, notwendige § 41a, volles Unterhaltsgeld
EGGUM	Short–term training for resettlers or German ethics	EGG bei § 41a Maßnahme v Aus–/Übersiedlern
UHG1M	Income maintenance ending former unemployment for short–term training in § 41a	Unterhaltsgeld bei notwendiger § 41a wegen vorheriger Arbeitslosigkeit
UHGMHG	Income maintenance amounting to unemployment benefits for necessary short–term training in § 41a	Unterhaltsgeld bei notwendiger § 41a in Höhe des ALG
UHGM328	Full income maintenance because of unemployment or in danger of loosing the job for necessary short–term training in § 41a	Volles Unterhaltsgeld bei notwendiger § 41a aufgrund von Arbeitslosigkeit oder Bedrohung von Arbeitslosigkeit
UHGMAH	Income maintenance amounting to un–employment assistance for necessary short–term training in § 41a	Unterhaltsgeld § 41a in Höhe der Arbeitslosenhilfe

(c) If the income maintenance variables are either missing or have any reasonable value corresponding to employment in the IAB-SLED–data, then the person is never considered as participating in the "Preparation, social skills and short–term training" treatment because we do not suppose individuals participation in preparation courses while working. Especially a training information corresponding to "12 sonstige Anpassungen der berufl. Kenntnisse" (other adjustment of working skills) is then not seen as such a treatment, but as a treatment in type b.

Provision of specific professional skills and techniques

(a) If the income maintenance variables show valid codes, a treatment is considered to be a "provision of specific professional skills" if the information of the type of programme FMASART* given in the FuU–data shows the coding.

Programme code	Label	Label in German
34	Basic training	Grundausbildungslehrgang (before 1986)
18	Other training institution	sonst. Übungs– und Trainingseinrichtung
21	Qualification below skilled worker level	Qualifikation unterhalb des Facharbeiterniveaus
24	Practical further education	berufspraktische Fortbildung
31	Further education of trainers and multidisciplinary qualification	Heran–/Fortbildung v. Ausbildungskräften/ berufsfeldübergreifende Qualifikation

(b) In many cases, the incomes maintenance payment indicate that individuals receive unemployment benefits. However, FuU–data may suggest that training occurred at the same time by indicating "other adjustment of working skills" ("sonst. Anpassung der berufl. Kenntnisse") because programmes can also be taken during unemployment. In this case, we assume that persons participate in courses which provide only specific professional skills. So the exact condition of this treatment is a coding of FMASART* to this type of treatment and a parallel transfer payment as documented below:

Programme code	Label	Label in German
12	Other adjustment of working skills	sonst. Anpassung der berufl. Kenntnisse

If this information corresponds to one of the following transfer payments the type of treatment is identified as „provision of specific professional skills".

Benefit code	Label	Label in German
ALGEH	Unemployment benefits for former development aid volunteers	Arbeitslosengeld für ehemalige Entwicklungshelfer
ALG101	Regular unemployment benefits	Arbeitslosengeld Code 101
ALGHKALG	Regular unemployment benefits and unemployment benefits for home comers	Arbeitslosengeld und Arbeitslosengeld für Heimkehrer
ALBSZ	Unemployment assistance for temporary soldiers	Arbeitslosenhilfe für Soldaten auf Zeit
HKALG	Unemployment benefits for home comers	Arbeitslosengeld für Heimkehrer
ALGHU	Unemployment benefits for political prisoners subject to §249g	Arbeitslosengeld für polit. Häftlinge gem. §249g
ALB7	Unemployment assistance for former development aid volunteers	Arbeitslosenhilfe für ehem. Entwicklungshelfer
EGGA	Benefits in case of language education	Eingliederungsgeld für Aus–/Übersiedler bei Arbeitslosigkeit
ALUEG	Benefits to bridge the time to retirement pension subject	Altersübergangsgeld
EGHI	Assistance in case of language education	Eingliederungshilfe bei Arbeitslosigkeit oder Sprachkurs für Spätaussiedler
DLUEG	Benefits/transfers to bridge the time to retirement pension paid by BA	Altersübergangsgeld– Ausgleichsbetrag von BA
DLUEGB	Benefits/transfers to bridge the time to retirement pension paid by Federal Ministry	Altersübergangsgeld– Ausgleichsbetrag Bund
ALUEGV	Benefits to bridge the time to retirement pension for former recipients of early retirement payments	Altersübergangsgeld für ehem. Bezieher von Vorruhestandsgeld
ALUEGS	Benefits to bridge the time to retirement pension for independent workers	Altersübergangsgeld für Selbstständige
ALUEGH	Benefits to bridge the time to retirement pension for former prisoners and hindered persons	Altersübergangsgeld für ehem. Häftlinge u. verhinderte Arbeitnehmer
ALUEGF	Benefits to bridge the time to retirement pension for former recipients of early retirement payments as of the 833rd day	Altersübergangsgeld für ehem. Bezieher von Vorruhestandsgeld ab dem 833 Tag
ALUEGB	Benefits to bridge the time to retirement pension as of the 833rd day	Altersübergangsgeld ab dem 833. Tag
ALG118	Regular unemployment benefits code 118	Arbeitslosengeld Code 118
ALG119	Regular unemployment benefits code 119	Arbeitslosengeld Code 119
ATGALG	Regular unemployment benefits	Arbeitslosengeld (andere)
ATGAUF	Regular unemployment benefits	Arbeitslosengeld (andere)
ALHIA	Unemployment assistance which follows unemployment benefits	Anschlussarbeitslosenhilfe an Arbeitslosengeld
ALHIB	Original unemployment assistance, no claim for unemployment benefits	Originäre Arbeitslosenhilfe (kein Anspruch auf Arbeitslosengeld)
ALHIEH	Unemployment assistance for former development aid volunteers	Arbeitslosenhilfe für ehem. Entwicklungshelfer
ALB8	Unemployment assistance which follows unemployment benefits for former development aid volunteers	Anschlussarbeitslosenhilfe an Arbeitslosenhilfe nur für Entwicklungshelfer
RV	Advanced pension payment	Rentenvorschuss
ALHISZ	Unemployment assistance for temporary soldiers	Arbeitslosenhilfe für Soldaten auf Zeit
ALHIHU	Unemployment assistance for political prisoners subject to §249g	Arbeitslosenhilfe für ehem. Häftlinge u. verhinderte Arbeitnehmer nach 249g AFG

(c) If the FuU–data shows a missing value or "12 sonstige Anpassung der berufl. Kenntnisse" (other adjustment of working skills) and the income maintenance variables indicate the following values, treatment were identified to be of the specific professional skills–type:

Benefit code	Label	Label in German
EGGUF	Benefits in case of further education for resettlers or German Ethics	Eingliederungsgeld bei notwendiger Fortbildung von Aus–/Übersiedlern
UHGTF	Income maintenance in case of part time further education 44 IIb	Unterhaltsgeld bei Teilzeitfortbildung 44 II b
UHGFAG	Income maintenance for further education, unemployment and conditions for income maintenance not met, income maintenance amounting to unemployment benefits is paid	Unterhaltsgeld bei Fortbildung, Arbeitslosigkeit, Zeiten für Unterhaltsgeld nicht erfüllt, Unterhaltsgeld in Höhe der Arbeitslosenhilfe
UHGF	Income maintenance for necessary further education for unemployed persons or persons whose jobs are in danger	Unterhaltsgeld bei notwendiger Fortbildung (arbeitslos oder bedroht)
UHGEH335	Income maintenance for development aid volunteers in further education measures code 335	Unterhaltsgeld für Entwicklungshelfer notwendiger Fortbildung (arbeitslos oder bedroht)
UHGF4	Complete income maintenance for further education due to unemployment	volles Unterhaltsgeld bei notw. Fortbildung wegen Arbeitslosigkeit
UHGTF4	Income maintenance because of necessary part time further education due to danger of loosing the job as of 1.1.94	Unterhaltsgeld bei notw. TZ–Fortbild wegen Bedrohung von Arbeitslosigkeit oder Berufsabschluss ab 1.1.94
UHGEH4	Income maintenance for unemployed development aid volunteers as of 94	Unterhaltsgeld für Entwicklungshelfer notwendiger Fortbildung (arbeitslos oder bedroht) ab 1994
UHGFA4	Income maintenance amounting to unemployment assistance because of necessary further education due to unemployment or danger of loosing the job as of 1.1.94	Unterhaltsgeld in Höhe der Arbeitslosenhilfe bei notw. Fortbildung wegen Arbeitslosigkeit ab 1994

Qualification for the first labour market via the education system

(a) If the income maintenance variables show valid codes (no missing) in case of the following programmes from the FuU–data, the type of treatment is recoded to a "Qualification via the educational system/retraining".

Programme code	Label	Label in German
29	Certification	berufl. Abschlussprüfung
32	Retraining	Umschulung

(b) In case of a missing of the benefit information indicating that participants are employed while preparing for a vocational exam or attending a retraining, the treatment is also coded to a qualification for the first labor market via the education system if the FuU–data shows the following codes:

Programme code	Label	Label in German
29	Certification	berufl. Abschlussprüfung
32	Retraining	Umschulung

(c) If the FuU–data shows a missing value or a treatment "other type of treatment" (12 sonstige Anpassung der berufl. Kenntnisse), but the benefit variables indicate one of the following codes referring to the receipt of subsistence during a retraining course, the treatment is considered to be a qualification for the first labor market via the education system:

Benefit code	Label	Label in German
UHGTU	Income maintenance for part time jobs and retraining	Unterhaltsgeld bei Teilzeit und Umschulung
EGGUU	Benefits in case of necessary further education for resettlers or German Ethics	Eingliederungsgeld bei notwendiger Umschulung von Aus–/Übersiedlern
UHGU	Income maintenance in case of retraining of unemployed persons or persons whose jobs are in danger	Unterhaltsgeld bei notwendiger Umschulung wegen Arbeitslosigkeit oder Bedrohung
UHGUAG	Income maintenance amounting to unemployment benefits because of retraining of former unemployed persons	Unterhaltsgeld in Höhe des Arbeitslosengeldes bei Umschulung und vorheriger Arbeitslosigkeit
UHGUAH	Income maintenance amounting to unemployment assistance because of retraining of former unemployed persons	Unterhaltsgeld in Höhe der Arbeitslosenhilfe bei Umschulung und vorheriger Arbeitslosigkeit
UGHU4	Income maintenance for necessary retaining of persons whose jobs are in danger or vocational exam as of 1.1.94	Unterhaltsgeld bei notwendiger Umschulung wegen Bedrohung von Arbeitslosigkeit oder Berufsabschluss ab 1994
UHGTU4	Income maintenance for necessary part–time retaining of persons whose jobs are in danger or vocational exam as of 1.1.94	Teilzeit–Unterhaltsgeld bei notwendiger Umschulung wegen Bedrohung von Arbeitslosigkeit oder Berufsabschluss ab 1995
UHGUA4	Income maintenance amounting to unemployment assistance in case of retraining due to unemployment as of 1.1.94	Unterhaltsgeld in Höhe der Arbeitslosenhilfe bei notwendiger Umschlung aus Arbeitslosigkeit, ab 1994

Training for precise job offers

(a) Given that the variable BTYP indicates that individuals are in employment and that the parallel benefit variable has no valid code, we expect these individuals to prepare themselves for a precise jobs in a firm if the type of treatment in the FuU–data shows the following codes:

Programme code	Label	Label in German
10	Training enterprise	Übungsfirma
11	Training studio	Übungswerkstatt
12	Other adjustment of working skills	sonst. Anpassung der berufl. Kenntnisse
31	Further education of trainers and multidisciplinary qualification	Heran–/Fortb. v. Ausbild.kräften/berufsfeldübergr. Qualif.
18	Other training centre	sonst. Übungs– und Trainingseinrichtung
21	Qualification below skilled worker level	Qualif. unterhalb Facharbeiterniveau
24	Practical further education	berufspraktische Fortbildung

Direct Integration in the first labour market

(a) Familiarisation into regular employment can be supported by a wage subsidy ("direct integration"), so that we onlyobserve regular employment and no income maintenance payments in the data. Treatment is then identified by the FuU–data. Therefore we identify "direct integration" only from the aggregated FMASRT–variables if they are coded by:

Programme code	Label	Label in German
33	Integration	Einarbeitung

Career advancement training

(a) "Career advancement training" is often implemented simultaneously to a regular employment. Hence the treatment variables FMASART* in the FuU–data should contain one of the following:

Programme code	Label	Label in German
14	Foreman	Industriemeister (< 97)
15	Master craftsman	Handwerksmeister (< 97)
16	Other master	sonstiger Meister (< 97)
26	Technician	Techniker (< 97)
27	Master of business administration	Betriebswirt (< 97)
28	Other promotion	sonstiger Aufstieg (< 97)
17	Qualification for promotion	Aufstiegsfortbildung (nur 97)

(b) If the benefit information exhibits the following values which refer to income maintenance during a career advancement training and if the FuU–data show a missing or "other adjustment of working skills" (12 sonstige Anpassung der beruflichen Kenntnisse), then we identify a career advancement if the benefit information shows one of the following values (including a retraining which implemented as a career advancement training financed by a loan):

Benefit code	Label	Label in German
UHGDF	Income maintenance paid as loan for advisable further education	Unterhaltsgeld als Darlehen bei zweckmäßiger Fortbildung
UHGDU	Income maintenance paid as loan for advisable retraining	Unterhaltsgeld als Darlehen bei zweckmäßiger Umschulung
UHGDEH	Income maintenance paid as loan for advisable further education of development aid volunteer	Unterhaltsgeld als Darlehen bei zweckmäßiger Fortbidung v. Entwicklungshelfern

Language training

(a) If the benefit information shows any valid code (no missing) and the treatment information from the FuU–data provides information that these individuals pass through a language training, then treatment is identified as a language training:

Programme code	Label	Label in German
35	Language training	Deutschlehrgang

(b) If the benefit information is missing because individuals are regularly employed while taking part in the training, the treatment is identified to be a language training if the FuU–data provide the following treatment information:

Programme code	Label	Label in German
35	Language training	Deutschlehrgang

(c) If the FuU–data do not provide a valid code for treatment or indicate that individuals participated in "other adjustment of working skills" (12 sonst. Anpassung der berufl. Kenntnisse), but the benefit information indicates clearly that benefit was paid for language training as indicated by the codes displayed below, the treatment is identified as a language training.

