IAB DiscussionPaper
Beiträge zum wissenschaftlichen Dialog aus dem Institut für Arbeitsmarkt- und Berufsforschung

No. 3/2005

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**Abstract**

Between 1991 and 1997 West Germany spent on average about 3.6 bn Euro per year on public sector sponsored training programmes for the unemployed. We base our empirical analysis on a new administrative data base that plausibly allows for selectivity correction by microeconometric matching methods. We identify the effects of different types of training programmes over a horizon of more than seven years. Using bias corrected weighted multiple neighbours matching we find that all programmes have negative effects in the short run and positive effects over a horizon of about four years. However, for substantive training programmes with duration of about two years gains in employment probabilities of more than 10 % points appear to be sustainable, but come at the price of large negative lock-in effects.

**Keywords:** Active labour market policy, matching estimation, programme evaluation, panel data

**JEL-Classification:** J68

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* The first author is also affiliated with CEPR, London, ZEW, Mannheim, IZA, Bonn, and PSI, London. Financial support by the Institut für Arbeitsmarkt- und Berufsforschung (IAB), Nuremberg, Germany (project 6-531A), is gratefully acknowledged. Conny Wunsch is also grateful for financial support from the IAB (doctoral thesis grant). The data originated from a joint effort with Stefan Bender, Annette Bergemann, Bernd Fitzenberger and Stefan Speckesser to make administrative data useful for research. The paper has been presented in seminars at the Universities of Frankfurt and St. Gallen. We thank Stefan Bender and Bernd Fitzenberger for helpful comments on a previous draft of the paper. The usual disclaimer applies.

# 1 Introduction

In the 1990s many continental European countries used active labour market policies (ALMP) as important tools to reduce Europe's notoriously high levels of unemployment, without having to go through the painful side effects of substantial reforms of the labour markets. Training was considered one of the most important and promising components of this policy (ILO, 1998). Recent evaluation studies surveyed for example by Fay (1996), Heckman, LaLonde, and Smith (1999), and Martin and Grubb (2001), however, do not appear to develop any consensus whether these hopes are justified.

Germany is no exception to these European trends. Quite to the contrary, Germany used training programmes extensively for two different policy purposes: In East Germany the goal was to qualify the labour force used to work in a centrally planned economy for the demands of a market economy. In West Germany, the goals were basically the same as in other OECD countries, namely to use training programmes to update and increase the human capital of those workers who drop out of the production process and become unemployed. Between 1991 and 1997, West Germany alone spent on average about 3.6 bn Euro per year on such training programmes.

Besides proposing improved versions of standard matching estimators for multiple programmes, we provide some answers to the question whether individual participants benefit from the fairly long and generous German public sector sponsored training (PSST) programmes for the unemployed using a microeconometric evaluation approach. We are particularly interested in the question, that even if there are positive effects of the different programmes in the short run (which cannot be taken for granted according to the evaluation literature for Germany and other countries), whether they can be sustained over a longer period of time. Since the German programmes are intensive and long by international standards, data that cover considerably more than one or two years after the programme are crucial for understanding their differential impacts on variables like individual employment. For this endeavour we use a new data base that we developed together with a team from the Institute for Employment Research and the University of Mannheim (see Bender et al., 2004) for the sole purpose of enabling the evaluation of German training programmes in the 1990s. With the new administrative data we not only can identify different programme types - impossible so far for Germany - but we can identify effects of the programmes for seven to eight years as well. Thus, we provide estimates for effects that go beyond the usual short-run effects omnipresent in the applied evaluation literature (for long-run effects of a US programme, see Hotz, Imbens, and Klerman, 2000).

Perhaps surprisingly, so far only little is known about the effectiveness of PSST in West Germany, basically because of a lack of appropriate data. Most of the previous studies use survey data from the German Socioeconomic Panel (GSOEP)[1], e.g. Hujer, Maurer and Wellner (1999b).[2] Although with this data it is possible to distinguish PSST from other forms of further vocational training, there are not enough observations to appropriately account for effect heterogeneity with respect to participants and different types of PSST.[3] On the other hand, even the few studies that use richer data (see below) do not exploit this information to analyse the different forms of PSST although there is substantial heterogeneity among them. Moreover, none of the studies conducted so far have analysed long-term effects of PSST beyond three years after the programme. The two studies that are closest to our study, in the sense of using data coming from the same large administrative data source, are Klose and Bender (2000) and Speckesser (2004).

Based on a less informative previous version of our data base, Klose and Bender (2000) analyse the effects of PSST for a cohort of participants

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1 The GSOEP data is a yearly questionnaire-based survey that started 1984. It provides individual data on personal and socioeconomic characteristics as well as retrospective information on the employment history and participation in training programmes. It is the most widely used data source for empirical analyses of the German labour market.

2 Hujer and Caliendo (2001) give a survey of studies available for Germany. Below, we present only a selection. In particular, we omit the first generation of West German evaluation studies, written in German and based on the GSOEP, e.g. Pannenberg (1995), Prey (1997, 1999), and Staat (1997). Many more studies are available for East Germany, where ALMP are used on a comparatively larger scale. Due to the very different labour market situations in East and West Germany, they are not directly relevant here.

3 Small sample sizes may be one reason why some authors (Pannenberg, 1995; Hujer, Maurer and Wellner, 1999a, c) using the GSOEP do not distinguish between PSST and other forms of further vocational training.

ending programmes in 1986. They construct a control group based on eligibility and a hierarchical matching approach. Based on treatment and control samples they estimate hazard rate models. They find ambiguous results concerning the employment effects of PSST. Speckesser (2004) uses the same evaluation period (1993-1994) and the same version of the new data base as we do but restricts his analysis to a special type of PSST and follows observations only until 1997.[4] He constructs a control group using propensity score matching and then applies nonparametric regression based on the predicted propensity scores to estimate differences in employment rates up to three years after the beginning of the programme. The effects are estimated for six subsamples stratified by unemployment duration before the programme and year of participation, thus yielding pretty small sample sizes.[5] He finds negative effects for up to one year after the beginning of the programme mainly corresponding to the time spent in the programme, but no significant effects later.[6]

For our study, as for any evaluation study, there is the question of identification strategies and estimation methods suitable for the specific situation. Angrist and Krueger (1999), Heckman and Robb (1986), and Heckman, LaLonde and Smith (1999) provide excellent overviews of available strategies. Because we argue that in our data we observe many of the major variables influencing selection as well as outcomes, we assume that labour market outcomes and selection are independent conditional on these observables (conditional independence assumption, CIA). For these reasons and since our sample is fairly large, we use matching estimators accounting for multiple treatments as proposed by Imbens (2000) and Lechner (2001, 2002a, b). Two of the advantages of this estimator are that it is essentially nonparametric and allows unrestricted effect heterogeneity. However, Abadie and Imbens (2004a) show that the usual one-to-one matching estimators may exhibit an asymptotic bias term. Therefore, we implement a weighted regression based bias removal procedure

[4] He evaluates further training of the form we will classify as short or long training in Section 3.2 yielding 536 observations in the treatment group.

[5] Sample sizes range from 57 to 121.

[6] The latter is not surprising: a feature of their data is a strongly decreasing number of observations after 1995.

on-top of the matching. Furthermore, we improve the efficiency of one-to-one matching by predicting the matched control observation by a weighted mean of similar observations.

This paper is based on unemployed individuals entering training in 1993 and 1994. The results confirm that all programmes have the expected negative lock-in effects in the months after they start (e.g. Van Ours, 2004, Gerfin and Lechner, 2002). However, in the longer run some training programmes appear to increase employment rates by more than 10% points. Furthermore, we also find that some shorter programmes are effective in the short run, but their effects decline as time goes by. This, however, is not true for a very intensive full-time programme with a duration of typically two years, called retraining, which qualifies for a different profession than the one currently held. The effects for this type of programme are not only large, but they are also sustainable over the complete eight year post-programme period we observe. Unfortunately, for this programme the lock-in effect is very substantial as well.

The plan of the paper is as follows: The next section gives the stylised facts of the German labour market policies and explains the institutional arrangements of the unemployment insurance system. Furthermore, it gives the details of the active labour market policies, with special attention to training. Section 3 discusses data issues, like definitions of programmes and the selection of the population as well as the sample. In Section 4, we discuss the selection processes into the programmes and provide descriptive statistics as well as estimates of a multivariate probit model to empirically characterise participants in the different programmes. In Section 5, we discuss our identification and estimation strategy. Section 6 contains the results for different outcome variables and different groups of participants, as well as sensitivity analyses. Section 7 concludes. Appendix A contains more information on the data. Finally, an appendix that can be downloaded from the internet (denoted as 'Internet Appendix' in this text) presents additional background information and several details the interested reader may find useful.[7]

[7] www.siaw.unisg.ch/lechner/lmw_fuu.

# 2 Labour market policies in Germany

## 2.1 The unemployment insurance system and the active labour market policy

In Germany, it is the Federal Employment Agency (FEA) which executes the passive and active labour market policy. In the period we are interested in, the early 1990s, the legal basis for the activities of the FEA is the Employment Promotion Act (Arbeitsförderungsgesetz, EPA) which regulates the policy measures available to the caseworkers in the labour offices.[8] Measures of passive labour market policy include different forms of income support during unemployment. Each employee covered by the social insurance system has to pay contributions to the unemployment insurance system (UI). The total UI contribution is shared equally between employer and employee. To acquire a legal entitlement to unemployment benefits (Arbeitslosengeld, UB) in general, an employee has to contribute for at least 360 calendar days within an entitlement qualification period of three years before the beginning of the unemployment spell. In addition, the potential claimant has to be registered with the labour office, available for job placement, willing to participate in ALMP measures, and he has to apply formally for UB (§§ 100-104 EPA).