Benefit code	Label	Label in German
EGHI	Assistance in case of unemployment or language course for resettlers or German Ethics	Eingliederungshilfe bei Arbeitslosigkeit oder Sprachkurs für Spätaussiedler
UHGVAK	Income maintenance in case of language courses for asylum seekers and refugees	Unterhaltsgeld bei Sprachlehrgang für Asylberechtigte und Kontingentflüchtlinge
UHGVA	Income maintenance in case of language courses for German Ethic or recipients of welcome benefits	Unterhaltsgeld für Aussiedler u Begrüßungsgabempfänger bei Sprachlehrgang
EGHIS	Other benefit for resettlers	andere Eingliederungsgeld
EGGSA	Benefits in case of full–time language courses for resettlers or German Ethics	Eingliederungsgeld bei Vollzeit–Sprachlehrgängen für Aus–/ Übersiedler
EGGSTA	Benefits in case of part–time language courses for resettlers or German Ethics	Eingliederungsgeld bei Teilzeit–Sprachlehrg für Aus–/Übersiedler
EGGSAK	Benefits in case of full time language courses for asylum seekers and refugees	Eingliederungsgeld bei Vollzeitsprachlehrg. für Kontingentflüchtlinge oder Asylbewerber
EGGSTK	Benefits in case of full time language courses for asylum seekers and refugees	Eingliederungsgeld bei Teilzeitsprachlehrg. für Kontingentflüchtlinge oder Asylbewerber

4 The aggregate impact of active labour market policy in Germany and the UK: Evidence from administrative data

4.1 Introduction[1]

Active Labour Market Policy (ALMP) plays a leading role in attempts to improve the situation for groups on the labour market, which are particular in need of counselling, information and training in both Germany and the United Kingdom. However, the policy design and the level of expenditure for ALMP vary widely between the two countries: While Germany spends on average 1.2% of its GDP for ALMP, the UK only spends 0.5% (data referring to 99, OECD 2001). How do the outcomes of these policies compare?

At the individual level, a programme can be considered as economically successful if the labour market situation of the participating individual improves significantly due to the participation in the programme and if the costs of the programme are lower than the benefits for the participants. Aggregate evaluation studies can be seen as a necessary complementary to interpret positive or negative microeconomic effects because they allow to estimate an employment effect of ALMP beyond the ceteris paribus effect on the treated and can include equilibrium price effects, behavioural changes, and other repercussions on the non–treated labour force or on the entire economy. These indirect effects of ALMP might consist of displacement effects (treated workers gain their jobs at the expense of non–treated workers), deadweight effects (subsidising a treatment, which would have occurred anyway), substitution effects (replacement of jobs for other types of non–treated workers because of relative wage changes), and tax effects (the effects of financing ALMP, see Calmfors 1994)

Microeconomic studies found out that the British ALMP –the New Deal programmes– substantially increased the employment outcome for participants (Dorsett 2001). Contrary, most microeconomic evaluation studies found that the German ALMP does not affect the employment prospects of the participants. This contribution aims at making a first step for a comparative macroeconomic evaluation study main ALMP programmes in Germany and the UK, the further training (Förderung der beruflichen Weiterbildung, FbW) and the job creation programmes (Arbeitsbeschaffungsmaßnahmen, ABM) in Germany and the New Deal for Young People (NDYP) in the United Kingdom, which are the biggest programmes in both countries.

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[1] This study is a part of the project "On the effectiveness of further training programmes. An evaluation based on register data provided by the Institute of Employment Research, IAB (Über die Wirksamkeit von Fortbildungs– und Umschulungsmaßnahmen. Ein Evaluationsversuch mit prozessproduzierten Daten aus dem IAB)". Financial support by the IAB under contract number 6–531 A of the Federal Employment Service and by the Anglo–German Foundation under contract number 1379 were gratefully acknowledged. I am grateful to Michael White, PSI, for giving me access to the New Deal Evaluation data base. Moreover, I want to thank Dorothe Bonjour and Genevieve Knight, PSI, and Bernd Fitzenberger, Michael Gebel and Norbert Schanne at Mannheim for their remarks. I greatly appreciated Reinhard Hujer's comments at the Annual Congress of the *Verein für Socialpolitik* in Zurich who suggested to consider both the current and the lagged level of ALMP in the estimation of macroeconomic outcomes in order to capture the time specific effectiveness of programmes. Of course, the usual disclaimer applies.

This paper conducts an empirical analysis of the macroeconomic employment effects of ALMP for Germany and the UK. It reconsiders the effects of ALMP on wages and employment with the application of the theoretical framework developed by Layard, Nickell and Jackman (1991) and Calmfors, Lang (1995).

It is well known that empirical evaluations of macroeconomic effects of ALMP faces severe identification problems because of the endogeneity of the ALMP variable, which is caused by the political system that answer to unemployment by allocation of ALMP. It is crucial to control for endogeneity because the simultaneous occurrence of high levels of ALMP and high unemployment could lead to biased estimates about its effects. This contribution suggests different procedures to control for endogeneity: either instrumental variables that are strictly exogenous are used in order to identify the exogenous variation of ALMP irrespective the level of unemployment or a dynamic panel data estimation on the basis of Arellano, Bond (1991) is implemented to control for the dynamic structure of the endogeneity of ALMP.

The remainder of the paper is as follows: Section 4.2 gives an overview of the institutional regulation of the programmes of further training and job creation in Germany and the NDYP in the United Kingdom. The paper shows the main incentive structures of the programmes, the intended functioning and describes the participation figures. Section 4.3 discusses the effects of ALMP on the wage setting and employment schedules in the model suggested by Layard, Nickell, Jackman (1991) and Calmfors, Lang (1995) and on the matching of unemployment and vacancies. It also surveys the empirical evidence, which has been found up to now for the two countries. Section 4.4 describes the data used for Germany and the United Kingdom and specifies the empirical model. It is then tested with quarterly regional panel data for the period 98–02. This section also describes the choice of instrumental variables and shows the results of the first step of the IV estimation. Section 4.5 concludes.

4.2 ALMP programmes

4.2.1 ALMP

Active Labour Market Policy (ALMP) consists of "selective interventions by the government in the pursuit of efficiency and/or equity objectives, acting indirectly or directly to provide work to, or increase the employability of people with certain disadvantages in the labour market". Following this definition, ALMP is predominantly:

- Policies implemented by the *Bundesanstalt für Arbeit* in Germany (Federal Employment Institute, in the following BA) under its regulatory framework, the Sozialgesetzbuch III (Social law book III, SGB III).
- National ALMP of the *Department for Work and Pensions* in the United Kingdom implementing the UK's Welfare to Work Strategy (New Deals) via the Employment Service (ES) besides other ALMP (Job–seekers Allowance, JSA, and the Restart Interviews).

Other programmes of ALMP might exist at the level of local municipalities in Germany, of the German Länder and at the European level, mainly co–financing the ALMP of the BA. Recent changes in the UK including the set–up of devolved administrations in Scotland and Wales and Regional Development Agencies (RDAs) could lead to more regional activities in the UK as well.

4.2.2 Germany

4.2.2.1 Basic regulation

In 98, the new Social Law Book (SGB) III replaced the basic regulation of ALMP in Germany, the former Labour Promotion Act (Arbeitsförderungsgesetz, AFG). For the general targets as well as for the organisation and implementation of ALMP in Germany, this reform had far reaching consequences.

Compared to the AFG, the SGB III rejects the role of ALMP as a part of growth policy: In its fundamentals in § 1 the objectives of ALMP are identified as the integration of disadvantaged groups and the improvement of the labour market situation of these target groups by increasing their human potential with advice, training measures, and special subsidies for professional integration or business start ups. The instruments of ALMP in Germany are now "more subsidiary" (Sell 1998: 545) and the SGB III puts more emphasis on the insurance principle, underlining that the promotion of employment opportunities of disadvantaged groups primarily aims at the reduction of income maintenance payments (SGB III, §§ 1).

4.2.2.2 Organisational change

Besides this more general "new orientation of ALMP", the introduction of the SGB III significantly modifies the allocation of ALMP measures by means of organisational and financial reforms of the internal structure of the BA as well as new or differently applied ALMP measures:

The former regulation implied separate budgets for either further training schemes or job creation measures (the main ALMP in Germany). With the reform of the organisational structure, an integration of both big funds in the local employment offices was introduced. With this gain in flexibility, the local employment offices are now enabled to implement programmes according to local requirements. The formerly homogeneous ALMP designs on the disaggregated level become more diverse with respect to design and implementation. Additional to the flexible shift between the different schemes of the SGB III, the new regulation allows that 10% of the regional budgets of ALMP can be utilised for "experimental" ALMP on the regional level (§ 10, SGB III) allowing the local employment offices to work out individual solutions and case management for individuals and specific problem groups (Sell 1998: 541).

4.2.2.3 Programmes

ALMP in Germany consists of three main policy areas: The first and most important aim is the integration or re–integration of problem groups by supporting individual vocational and further training, the second policy instrument enables the creation of temporary or permanent employment opportunities with a broad variety of wage–cost subsidies and the third target area are grants for occupational or regional mobility. The most important programmes of these areas are described in the following sections and are summarised in table 4.1.

Training

Among the ALMP programmes, *further training* is the most important. It aims at the integration of unemployed persons and those at risk of becoming unemployed by providing recognised vocational qualifications including schemes for individuals who completed first vocational training aiming at assessment, maintaining, extension or adaptation of vocational skills to technical developments. Participants may be granted an "income maintenance payment" (Unterhaltsgeld) if they have been previously in employment subject to social contributions for a minimum length during a set period of time or if they have received unemployment benefits or assistance. Under certain conditions, these payments can be extended to persons who return to the labour market. The income maintenance payment is equal to unemployment allowance, i.e. 60–67% of the previous net wage. Training measures are usually carried out by private training centres, which offer programmes for specified target groups and market segments to the local employment offices. The selection of the appropriate participants among the unemployed lies in the responsibility of the local employment office. The duration of measures varies between 3 and 8 months for further training and up to 24 months for re–training in a new profession.

Measures to *improve prospects of integration* are financially supported short–term training courses or practical activities that improve the prospects of unemployed workers for integration by the assessment of the suitability of the unemployed person for employment or training. Furthermore, they can comprise job–application training, counselling on job–search possibilities or measures, which investigate the unemployed person's willingness and ability to work. This new instrument of ALMP introduced with the SGB III aims at the support for individual job–search activity.

Job creation programmes

Among the target area of job creation, the most important programme is *Job creation (ABM)*. ABM aims at the creation of temporary employment for long–term unemployed (> 12 months) in projects that "have to benefit the community" and " must be additional", meaning that they would not have carried out without the subsidy. ABM is co–financed: between 30% and 75% of the total wage costs (i.e. the wages plus the employers' shares of the social insurance contributions) are subsidies by the BA and where the implementing body (public or private legal entities) incurs further costs.

These general rules are only rough guidelines: In many cases additional loans or subsidies are given to the implementing body. The regulation gives priority to projects that considerably improve the chances for permanent jobs that support structural improvement in social or environmental services or that aim at the integration of extremely hard–to–place individuals. The gross wage must not exceed 80% of a comparable unsubsidised job. The duration of ABM is in most cases restricted to 1 year, but can be extended up to 36 months if permanent employment is offered subsequently.

Another job creation programme, *structural adjustment measures* aims at the temporary re–integration of long–term unemployed, too. This programme applies less severe eligibility criteria: Individuals who are only threatened by unemployment may participate, too. A priority is given to individuals, who cannot be placed in regular employment without subsidies in foreseeable future, i.e. long–term unemployed.

The wage cost subsidy is a flat rate equal to the amount of unemployment allowance or assistance the individual would have received if unemployment had continued. Temporary employment is mainly supported by means of this programme if the projects conserve or improve the environment or provide social services or youth aid. The implementing institutions, public institutions or private companies, pay the remaining personnel and material costs.

Wages for participants in this programme must not exceed 80% of equal unsubsidised employment. The subsidy is paid for 36 months and can be extended up to 48 months if the participants are regularly employed after the end of the programme. Besides these two main programmes, quite a variety of wage cost subsidies exist which are targeted to long–term unemployed (*Integration subsidies, recruitment subsidies for business start–ups, employment assistance for the long–term unemployed*) as well as programmes which are intended to increase the geographical mobility of the individuals (*mobility allowances*) or to improve the transition to self–employment. As data for these programmes are usually not available at the level of regions these programmes are not considered in the analysis in section 4.4 and therefore are not described in details here. A detailed description of these programmes can be found for example in Fitzenberger/Speckesser (2000).

4.2.2.4 Participation

Table 4.2 shows the participation figures in ALMP programmes according. Here, considerable changes are noticeable over the last 9 years in participant inflows into measures for both East and West Germany: Inflows into ABM and structural adjustment measures in the public sector ("traditional SAM") vary between 0.2 and 0.35% of the total civilian labour force between 93 and 01 in West and 2.5 and 4.7% in East Germany.

There are two peaks over the last years for both East and West Germany in 94 and 98, suggesting that the participation and the extent of the programme depend on the election to the federal parliament. In total, significantly less than 1% of the total civilian labour force starts an ABM programme or a traditional SAM every year (except for the two peaks). The inflow into programmes aiming at the integration of hard–to–place individuals into the regular labour market shows the same pattern. This category, including integration subsidies, recruitment subsidies for business start–ups, integration contracts and the employment assistance for long–term unemployed as well as special SAMs for the private sector, employed about .31% of the total civilian labour force in West Germany and 1,58% in East Germany in 01 (0.5% of total civilian labour force countrywide), again with a peak in the election year 98.

Further training, the most common ALMP intervention, shows participation figures like the ABM programme (1.1% of total civilian labour force in 98) in the eastern part. In the western part, further training involves twice as many participants compared to ABM and traditional SAM, the relative size in East Germany is just opposite after a decline from 3.87% to 2.35% between 93 and 01, indicating that the first and extensive use of further training for the purpose of qualification to new occupations in the market economy ended.

A declining usage of further training among the German ALMP can be observed. For the ABM scheme, this trend cannot be found because of two peaks in election years (which is also true for wage subsidies for regular employees in 98).

Table 4.1 Main ALMP programmes in Germany

Training

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 01	Costs, 01
Further training and Re–training	Improving qualification of unemployed	§§ 77–96; §§ 153–156	• Unemployed with "training necessity" • Re–entrants from inactivity	• Course fees • Income maintenance payment for participants (equal to unemployment benefit) • Accommodation/child care	a) 2 to 8 months b) 24 months	Total: 344,800 East: 135,800 West: 209,000	Total: 6,982 Mill. € East: 2,795 Mill. € West: 4,187 Mill. €
Improving prospects of integration	Improvement of job search	§§ 48–51	Unemployed	• Course fees • Accommodation/child care	½ to 2 months	n.a.	n.a.

Subsidised Employment

Programme name	Aim	SGB III	Target group	ALMP support	Duration	Participants, 01	Costs, 01
Job creation measures (ABM)	Temporary integration in "additional" employment in the public interest	§§ 260–217; § 416	• Long–term unemployed	• Wage cost subsidy (30–90% of the wage costs) • Wages must not exceed 80% of equal unsubsidised employment • Financial support for institutions	• 12 to 24 months • 36 months (if creation of permanent employment follows)	Total: 166,643 East: 116,024 West: 50,619	Total: 2,976 Mill. € East: 2,113 Mill. € West: 863 Mill. €
Structural adjustment measures (SAM)	Temporary integration in "additional" employment improving social service and the environment	§§ 272–279; § 415	• Long–term unemployed • Unemployed • Persons at risk of becoming unemployed	• Wage cost subsidy (equivalent to individual unemployment benefit) • Social insurance contributions • Wages must not exceed 80% of equal unsubsidised employment	• 36 months • 48 months (if creation of permanent employment follows)	Total: 76,466 East: 65,767 West: 10,699	Total: 871 Mill. € East: 744 Mill. € West: 121 Mill. €

Table 4.2 Participation entries in ALMP as percentage of total civilian labour force

		93	94	95	96	97	98	99	00	01
Job–creation measures (ABM) and traditional structural adjustment measures (SAM)	West	0.20	0.31	0.30	0.31	0.26	0.35	0.30	0.28	0.23
	East	4.12	4.74	3.74	3.80	2.54	4.39	3.14	2.80	2.16
	Total	0.97	1.17	0.97	0.99	0.71	1.15	0.87	0.78	0.61
Regular employees receiving ALMP subsidies*	West	0.09	0.09	0.20	0.16	0.21	0.44	0.43	0.38	0.31
	East	0.68	0.53	0.84	0.60	1.46	3.38	2.66	1.70	1.58
	Total	0.20	0.18	0.32	0.25	0.46	1.02	0.88	0.64	0.56
Further training	West	1.12	0.98	1.30	1.23	0.89	0.86	0.95	1.05	0.80
	East	3.87	3.82	3.44	3.62	2.20	2.42	2.25	2.66	2.35
	Total	1.66	1.53	1.72	1.70	1.15	1.17	1.21	1.37	1.11
Total	West	1.41	1.38	1.80	1.71	1.37	1.66	1.16	1.04	0.85
	East	8.66	9.09	8.03	8.01	7.17	12.93	6.23	4.88	4.05
	Total	2.84	2.88	3.01	2.94	2.51	3.87	2.96	2.79	2.28

Source: Amtliche Nachrichten der Bundesanstalt für Arbeit (Official Bulletin of the Federal Employment Institute), 1, 1999: 14ff, own calculations

4.2.3 The New Deal in the UK

4.2.3.1 Introduction of the New Deal

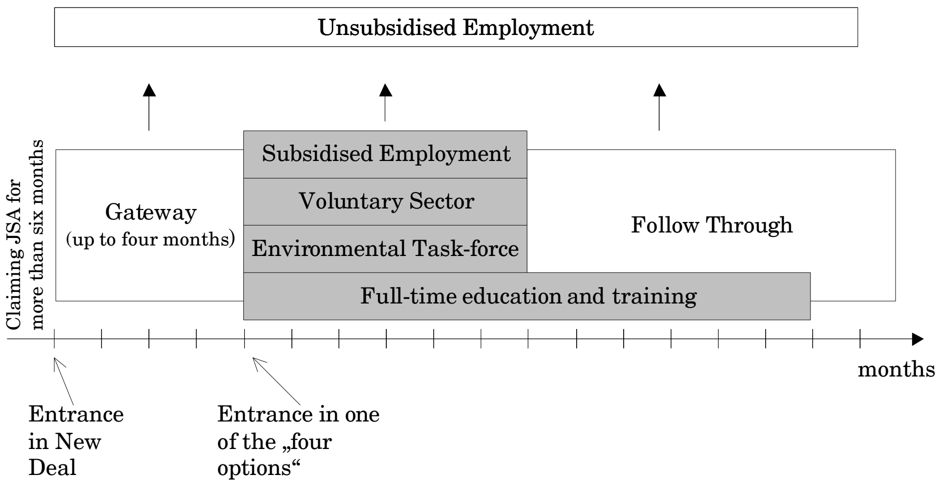
The New Deal for Young Unemployed People (NDYP)[2] was introduced in April 98. Similar to other programmes, which were introduced under the New Deal policy of the Labour government after 97, this programme was tested in a three–month period before in selected regions of the UK ("pathfinder regions") for collecting first experience with the programme. The NDYP is intended to encourage young people to actively seek work and to provide them necessary skills, opportunities and motivation. The NDYP basically consists of three stages (see figure 4.1):

- The "Gateway" period: After a period of six months of open unemployment, people enter the NDYP in a stage called the "Gateway" period which can last up to four months. During this period, participants on the NDYP are offered intensive assistance in the job search process.
- Individuals who are not successful in finding employment in the "Gateway" period are assigned to any of the ALMP programmes of the NDYP after the four–month period. This stage of the NDYP is called "the options". Individuals either are offered a subsidised job in a private sector form, a job placement in the voluntary sector or to the environmental task–force or start full–time education and training. The "options" are supposed to last up to a period of 6 months, with training lasting up to one year. Individuals leaving an option without placement to regular employment outside the NDYP then start a third phase.

[2] An extensive description of the New Deals can be obtained under http://www.newdeal.gov.uk/newdeal.asp?DealID=1824

- The third stage is called the "Follow through". During this period the employment service offers additional assistance in finding regular employment to ensure that New Deal clients are helped throughout their participation on an option to progress towards the goal of finding and sustaining work, and are given further assistance if they return to unemployment. If individuals do not find adequate work in the private sector after a period of six months, they may start a second option.

Figure 4.1 The three stages of the NDYP



In principle, unemployed young people enter the NDYP after a period of six months. There are however exceptions to this period; if individuals start regular employment after participating in the gateway and become unemployed again within a period of 13 weeks, they do not have to wait another six months of unemployment before re–qualifying for the NDYP.

4.2.3.2 The options

Before participants start any of the options, a personal adviser will set up an induction session in order to make participants understanding the aims of the option and knowing what they can expect to gain from working or learning in either one of the options. Participants are offered a personal development plan, which should ideally match their individual needs. There will also be support available during the whole time, with job search help and advice and regular progress checks. When individuals start an option, they either receive a wage from the employers, or an allowance equal to their unemployment benefit (Job Seekers Allowance, JSA) entitlement plus a grant of up to GBP 400 (€ 650) for the whole period granted as weekly supplements to the benefit. The four options are:

Employment option (EMP)

The employment option EMP offers subsidised employment in the private sector. Employers receive a direct payment of GBP 60 (€ 100) per month plus GBP 750 (€ 1,200) towards the costs of providing training to the participants for at least one day per week. This new deal programme is the private sector centred option as it is explicitly targeted at tackling skills shortages at the same time as helping job–seekers to find regular employment. Participants on the employment options are paid with normal wages, only being subsidised by the NDYP. The employment option is focused on the most employable fraction of participants on the NDYP previously screened in the Gateway phase.

Voluntary Sector Work (VS)

Individuals starting a VS option work up to six months in the voluntary sector, developing skills and gaining experience for future jobs. Voluntary sector work has to benefit the community, ranging widely between social care work in private household and public charities benefiting for children and disabled as well as work in arts and media related projects for public administration. Participants on the VS option are guaranteed an allowance equivalent to the Job–seekers Allowance (JSA) plus GBP 400 (€ 650) on top for six months. Other benefits such as housing and council tax benefits are continued if applicable. On average, voluntary sector work offers one full day of training per week which is intended to lead to a recognised qualification besides the work experience the participants gather in VS (approx. 30 to 40 hours per week).

Environment Task Force (ETF)

The Environment Task Force (ETF) has the same structure as the VS option providing work experience for a period up to six months in the voluntary sector and one day of full–time education per week, but has a focus on environmental work. Environmental work is usually carried out by non–profit organisations close to the public sector and to local authorities improving run–down estates, save energy and water, repair homes, recycle waste and look after the countryside. As in the VS scheme, individuals are offered one day of full–time training during this period. The benefit level is comparable to the JSA, plus an extra GBP 400 (€ 650) once in the period of six months.

Full–time education and training (FTET)

If participants are not quite ready to move directly into work, they can spend up to 12 months in full–time education or training leading to a recognised qualification. Participants may start this option if they do not have training up a medium skilled qualification or in some circumstances after completion of vocational training if further training is necessary for finding a job. Participants in this option start full–time education and training. If necessary the FTET option is combined with general training such as reading and writing. The training option can last up to 12 months and participants are granted income maintenance equivalent to the JSA.

4.2.3.3 Participation

The NDYP started in January 98 in selected pathfinder areas and nationwide with the beginning of April 98. 35,911 individuals started the Gateway in April 98. After half a year, participation entries stabilised at around 15,000 new entries per month and remained at this figure afterwards (www.dwp.gov.uk/asd/online.html). Participation outflows from the NDYP are relatively low at the beginning, but also average near 15,000 per month after April 99.

The composition of the NDYP participation varies significantly over time (see table 4.3). At the beginning, the employment option had participation stocks varying between 10,787 in the quarter when the NDYP started and a peak of 11,636 persons on EMP in the third quarter of 99. Over the next five quarters, participation in EMP declined to 4,481. Participation in full time education and training started with 23,190 persons in the first quarter of 99 and declined constantly over time to near half of this figure in the first quarter of 01. On the contrary, the subsidised employment in the public sector provided by either voluntary sector work or the option of the environment task force remained relatively stable with participation figures around 7,850 and 6,950 at the beginning of the NDYP and now 6,248 in the VS option and roughly 5,600 in the ETF scheme. The full–time education and training option therefore can be considered as the most important of these four ALMP programmes with almost one third of the participants going through this scheme.

Table 4.3 Quarterly participant stock on NDYP

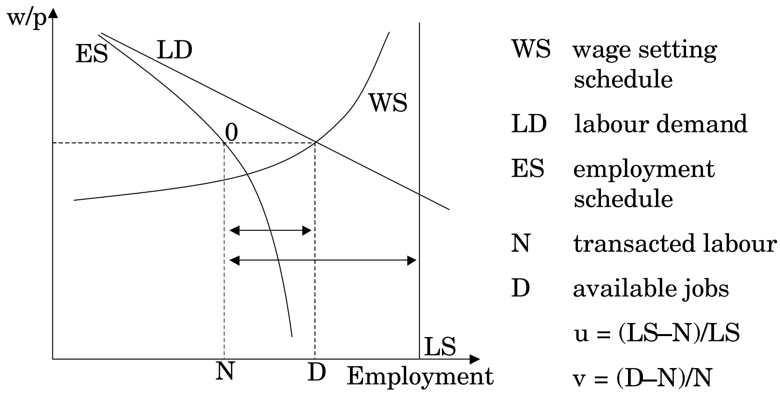
Quarter	Gateway	Options				Follow Through	Total
		EMP	FTET	VS	ETF		
99–1	74,550	10,787	23,190	7,848	6,952	14,308	137,635
99–2	71,837	11,633	20,922	7,980	7,278	20,243	139,893
99–3	65,513	11,636	19,384	7,644	7,327	24,656	136,160
99–4	60,873	10,893	17,779	7,168	7,023	22,667	126,403
00–1	63,662	10,664	17,806	7,971	7,537	20,266	127,906
00–2	59,875	6,328	15,701	8,027	7,109	19,688	116,728
00–3	50,706	5,758	14,990	7,302	6,543	20,151	105,450
00–4	50,012	4,481	13,243	6,432	6,097	18,938	99,203
01–1	51,959	4,288	12,207	6,248	5,593	17,667	97,962

Source: www.dwp.gov.uk/asd/online.html

4.3 Expected outcomes

A modified version of the Layard, Nickell, Jackman framework (1991) was introduced by Calmfors, Lang (1995) to derive the macroeconomic effects resulting from ALMP based on a union wage–setting model. This model makes a distinction between regular employment and participation in programmes. Both labour demand and wage setting are affected by ALMP. The model (see figure 4.2) assumes price setting firms, non–competitive wage determination and formulates a wage–setting schedule and a labour demand schedule. Regular employment outside ALMP programmes then depends on real wages (*w/p*) and employment, both the employers (*LD* labour demand) and the unions (*WS* wage–setting) curves are determined by outside options of the workers, the firms and the relative bargaining power. Labour demand depending on technology, payroll taxes and product market competition of the firm is not equal to the employment schedule (*ES*), because some vacancies remain unfilled even after bargaining (mismatch, indicated by the distance between *N* and *D*).