The minimum duration of UB entitlement is 156 days. The maximum duration increases with the total duration of insured employment within an extended entitlement qualification period of seven years, and age.[9] Unemployed individuals entitled to UB receive 68 % of their average income in the three months prior to the unemployment spell if they have at least one dependent child and 63 % without children (§§ 111-115 EPA). Additional labour income can be earned up to some maximum amount but reduces the amount of UB received accordingly (§ 115 EPA). The UB payment can be suspended for up to eight weeks if the unemployed refuses to accept a *suitable* job offered by the labour office (where suitability is de-

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[8] The EPA was enacted in 1969. Since then it was subject to various amendments. On January 1st, 1998, the EPA was abolished and replaced by Social Code III. However, since this paper analyses public sector sponsored training programmes in 1993-1994, we refer to the EPA legislation effective in 1993-1994 everywhere in this paper. The legislation relevant for 1993 is taken from BA (1993a), that for 1994 from BA (1995a).

[9] § 106 EPA. For an UE below age 42, the maximum duration is 312 days, above age 54 it is 832 days.

fined by the EPA and the FEA) or to participate in (most) ALMP measures, or if he prematurely quits such a measure (this is of course not relevant if he finds a job; § 119 EPA).

Participation in ALMP measures has direct implications for UB entitlement. Times in which individuals participate in training and receive income support from the FEA count in the same way towards future benefits as insured employment does for both the acquisition and the duration of an UB claim (§ 107 (1) No. 5d EPA). This implies that participating in public sector sponsored training can lead to the acquisition of a new UB claim or to the prolongation of an existing one.

Unemployed individuals having exhausted their UB and not yet acquired a new claim can receive unemployment assistance (Arbeitslosenhilfe, UA) if they register with the labour office, are available for job placement, are willing to participate in ALMP measures, and if they are needy (means test for the unemployed and his/her partner). The UA payment amounts to 58 % of the average income in the three month prior to the unemployment spell with at least one dependent child and to 56 % otherwise. As with UB, additional earnings while receiving UA will reduce the payment accordingly.

According to the EPA, ALMP in Germany aims at achieving and maintaining a high level of employment in the economy, as well as at improving the employment structure in order to encourage economic growth. In particular, these measures seek to prevent or reduce unemployment and underemployment, to improve job-related mobility, to prevent or eliminate adverse effects of structural change in the economy, to improve the labour market integration of disadvantaged people, and to eliminate gender discrimination in the labour market.

**Table 2.1: Expenditure on active and passive labour market policies 1991-1997**

| | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 |
|---|---|---|---|---|---|---|---|
| Total expenditure in million DM | 48912 | 55125 | 69286 | 70619 | 76816 | 84795 | 83673 |
| | Shares of total expenditure for active and passive labour market policy in % | | | | | | |
| Training | 13 | 12 | 10 | 9 | 10 | 10 | 8 |
| Temporary wage subsidy | 1 | <1 | <1 | <1 | <1 | <1 | <1 |
| Short time work | 1 | 2 | 5 | 2 | 1 | 1 | 1 |
| Job creation schemes | 6 | 6 | 4 | 4 | 4 | 4 | 3 |
| Early retirement | 1 | <1 | <1 | <1 | <1 | <1 | <1 |
| Rehabilitation programmes | 7 | 7 | 6 | 5 | 4 | 4 | 4 |
| Unemployment benefits | 33 | 36 | 43 | 47 | 46 | 46 | 47 |
| Unemployment assistance | 14 | 14 | 15 | 18 | 19 | 21 | 23 |
| Other expenditure | 25 | 23 | 18 | 16 | 15 | 14 | 13 |
| Unemployment rate in % | 6.2 | 6.4 | 8.0 | 9.0 | 9.1 | 9.9 | 10.8 |

Notes: Expenditures in million DM (approx. 500,000 Euro) for West Germany. Training: further training, retraining, short programmes according to §41a EPA (abolished at the end of 1992). Temporary wage subsidies are subsidies during the phase of initial skill adaptation in a new job (*Einarbeitungszuschüsse*). Short time work: *Kurzarbeit*. Job creation schemes (JCS): *Arbeitsbeschaffungsmassnahmen*. Early retirement: V*orruhestand/ Altersteilzeit/ Altersübergangsgeld*. Unemployment benefits (UB): *Arbeitslosengeld*. 'Other expenditure' mainly includes counselling and job placement services as well as administrative costs of the FEA.

*Sources: BA (1993b, 1995b, 1996-1998).*

Besides counselling and job placement services, the most important instruments of German ALMP in the 1990s were training programmes, short time work, job creation schemes, early retirement schemes, and rehabilitation programmes. Table 2.1 displays the expenditure for different measures of passive and active labour market policies in West Germany for the years 1991-1997. There was first relatively moderate and then rising unemployment and most of the expenditure was devoted towards UB and UA. The structure of expenditures for ALMP was relatively stable. Training was by far the most utilised instrument, followed by rehabilitation programmes. Table 2.2 presents corresponding numbers of participants in those ALMP measures that were quantitatively most important in West Germany in the years 1991-1997.

*Training programmes* - which are the subject of this study - have always played an important role in West Germany. They are supposed to adjust the skills of an individual to the current and future requirements of the labour market. Durations range from a few days to three years. The objectives and different types of these training programmes are described in more detail below in Section 2.2. In 1991 about 600,000 individuals participated in training measures. There was a significant decline in 1993 to about 350,000 participants and to 275,000 participants in 1997 due to a policy change.

**Table 2.2: Participants in the quantitatively most important ALMP measures 1991-1997**

| | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 |
|---|---|---|---|---|---|---|---|
| Training (total) [a] | 601 | 582 | 350 | 308 | 402 | 378 | 275 |
| Further training (in % of total) | 71 | 72 | 76 | 73 | 77 | 77 | 76 |
| Short programmes (in % of total) | 9 | 8 | - | - | - | - | - |
| Retraining (in % of total) | 12 | 14 | 21 | 24 | 20 | 20 | 21 |
| Temporary wage subsidy | 9 | 5 | 3 | 3 | 3 | 3 | 3 |
| Job creation schemes [b] | 83 | 78 | 51 | 57 | 70 | 70 | 59 |
| Short time work [b] | 145 | 283 | 767 | 275 | 128 | 206 | 133 |

Notes: [a] Total number of inflows in 1000 persons. [b] Yearly average in 1000 persons. Short programmes are courses according to § 41a EPA (abolished at the end of 1992). Temporary wage subsidies are subsidies during the phase of initial skill adaptation in a new job (*Einarbeitungszuschüsse*). Job creation schemes (JCS): *Arbeitsbeschaffungsmassnahmen*. Short time work (STW): *Kurzarbeit*.

*Sources: BA (1993b, 1995b, 1996-1998).*

*Short time work* (Kurzarbeit, STW) can reduce layoffs due to temporary unanticipated reductions in a firm's labour demand. Workers in STW work only a few hours per week or month and receive income support to supplement their reduced labour income. With 767,000 participants STW was used extensively in 1993 when the recession of the world economy started to affect West Germany and, as a result, unemployment increased significantly. In contrast, in the other years in the period 1991-1997, the number of participants did not exceed 285,000.

*Job creation schemes* (JCS) provide additional jobs outside the regular labour market which have to be in the interest of the public. Additional means that the job would not have been provided otherwise and that it does not compete with any job in the regular labour market. In contrast to East Germany, JCS only play a minor role in West Germany. The number of participants declined from 83,000 in 1991 to 59,000 in 1997 with a temporary increase to 70,000 in 1995/96. Other ALMP measures less important in West Germany but extensively used in East Germany are early retirement schemes which seek to reduce unemployment directly by reducing the labour supply of older individuals.

*Rehabilitation programmes* range from different kinds of training to wage subsidies, and they are specifically targeted at (re)integrating disabled people and individuals with certain kinds of health limitations into the labour market.

## 2.2 Training as a part of the active labour market policy

In Germany, training consists of heterogeneous instruments which differ largely in the form and the intensity of the human capital investment as well as in their respective duration. Five groups of training programmes can be distinguished: (i) short programmes,[10] (ii) vocational training,[11] (iii) further training, (iv) retraining, and (v) German language courses.[12] Due to data limitations, the subject of this study are further training and retraining programmes that are now described in more detail: Further training comprises a variety of different forms of training. The courses offered either (a) assess, maintain or improve the occupational knowledge and skills of the participant, (b) adjust skills to technological changes, (c) facilitate a career improvement, or (d) award a first professional degree (§§ 41, 43 EPA). The duration of a full-time course that does not award a professional degree should in general not exceed one year but it can be extended to a total of up to two years if this is deemed appropriate.[13]

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[10] Short programmes were courses according to §41a EPA which had a maximum duration of nine weeks and provided information on the services available from the FEA, an initial skills assessment as well as basic job search assistance. These measures were abolished at the end of 1992. Thus, they are not part of our analysis.

[11] In some special cases the FEA supports regular vocational training in the German apprenticeship system through payment of income support.

[12] Immigrants from Eastern Europe with German origin who participate in such courses can receive income support for up to six months from the FEA which also pays for the direct programme costs.

[13] § 10 Anordnung des Verwaltungsrates der Bundesanstalt über die individuelle Förderung der beruflichen Fortbildung und Umschulung (A FuU).

One form of further training, belonging to category (a) or (b), are courses in so-called *practice firms* which simulate - though under very realistic conditions - working in a specific field of profession. There are two forms of practice firms which either simulate the commercial part of a company (administration, accounting, customer relations, etc.) or the manufacturing part.[14] The mean duration of courses in practice firms was seven months in 1994, 12 % of participants did spend no more than three months in practice firms.[15]

*Career improvement* measures which enable participants to obtain a higher professional degree (e.g. master craftsman, technician or a (below university) degree in business administration) had a mean duration of ten months in 1994, with 24 % having a duration of more than one year. In 1994, participants in courses that award a first professional degree spent 13 months on average in the programme. However, the dropout rate was rather high at 19 %.

*Retraining* enables working in a different profession than the one currently held by qualifying for a new professional degree (§ 47 EPA). A full-time retraining measure has to reduce the duration of a regular vocational training course in the German apprenticeship system by at least one year.[16] The mean duration in 1994 was 22 months, 20 % of the participants spent more than two years in the programme. On average, only about two thirds of the participants completed the programme successfully.[17]

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[14] For the commercial part, there exists a Germany- and Europe-wide network of practice firms that trade 'virtual' goods and services with each other to provide realistic conditions for participants who are the practice firm's employees. The skills acquired correspond to what is required for the specific job held within the practice firm, e.g. that of an accountant. Courses in practice firms representing the manufacturing part, on the other hand, are very heterogeneous ranging from specialist training in technical professions over obtaining a driver's licence for special vehicles to just practising bricklaying.