Figure 4.2 Layard–Nickell–Jackman–Framework

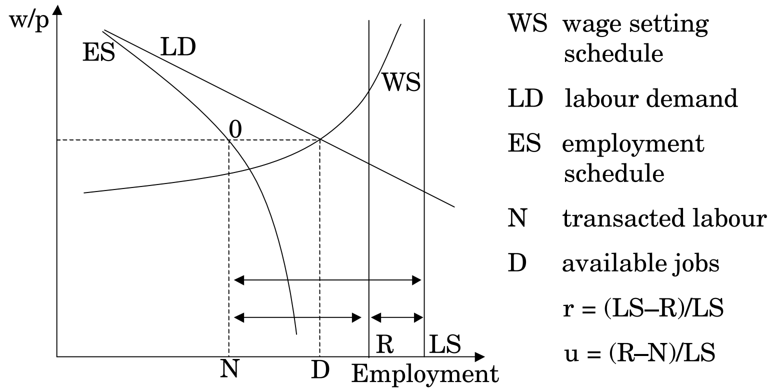


The intersection of wage setting and employment schedule shows the planned level of employment and wages (*D*). This may differ from the level of full employment as indicated by the distance between *D* and *LS* (*LS* is the exogenous labour supply). It is important to note that the aggregate wage setting in this model is only affected by the level of structural unemployment, i.e. not by short–term unemployment caused by mismatch. Mismatch *D–N* does not affect the wage setting and the planned level of employment, but of course the realised employment.

Following Calmfors (1994) both wage setting and labour demand are affected by ALMP, either because the job search is improved, the productivity of workers is affected by the implementation of ALMP or because of the changes of the bargaining power of the unions caused by ALMP to be discussed in the following. Possible effects of ALMP can be identified within this framework by deriving separately effects on wage setting and labour demand. The equilibrium in this system is point *0*, where *N* people are employed and the unemployment rate is given by $u = (LS–N)/LS$. The distance *ND* gives the number of available vacancies, i.e. the total number of jobs which could be filled, so that the vacancy rate is represented by $v = (D–N)/D$. An increase in the unemployment can be caused either by an upward shift in the wage setting schedule *WS*, a downward shift of the employment schedule *ES* or by a shift of the effective labour demand *LD* to the left, i.e. reducing the number of employed.

Extending this framework, Calmfors (1994) distinguishes between regular employment and the employment in ALMP programmes. Hence, the effective labour supply is reduced by the number of individuals in programmes.

Figure 4.3 Calmfors' LNJ–Framework



As the exogenous labour supply is reduced, R – the remaining labour supply outside the ALMP programme – indicates the effective labour supply. Within this framework, any increase in ALMP participation corresponds to a leftward shift of the R curve to any point R' and to a reduction of the structural unemployment. If nothing else happened, the effect would result in a *reduction of the open unemployment*, which usually is referred to as the "bookkeeping effect" of ALMP. Note that the mismatch unemployment is not affected by ALMP if the effect of ALMP consisted only of this reduction.

However, because of the wage setting schedule in the model assumed to depend on the overall employment level, a reduction of the effective labour supply – corresponding to a reduction of the structural unemployment – increases the employment rate as a percentage of the "remaining" total labour force and might increase the bargaining power of the unions. With ALMP reducing the effective labour supply, the unions tend to request higher wages. Thus, a reduction of the level of structural unemployment by ALMP programmes (indicated through the leftward shift of the R curve) could increase the aggregate wage pressure by the unions and correspond to a leftward shift of the wage–setting schedule. Consequently, an expansion of ALMP programmes would results in an even reduced employment level as the wages increase. This effect will be discussed in details in the next section. Other effects on the wage setting schedule arise from "crucial design features" (Calmfors 1994) such as the level of benefit, duration of the programme and eligible groups.

Other possible effects can occur via changes in the labour demand or the employment schedule: As a fraction of all vacancies usually remain unfilled and mismatch by the distance between N and D is included in the model, the effects of ALMP are often discussed with respect to the matching efficiency on the labour market: By increasing information on the labour market with additional signalling devices (e.g. certificates acquired in further training programmes) or by extending the placement activity, the matching efficiency is improved.

The consequence is, that even if we do observe unchanged wages, employment could increase at the same time – at least theoretically. Many of the ALMP evaluations so far carried out discuss the ALMP outcomes via this mechanism, see for example Bellmann, Lehmann (1991), Burda, Boeri (1993), Hagen, Steiner (2000, 2002), Schmid, Speckesser, Hilbert (2001), Buettner, Prey (1998) and Prey (1999) for Germany and Bellmann, Lehmann (1991), Lehmann (1993) and Riley, Young (2001) for the United Kingdom. Calmfors, Lang (1995) term this effect "the iron law" of ALMP. The effect of ALMP on matching will be discussed in the second part of this section on the basis of a simple matching function.

4.3.1 Effects of ALMP on wage–setting

Three possible effects on the wage setting process can be derived from the previously mentioned way how unions internalise the effects of ALMP benefit levels and altered search or drop–out behaviour in the utility function:

1. If the instantaneous utility in ALMP is bigger than the utility of open unemployment this raises the wages: If programmes are entered directly by individuals laid off from employment, the effect on the wage setting schedule is higher than in the case of being unemployed for a certain time (see Calmfors, Lang 1995) because the alternative utility then differs by the discount factor as individuals have to wait at least one period in open unemployment before profiting from the higher utility in the programme.
2. The second effect on wages comes via the reduced outflows from the labour market and an increased competition among the job–seekers ("competition effect"): If the proportion of the job–seekers in programmes increases and the programmes are supposed to attach closer to the labour market than open unemployment, the flow out of the labour force is reduced. For given employment, this tends "to keep up the number of job–seekers and hence to reduce the re–employment probability for the individual" (ibid. 609).
3. The third effect of this model however weakens the wage–reducing outcome from the competition effect. Supposed the dropout from the labour force is reduced and the benefit level is higher in the case of remaining a job–seeker either in a programme or in open unemployment, a reduction of the dropout leads of course to a higher alternative utility and increases the wages.

The effects which Calmfors, Lang (1995) derive from this theoretical framework are ambiguous – because ALMP can either lead to an increase or decrease of the wage setting schedule and the aggregate employment level. However two clear effects can be derived: 1) the influence on wages is lower, the later a possible treatment of ALMP occurs. This however would imply that already a certain fraction of individuals dropped out of the labour force and the competition effect might be lower than in the case of a programme concentrated on an earlier stage of unemployment. 2) If formerly employed are never eligible to start an ALMP participation, ALMP does not appear as alternative utility in the wage bargaining process. In this case, an effect on wage bargaining will not arise.

4.3.2 Labour demand

Deadweight and substitution effects

Many job creation programmes are considered to have deadweight and substitution effects meaning that either hirings from the target group would have occurred also in the absence of the programme or that workers gained their jobs because they substitute workers from other categories as their relative wage costs are changed. For further training substitution effects are supposed to be lower than for job creation programmes.

In practice the distinction between further training and wage–subsidies is not clear, meaning that especially on–the–job–training can have the same effects on relative wages as a normal wage subsidy. A quantification of substitution effects has not yet been worked out empirically. There is however on the basis of simple regression models little evidence that it could be substantial at least for wage subsidy programmes (see Calmfors 1994: 19).

Productivity effects

ALMP is assumed to counteract possible negative effects of unemployment – and especially long–term unemployment – on the productivity of the workforce. Training for unemployed is aimed at rising or maintaining the productivity level. This is for example one of the reasons for the long–lasting use of ALMP in East Germany even after it became obvious that unemployment was not temporary but persistent.

Thinking of training programmes for the unemployed as improving the technology the effect on the labour demand schedule would be a rightward shift indicating so that regular employment would be increased because the unit wage costs decrease. A positive effect however does not arise from this in any case: If output depends on the amount of labour in efficiency units and labour becomes more efficient, then the employment effect is uncertain. Besides, the reaction of wages to the technological changes is unclear. According to Calmfors (1994) it is empirically uncertain how the effects of ALMP on employment via the technological progress work out. However, productivity changes could increase the employment level as a whole for a heterogeneous workforce because the reallocation of workforce from a low productivity sector with excess labour supply to a high productivity sector with excess demand can be increased.

4.3.3 Matching

The effects of ALMP are also evaluated with respect to the relationship between unemployment and open vacancies,[3] often referred to as the *Beveridge* curve. Labour market matching in this context is defined as a production function of unemployment and vacancies. ALMP in this context could in-

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[3] For a general discussion of the unemployment/vacancy relationship see Blanchard, Diamond (1989).

crease or decrease the productivity parameter of this production function. If job–seekers are heterogeneous, ALMP increases the probability of programme participants to find employment so that

$$s_r = c \cdot s_u \tag{1}$$

with s_u the matching probability of an unemployed person and s_r the probability of an ALMP programme participant. The assumption of $c > 1$ gives the statement of the effects of ALMP on the individual search efficiency. Note that this efficiency parameter would correspond with a microeconomic increase in individual employment characteristics, which might not in any case be warranted. Furthermore, consider a constant stock of vacancies in the period *t* given as V_t. Then, the transition from unemployment and ALMP to employment follows a matching function of the form

$$h_t = c_0 m(V_t; UE_t) \tag{2}$$

where new hiring h_t from either unemployment or ALMP can be specified as a function of vacancies and job–seekers UE_t, which in this case are both ALMP participants and unemployed job–seekers. c_0 is a general technology parameter, m is a matching function that can be specified for example as a Cobb–Douglas function[4] with constant returns to scale, so that

$$m = (V_t)^d (UE_t)^{1-d} \tag{3}$$

The theoretically expected effect of ALMP would be an increase in the search efficiency of the respective share of job–seekers coming from the programme by a positive quantity because a) additional vacancies are generated by measures or b) placements facilitate the matching process or c) stock of human capital is improved and search costs as a part of total wage costs decrease, so that an increasing employment can be observed.

$$h_t = c_0 \, c_t^* m(V_t; UE_t)$$

where c_t^* indicates a technological increase in efficiency caused by ALMP with:

$$c_t^* = \left(1 + \sum_{j=1}^{p} k_j M_j\right). \tag{4}$$

k_j exhibits the influence of the aggregate participation M_j in $j = 1,\ldots,p$ different ALMP programmes on the search efficiency. Reformulation of (3) in outflows per unemployed yields to:

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4 The empirical analysis of ALMP effects on the basis of this framework often apply specifications of the matching function as a Cobb–Douglas type (see for example Bellmann, Lehmann 1991 for Germany and Blanchard, Diamond 1989 for the USA).

$$f = \frac{h}{UE} = c^{*}\left(\frac{V}{UE}\right)^{d}. \tag{5}$$

Assuming the ALMP influence on the search efficiency to be small, the permissible approximation $\ln\left(1+\sum_{j=1}^{p} k_j M_j\right) \approx \sum_{j=1}^{p} k_j M_j$ leads to a log–linear model that allows the estimation of the ALMP effects as linear in the parameters with:

$$\ln\left(\frac{h}{UE}\right) = \ln(c_0) + \sum_{j=1}^{p} k_j M_j + d \ln\left(\frac{V}{UE}\right). \tag{6}$$

Outflows from unemployment then can be regarded as a function of the general efficiency parameter and the available vacancies on regional labour markets over the unemployed as a measure of labour market tightness and the increased technology of the ALMP. If ALMP is supposed to increase the search efficiency, then the overall outflows from unemployment increase. This implies an increase in employment while the open vacancies decline – given constant outflows from employment to unemployment. Hence the matching of unemployment and open vacancies is improved. In terms of the matching function regarded as a production function, the ALMP influence can be seen as an efficiency parameter increasing total factor productivity.

4.3.4 Previous empirical evidence

Previous evidence reported here focuses on studies, which were explicitly worked out for Germany and the UK on the basis of *regional data* in order to identify the macroeconomic impact. For the 90s, six evaluations studies have been conducted for Germany. For the UK, only two comparable evaluation studies exist about the effects of the NDYP. Therefore, an evaluation study from a programme of the 80s will also be discussed here to find out more about macroeconomic effects in the UK. The NDYP study considers the effects of ALMP on regional or local unemployment outflows of the ND specific target group as compared to the non–eligible group of participants and applies difference–in–difference estimators.

Germany

Bellmann, Lehmann (1991) evaluate the outcomes of ALMP on the aggregate regional outflows of specific age–groups distinguished by having experienced either short–term or long–term unemployment for the United Kingdom and for West Germany over the 80s. Based on administrative data for both countries, they implement estimations of an outcome equation specified as a linear regression in a panel data set. They apply a Hausman test in order to find the correct specification of the panel estimation (fixed effect model). The units of observation for Germany are local employment office districts (N = 142) in West Germany with a quarterly frequency for the years 79–

88. In this study, the effects of different programmes of ALMP are investigated, either Job Creation schemes, further training and wage subsidies. It can be shown that neither one of these policies significantly affects the outflows from unemployment, except job creation, which decreases outflows from short–term unemployment, but has no significant effect on outflows from long–term unemployment either.

Pannenberg, Schwarze (1996) make use of on an extended wage curve approach and analyse how the level of ALMP for East German regions affects the average regional wages in the years 92–94. Based on data of the German Socio–economic Panel (GSOEP), information is aggregated to local employment office districts for East Germany (N = 39). The results indicate that a standard wage curve relation does not hold for East Germany. The extended wage curve however shows a significantly negative effect of the aggregate number of job–seekers on the wage pressure, indicating that equilibrium unemployment could have been reduced due to carrying out further training in East Germany.