[15] If not stated otherwise, the numbers reported in this section originate from own calculations based on a sample of participants in public sector sponsored training. For a description of the data see Section 3.1.

[16] § 10 AFuU. Durations of apprenticeships range from two to three years.

[17] 1994: 67.4 %, dropout rate 28.3 %, 4.2 % failed.

**Table 2.3: Original and target professions of participants in retraining 1994 (shares in %)**

| | Original professions of retraining participants | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Target profession | None | Agricul-ture | Mining | Manu-facturing | Engi-neering | Services | Other | Total | No. of obs. |
| Missing | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 3 |
| Agriculture | 4 | 0 | 14 | 0 | 0 | 3 | 0 | 3 | 16 |
| Mining | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0.2 | 1 |
| Manufacturing | 31 | 40 | 29 | 34 | 19 | 5 | 0 | 27 | 143 |
| Engineering | 3 | 0 | 14 | 6 | 23 | 1 | 0 | 4 | 22 |
| Services | 58 | 60 | 43 | 56 | 58 | 86 | 100 | 62 | 337 |
| Other | 3 | 0 | 0 | 3 | 0 | 5 | 0 | 3 | 18 |
| % of all observ. | 55 | 2 | 1 | 18 | 5 | 18 | 0.4 | 100 | 540 |
| No. of obs. | 297 | 10 | 7 | 99 | 26 | 99 | 2 | 540 | |

*Source: Sample of participants in public sector sponsored retraining (for details about the data, see Section 3.1).*

Table 2.3 shows the original and target professions for a sample of retraining participants in 1994. Almost two thirds of the participants were trained towards a profession in the service sector. Data on all retraining participants reveal that most of these individuals were trained as office workers, or as workers in the social or health services (BA, 1995b). The second largest group (more than one fourth) are target professions in the manufacturing sector, with most participants trained as locksmiths, mechanics, electricians and construction workers (BA, 1995b). One striking fact apparent in Table 2.3 is that 55 % of the retraining participants seem not to have any formal professional degree before entering the program. This fact can be observed not only for 1994. Normally, participation in retraining requires a first professional degree; otherwise the individual can only participate in other forms of training which, for example, award a first professional degree. However, it seems that it was common practice to refer individuals without any formal professional degree but presumably with a substantial record of work experience in a certain field of profession to retraining.

Participation in further training and retraining can be supported by the FEA through payment of a maintenance allowance (MA)[18] and by bearing the direct costs of the programme such as course fees and study material, as well as covering parts of additional expenses for child care, transportation and accommodation. In 1994, expenditure of the FEA for further training and retraining amounted to 4.2 bn DM for payment of MA plus 2.1 bn DM for programme costs (in total about 3.1 bn Euro; BA, 1995b).[19] Both full-time and part-time, and in some very rare cases also distance learning courses are supported.[20] In addition to pure classroom training a course can include on-the-job training (OJT). This is frequently the case in courses that award a professional degree since OJT is mandatory in the German apprenticeship system with only very few exceptions.

Target groups of further training and retraining are defined by eligibility rules. In the period under consideration, FEA support for training was restricted to individuals with a first professional degree or a minimum number of years of work experience.[21] In addition, the potential participant had to be either unemployed, directly threatened with unemployment, or without any professional degree. Since FEA support of further training and retraining measures is funded out of UI contributions, an additional requirement was a minimum amount of insured employment (two years) or, alternatively, receipt of UB or UA before entering the programme (§ 46 EPA). Individuals who did not meet these additional requirements could only apply for reimbursement of the costs of the programme.[22]

[18] Until 1993 the amount of MA received was 73 % of the previous net income with at least one dependent child and 65 % without children. In 1994, the replacement rates were reduced to 67 % and 60 %, respectively, which is the same amount as unemployment benefits.

[19] More disaggregated information about the costs of specific programmes is not publicly available in Germany.

[20] In 1994 more than 95 % of retraining programmes and courses in practice firms were full-time courses. For other forms of further training, the fraction of part-time courses ranged from 7-15 %.

[21] Until the end of 1993 the requirement was a formal professional degree plus three years of work experience, or no degree but at least six years of work experience. From 1994 on, the work experience requirement was abolished for individuals with a formal professional degree and reduced to three years for all others.

[22] §§ 42, 44-45 EPA. Until the end of 1993 individuals who did not meet these requirements had the possibility to apply for MA as a loan.

# 3 Defining the estimation sample and the programmes

## 3.1 The new database

We use administrative data from three different sources which have been made available to the scientific community only recently: the IAB Employment Subsample (ES), the benefit payment register (BPR), and the training participant data (TPD).[23] Table 3.1 provides a description of the main features of these data sets. The three data sets were merged to obtain an integrated data base that covers not only participant information but as well the full history of insured employment and benefit receipt for both participants and nonparticipants in public sector sponsored training. The merged data base contains information for 208,928 individuals (54,756 of whom registered as training participants in the TPD) from 1975 to 1997. Here, we use supplementary data on the employment history and a record of benefit receipt up to the year 2001 for the individuals included in the original data sets as well.[24]

The outcome of this exercise of making administrative data that were collected for different purposes available to the scientific community is a data base that is the most comprehensive one in Germany with respect to training conducted prior to 1998. It contains many, if not most, variables influencing the selection process into these programmes (see the appendix for a list of variables used in our analysis), it allows a fairly precise measurement of interesting outcome variables, particularly those related to individual employment status, it contains information about different programme types and it has a decently large number of observations for the major programme groups. Finally, it covers a period of more than 25 years.

[23] The common German abbreviations for these data sources are IABS, LED and FuU. A detailed description of the ES is provided by Bender et al. (1996) and Bender, Haas and Klose (2000). For the TPD see Miquel, Wunsch and Lechner (2002).

[24] Following the abolishment of the EPA and introduction of Social Code III on January 1st, 1998, data collection and processing has been changed as well. The new data are similar to the data formerly included in the IABS and in the LED. See Internet Appendix IA.2 for a comparison of the different definitions of the outcome variables before and after this break in data collection.

**Table 3.1: Combined data sources used**

| | ES | BPR | TPD |
|---|---|---|---|
| Source | Employer supplied mandatory social insurance entries. | Benefit payment register of the FEA 1975-1997. | Questionnaires filled in by the labour officer for statistical purposes (*ST35*). |
| Population | 1 % random sample of persons covered by social insurance for at least one day 1975-1997. Self-employed, civil servants, university students are not included. | Recipients of UA, UB, or MA, 1975-1997. | Participants in further training, retraining, short programmes (§ 41a EPA), German language courses and temporary wage subsidies 1975-1997. |
| Available information | Personal characteristics and history of employment. | Information about the receipt of benefit payments, mainly *UB, UA, MA*. | Personal characteristics of participants and information about training programmes. |
| Important variables | Gender, age, nationality, education, profession, employment status, industrial sector, firm size, earnings, regional information. | Type and amount of benefits received. | Type, duration and result of the programme, type of income support paid during participation. |
| Structure | Spells based on daily information. | Spells based on daily information. | Spells based on monthly information. |

Note: The merged data is based on monthly information. For detailed information on the merging and recoding procedures see Bender et al. (2004). The creation of this data base is a result of a three year joint project of research groups at the Universities of Mannheim (Bergemann, Fitzenberger, Speckesser) and St. Gallen (Lechner, Miquel, Wunsch) as well as the Institute for Employment Research of the FEA (Bender).

Of course, there are several drawbacks as well, four of those could be important: First, there are several groups of individuals, like nonworking recipients of social assistance, self-employed, and civil servants ("Beamte"), who are not paying social insurance contributions and are thus not covered by these data. Second, employment that is not subject to social security contributions cannot be observed, and it is impossible to distinguish between subsidised employment (like in job creation schemes) and regular employment in the first labour market. Third, the training information prior to 1993 does not appear to be complete and correctly coded. Fourth, individual information about the unemployed as assessed by the caseworker (like in Gerfin and Lechner, 2002) is missing. Despite these drawbacks, given that so far evaluation studies for Germany relied on much smaller survey data requiring substantial aggregation across programmes, this data base must be considered a very substantial improvement in several dimensions, like sample size, selection and outcome information, and programme heterogeneity.

## 3.2 Definition of programmes and programme participation

When aggregating the specific training programme types into groups we use the following criteria: homogeneity of subprogrammes with respect to selection, to contents and to organisation, sample size, and information available to reliably distinguish subprogramme types. Table 3.2 shows the resulting five different groups plus a residual category. Because of sample size considerations, only the first four groups are subject of this evaluation.

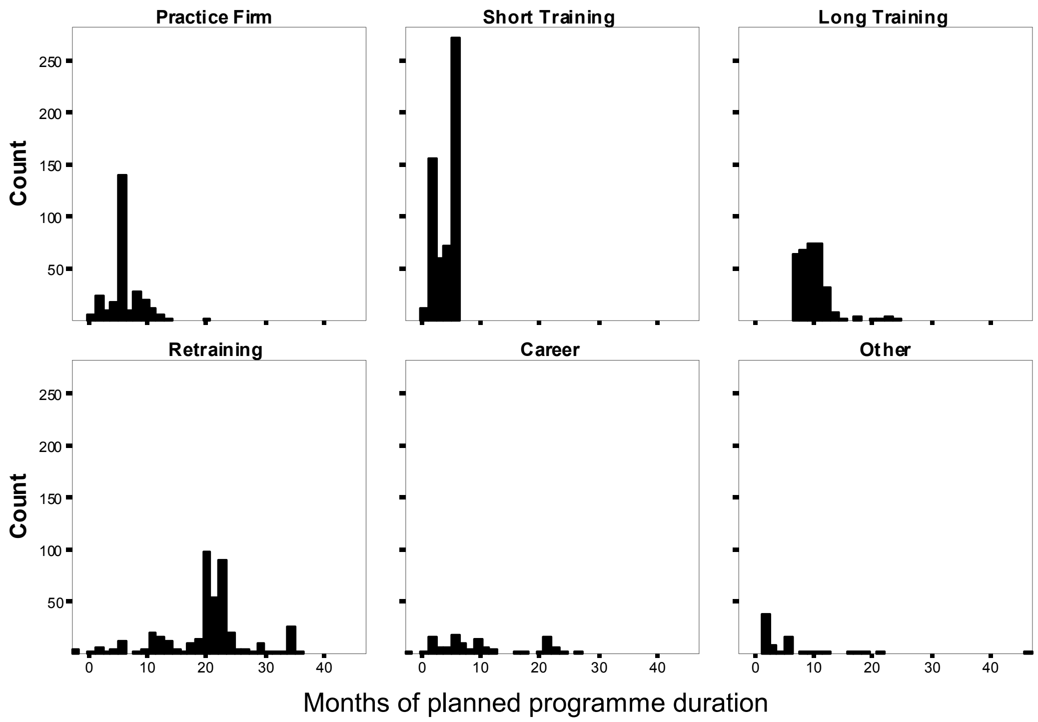
**Table 3.2: Definition of programme types**

| Programme | Description |
|---|---|
| Practice firm | Further training that simulates a job in a specific field of profession. |
| Short training | Further training (i) with the aim of a general adjustment of working skills in the profession held; (ii) to obtain an additional qualification in the profession held; (iii) to obtain a first professional degree; planed duration ≤ 6 months. |
| Long training | Same types as short training with a planed duration > 6 months. |
| Retraining | Training to obtain a new professional degree in a field other than the profession currently held. |
| Career improvement | Further training to obtain a higher professional degree, e.g. master craftsman, technician, or a (below university) degree in business administration. |
| Other | German language courses: for immigrants from Eastern Europe with German origin; participants receive income support during participation. |
| | Temporary wage subsidies: for individuals with reduced productivity e.g. due to long-term unemployment who take up a regular job during the phase of initial skill adaptation (Einarbeitungszuschüsse) for usually 6 month, sometimes up to 12 months; 30-50 % of the wage. |
| | Training while being employed. |

Note: After selecting the sample of interest, sample sizes for *career improvements* are too small. *Other* is a residual category that comprises very heterogeneous, small programmes. Therefore, those two groups are not evaluated.

The programmes considered here do not only differ with respect to the type of training received, but they also differ substantially with respect to the planned duration of a programme. Figure 3.1 indicates that typical German programmes are much longer than for example Swiss programmes (see Gerfin and Lechner, 2002). Ignoring *other* and *career improvement* which are not subject of our analysis, Figure 3.1 shows that even *short programmes* typically have a duration of about five months (mean: four), *long programmes* are clustered at nine or twelve months, and *retraining* has a typical duration of 21 months to two years, with some programmes even planned for three years. Thus, these programmes in-tend substantial investments in human capital. Although there is a clear peak at six months for *practice firms*, their duration appears to be much more heterogeneous than for the other programmes.

**Figure 3.1: Distribution of the planned programme duration**




Note: This is the planned duration of a programme determined before the programme starts.

Next, we define participation in one of the six programme groups. Since the programme participation data (TPD) is of good quality only after 1992, we consider programme participation between 1993 and 1994. This allows us to focus on fairly recent programmes while at the same time still having an observation period that allows us to detect long-run effects.[25] A person is included in our evaluation sample if she starts an unemployment spell between 1993 and 1994. The treatment group consists of all persons entering a programme between the beginning of the first unemployment spell after 1992 and the end of 1994.[26] If there are multiple treatments

[25] Furthermore, since we observe only training spells after the participant left training, and some courses have a duration of more than two years, and there is no training information after 1997, concentrating on the years 1993 and 1994 does not lead to a selective under representation of long training spells.

[26] For a figure showing the start date distribution of this defining UE spell see the internet appendix.

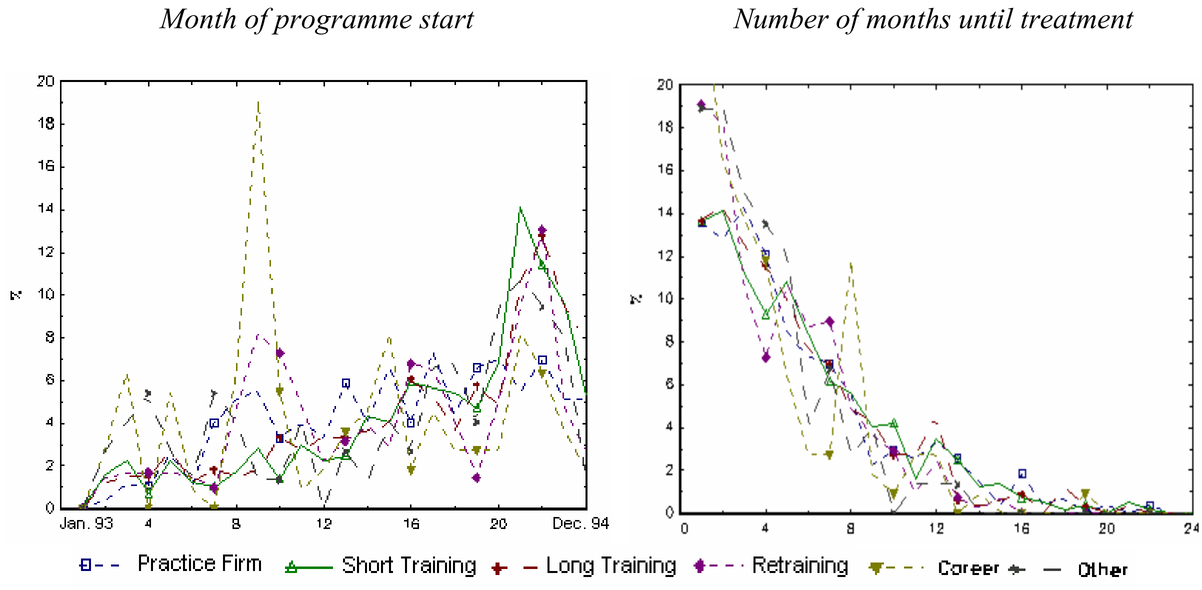
over time only the first one is included in the analysis if it occurred between 1993 and 1994. Clearly, the crucial issues here are how we define the nonparticipation status and the disregard of second, third, etc. programmes.

Taking up the arguments in Fredriksson and Johansson (2003), the fact that we condition the nonparticipation status on ending unemployment without entering a programme (or not ending unemployment at all) before 1995 might lead to some bias in our results in favour of the so-defined nonparticipation status. This bias should be severe if nearly every unemployed has to participate. However, as will become clear from the descriptive statistics, this is by far not the case.

The left panel of Figure 3.2 shows the distribution of starting months in the two-year window we consider. Partly due to the construction of our sample, the probability of treatment increases over time. The right panel of Figure 3.2 shows the months it takes until participation after the beginning of the 'defining' unemployment spell (the first UE spell between 1993 and 1994). With the exception of *career improvement*, which is not considered in the evaluation, the start date distribution is pretty homogenous across treatments. Nevertheless, *retraining* appears to be used very early in the spell, because about 45 % of the participants start within in the first three months. Note however, again, that the combination of our definition of 'defining' UE spell beginning 1993 or 1994 and training be observed not later than Dec. 1994, clustering in the first two months is rather natural.

Given our definition of a small treatment window (although in many cases, much smaller windows are used in the literature, e.g. Gerfin and Lechner, 2002), it is particularly important for the interpretation of our results which share of the control groups receives treatment as well (similar to the problem of substitution bias in an experiment). Furthermore, there is the issue of programme careers, i.e. UE participating in more than one programme over time. The conceptual problem with analysing the effect of e.g. the second participation is that it might be subject to sample selection influenced by the effect of the first programme. Thus, such an analysis of the effects of sequences of programmes requires a dynamic evaluation approach as suggested by Miquel and Lechner (2002), or Lechner (2004), which is not feasible with our data without further aggregation of programme types, which is undesirable for obvious reasons.

**Figure 3.2: Monthly distribution of start dates (in %)**




Note: The treatments *other* and *career* are not considered in the evaluation below. The right panel shows the number of months until participation after the beginning of the 'defining' unemployment spell.

**Table 3.3: Participation in different programme types in % of participants in subsamples until 1997**

| | Treatment status in study (first treatment) | | | | | | |
|---|---|---|---|---|---|---|---|
| Programme participation between 1993 and 1997 | Nonparticipation | Practice firm | Short training | Long training | Retraining | Career improvement | Other |
| Practice firm | 1.6 | 3.7 | 0.9 | 1.2 | 0.5 | 0.9 | 1.4 |
| Short training | 4.8 | 4.0 | 7.3 | 2.7 | 1.2 | 3.6 | 1.4 |
| Long training | 3.2 | 4.4 | 4.7 | 4.9 | 1.5 | 2.7 | 2.7 |
| Retraining | 1.2 | 1.5 | 4.4 | 1.5 | 1.0 | 0.0 | 1.4 |
| Career improvement | 0.2 | 0.4 | 0.7 | 0.0 | 0.0 | 0.9 | 0.0 |
| Other | 0.5 | 0.7 | 1.2 | 0.0 | 0.2 | 0.0 | 2.7 |
| Total other treatments than first treatment | 11.4 | 11.0 | 11.9 | 5.5 | 3.4 | 7.3 | 6.8 |

Note: Entries show the fraction (%) of members of the subsamples stated in the columns who participated at least once in the treatments stated in rows after their first treatment (programme participants) or after 1994 (nonparticipants). Due to data restrictions only training spells completed by the end of 1997 are observable.

Table 3.3 shows the share of observations defined by the first treatment in 1993 and 1994, or the absence of it, who participate in additional programmes. First of all, note that only about 11% of nonparticipants receive some sort of training until the end of 1997. A similar share of the shorter programmes, *practice firm* and *short training*, shows about the same amount of other programmes, but more than one third of those participants in *practice firms* and two thirds of those in *short training* who participate more than once, participate in a programme belonging to the same programme group. For the other programmes, in particular for the longest programme *retraining*, subsequent participations occur only in rare cases (3%). To conclude, Table 3.3 provides clear evidence that the effect we will estimate are very close to the 'pure' effect of the programme used to define the treatment status.