Büttner, Prey (1998) and the following study by Prey (1999) analyse to which extent ALMP influences the labour market mismatch in West Germany measured as the structural rate of unemployment. The analysis is based on regional data corresponding to Planning regions (N = 74) for 86–93. Endogeneity of ALMP is considered in the outcome equations by either applying fixed effects panel data estimations or by the use of instrumental variables estimations in which the level of ALMP is instrumented by data of political majorities in the regions or other structural variables. The effects of ALMP on the regional labour market mismatch parameter are estimated with different specifications of the outcome equation, either on the basis of fixed effect models or with IV estimations identifying the exogenous variation of ALMP in the first step. To control additionally for dynamic forms of endogeneity, the authors make use of the Arellano and Bond (1991) dynamic panel data estimator which allows to instrument the first differences of lagged endogenous variable with the lagged levels of the dependent variable and further strictly exogenous variables. The results of this analysis are mixed: based on the fixed effects estimations there are neither effects of job creation nor of further training whereas the dynamic panel data estimations indicate that job creation reduced mismatch while further training had no significant effect.

Schmid, Speckesser, Hilbert (2001) evaluate the outcomes for regional structural unemployment, estimating the effect of ALMP on the regional level, share and change of unemployment and on the aggregate regional outflows from unemployment. The units of observation of this study are local employment office districts (N = 142) for the years 94 – 97 in West Germany. Job Creation programmes have only insignificant effects on the outflows from unemployment whereas further training reduces the outflows significantly. If the outcome is measured with respect to the level and the share of long–term unemployment defined, the effect becomes positive for further training and insignificant for Job Creation schemes. A significant effect of wage subsidies cannot be found in any of the specifications on either long–term unemployment or outflows from unemployment. A causal effect of the length of the schemes cannot be found in the individual specifications. It can however be shown that regional implementation variables significantly affect the outcomes and that the regional policy variation matters.

Hagen, Steiner (2000) evaluate the effects of ALMP on the regional level of unemployment for east German local employment districts (N = 35) on the basis of a monthly panel for the years 93–99. The data cover regional inflows into and outflows from unemployment and variables for ALMP covering further training and job creation schemes. The empirical model is derived from a unemployment/vacancies relation considering ALMP to have an impact on the regional job matching. This model assumes the outflows from unemployment and inflows into unemployment to be affected by the ALMP. The evaluation of the effects of ALMP on inflows into and outflows from unemployment is carried out with fixed effects estimations, which should allow controlling for the endogeneity of ALMP. The authors additionally include polynomials of lagged variables of the level of ALMP in the region to control for the long–term effect of ALMP on the regional labour market outcomes. The results indicate a positive effect of ALMP on the level of unemployment in the long run, i.e. the unemployment level increased.

Hujer, Blien, Caliendo and Zeiss (2002) evaluate the outcomes of ALMP on the regional job–seekers rate. On the basis of regional data for 175 local employment office districts, the authors estimate the effects on the basis of quarterly data. The endogeneity problem is controlled for by several sets of instrumental variables, which are included in the first step. Sensitivity checks with respect to the set of instruments are performed. In order to control for bias arising from estimations of dynamic panel models, which are justified in the presence of highly frequent data, the study applies the estimator developed by Arellano, Bond (1991). The results are separately estimated for east and West Germany: For West Germany, the authors find a reducing effect of further vocational training on the job–seekers rate and job creation schemes whereas for East Germany no significant effect could be found. The authors conclude that job creation schemes and other extensively implemented ALMP programmes were not able to overcome the essential mismatch problem in East Germany.

UK

Lehmann (1993) and Bellmann, Lehmann (1991) examine the effects of the restart programme in the United Kingdom on the outflows from unemployment for specific target groups. In several specifications, a significant increase in the outflow rates from long–term unemployment was found which however was counteracted by an adverse effect on the outflows from short–term unemployment. The total effect on the outflows was positive and significant.

For the NDYP two comparable evaluation studies have been published investigating the effects of ALMP on regional or local unemployment outflows of the ND specific target group as compared to the non–eligible group of participants. The study by Riley, Young (2001) makes use of the New Deal evaluation data base (NDED) and estimates the impact of the NDYP on all outflows from the programme compared to non–participation applying a difference–in–differences estimator.

The authors consider outflows from unemployment to different destinations and also assess the total effect of the NDYP. The flow models are specified as matching functions describing the production of matches made between vacancies and job–seekers. The study derives the total effect of ALMP on the unemployment level by estimating the impact of ALMP on inflows into unemployment.

Data refer to 144 ND units of delivery[5] taken from the NDED, which is matched with vacancy data and social insurance data. Data then are aggregated to 95 modified UoDs because the information is not fully compatible between the different data sets and some regional units had to be aggregated. The panel covers 61 months. The authors can identify the macroeconomic effect with respect to outflows from unemployment using difference–in–differences estimators. The DiD estimators compare the effects of the introduction of the ND on the target group relative to a near non–target group before and after the introduction of the NDYP. Because of the recent introduction of the NDYP as well as the restriction to a specific target group, DiD is assumed to be an adequate instrument to evaluate the NDYP. However, there are two restrictions to this methodology because 1.) the comparison group might also be affected by the introduction of the programme if they can be substituted by the participants of the programme and 2) this methodology does not allow to estimate the long–term effects of the NDYP.

On the basis of regressions on the inflow into unemployment and the outflow from unemployment into regular jobs this evaluation study finds that positive effects on outflows are partially counteracted by positive effects on the inflows into unemployment. However, the authors find an overall reducing effect of 35,000 on the unemployment of the target group.

The evaluation study by Dolton, Balfour (2000) also refers to the regional data of 144 Units of Delivery and has focus on cohorts of entrants in the NDYP programme between April – June 98 and July – September 98, after the nation–wide introduction of the ND. These authors make use of the regional variation among the 144 UoDs and their region specific labour market conditions resulting in a regional specific accommodation ratio of the options. The evaluation estimates the outcomes with respect to hirings to four different points in time for the respective cohort as a percentage of all unemployed. Within the panel model, the authors control for additional heterogeneity by including a number of other variables. The regional specific heterogeneity is modelled as a random effect model. In order to cope for remaining endogeneity, the authors instrument for the allocation of NDYP programmes and the costs. The study does not specify the model as a dynamic panel model because the time periods covered did not allow the adequate modelling. The fit of the equations is used as an indicator that the matching process is adequately explained. The results with respect to matching indicate that only a limited effect of the employment option can be found in the estimations.

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5 The New Deal "units of delivery" (UoD) are regional subdivisions exclusively implemented for the New Deal programmes. They do not match properly to other regional subdivisions in the UK such as the Travel-to-work areas (TTWA) or the local authority districts (LAD).

4.4 Empirical Analysis

4.4.1 Data

4.4.1.1 Germany

Data for the estimation of the effects of ALMP on the regional extended unemployment and the matching function for Germany were taken from the published administrative data of the regional employment offices for the period 98 – 01, available for monthly and annual frequencies.[6] The estimation is restricted to West German regions in this paper. Data become available for 141 West German employment offices districts with respect to the structure of employment, unemployment and participation in ABM, SAM and FbW. Besides, detailed information about the structure of unemployment and an indicator for the implementation capacity of the employment office (unanswered claims for ALMP or unemployment benefit) can be obtained from the same source. However, as the organisational unit of "local employment offices" does not match to any of the other administrative units in Germany (*Kreise, Regierungsbezirke, Länder*), data from other sources had to be aggregated to the level of employment office districts. This additional information consists of political party shares in the regions and is obtained from published sources of the statistical offices of the Bundesländer for the aggregation level of *Kreise* (counties). We measure either the share of the conservative CDU in the regional representative assembly (*Kreistage, Bügerschaften*) or the share of the social democratic SPD. Topographical data for *Kreise*, i.e. the population figures and the surface, were obtained from the *Bundesamt für Bauwesen und Raumordnung* (Federal Institute for Environmental Planning).

As the different regional units of observation do not match perfectly, an approximate match of data from the different sources became necessary: Although a regional employment office district can consist of up to 10 *Kreise*, an aggregation to this unit can not always be achieved, because some *Kreise* belong to more than one employment office district had to be split. This is especially the case in northern Germany. In cases, in which one employment office district consists of several Kreise, the covariates – especially the political party information – was weighted according to the number of inhabitants of a region in order to represent the political power in the local employment office district appropriately.

Based on the employment office districts, ALMP participation were specified for the empirical models as the stock of participants in a region in either one of the programmes as a percentage of all unemployed and programme participants in this region (these ratios are referred to as accommodation ratios in the literature, see below section 4.4.2.1). Information of the unemployed was taken from the unemployment register although this does not match exactly to the international definitions of unemployment: only unemployed who register are recorded.

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6 http://www.arbeitsamt.de/hst/services/statistik/detail/index.html

Data for the political majorities turned out to be most significant when interacted with the regional topographical information and were transformed into dummy variables displaying the regional topographical type and the political majority within one variable (see first step of the IV estimations in the Appendix).

The first version of this analysis only used the annual totals for the regional aggregates and estimated the effects of ALMP for the years 98–01. In this paper, we also report results based on quarterly data that was obtained from the same source. However, informative covariates that could in principle serve as instruments in order to overcome the endogeneity, are not available from this source, so that we have to restrict the estimations of the macroeconomic effects based on quarterly data to dynamic panel estimations without instrumenting for ALMP. The job match is defined as the regional outflow from unemployment (reduced by the programme entrants in any of the ALMP programmes) as percentage of the stock of extended unemployment (i.e. unemployed as well as programme participants) in the same quarter. The vacancy rates describe the vacancy stocks as percentage of regional extended unemployment[7].

4.4.1.2 UK

The analysis for the UK is based on data taken from the New Deal Evaluation Database (NDED), the Quarterly Labour Force Survey (QLFS) for the period 1/98 to 2/01 and claimant count data from the Office for National Statistics of the United Kingdom (ONS).

The release of the NDED used for this evaluation covers data for all entrants into the NDYP between the beginning of the programme in January 98 and September 01 throughout the United Kingdom. The data provide monthly cross sections of all participants on the New Deal, but do not record full continuous information between these dates. The available data offer information about the individual status related to the New Deal on the date of observation. The data record whether a person started New Deal or the Gateway, whether he/she is on one of the four options, between options, on the follow trough or finished the new deal. On the basis of all individuals on New Deal, a cross section of the New Deal participation can be calculated for each month and quarter. Besides the information on ND, variables on the regions where individuals participate in the New Deal and the reason why they left the programme were taken from the ND database. Information was adjusted to quarterly averages. The individual information then was aggregated to the level of Travel–to–work areas in order to obtain a panel for 297 regions. Regions in Northern Ireland were excluded from the analysis. With quarterly observations from the first quarter of 98 to the second quarter of 01, the panel consists of 4,158 observations. However the ND started with the second quarter of 98 in most of the regions resulting in balanced panel 3,861 observations. Within the regions, accommo-

[7] The literature suggests several strategies how to model appropriately the matching of unemployment and vacancies, mainly justified by job search theories (Petrongolo, Coles 2002). As the results of this study however did not change if we tried different vacancy rates and different definitions of the unemployment outflow, we retuned to this simple specification of the matching function (see below for the exact form).

dation ratios were calculated on the basis of the regional registered unemployment and the participants on the NDYP.

Information about the regional unemployment stocks and flows as well as on regional vacancies were taken from the unemployment count data of the UK available through the statistical office. We define matching of unemployment and vacancies by the outflows from unemployment to employment as percentages of the regional extended unemployment based on unemployment count data. Vacancies rates are defined as the average stock of vacancies available in a region within a quarter as a percentage of regional extended unemployment.

4.4.2 Empirical model

4.4.2.1 Effects on extended unemployment

The basic empirical specification of the estimation in this analysis is given by

$$(u+r)_{i,t} = a_i + a_t + \sum_{j=1}^{p} b_{j,t-t} g_{i,t-t}^{j} + e_{i,t} \tag{7}$$

where u is the rate of unemployment as percentage of the total labour force, r is the rate of participants in programmes of ALMP as percentage of the total labour force, the rate of non–employment in the region i at time t and $t \in \{0,1,...,t-1\}$ indicates possible lags of the ALMP effects. If the outcome is specified in this form, we control for the bookkeeping effect of ALMP. a_i is a regional specific fixed effect, a_t is a time effect. The policy variables of any of the p programmes are specified as $g_{i,t}^{j}$ representing the accommodation ratios

$$g_{i,t} = \frac{r_{j,i,t}}{\sum_{j=1}^{p} r_{j,i,t} + u_{i,t}} \tag{8}$$

of the p different programmes of ALMP in the regions i at time t. The specification of the programme participation in accommodation ratios measures the participation in any of the programmes of ALMP as a percentage of the total proportion of job–seekers without employment (i.e. unemployed and programme participants), $e_{i,t}$ is the error term.

If ALMP had an effect on aggregate employment, either one of the policy variables should indicate a negative impact on the non–employment rate. Assuming that ALMP does not affect the outcome instantaneously, the model is also tested with lagged variables of the ALMP accommodation ratios, too.

In the above–mentioned specification of the outcome equation, severe problems of endogeneity arise. First and foremost, the allocation of programme participation in ALMP in a region depends on the level of unemployment in this region, so that a problem of reverse causality exists. The government certainly reacts to the unemployment problems at the regional level, so that the estimates of the impact of ALMP on employment are subject to simultaneity bias, especially if it is measured as a percentage of the total labour force. Taking the accommodation ratios (i.e. measuring programme participation as a percentage of all non employment in a region) might be less endogenous (Calmfors, Skedinger 1994) as a priori it is not clear whether increases in unemployment should be expected to lead to more or less than proportional increases of programme participation as a share of all job–seekers. However, taking the accommodation ratio might not be sufficient for controlling on endogeneity: As most of the participants in any ALMP are recruited from the pool of unemployed job–seekers, the accommodation ratio in the region i at time t reflects the previous unemployment problem and remains endogenous. In an econometric analysis, the simultaneity problem leads to biased estimators of the effect of ALMP due to $Cov\left(g_{i,t-t}^{j}, e\right) \neq 0$ at any point in time t-t.

As the correlation between the amount of ALMP allocated to a region and the outcome can be considered to be positive, i.e. ALMP is higher in regions with a high level of unemployment, the estimated coefficient is likely to be upward biased. If we suppose for example the impact of ALMP on unemployment to be negative, the estimated coefficient might be higher than its true parameter if the endogeneity is not controlled for sufficiently in the econometric analysis, and hence, the effect of ALMP may be underestimated.

4.4.2.2 Modelling ALMP allocation

The accommodation ratio of ALMP is endogenous because it depends on the level of non–employment in a region (the political system "answers" to unemployment by ALMP allocation). The success criterion $(u+r)$ then determines the allocation of ALMP with Ω and is supposed to have a lagged influence on the level of ALMP. Besides, there exists an autonomous policy choice for ALMP, which depends on exogenous covariates Z set by political planners in regions and the conditions of implementation in the employment service offices. The allocation of any of the ALMP programmes then would depend on $(u+r)$ and Z as follows:

$$g_{i,t}^{j} = g\left(pZ_{i,t-t}^{j}, \Omega[u+r]_{i,t-t}\right) \tag{9}$$

with $g_{i,t}^{j}$ the accommodation ratios of any of the j=1,…p ALMP programmes, $t \in \{0,1,\ldots,t-1\}$, $g(.)$ an unspecified function of the ALMP allocation as the policy response to the local unemployment problem and π showing the amount of ALMP allocation depending on exogenous covariates. Taking the policy response of ALMP into consideration, the estimation of the empirical model from equation (7) yields:

$$(u+r)_{i,t} = a_i + a_t + \sum_{j=1}^{p} b_{j,t-t}\left(g\left(pZ^{j}_{i,t-t}, \Omega[u+r]_{i,t-t}\right)\right) + e_{i,t} \qquad (10)$$

The estimated coefficients of (7) would not only show the effect of the exogenous variation of ALMP $p\ Z^{j}_{i,t-t}$ (the autonomous policy choice), but also the effect that the level of ALMP always depends on $\Omega[u+r]_{i,t-t}$, the level of the outcome variable. Therefore, a substantial part of the variation of ALMP is caused by a variation in $\Omega[u+r]$. As Ω is supposed to be different from zero, any coefficient estimate $\hat{b}_{p,t-t}$ without taking into consideration the endogeneity will report a biased ALMP effect: Effectively, one is correlating $(u+r)$ with itself.

With respect to this, we implement two different solutions in order to control for endogeneity in the context of the estimation of an ALMP effect on the extended unemployment rate:

- We estimate (7) as a fixed effects model, in order to remove unobserved, however stable regional specific effects. This model considers the regional policy response of the ALMP allocation, if we assume it to depend on the variation of $\Omega[u+r]$ within a region in a *stable* relationship. This could e.g. be justified e.g. by a fixed formula implemented by the administration. The fixed effects model the removes a stable allocation formula as long as it is unchanging, which is plausible for the short–term period we observe. Estimation by fixed effects would then lead to estimators of $\hat{b}_{k,t-t}$ that only describe the influence of the stable, exogenous component of ALMP – irrespective the variation of $\Omega[u+r]$ – on the regional unemployment outcome. In this sense, fixed effect models lead to unbiased estimators.
- Another identification strategy applied in the following is the explicit modelling of the exogenous component of any of the ALMP programmes $p\ Z^{k}_{i,t-t}$ by two–stage least squares estimation with panel data. Here, we assume regional indicator variables such as the workload of the regional employment office, regional policy variables and long–term structural variables of the region to be influential on the allocation of ALMP. We model the influence of these indicator variables on the ALMP allocation in the first step and the estimate in the second step the influence of the predicted "exogenous" component of ALMP on the outcome variable. However, the instrumental variables approach critical depends on the availability of informative covariates that serve as instruments and capture the endogeneity in an appropriate way, which is however difficult on the basis of regional data, because many structural variables only exist on more aggregated units of observations. ALMP implementation variables provided by of the implementing institutions (i.e. public and private training centres) of the programmes – which would be the most promising instrument – does not exist in any of the regions. The instrumental variables estimates reported in the next section therefore should be understood mainly as explorative. Basically, the instrumenting strategy

is doomed to failure due to the lack of informative data for both countries. In the context of the dynamic modelling of the ALMP outcomes on employment or matching, the instrumental variable approach could not be implemented because the policy variables depend on the election cycles (up to 5 years): In many of the cases, the political variables offer to few variation and are basically omitted in fixed effects models due to missing variation.

The estimation of the matching function – based on quarterly data in both countries – therefore could not be achieved by applying the explicit modelling of the ALMP allocation as a first step of an IV estimate and refers only to static or dynamic panel estimators.

4.4.2.3 Dynamic model: Matching

The second approach implemented here is the estimation of the regional matching function as described in section 4.3.2 where we regress the outflow rate from unemployment on a constant term, the accommodation ratios of ALMP and an indicator of vacancies over unemployment. As monthly data are available for both countries, Germany and the UK, we are allowed to estimate the effects of ALMP on matching with dynamic models of a quarterly frequency. Of course, the dynamic modelling can be regarded as preferable compared to the static estimation of ALMP effect on the regional extended unemployment rate as shown above: It (i) models the employment dynamics more appropriately and (ii) takes into account that hirings adjust slowly to their long–term level, which can be considered by including the lagged levels of the depending variables into the model. However, we cannot instrument for the exogenous component of ALMP any longer, as there are too few informative covariates at the disaggregated level of employment offices for quarters. Therefore, we had to restrict the macroeconomic analysis of the matching function according to (6) to (i) static and (ii) dynamic fixed effects panel models.

Then, the dynamic model (6) becomes:

$$\ln\left(\frac{h}{UE}\right)_{i,t} = a_i + a_t + a_q\left(\frac{h}{UE}\right)_{i,t-t} + \sum_{j=1}^{p} b_{j,t-t} g^{j}_{i,t-t} + d\ln\left(\frac{V}{UE}\right)_{i,t} + e_{i,t} \qquad (11)$$

where $t \in \{0,1,\dots 5\}$ and $(h/UE)_{i,t-t}$, the lagged endogenous variable, is included. a_i and $e_{i,t}$ are assumed to be independent for each i over all t. Note that we now also consider further lags of the ALMP accommodation rations $g^{j}_{i,t-t}$ in the model, because ALMP usually lasts significantly longer than one quarter, i.e. the outcomes of the policies should be modelled for at least one year relative to the policy intervention because many programmes last up to that duration (or might last even longer). The policy effect of ALMP then basically consists of the cumulative effect of the coefficients of the lagged policy variables.

The basic problem of dynamic panel models consists of a correlation of a right hand side regressor with the individual effect (Baltagi 2001: 129 ff.). As before, the parameter estimates of the dynamic panel model are again biased due to endogeneity: Since $h/UE_{i,t}$ depends on a_i, then $h/UE_{i,t-1}$ is also a function of a_i, too, and $h/UE_{i,t-1}$ (a right hand regressor) is correlated with the error term. Any OLS or within–transformation estimate of (11) will be biased.

In the case of fixed effect estimators, the within–transformation wipes out the individual fixed effect a_i,. However $(h/UE_{i,t-1} - \overline{h/UE_{i,-1}})$ with $\overline{h/UE}_{i-1} = \sum_{t=2}^{T} \overline{h/UE}_{i,-1}/(T-1)$ will still be correlated with $(e_{i,t} - \bar{e}_i)$ even if the $e_{i,t}$ are not serially correlated, because $h/UE_{i,t-1}$ is correlated with $\bar{e}_i$ by construction (ibid. 130): $e_{i,t-1}$ is correlated with $h/UE_{i,t-1}$. The within estimator will be biased of $s\,(1/T)$, but would be consistent as long as T being large. However, for a typical panel where N is large and T is fixed, the within estimator is biased and inconsistent. Only if $T \to \infty$ the within estimator will be consistent for a_q and any of the b. The study by Arellano, Bond (1991) states that instruments in order to overcome the endogeneity problem in dynamic panel data estimates can be obtained if one utilises the orthogonality conditions between the lagged values of $h/UE_{i,t}$ and the disturbances $e_{i,t}$.

The final specification of the empirical modelling of the matching function includes five lagged levels of the dependent variable, so that

$$\begin{aligned} \ln\left(\frac{h}{UE}\right)_{i,t} &= a_i + a_t + a_1\left(\frac{h}{UE}\right)_{i,t-1} + a_2\left(\frac{h}{UE}\right)_{i,t-2} + a_3\left(\frac{h}{UE}\right)_{i,t-3} + a_4\left(\frac{h}{UE}\right)_{i,t-4} + a_5\left(\frac{h}{UE}\right)_{i,t-5} \\ &\quad + \sum_{j=1}^{p} b_{j,t-t}\, g^{j}_{i,t-t} + d\ln\left(\frac{V}{UE}\right)_{i,t} + e_{i,t} \end{aligned} \tag{12}$$

This model can be estimated in first differences earliest for the period 7 we observe. Then, it becomes

$$\begin{aligned} \ln\left(\frac{h}{UE}\right)_{i,7} - \ln\left(\frac{h}{UE}\right)_{i,6} &= a_{t,7} - a_{t,6} + a_1\left(\ln\left(\frac{h}{UE}\right)_{i,6} - \ln\left(\frac{h}{UE}\right)_{i,5}\right) + a_2\left(\ln\left(\frac{h}{UE}\right)_{i,5} - \ln\left(\frac{h}{UE}\right)_{i,4}\right) \\ &\quad + a_3\left(\ln\left(\frac{h}{UE}\right)_{i,4} - \ln\left(\frac{h}{UE}\right)_{i,3}\right) + a_4\left(\ln\left(\frac{h}{UE}\right)_{i,3} - \ln\left(\frac{h}{UE}\right)_{i,2}\right) \\ &\quad + a_5\left(\ln\left(\frac{h}{UE}\right)_{i,2} - \ln\left(\frac{h}{UE}\right)_{i,1}\right) + \sum_{j=1}^{p} b_{j,t-t}\, g^{j}_{i,7-t} - g^{j}_{i,6-t} \\ &\quad + d\left(\ln\left(\frac{V}{UE}\right)_{i,7} - \ln\left(\frac{V}{UE}\right)_{i,6}\right) + e_{i,7} - e_{i,6} \end{aligned} \tag{13}$$

As the error structure $e_{i,7} - e_{i,6}$ is by construction not correlated with

$$\ln\left(\frac{h}{UE}\right)_{i,1}, \ldots, \ln\left(\frac{h}{UE}\right)_{i,5},$$

valid instruments for the lagged endogenous variables are provided by these lagged levels.

For period 8, the second period in which we can observe the relationship, the lagged levels $\ln(h/UE_{i,1}),...\ln(h/UE_{i,6})$ serve as valid instruments. For period 9, the third period in which we can estimate the mode, we again add a further lag of the dependent variable to the set of instruments, so that $\ln(h/UE_{i,1}),...\ln(h/UE_{i,7})$ serve as instruments. For the following periods, all levels of the dependent variables up to the time *t*–2 serve as instruments for the differences in the lagged dependent variables as long as the $e_{i,t}$ are not serially correlated. This needs to be tested for in the empirical analysis[8].

Additional instruments could in principle be obtained by using the lagged levels of the other covariates

$$g^j_{i,1},...g^j_{i,5} \text{ and}$$

$$\ln\left(\frac{V}{UE}\right)_{i,1},...\ln\left(\frac{V}{UE}\right)_{i,5}$$

as suggested in Arellano, Bond (1991). We however decided not to use the other covariates as instruments because of the endogeneity problems of the ALMP allocation discussed above.

One could argue that a number of important regional characteristics are omitted in this specification, especially the capital stock and other public policies as the tax rates and public sector employment. However, regional variation is covered by fixed effects and can be assumed to be time constant for the short period we observe (98–02).

4.4.3 Estimation and results

4.4.3.1 Germany

The empirical analysis of ALMP in Germany is based on data of the local employment office districts for either the years 98– 01 or the quarterly data of the first quarter 00 to the second quarter 02. Table 4.4 shows the basic characteristics of the annual data, indicating that over the period 98–01 considered here, the extended unemployment rate $u+r$ varies between 3.4% (Freising near Munich) and 18.5% (Gelsenkirchen) in West Germany with a mean of almost 10% of the civilian labour force. Extended unemployment comprises both unemployment and programme participation as a percentage of the labour force. We observe vacancy rates above the average in southern Germany (especially in the suburban districts near Munich and Stuttgart): over 60% in Freising and between 50% and 60% in Munich and Landshut.

8 As the $\Delta e_{i,t}$ are differences of serially uncorrelated errors, $E(\Delta e_{i,t}, \Delta e_{i,t-1})$ might not be zero. Following Arellano, Bond (1991: 281), this first order autocorrelation does not imply that the estimates are inconsistent. However, the consistency of the estimator hinges on the assumption that $E(\Delta e_{i,t}, \Delta e_{i,t-2}) = 0$. The related tests of autocorrelation of first and second order (AR[1]–test and AR[2]–test) are reported in tables 4.9 and 4.12.

For the estimation of the regional matching function, we refer to quarterly data, which again shows the huge regional variation in Germany: With a maximum of 33.6% of all extended unemployment, the Bavarian region Passau shows the highest exit rate from unemployment in the third quarter of 01, whereas Gelsenkirchen had the lowest rate of 8.23% of all extended unemployment in the fourth quarter of 01.

Accommodation ratios for the most important ALMP programmes differ significantly across the regions. Employment office districts in the north implement relatively more job creation programmes while local offices in the south usually focus on further training. Descriptive statistics for other regional indicators can be obtained from table 4.4 also displaying the basic information about the regional labour market structure, particularly the age structure of unemployment (unemployed below the age of 25 as percentage of all regional unemployment and the unemployed aged above 55 as percentage of all regional unemployment. Other covariates used in the first step of the IV estimates of the ALMP outcomes on regional extended unemployment are the ethnic structure of unemployment, whether individuals were in employment in the service sector before and the age and ethnic structure of the total labour force (namely the share of the labour force aged below 25 as percentage of all, the share of women and the share of foreigners as percentage of the total labour force).