## 3.3 Selection of population and sample

When choosing the appropriate population, we aim at having a homogenous group of people covering the prime age part of the population of West Germany. Thus, we do not consider the capital, Berlin, because the regional information for Berlin is not precise enough to attribute a particular individual to the former East German or West German parts of the town. Furthermore, we aim to ensure that all people are eligible: We require that everybody was employed at least once prior to programme participation and that they were receiving UB or UA in the month of and before the programme starts.[27] This, however, requires the use of variables which are measured relatively to the programme start. We follow one of the approaches suggested by Lechner (1999) and simulate start dates for nonparticipants by drawing start dates from the empirical distribution for

27 'Employed' means that we observe the person at least once in an insured employment spell in the ES. With respect to eligibility receipt of UB or UA directly before entering a programme is not sufficient. Individuals must also meet the requirement of either having a formal professional degree plus three years of work experience (since 1994 zero years), or alternatively, at least six years (since 1994 three years) of work experience, where times of registered unemployment also count as work experience up to half of the required minimum number of years. Since we also require individuals to be employed at least once before the programme, the only group of non-eligibles we do not exclude from the sample are individuals without any professional degree that have not (yet) acquired sufficient work experience. Insufficient work experience might also affect eligibility of individuals having a professional degree who have been assigned a simulated programme start date in 1993. However, both groups are very small so that eligibility is captured sufficiently well by our selection criteria. One might argue that we are overly strict in selecting our sample since we disregard short episodes of individuals not observed in the data which may be due to suspension of UB or UA for up to eight weeks. However, the reasons for not being observed in the data are very heterogeneous (self-employment, receipt of social assistance, out of labour force, etc.) we prefer to exclude these cases altogether.

participants and then ensuring that this date does not lie before the beginning of the 'defining' UE spell or after the end of the person's last spell that is observed in the data. Nonparticipants that do not satisfy this criterion are excluded from the sample. To avoid most influences coming from retirement, early retirement and primary education, we also impose an age restriction (20-55 years) before entering the programme. Concentrating on the main body of the active labour force we furthermore exclude trainees, persons in apprenticeships, persons whose last employment was less intensive in terms of hours than half of a full-time equivalent, and persons who were home workers before the 'defining' UE spell.

**Table 3.4: Sample selection rules**

| | Nonparticipation | Practice firm | Short training | Long training | Retraining | Career improv. | Other |
|---|---|---|---|---|---|---|---|
| Persons entering unemployment between Jan. '93 and Dec. '94 | 36965 | 324 | 644 | 380 | 497 | 130 | 103 |
| Simulated programme start after the entry in unemployment (UE) and before the end of the observation period | | | | | | | |
| Remaining observations | 26022 | 324 | 644 | 380 | 497 | 130 | 103 |
| Eligibility: Only individuals receiving UB or UA in the month of and before the programme start | | | | | | | |
| Remaining observations | 13091 | 309 | 618 | 350 | 450 | 118 | 92 |
| Personal characteristics : | a) 20 ≤ age ≤ 55;<br>b) no trainees or apprentices;<br>c) at least one observation of employment;<br>d) no home workers;<br>e) no part-time worker less than half of a full-time work | | | | | | |
| Final sample | 9197 | 273 | 572 | 329 | 413 | 110 | 74 |

Note: All variables are measured before or in the same year as the start of the programme.

Table 3.4 shows how the sample shrinks imposing these criteria successively. We end up with a sample of about 9,000 nonparticipants and about 270 to 570 participants in the four programme groups we consider in the econometric analysis. The number of participants in *career improvement* and *other* is too small to compute a precise treatment effect.

# 4 The determinants of programme participation

## 4.1 Eligibility, assignment and self-selection into programmes

As in every evaluation study, the key to address the sample selection (endogeneity) problem is to obtain an understanding of how different individuals end up in different programmes. Instead of postulating a complete structural model for the selection process, we discuss the main determinants of selection and then explain which observable variables are used to capture them. The determinants can be divided into two groups: those required by legislation (eligibility), and those that may be underlying the decisions of the caseworker and the unemployed.

Beginning with the role of the legislation, remember that to become eligible for FEA support an unemployed must hold a first professional degree or have a minimum number of years of work experience.[28] In addition, the potential participant has to be either unemployed, directly threatened with unemployment, or without any professional degree. If not receiving UB or UA directly before entering a programme, individuals must be employed for at least two years within the three years prior to the programme. As discussed in Section 3.3 (in particular in footnote 35) our selected sample fulfils the eligibility rules.

When these conditions are met, then the unemployed *could* be offered a programme by her caseworker. Before going into the details of the determinants underlying the selection decisions of both parties, it is helpful to understand the rules of their interaction. The unemployed and her caseworker meet at least every three months in order to discuss the job search efforts of the unemployed since their last meeting, new job offers available, potential benefits of participating in labour market programmes, as well as potential adaptations of their strategy for getting the unemployed back to work.[29] Usually it is the caseworker but it may also be the unemployed herself who proposes participation in training to improve her chances of finding a job. In any case, the unemployed must apply for FEA support before the beginning of the programme, and the caseworker decides whether or not she will receive support. There is no legal entitlement to FEA support, and caseworkers have a considerable amount of discretion

[28] The exact requirement is a formal professional degree plus three (since 1994 zero) years of work experience, or no degree but at least six (since 1994 three) years of work experience.

[29] The caseworker can schedule a meeting at any time but at least every three months, e.g. in order to check the availability of the unemployed for job placement, or to discuss new job offers or participation in labour market programmes. Attendance is compulsory for the unemployed. See § 132 EPA.

in making their decision about programme participation. However, they have to use this discretion in accordance with the objectives of the EPA as well as the specific aims of the programme (§ 33 EPA). They also have to consider the situation and development of the labour market, and they have to act based on the principle of economic efficiency. In addition, caseworkers have to take into account the aptitude of the applicant for specific jobs and her chances for completing a specific programme successfully (§ 36 EPA). In particular, the caseworker's decision has to be guided by the consideration which of the measures available have the highest chances for success and are the least costly, that is, most efficient for a specific individual (§ 7 A FuU).

Usually the caseworker decides in consultation with the potential participant whether or not and if so what kind of training programme would be appropriate based on an assessment of the employment prospects of the UE. Since the willingness to participate in labour market programmes is a precondition for receipt of UB and UA, UE who refuse to apply or, having applied, refuse to participate in a training measure risk suspension of their benefits for up to eight weeks.[30]

Given our knowledge about the 'average' selection process, the caseworker's decision about referral of applicants to specific programmes may be guided by two objectives: efficiency or equity. Caseworkers pursuing efficiency goals assign those individuals to the programmes that are expected to benefit most from them. In contrast, equity goals require caseworkers to select the neediest individuals into the programmes, where neediness is defined by some criterion, e.g. a high risk of becoming long-term unemployed. The factors relevant for pursuing the latter policy can probably be best approximated in our data by the employment and unemployment history (Heckman and Smith, 1999, 2004, point out the importance of this information in the context of analysing participation in the Job Training Partnership Act (JTPA) in the United States) as well as the economic situation of the individual, which are largely determined by the last job, educational attainment, nationality and family status since these

[30] They may even loose their entitlement altogether if benefits have already been suspended before (§ 119 EPA).

variables govern chances in the labour market. These factors may also be related to the effect maximising strategy. In addition, we would expect that participation declines with age, because the amortisation period of the human capital investment shrinks. Furthermore, as mentioned for example again by Heckman and Smith (1999), the state of the local economy may also be a factor influencing the decision of sending somebody into a programme or not. The caseworker may, however, be supply constrained and not able to offer what he considers best. Yet this is not so important here, because it can plausibly be assumed that conditional on all other variables like the regional information, this variable is not correlated with the outcomes.

From the point of view of the unemployed, his decision whether or not to participate in a programme is guided by considerations very similar to those of the caseworker. There are, however, additional reasons for joining or not joining a programme: If the unemployed sees no chance to find a job anyway, with or without a programme, he may prefer not to join a programme which reduces his leisure time. Again, we capture this fact by using his (un)employment history as well as regional variables as a proxy. Finally, legislation also provides a rather strong incentive to participate in training that is supported by payment of MA: times of receipt of MA can extend existing or renew exhausted UB entitlements. To be able to control for this fact, we have constructed two variables from the (un-)employment histories indicating the UB claim at the beginning and at the end of a spell.

In our data all the factors determining participation mentioned so far can be captured in most cases by very detailed proxy variables, in fact much more detailed than usually available in many administrative datasets used for evaluation purposes (see Table A.1 in the appendix for details about all variables used). However, as already noted in the previous section, in our data there is no information about the caseworker's direct assessment of the strengths and weaknesses of the UE, for example with respect to his motivation and ability. As usual for these variables, we have to rely on their indirect effects, i.e. on their effect on the employment and the earnings history that materialised in the past.

## 4.2 The empirical determinants of programme participation

Table 4.1 shows descriptive statistics for selected socio-economic variables for the different subsamples defined by treatment status. Concentrating on the first five groups included in the econometric analysis, the results in this table can be summarised as follows: Participants in *retraining* are on average 31 years old, and thus much younger (about five years) than other unemployed which is completely in line with the idea that substantive human capital investments are most beneficial if the productive period of the new human capital is fairly long. Another interesting feature is that the share of foreigners in the programme is only about half the share of foreigners in the group of nonparticipants. Participants in *practice firms* and *retraining* are less educated and skilled than the rest. The mirror image of this observation is that participants in *short* and *long training* appear to have the best a priori chances on the labour market, although the education level of those in *short training* is somewhat lower than for those in *long training*. Correspondingly, earnings are somewhat higher for participants in *short* and more strongly in *long training* than in *practice firms* and *retraining*. Earnings of the latter two groups are almost the same as for *nonparticipants*. For the two variables indicating remaining UB claims, Table 4.1 does not show much variation, though the average remaining claim for participants in *retraining* is about two months shorter than for the other UE. Finally, note that regional and male-female differences are fairly small.

The lower part of Table 4.1 refers to one of the main outcome variables used in this study, namely whether an individual is employed in particular months before and after the programme. Note that when we go back in time, the sample size decreases because more and more young people did not yet have their first employment subject to social security contributions which is the key requirement to be included in the population from which the data is drawn.