The descriptive statistics are based on lagged values because the variables are included as lags in the IV estimation. Additionally, there are indicators for the good organisation of a regional employment office (the number of unanswered requests by unemployed as a percentage of all extended unemployed), the degree of urbanisation (indicators for the population density) and finally terms of regional population density (medium density, city centres and rural areas) interacted with a share of the social democratic party in the regional councils above 40% of all voters to time t–2. These variables are included in the IV estimates in order to identify the exogenous variation of ALMP with policy variables.

Table 4.4 Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.
regional extended unemployment (u+ r)	9.9911	3.0000	3.3675	18.5129
Exits from unemployment (without exits to ALMP programmes as % of extended unemployment) h / UE	76.9017	32.8575	23.1911	272.4718
Vacancies as % of extended unemployment (V / UE)	15.9682	9.4722	3.1397	71.3274
Accommodation ratio FBW γ^{fbw}	7.4321	1.5308	2.7662	14.2374
Accommodation ratio ABM γ^{abm}	1.7339	0.9640	0.1397	5.8671
Accommodation ratio SAM γ^{sam}	0.3433	0.5012	0.0000	3.0498
Unemployed below the age of 25 as % of all unemployment (lag –1)	12.4804	1.8304	6.5847	18.3296
Unemployed above 55 as % of all regional unemployment (lag –1)	22.3561	4.2904	11.0323	38.5475
Share of non–German unemployed as % of all regional unemployment (lag –1)	5.0648	3.6149	0.0329	22.7204
Share of non–German female unemployed as % of all regional unemployment (lag –1)	2.9782	2.2283	0.0205	14.2468
Unemployment of service sector occupations as % of all regional unemployment (lag –1)	14.9349	2.7351	9.2035	26.0338
Share of the labour force aged below 25 as % of total labour force (lag –1)	12.7683	1.9906	8.5610	19.3276
Share of the female labour force as % of total labour force (lag –1)	43.6704	2.1026	37.2093	49.3413
Share of the non–German labour force 25 as % of total labour force (lag –1)	7.7899	3.9929	1.7778	22.0273
Share of unanswered requests as % of total	5.9500	3.1611	0.8864	22.7498
City centres	0.5213	0.4998	0.0000	1.0000
Rural areas	0.2030	0.4024	0.0000	1.0000
Medium population density interacted with share of left party above 40 % (lag –2)	0.0934	0.2911	0.0000	1.0000
City centre interacted with share of left party above 40 % (lag –2)	0.3286	0.4700	0.0000	1.0000
Rural area interacted with share of left party above 40 % (lag –2)	0.0248	0.1557	0.0000	1.0000

As aforementioned, we account for the endogeneity ALMP by estimating IV. The IV approach chosen here makes use of variables that model the decision of policy makers on the regional level and/or the local capacity for the implementation of ALMP based on regional characteristics, especially the ethnic structure and the age groups of unemployment and the labour force, the degree of urbanisation and the political majorities on the local level. The instruments are justified by institutional reasons. The structural problems of a regional labour market can be supposed to influence the implementation of policies in the long–run independently form the short–term unemployment figure. The selected variables can be motivated as follows:

- The age structure of the labour force and unemployment indicates the regional demographic structure, i.e. if the regional labour market has to adjust to either more or less tight supply problems in the long run.

- The share of unemployed who were formerly working in the service sector is taken into consideration in order to show the degree of tertiarisation of the regional labour market
- The share of unemployed who are non–German citizens offers information about the regional need to adjust language and occupational skills.
- Information on the regional rate of unanswered requests by unemployed and participants in ALMP programmes as a percentage of all extended unemployment indicates how efficient the local employment office implements ALMP. This variable especially shows the capacity of the local employment office, the availability of staff and the regional capacity to implement ALMP.
- Urban and rural areas are supposed to follow a different implementation strategy of ALMP: In urban areas, more carriers of ALMP exist supplying a greater variety of ALMP programmes that in the long–run is decisive for the ALMP allocation. We assume the supply structure of ALMP in a region to be most crucial for the exogenous variation of ALMP.
- Political planning on the regional level is important for the exogenous variation of ALMP because the social democratic party is supposed to be more in favour of demand oriented ALMP (Job Creation) whereas the right wing party CDU prefers supply oriented ALMP (i.e. FbW). However the political process differs between the centres of cities and rural areas, so that political majorities (an SPD share of seats in *Kreistagen* of more than 40%) is interacted with information about the regional population density: The politics are supposed to be different in medium urbanised areas compared to rural areas and city centres.

The first step estimations of a 2 SLS dummy variable regression can be found in table 4.A1 in the Appendix. Especially the regional labour force characteristics turn out to be significant (share of women and non–German unemployed as percentage of all unemployment) as well as the political planning in regions: The estimated coefficients show that strong left wing parties at the regional level implement significantly less further training in medium urbanised areas and city centres and implement significantly more job creation schemes in the centres compared to the base category of weaker left wing party shares – the allocation of structural adjustment programmes however is not affected by any of the variables indicating the regional political majorities[9]. The first stage of the IV–estimates instruments for the level of the accommodation ratios of ALMP and includes dummy variables for each region and period. The two stage estimator (table 4.6) implements least squares dummy variables (LSDV) for estimating the model and includes dummies for each region in the first stage as suggested by Büttner, Prey (1998: 403).

The results of the static fixed effects model show that further training has a reducing effect on the extended level of unemployment as well as the implementation of structural adjustment programmes. However, the effect disappears after one year: The coefficients of the lagged variables are

9 The t–statistics of all the estimations reported in this paper are based on heteroskedasticity consistent standard errors. Here – as in all subsequent estimations – the p–value of the chi–square test indicates the significance of the null–hypothesis that all coefficients have a joint influence of zero. It is rejected in any of the specifications.

insignificant for either the further training or the structural adjustment programmes. Except for the short–term static fixed effects model, the coefficients of ALMP programmes are insignificant for all specifications of the outcome equation with respect to the level of extended unemployment (tables 4.5 and 4.6).

Table 4.5 Effects of ALMP on extended unemployment (FE)

u + r	Coef.	t	Coef.	t
γ^{fbw}	–0.107	–3.330		
γ^{abm}	0.024	0.420		
γ^{sam}	–0.271	–2.060		
$\gamma^{fbw}{}_{(t-1)}$			–0.040	–1.090
$\gamma^{abm}{}_{(t-1)}$			0.100	1.610
$\gamma^{sam}{}_{(t-1)}$			0.041	0.280
_cons	7.982	24.260	16.031	23.050
N		564		423
(df) Wald–Test		(146) 1112.87		(145) 1623.07
P–value		.00		.00

Table 4.6 Effects of ALMP on extended unemployment (IV)

u + r	Coef.	t	Coef.	t
γ^{fbw}	0.117	1.300		
γ^{abm}	0.013	0.080		
γ^{sam}	0.351	0.970		
$\gamma^{fbw}{}_{(t-1)}$			0.050	0.610
$\gamma^{abm}{}_{(t-1)}$			0.212	1.130
$\gamma^{sam}{}_{(t-1)}$			0.410	0.970
_cons	16.572	16.490	8.552	6.670
N		564		423
(df) Wald–Test		(146) 1000.35		(145) 3636.95
P–value		.00		.00

The estimation of the matching function is based on quarterly data for the period first quarter 00 to the second quarter 02. Table 4.7 provides a short descriptive overview of the data and again indicates the wide variation of the ALMP accommodation across regions.

Table 4.7 Descriptive Statistics – quarterly data (3/00 – 6/02)

Variable	Mean	Std. Dev.	Min.	Max.
Accommodation ratio ABM (γ^{abm})	1.48	0.84	0.06	6.13
Accommodation ratio SAM (γ^{sam})	0.38	0.56	0.00	4.81
Accommodation ratio FBW (γ^{fbw})	7.35	1.51	3.26	15.42
Unemployment (entry)	2,688.46	1,525.27	820.67	12,458.67
Unemployment (stock)	16,882.61	10,994.49	3,343.00	80,536.67
Extended unemployment (stock)	19,845.72	12,769.52	4,000.00	96,452.67
Total vacancies (stock)	3,068.18	2,768.41	516.67	29,772.00
Civilian Labour Force (total)	227,595.60	137,546.40	70,226.00	1,159,238.00
Unemployment rate (% of the civilian labour force)	8.79	2.78	2.82	17.88
Unemployment exit rate (% of the extended unemployment)	14.68	3.40	6.27	33.65
Vacancy rate (% of extended unemployment)	17.08	10.98	3.71	111.42

In the following, we report the estimated coefficients of the ALMP effects on regional mismatch, which have been estimate either by fixed effects panels models or by the dynamic models applying the estimator by Arellano, Bond (1991). In the short term, all programmes significantly increase the outflows from unemployment. That is surprising because we control for the bookkeeping effect of ALMP programmes with respect to the outcome variables. If we simultaneously estimate the effects of ALMP programmes implemented in the same and in earlier periods in order to estimate the cumulated effect of ALMP, we find negative effects of the lagged ALMP variables which in all cases out weight the positive effects of the ALMP implemented in the quarter, in which we measure the outcomes. If we consider further lags of the ALMP variables, this results becomes more emphasised: Practically all positive effects of ALMP in the short run are counteracted by the long–run effects, so that the cumulated ALMP effect does not affect significantly the regional matching. A closer look at the coefficients e.g. in the specification including ALMP variables for up to three lags shows that the coefficients for further training, which are jointly significant, sum up to exactly 0.01 in the long run, which indicates that if we increase the proportion of further training as percentage of all regional extended unemployment by 1 percentage point, the hiring rate would increase exactly by 0.01%. The results for structural adjustment programmes are relatively better with cumulated coefficients amounting to 0.03%, and they are better for the job creation programme, too (0.1%).

Table 4.8 Effects of ALMP on matching (Fixed effects)

log (h / UE)	Coef.	t	Coef.	t	Coef.	t	Coef.	t
T	0.20	0.85	0.00	–3.14	0.00	–0.89	0.00	0.67
log (V / UE)	0.20	13.99	0.19	12.38	0.19	11.47	0.19	11.26
γ^{abm}	0.03	2.95	0.06	5.96	0.06	4.45	0.06	4.15
γ^{sam}	0.12	6.49	0.15	9.66	0.17	9.17	0.13	5.80
γ^{fbw}	0.01	4.26	0.01	3.89	0.02	3.66	0.01	3.24
$\gamma^{abm}{}_{(t-1)}$			–0.07	–7.17	–0.07	–4.95	–0.03	–1.55
$\gamma^{sam}{}_{(t-1)}$			–0.13	–8.46	–0.13	–6.63	–0.10	–3.76
$\gamma^{fbw}{}_{(t-1)}$			–0.01	–4.26	–0.01	–2.70	–0.01	–1.41
$\gamma^{abm}{}_{(t-2)}$					0.01	1.36	–0.03	–2.10
$\gamma^{sam}{}_{(t-2)}$					0.02	1.08	0.00	0.04
$\gamma^{fbw}{}_{(t-2)}$					0.00	0.74	0.00	–0.68
$\gamma^{abm}{}_{(t-3)}$							0.04	2.90
$\gamma^{sam}{}_{(t-3)}$							0.03	1.31
$\gamma^{fbw}{}_{(t-3)}$							0.01	2.83
Number of obs.		1410.00		1269.00		1128.00		987.00
(df) Wald–Test		(145) 9664.25		(148) 10244.56		(151) 7989.41		(154) 8303.68
P–value		.00		.00		.00		.00

In further lags of the dependent variable are included into the model and are estimated by the dynamic panel model, the positive macroeconomic effects of the job creation scheme and the structural adjustments fully disappear in the long–run, for ABM , the effect amounts to –0.01% and for further training to +0.02%. The significance of the estimated employment effects critically depends on the specification. The coefficients themselves are very small in general.

Table 4.9 Effects of ALMP on matching (Dynamic Panel)

log (h / UE)	Coef.	t	Coef.	t	Coef.	t	Coef.	t
t	–0.02	–5.11	–0.02	–5.03	–0.03	–5.00	–0.02	–3.58
log (V / UE)	0.13	4.71	0.12	4.49	0.11	4.10	0.11	4.03
log (h / UE) $_{(t-1)}$	–0.67	–8.06	–0.70	–7.80	–0.73	–7.83	–0.70	–7.71
log (h / UE) $_{(t-2)}$	–0.43	–7.10	–0.43	–6.82	–0.43	–6.52	–0.41	–6.35
log (h / UE) $_{(t-3)}$	–0.40	–7.20	–0.41	–7.19	–0.42	–7.03	–0.43	–7.79
log (h / UE) $_{(t-4)}$	0.19	3.52	0.17	3.01	0.17	2.97	0.17	2.99
log (h / UE) $_{(t-5)}$	0.17	2.55	0.19	2.87	0.19	2.75	0.15	2.18
γ^{abm}	0.02	1.28	0.01	1.04	0.00	0.16	0.01	0.43
γ^{sam}	0.06	3.92	0.06	4.15	0.05	2.14	0.04	1.97
γ^{fbw}	0.00	0.85	0.00	0.22	0.00	0.26	0.00	0.68
γ^{abm} $_{(t-1)}$			–0.02	–1.45	0.00	–0.18	0.00	0.15
γ^{sam} $_{(t-1)}$			–0.03	–1.31	–0.02	–1.09	–0.01	–0.40
γ^{fbw} $_{(t-1)}$			0.01	2.08	0.01	1.88	0.01	2.57
γ^{abm} $_{(t-2)}$					–0.04	–2.77	–0.03	–2.34
γ^{sam} $_{(t-2)}$					0.00	–0.18	0.00	–0.17
γ^{fbw} $_{(t-2)}$							0.00	–0.98
γ^{abm} $_{(t-3)}$							0.01	0.52
γ^{sam} $_{(t-3)}$							0.02	0.66
γ^{fbw} $_{(t-3)}$							0.01	3.14
Number of obs		564.00		564.00		564.00		564.00
Number of groups		141.00		141.00		141.00		141.00
(df) Wald–Test		(10) 1585.40		(13) 1842.18		(16)2130.44		(19) 2466.93
P–value		.00		.00		.00		.00
AR(1)–test (H0 no)		–3.16		–2.38		–2.12		–2.34
AR(2)–test (H0 no)		0.68		0.43		–0.02		0.02

4.4.4.2 UK

The basic characteristics of the empirical data for the UK are summarised in table 4.10. The programmes of the NDYP have much lower participation figures than the German ALMP programmes: With an average accommodation of 0.5%, the participation in the employment option of NDYP is much lower than participation in the big German programmes (further training has participant stocks of 7.4% of the regional extended unemployment). However, there are regions where the participation in NDYP options reach higher levels, for example Keswick where in the third quarter 98 the accommodation ratio of the employment option peaks with 5.8%. On average the training option is the most important programme with an average participation of 0.88% of the regional extended unemployment, followed by the employment option. The ETF and VS options are less important: Participation here has an average value of 0.4% and 0.36% of the regional extended unemployment. Over the period of observation, this picture does not change very much: The training option is the most important programme since the start of the NDYP, however reaches its maximum already at the beginning of the NDYP in the fourth quarter 98 with an average participation figure of roughly 1.3% of the regional extended unemployment. After 98, the participation in the training option decreases to 0.8% in the second quarter 01. The employment option decreases only slightly over time, from an accommodation ratio of almost 0.8% to 0.6% in 01.

Table 4.10 Descriptive Statistics – quarterly data (6/98 – 6/01)

Variable	Mean	Std. Dev.	Min.	Max.
γ^{eo}	0.5800	0.5250	0.0000	5.8201
γ^{to}	0.8814	0.7022	0.0000	4.9645
γ^{vs}	0.4037	0.3729	0.0000	3.1987
γ^{et}	0.3621	0.4090	0.0000	4.8561
h / UE	26.3935	7.2018	9.0596	71.2439
V / UE	78.2135	56.0496	0.6501	734.5795

The estimations of the fixed effects model are reported in table 4.11. The effects of the NDYP on the regional outflow rate to employment are again estimated by including different lags of the ALMP variables in the outcome equation. In the first column we do find significant effects of the employment and the voluntary sector option in the same quarter, indicating that these policies significantly increased the outflow from unemployment. Even in the long–run, if we include further lags into the estimated form, these effects do not disappear and the cumulated effects remain positive, however very tiny.

Again, the coefficients can be interpreted as elasticities, meaning that a 1% increase in the accommodation ratio of any the training option would increase the outflow rate from unemployment by a fraction of 0.01% (table 4.11, cumulated coefficients) and to the same by the voluntary sector work and the employment option.

Table 4.11 Effects of ALMP on matching (Fixed effects)

log (h / UE)	Coef.	t	Coef.	t	Coef.	t	Coef.	t
t	0.01	15.51	0.01	16.01	0.01	12.41	0.01	10.19
log (V / UE)	0.28	20.93	0.27	18.80	0.24	16.27	0.25	14.85
γ^{eo}	0.01	4.54	0.03	8.05	0.03	7.31	0.03	6.61
γ^{to}	0.00	–0.05	0.00	2.45	0.01	5.91	0.01	5.00
γ^{vs}	0.01	3.05	0.02	4.91	0.02	5.45	0.02	4.85
γ^{et}	0.00	0.16	0.00	0.74	0.01	1.49	0.01	1.47
$\gamma^{eo}_{(t-1)}$			–0.03	–10.40	–0.03	–8.62	–0.03	–5.89
$\gamma^{to}_{(t-1)}$			0.00	–1.53	–0.02	–9.07	–0.03	–9.45
$\gamma^{vs}_{(t-1)}$			–0.01	–1.86	–0.01	–2.55	–0.01	–2.26
$\gamma^{et}_{(t-1)}$			0.00	0.33	–0.01	–2.20	–0.01	–1.99
$\gamma^{eo}_{(t-2)}$					0.01	1.83	–0.01	–1.91
$\gamma^{to}_{(t-2)}$					0.02	11.52	0.03	11.48
$\gamma^{vs}_{(t-2)}$					0.00	0.67	0.00	–0.07
$\gamma^{et}_{(t-2)}$					0.01	2.54	0.01	1.65
$\gamma^{eo}_{(t-3)}$							0.02	5.20
$\gamma^{to}_{(t-3)}$							–0.01	–4.88
$\gamma^{vs}_{(t-3)}$							0.00	0.49
$\gamma^{et}_{(t-3)}$							0.00	0.14
Number of obs.		3876		3599		3322		3045
(df) Wald–Test		(282) 16821.3		(286) 15375.36		(290) 16132.7		(294) 14514.78
P–value		.00		.00		.00		.00

Table 4.12 finally reports the results of the dynamic panel estimations. Allowing for five lags of the dependent variables in the response for ALMP in order to model the long–term adjustment of the outflow rate to its long–term level and including lags of the ALMP accommodation ratios into the estimation finally shows that the cumulated effect estimated in the static model amounting to 0.01% is exactly found in this specification, too, however only in the short run. In the long–run, there is no effect at all.

Table 4.12 Effects of ALMP on matching (Dynamic Panel)

log (h / UE)	Coef.	t	Coef.	t	Coef.	t	Coef.	t
t	0.04	10.89	0.04	10.23	0.04	10.94	0.04	11.41
log (V / UE)	0.17	11.44	0.17	11.47	0.16	10.89	0.16	10.94
log (h / UE) $_{(t-1)}$	–0.27	–6.43	–0.28	–6.09	–0.30	–6.66	–0.30	–6.50
log (h / UE) $_{(t-2)}$	–0.51	–16.77	–0.52	–15.69	–0.55	–16.34	–0.55	–16.04
log (h / UE) $_{(t-3)}$	–0.22	–6.05	–0.23	–6.22	–0.25	–6.63	–0.25	–6.67
log (h / UE) $_{(t-4)}$	0.29	8.99	0.29	8.64	0.26	7.72	0.26	8.09
log (h / UE) $_{(t-5)}$	–0.20	–6.41	–0.20	–6.53	–0.18	–5.89	–0.19	–5.92
γ^{eo}	0.01	3.53	0.01	3.60	0.01	3.15	0.01	2.86
γ^{to}	0.01	3.30	0.01	3.40	0.01	4.27	0.01	3.78
γ^{vs}	0.00	1.41	0.00	1.20	0.01	1.62	0.00	1.46
γ^{et}	0.01	2.47	0.01	2.26	0.01	2.75	0.01	2.69
$\gamma^{eo}{}_{(t-1)}$			0.00	–0.83	0.00	–0.19	0.00	–0.51
$\gamma^{to}{}_{(t-1)}$			0.00	0.17	0.00	–1.88	0.00	–2.06
$\gamma^{vs}{}_{(t-1)}$			0.00	0.97	0.00	1.13	0.00	0.98
$\gamma^{et}{}_{(t-1)}$			0.00	–0.72	0.00	–0.87	0.00	–0.52
$\gamma^{eo}{}_{(t-2)}$					0.00	–1.42	0.00	–0.68
$\gamma^{to}{}_{(t-2)}$					0.01	4.33	0.01	4.02
$\gamma^{vs}{}_{(t-2)}$					0.00	0.33	0.00	0.24
$\gamma^{et}{}_{(t-2)}$					0.00	1.24	0.00	1.08
$\gamma^{eo}{}_{(t-3)}$							0.00	–0.67
$\gamma^{to}{}_{(t-3)}$							0.00	–0.74
$\gamma^{vs}{}_{(t-3)}$							0.00	0.01
$\gamma^{et}{}_{(t-3)}$							0.00	1.10
Number of obs		2213.00		2213.00		2213.00		2213.00
Number of groups		277.00		277.00		277.00		277.00
(df) Wald–Test		(11) 8533.83		(15) 9677.07		(19) 10855.46		(23) 11409.83
P–value		.00		.00		.00		.00
AR(1)–test (H0 no)		–6.58		–6.12		–5.79		–5.75
AR(2)–test (H0 no)		–0.1		0.02		1.31		1.45

4.5 Conclusion

This paper describes the institutional design of ALMP programmes in details and compares it with the outcomes theoretically expected. In theory, no clear effects can be derived. An empirical analysis of the macroeconomic effects of ALMP on the basis of regional aggregate data for German local employment office districts and British travel to work–areas brought no evidence for macroeconomic effects in the short or in the long–run:

- Both countries dispose over a variety of ALMP programmes among greater policy areas that differ with respect to duration, the way of integration into regular employment and the level of benefits that are granted to the participants. Training programmes are more important in both Germany and the UK, and they are supposed to have a positive impact because of a structural improvement of the labour force, the creation of more transparency in the matching of unemployed and vacancies. On the other hand, the levels of benefit in further training programmes in both countries are lower than for subsidised employment, so that wage increasing effects are supposed to be lower for training than for Job Creation and direct wage subsidies. The main differences between Germany and the UK are the participation figures: the NDYP in the UK is a programmes targeted to a restricted group of participants with durations usually less than one year. German ALMP programmes are targeted to unemployed later in the life cycle. The institutional analysis could make clear that the level of benefits associated with programmes and the duration is much more generous in Germany than in the UK.
- The conclusion of the theoretical part indicates that the effects of ALMP on the macroeconomic level are uncertain, because positive and negative effects occur simultaneously: On the one hand, the participation in ALMP programmes increases the participation on the labour market and decreases possible drop–outs. This effect is certainly positive for aggregate employment because a more numerous workforce leads to more competition between insiders and outsiders and unambiguously reduces the bargaining power of unions and their wage claims. On the other hand, a high level of benefit – especially in the German case – leads to a higher alternative wage in the case of being laid off and increases the wage claims of unions. Finally the effects of ALMP are usually supposed to be positive with respect to matching.
- The empirical analysis applies regional aggregate data for the UK and Germany in order to assess the macroeconomic impact of ALMP. To control for the endogeneity of ALMP, it is necessary to use instruments which have an impact on the extent of ALMP and which at the same time do not influence directly the state of the labour market. However, in both countries necessary complementary information on regional aggregates are hardly available. The results are mixed:

1. In Germany, we find a reducing effect of further training on the extended unemployment rate in the short run in fixed effects models, which is however not confirmed by estimations which explicitly instrument of the ALMP accommodation. The estimations of the dynamic panel models of the matching function shows that structural adjustment programmes improve matching in the short run (a result which was also found in Büttner, Prey (1998) for other regional aggregates), but is exactly zero in the long–run.
2. For the British NDYP programmes, we applied static and dynamic panel models in order to estimate the effects of the NDYP on matching. The static specifications indicate an improving effect of the options in the long–run, which however vanishes in the dynamic model. If we include the lagged dependent variable into the matching model, we can only estimate very small short–term effects of the NDYP.

Summarising these findings, one can clearly state that macroeconomic effects of ALMP are very weak. The estimated coefficients are very small on average and not significant or near zero in the long run. We can conclude that ALMP has no effects on the macroeconomic employment outcome. If at all, the macroeconomic effects are presumable found in other areas, such as the effects on aggregate demand and regional incomes.

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4.7 Appendix

Table 4.A1 Instrumental variable estimations

	Accommodation ratio ^{fbw}		Accommodation ratio ^{abm}		Accommodation ratio ^{sam}	
	Coef.	t	Coef.	t	Coef.	t
unemployed below the age of 25 as % of all unemployment (lag –1)	0.080	0.820	-0.022	-0.280	-0.039	-1.230
unemployed above 55 as % of all regional unemployment (lag –1)	0.163	3.650	0.037	1.490	-0.004	-0.420
share of non-German unemployed as % of all regional unemployment (lag –1)	0.234	1.060	-0.111	-0.870	0.096	1.760
share of non-German female unemployed as % of all regional unemployment (lag –1)	-0.351	-0.990	0.186	0.900	-0.196	-2.400
unemployment of service sector occupations as % of all regional unemployment (lag –1)	0.107	1.010	-0.047	-0.910	-0.009	-0.380
share of the labour force aged below 25 as % of total labour force (lag –1)	-0.439	-2.990	-0.074	-0.900	-0.113	-3.160
share of the female labour force as % of total labour force (lag –1)	-0.460	-4.310	-0.185	-2.580	-0.065	-2.220
share of the non-German labour force 25 as % of total labour force (lag –1)	-0.969	-3.900	-0.208	-1.840	0.100	2.190
share of unanswered requests as % of total	0.050	1.610	-0.009	-0.640	0.001	0.120
city centres	-0.066	-0.400	0.094	1.480	0.032	1.210
rural areas	-0.089	-0.480	-0.245	-2.550	-0.066	-1.470
medium population density interacted with share of left party above 40 % (lag –2)	-0.380	-2.010	-0.070	-0.770	-0.006	-0.150
city centre interacted with share of left party above 40 % (lag –2)	-0.283	-2.300	0.117	1.730	0.023	0.800
rural areasinteracted with share of left party above 40 % (lag –2)	-0.248	-0.900	0.167	0.680	-0.003	-0.040
share of unanswered requests as % of total	37.984	5.690	13.065	2.940	4.371	2.250
N		423		423		423
(df) Wald–Test		(156) 13882.44		(156) 41260.44		(156) 42953.04
P–value		.00		.00		.00
R^2		0.9403		0.9537		0.9662

Hiermit erkläre ich, dass ich die Dissertation selbständig angefertigt und mich anderer als der in ihr angegebenen Hilfsmittel nicht bedient habe, insbesondere, dass aus anderen Schriften Entlehnungen, soweit sie in der Dissertation nicht ausdrücklich als solche gekennzeichnet und mit Quellenangaben versehen sind, nicht stattgefunden haben.

Mannheim, den 28. März 2004