Due to selecting a sample of unemployed before the programme, the rates decline when approaching 1993/4, starting with a stable level close to 70 %. They are low during the programme (*retraining* has a mean duration of almost two years) and recover thereafter. None of the groups reaches their initial level in 1990/2 by 2001, although participants in *retraining* come close. The rate of recovery for *nonparticipants* is particularly low, already foreshadowing the results of the econometric part below.

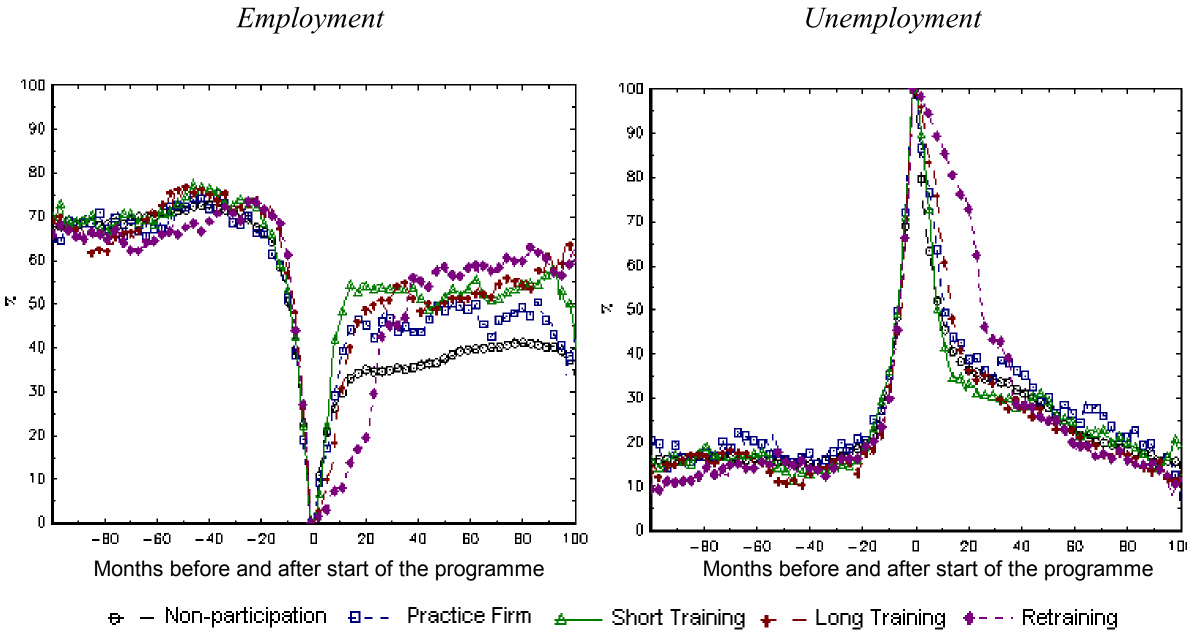
**Table 4.1: Descriptive statistics of selected variables by treatment status (shares in %)**

| | | Non-participation | Practice firm | Short training | Long training | Retraining | Career improvement | Other |
|---|---|---|---|---|---|---|---|---|
| Number of observations | | 9197 | 273 | 572 | 329 | 413 | 110 | 74 |
| | | Personal characteristics | | | | | | |
| Women | | 41 | 33 | 37 | 39 | 38 | 24 | 27 |
| Age[++] * | | 37 | 36 | 35 | 35 | 31 | 32 | 32 |
| Nationality: German | | 81 | 87 | 91 | 92 | 89 | 92 | 92 |
| Education: no university entrance degree, no professional degree | | 25 | 18 | 15 | 9 | 24 | 5 | 5 |
| Polytechnical or university degree | | 6 | 0 | 6 | 14 | 3 | 10 | 2 |
| | | Position in last job | | | | | | |
| Salaried employee | | 28 | 34 | 38 | 57 | 23 | 41 | 39 |
| Unskilled worker | | 40 | 36 | 31 | 19 | 53 | 9 | 2 |
| | | Last monthly earnings | | | | | | |
| Salary in EUR* | | 1680 | 1640 | 1773 | 1889 | 1640 | 2072 | 1781 |
| | | Remaining UB claim (before entry in the programme) | | | | | | |
| Remaining UB claim (months)* | | 8 | 7 | 8 | 7 | 5 | 7 | 7 |
| | | Legal UB claim at the beginning of the last unemployment spell before the programme | | | | | | |
| Legal claim* (months) | | 13 | 11 | 12 | 11 | 9 | 10 | 10 |
| | | Programme information | | | | | | |
| Planned programme duration* (months) | | | 6 | 4 | 10 | 20 | 10 | 5 |
| | | Regional information | | | | | | |
| North-Rhine-Westphalia | | 31 | 21 | 28 | 36 | 35 | 23 | 28 |
| Rhineland-Palatinate, Hesse, Saarland | | 17 | 23 | 19 | 18 | 17 | 17 | 19 |
| Baden-Württemberg, Bavaria | | 30 | 26 | 34 | 21 | 22 | 42 | 38 |
| UE rate[++] ≤ 5 % | | 1 | 2 | 2 | 0 | 1 | 2 | 4 |
| 5 % < UE rate ≤ 10 % | | 64 | 67 | 70 | 62 | 61 | 72 | 76 |
| UE rate > 10 % | | 35 | 31 | 28 | 38 | 38 | 26 | 20 |
| | | Employed in | | | | | | |
| January 1990 | N = 9559 | 69.5 | 68.0 | 67.0 | 71.3 | 66.9 | 81.4 | 70.5 |
| 1992 | N = 10609 | 69.1 | 67.5 | 70.0 | 73.3 | 68.5 | 80.6 | 68.1 |
| 1993 | N = 10870 | 60.9 | 55.7 | 60.5 | 64.7 | 57.8 | 62.7 | 63.0 |
| 1995 | N = 10940 | 17.8 | 28.6 | 29.9 | 19.8 | 9.4 | 32.7 | 73.0 |
| 1997 | N = 10872 | 30.1 | 43.9 | 49.4 | 52.6 | 46.1 | 60.6 | 70.3 |
| 1999 | N = 10670 | 33.6 | 47.4 | 49.4 | 51.2 | 56.8 | 56.9 | 59.5 |
| 2001 | N = 10670 | 36.3 | 47.0 | 52.8 | 54.9 | 60.0 | 61.5 | 59.5 |

Note: *Numbers marked by an asterisk are means (rather than proportions).
[++]Measured in the year of the beginning of the programme. The sample used for the table is the one after all selection steps described in Section 3, but before imposing the common support requirement. For a detailed list of variables see the appendix A.1.
'*N* =' means the number of individuals for whom this information is available at that point in time.

Figure 4.1 provides another look at the monthly employment rate - now centred at the actual or simulated beginning of the programmes - and also the corresponding numbers for the other outcome variable of interest, namely registered unemployment.[31] Prior to training, all curves are on a fairly similar level, although the employment level of the future *retrainees* is the lowest one for most of the time (and among the highest after the programme). Since being unemployed is a precondition for participation, the shapes for all subgroups exhibit the usual Ashenfelter's dip (Ashenfelter, 1978) starting about two years prior to the actual or simulated programme starts. It must be pointed out that this dip is a purely statistical phenomenon coming from the way we selected the sample by enforcing the eligibility criterion for participants and nonparticipants.

**Figure 4.1: Employment and unemployment rates by participation status**




Note: Timing relative to observed or simulated starting dates of programmes. Note that after 80 months the sample size starts to decline rapidly.

After training, the different rates recover quickly, the speed mainly depending on the average programme duration. The surprising finding is that for *nonparticipants* the recovery of the employment rate suddenly slows down about one year after the simulated programme start at a level of

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[31] Note that we define registered unemployment as receipt of UB or UA in the respective month.

about 35 to 40 %. This timing coincides with the end of the benefit period for this group, thus suggesting that a large group of *nonparticipants* leaves the labour force after benefit exhaustion. This is reinforced by considering unemployment for which no large differences are observed after the initial programme period is over (note that programme participation increases the benefit period), therefore more of the nonworking people remain registered as UE thus probably making up the difference in employment rates. Clearly, combining the fact from Table 4.1 and Figure 4.1 that retrainees are no good a priori risks on the labour market together with the observation that their long-run employment rates are among the highest, already suggests the likelihood of a positive effect for this programme.

**Table 4.2: Differences of mean probabilities for different values of the covariates (in %-points)**

| Changes in covariates | Nonparticipation | | Practice firm | | Short training | | Long training | | Retraining | |
|---|---|---|---|---|---|---|---|---|---|---|
| | mean | std. | mean | std. | mean | std. | mean | std. | mean | std. |
| Women - men | 1.84 | 1.25 | -1.24 | 0.85 | 0.37 | 0.96 | -0.73 | 0.96 | -0.25 | 0.98 |
| Nationality: German - foreigner | -5.85 | 1.47 | -0.01 | 0.93 | 1.97 | 1.24 | 0.99 | 1.32 | 2.89 | 1.14 |
| Education: Low - high | 3.26 | 1.24 | 0.09 | 0.85 | -0.87 | 0.99 | -1.10 | 1.06 | -1.38 | 0.87 |
| Employment states in each of the 5 years before 1993: unemployment - employment | -5.49 | 8.49 | 3.57 | 6.00 | 2.67 | 7.43 | -6.40 | 4.50 | 5.65 | 8.24 |

Note: Probabilities are computed for every individual at each value of the covariates in question given the estimated coefficients. Others covariate not explicitly mentioned in the first column are only changed if logically required. For example, changing unemployment states change many variables at the same time (see internet appendix for details). Standard errors of the mean differences over the sample (which should converge to a normal distribution) are based on 250 draws from the asymptotic distribution of the estimated MNP coefficients.

To obtain a better understanding of the empirical selection process and for later use in the matching estimator, we estimate a multinomial probit model for the different treatment states. The Internet Appendix contains all details of its implementation and the coefficient results. Here, we only report some simulations based on this model showing the magnitude of changes in the impacts that some important exogenous variables have on the estimated probabilities. The results for some selected covariate scenarios are presented in Table 4.2. They more or less confirm the impression from the descriptive statistics. Whereas differences in sex and employment history are not statistically significant, foreigners are more likely to participate, in particular in *retraining*. Quite surprising a lower education makes nonparticipation more likely.

# 5 Econometrics

We base our analysis on the prototypical model of the microeconometric evaluation literature with multiple treatments: An individual chooses between several states, like participation in a specific training programme or non-participation in such a programme. Potential participants in the programmes are assigned hypothetical outcomes for all states. This model is based on the binary potential outcome model (Fischer, 1935, Neyman, 1923, Roy, 1951; Rubin, 1974, 1977) extended by Imbens (2000) and Lechner (2001) to multiple, mutually exclusive states. Here, we consider outcomes of six different states denoted by $\{Y^0, Y^1, Y^2, Y^3, Y^4, Y^5\}$[32]. The different states are called treatments in the following to stick to the terminology of that literature. For any individual only one component of $\{Y^0, Y^1, Y^2, Y^3, Y^4, Y^5\}$ is observable. Participation in a particular treatment *m* is indicated by the realisation of the random variable S, $S \in \{0,1,2,3,4,5\}$. This notation allows us to define average treatment effects for pair-wise comparisons of the effects of different states under the usual assumptions (see Rubin, 1974; note that we are not interested in the residual category):

$$\gamma_0^{m,l} = E(Y^m - Y^l) = EY^m - EY^l; \tag{1}$$

$$\theta_0^{m,l} = E(Y^m - Y^l | S = m) = E(Y^m | S = m) - E(Y^l | S = m); \qquad m \neq l; m, l \in \{0,1,2,3,4\}. \tag{2}$$

$\gamma_0^{m,l}$ denotes the expected (average) effect of treatment *m* relative to treatment *l* for a participant drawn randomly from the population (average treatment effect, ATE).[33] ATEs are symmetric ($\gamma_0^{m,l} = -\gamma_0^{l,m}$). $\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of par-

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[32] The last state '5' contains *career improvement* and *other* and will be ignored in the estimation part.

[33] If a variable *Z* cannot be changed by the effect of the treatment then all what follows is also valid in strata of the data defined by different values of *Z*.

ticipants in treatment *m* only (ATE on the treated, ATET). ATETs are not symmetric, if participants in treatments *m* and *l* differ in a way that is related to the distribution of X, and if the treatment effects vary with X.

## 5.1 Identification

ATEs and ATETs are generally not identified so that additional assumptions are needed. We already noted that our data compiled from different administrative records are so rich that it seems plausible to assume that we observe all important factors that jointly influence labour market outcomes and the process selecting people into the five different states (selection on observables). Therefore, we assume that treatment participation and treatment outcome are independent conditional on a set of (observable) attributes (conditional independence assumption, CIA). In other words, there are no exogenous variables left out that are both correlated with potential outcomes and the participation decision. Expression (3) formalizes the CIA on subspace $\chi$ of the attribute space:

$$Y^0, Y^1, ..., Y^m \amalg S \mid X = x, \forall x \in \chi \tag{3}$$

where $\amalg$ denotes independence. This assumption requires the researcher to observe all characteristics that jointly influence the outcomes as well as selection into treatments. In addition, CIA requires that all individuals that are part of the evaluation could participate in all states (i.e. $0 < P(S = m \mid X = x)$, $\forall m = 0, ..., 4$, $\forall x \in \chi$).

## 5.2 A matching estimator

Lechner (2001) shows that the CIA identifies all effects defined in this section and that expression (3) implies independence not only conditional on *X* but also conditional on the marginal probabilities of the states (conditional on *X*), denoted as $[P^0(X), P^1(X), P^2(X), P^3(X), P^4(X)]$.[34] Based on this insight, Lechner (2001, 2002a, b) proposes and applies different matching

[34] Depending on the effect to be estimated, we need to condition only on a subset or functions of these probabilities. For all details the reader is referred to Lechner (2001). All details of the estimation of the conditional probabilities can be found in the internet appendix. In addition to the propensity score, one may condition on attributes included in it to ensure that a misspecification in the functional form of the marginal probabilities has only a minor impact.

estimators for that problem. Here, we use an improved version of the estimator implemented by Gerfin and Lechner (2002), because it is simple, seems to perform reasonably well and appeared to be quite robust in different practical applications (e.g. Larsson, 2003; Gerfin, Lechner and Steiger, 2004). Moreover, it was subjected to Monte Carlo studies (e.g. Lechner, 2002b) investigating small sample problems and sensitivity issues. The different steps of the estimator are described in Table 5.1. In the first step, the multinomial probit model is used to estimate the choice probabilities conditional on the attributes. Step 2 ensures that we estimate only effects in regions of the attribute space where two observations from any two treatments could be observed having similar participation probabilities ('common-support'). Otherwise the estimator will give biased results (see Heckman, Ichimura, Smith and Todd, 1998). Note that if we are only interested in pair-wise effects the current implementation would be unnecessarily strict, since making sure that there is an overlap for each pair would be sufficient. Our implementation has the advantage that we evaluate all programmes on the same support. In total, the common support criteria discarded only about 6 % of participants in *retraining*, 9 % in *practice firms*, 13 % in *short training*, 19 % in *long training*, and 24 % in *nonparticipation*. As opposed to the high number for *long training*, note that the high number for nonparticipants is not worrying because they have no implication for estimating programme ATETs which are the most interesting quantities. Independent of the common support issue, ATE's for the nonparticipants cannot be estimated, because the simulation procedure for start dates already renders a group of nonparticipants not representative for the population of nonparticipants. The unemployed we are losing for *long training* are most likely older men with a polytechnical degree and a comparatively high salary in technical occupations (see the Internet Appendix for details).

**Table 5.1: A matching protocol for the estimation of $\theta_0^{m,l}$ and $\gamma_0^{m,l}$**

| | |
|---|---|
| Step 1 | Specify and estimate a multinomial probit model to obtain $\left[\hat{P}_N^0(x), \hat{P}_N^1(x), \hat{P}_N^2(x), \hat{P}_N^3(x), \hat{P}_N^4(x)\right]$. |
| Step 2 | Restrict sample to common support: Delete all observations with probabilities larger than the smallest maximum and smaller than the largest minimum of all subsamples defined by S. |
| Step 3 | Estimate the respective (counterfactual) expectations of the outcome variables.<br>For a given value of m and l the following steps are performed:<br>a-1) Choose one observation in the subsample defined by participation in m and delete it from that pool.<br>b-1) Find an observation in the subsample of participants in l that is as close as possible to the one chosen in step a-1) in terms of $\left[\hat{P}_N^m(x), \hat{P}_N^l(x), \tilde{x}\right]$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again.<br>c-1) Repeat a-1) and b-1) until no participant in m is left.<br>d-1) Compute the maximum distance (d) obtained for any comparison between treated and matched comparison observations.<br>a-2) Repeat a-1).<br>b-2) Repeat b-1). If possible, find other observations in the subsample of participants in l that are at least as close as R * d to the one chosen in step a-2) (to gain efficiency). Do not remove these observations, so that they can be used again. Compute weights for all chosen comparisons observations that are proportional to their distance. Normalise the weights such that they add to one.<br>c-2) Repeat a-2) and b-2) until no participant in m is left.<br>d-2) For any potential comparison observation, add the weights obtained in a-2) and b-2).<br>e) Using the weights $w(x_i)$ obtained in d-2), run a weighted linear regression of the outcome variable on the variables used to define the distance (and an intercept).<br>f-1) Predict the potential outcome $y^l(x_i)$ of every observation in l and m using the coefficients of this regression: $\hat{y}^l(x_i)$.<br>f-2) Estimate the bias of the matching estimator for $E(Y^l \mid S=m)$ as: $\sum_{i=1}^{N} \frac{1(S=m)\hat{y}^l(x_i)}{N^m} - \frac{1(S=l)w_i\hat{y}^l(x_i)}{N^m}$.<br>g) Using the weights obtained by weighted matching in d-2), compute a weighted mean of the outcome variables in l. Subtract the bias from this estimate.<br>h) Compute the treatment effect by subtracting the weighted mean of the outcomes in the comparison group (l) from the weighted mean in the treatment group (m). |
| Step 4 | Repeat Step 3 for all combinations of *m* and *l*. |

Note: Lechner (2001) suggests an estimator of the asymptotic standard errors for $\hat{\gamma}_N^{m,l}$ and $\hat{\theta}_N^{m,l}$ conditional on the weights that we use here. $\tilde{x}$ includes the date of the beginning of the programme, sex, three dummies indicating if the individual is employed 12, 24 and 48 months before the programme. $\tilde{x}$ is included to ensure a high match quality with respect to these critical variables. R is fixed to 90% in the application. Note that once we estimate all $E(Y^l \mid S=m)$ for all *m*, they can be directly used to obtain $E(Y^l)$.

In the matching algorithm implemented by Gerfin and Lechner (2002) the same comparison observation may be used repeatedly in forming the comparison group (matching with replacement). This modification of the 'standard' estimator is necessary for the estimator to be applicable at all when the number of participants in treatment *m* is larger than in the comparison treatment *l*. Since the role of *m* and *l* could be reversed in this framework, this is always the case when the number of participants is not equal in all treatments. However, when there are other comparison observations which are similar to the matched comparison observation, there are easy efficiency gains (without paying a too high price in terms of additional bias) by taking these 'very close' neighbours into account and forming an 'averaged matched comparison' observation. Of course, there are many ways to do this in practice (also note the similarity to the idea of kernel matching). Here, our basic consideration is that we are not prepared to incur much additional bias, because the variance of the estimator is visible after the estimation, and the bias generally is not. To be conservative in this respect, we consider observations which have a distance to 'their' treated observation of no more than 90 % (called *R* in Table 5.1) of the worst match we obtain by one-to-one matching (after enforcing common support; *R=0* is the case of one-to-one matching; *R* corresponds to a bandwidth choice in kernel weighting).[35] To be even more conservative, we weight the observations proportionally to their distance from the treated (corresponding to a triangular kernel). The results are not too sensitive to the exact way the weighting is implemented. When *R* is reduced the means change little, but the estimated variances increase.

However, Abadie and Imbens (2004a) show that dependence on the dimension of the continuous conditioning variables, the usual one-to-*K* matching estimators where K is a fixed number, may exhibit an asymptotic bias, because matches are not exact. Although our weighted matching estimator is smoother and thus probably less subject to this problem, we follow their proposal and implement a weighted regression based bias removal procedure on top of the matching. The regression is done in the comparison sample only. Outcomes are predicted for the attributes ob-

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[35] As for the choice of bandwidth of kernel-matching, there are no theoretical results available for choosing *R* (see Imbens, 2004).

served in treated and control samples. Specifically, the outcome variable is regressed on the propensity score and the additional variables with weights coming form the matching step (see Imbens, 2004). The difference between the mean of the predicted outcomes using the observed $X$ of the treated and the weighted $X$ of the comparison observations gives an estimate of the bias (see Table 5.1 for the exact implementation). Without the theoretical justification given by Abadie and Imbens (2004b), a somewhat similar procedure has been used by in Rubin (1979) and Lechner (2000).

For the sake of brevity we do not document the matching quality explicitly, but the weighed matching estimator roughly balances the covariates. Detailed results are available in the Internet Appendix.

We used the same standard errors as Gerfin and Lechner (2002) which are conditional on the weights for the comparison observations, because in Monte Carlo simulations they showed (e.g. Lechner, 2002b) reasonable performance in finite samples (their generalisation to non-integer weights as used here is trivial). Unfortunately, alternatives are either not valid, as for example the bootstrap (see Abadie and Imbens, 2004b), or have not been adapted to the weighted matching estimators with estimated regressors and have unknown operational characteristics in finite samples (like the matching-within-the-treated estimators suggested by Abadie and Imbens (2004a).

# 6 The effects of training

## 6.1 Measurement of the outcomes in the labour market

According to German legislation one of the objectives of active labour market policy is to increase reemployment chances and to reduce probabilities of remaining unemployed. Therefore, important outcome variables are those relating to the employment status, like registered unemployment and different types of employment.[36] Some types of employment require a certain quality, approximated for example by the job's duration and earnings compared to the previous job. Furthermore, as a crude measure for individual productivity gross earnings are considered as well.

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[36] Here 'registered unemployment' denotes all individuals receiving UB or UA.

Effects are measured beginning in the month after the programme started (with simulated start dates for nonparticipants). Focusing on the beginning instead of the end rules out that programmes appear to be successful, just because they keep their participants busy by making them stay in the programme. We consider a programme to be most successful if everybody would leave for 'good' employment immediately after starting participation. Whenever a person participates in any of the programmes he is considered as registered unemployed (and not employed).

## 6.2 Mean effects of programmes for their participants

Table 6.1 shows the means of the outcomes in the various groups, the estimated counterfactual expectations and pair-wise comparisons between the programmes and nonparticipation. We concentrate on the outcome *employment two and seven years after participation started*.

Columns (3) and (4) give the exact sample sizes (after imposing common support) available at each point of (process) time. Note that the small decrease in sample size in year seven is due to programme participants who could not be observed for all seven years. Therefore, there is loss in precision which becomes particularly relevant after eight years (to be considered in the graphs below).

Columns (5) and (8) show the observed mean outcomes for the participants in programme *m* (5) as well as the observed mean outcomes for participants in programme *l* (8). Column (6) shows the estimated mean counterfactual outcome of treatment *m* for population *l*. Column (7) shows the respective estimated mean counterfactual outcome of treatment *l* for population *m*. Note that over time, employment is generally increasing, because the sample is conditioned on being unemployed in month zero.

The comparison of column (5) to column (6) and of column (8) to column (7) reveals the magnitude of the selection bias corrected for by the estimation procedure. It is up to a magnitude of about 9%-points for some comparisons. From the direction of the selection correction, we can infer whether one group has a priori better or worse chances on the labour market than the other. It turns out that participants in *short training* and to a lesser extent in *long training* have better a priori chances than *non-participants* and participants in *practice firms*. The same holds true for participants in *retraining* compared to *nonparticipants*. Note that due to sampling error in the estimates, the results do not allow for a complete ranking of all populations.

**Table 6.1: Estimated employment effects two and seven years after the beginning of the programme**

| Outcome | Month after beginning | Sample size m | Sample size l | $E(Y^m \mid S=m)$ | $E(Y^m \mid S=l)$ | $E(Y^l \mid S=m)$ | $E(Y^l \mid S=l)$ | $\theta_0^{m,l}$ | $-\theta_0^{l,m}$ | $\gamma_0^{m,l}$ |
|---|---|---|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Practice Firm (m) compared to nonparticipation (l) | | | | | | | | | | |
| Employed | 24 | 246 | 6910 | 42.3 | 39.7 | 38.3 | 34.2 | 4.0 | 5.5 | 5.4 |
| | 84 | 242 | 6772 | 48.8 | 51.2 | 44.5 | 41.0 | 4.3 | **10.2** | *8.9* |
| Practice Firm (m) compared to short training (l) | | | | | | | | | | |
| Employed | 24 | 246 | 501 | 42.3 | 43.5 | 50.6 | 54.1 | *-8.3* | **-10.6** | *-9.4* |
| | 84 | 242 | 494 | 48.8 | 52.0 | 51.5 | 54.7 | -2.7 | -2.7 | -1.3 |
| Practice Firm (m) compared to long training (l) | | | | | | | | | | |
| Employed | 24 | 246 | 267 | 42.3 | 53.2 | 44.0 | 49.4 | -1.7 | 3.8 | *-10.8* |
| | 84 | 242 | 263 | 48.8 | 52.3 | 45.8 | 54.4 | 3.0 | -2.1 | 1.0 |
| Practice Firm (m) compared to retraining (l) | | | | | | | | | | |
| Employed | 24 | 246 | 386 | 42.3 | 43.3 | 32.1 | 35.0 | **10.2** | 8.3 | 9.2 |
| | 84 | 242 | 381 | 48.8 | 49.0 | 62.5 | 62.7 | **-13.7*** | **-13.7** | *-11.6* |
| Short training (m) compared to nonparticipation (l) | | | | | | | | | | |
| Employed | 24 | 501 | 6910 | 54.1 | 48.5 | 36.5 | 34.2 | **17.6*** | **14.3*** | **14.8*** |
| | 84 | 494 | 6772 | 54.7 | 50.9 | 46.8 | 41.0 | **7.9** | **9.9*** | **9.6*** |
| Short training (m) compared to long training (l) | | | | | | | | | | |
| Employed | 24 | 501 | 267 | 54.1 | 58.0 | 50.2 | 49.4 | 3.9 | *8.6* | -0.9 |
| | 84 | 494 | 263 | 54.7 | 52.7 | 46.4 | 54.4 | *8.3* | -1.7 | 2.2 |
| Short training (m) compared to retraining (l) | | | | | | | | | | |
| Employed | 24 | 501 | 386 | 54.1 | 53.9 | 32.5 | 35.0 | **21.6*** | **18.9*** | **19.0*** |
| | 84 | 494 | 381 | 54.7 | 53.8 | 62.5 | 62.7 | *-7.8* | **-8.9** | *-9.2* |
| Long training (m) compared to nonparticipation (l) | | | | | | | | | | |
| Employed | 24 | 267 | 6910 | 49.4 | 52.7 | 41.1 | 34.2 | **8.3** | **18.5*** | **16.8*** |
| | 84 | 263 | 6772 | 54.4 | 48.8 | 48.6 | 41.0 | 5.8 | 7.8 | 7.1 |
| Long training (m) compared to retraining (l) | | | | | | | | | | |
| Employed | 24 | 267 | 386 | 49.4 | 47.7 | 29.8 | 35.0 | **19.6*** | **12.7** | **21.6*** |
| | 84 | 263 | 381 | 54.4 | 52.5 | 64.2 | 62.7 | *-9.8* | *-10.2* | **-12.1** |
| Retraining (m) compared to nonparticipation (l) | | | | | | | | | | |
| Employed | 24 | 386 | 6910 | 35.0 | 30.4 | 43.2 | 34.2 | **-8.2** | -3.8 | -4.3 |
| | 84 | 381 | 6772 | 62.7 | 61.1 | 47.0 | 41.0 | **15.7*** | **20.1*** | **19.3*** |

Note: **Bold** numbers indicate significance at the 5% level, numbers in *italics* relate to the 10 % level and * to the 1 % level.

The estimates of the mean effects of treatment *m* compared to treatment *l* for the subpopulation observed in the respective state can be computed directly from columns (5) to (8) and are reported in columns (9) to (10) together with an indicator of their asymptotic significance. Column (9) show the mean effect for participants in treatment *m* (difference between column (5) and (7)), while column (10) displays the results for participants in treatment l (difference between column (6) and (8)). Column (11) shows the effects for the joint population of participants and the nonparticipants. Since it also considers populations other than *m* and *l*, it can be larger or smaller than the mean effects for the populations *m* and *l*.

The results in columns (9) to (11) show that in the long and short run almost all programmes have positive effects compared to *nonparticipation,* although not always significant. The exception is *retraining* due to a lock-in effect which is most severe for this particularly long programme. In contrast to *retraining*, *long* and *short training* have short-run employment effects in the range of 8-18 %, falling to about 6-8 % after seven years (not significant for *long training*).

The pattern for *retraining* is quite different. After a negative lock-in effect, the employment effect rises to almost 16 % after seven years suggesting a substantial and sustainable impact of this expensive programme. In the long run, *retraining* dominates *practice firms*, but although it seems to have larger effects compared to *short* and *long training*, the latter are only significant at the 10 % level. Among the programmes, the effects for *practice firms* appear to be hardest to pin down. Finally, note that the comparison between *short* and *long training* reveals hardly any positive returns of the additional investment in time and money required for *long training.* In fact, the results are quite positive for a 'short' programme with a maximum duration of six months and a much lower average duration, although due to lacking cost data, other than very rudimentary cost-benefit considerations are impossible.

Finally, Table 6.1 is informative about the question whether, on average, caseworkers send those types of unemployed into the specific programmes that can expect the highest return from it. If this presumption is true, the effect must be larger for the respective participants (ATET) than for the participants in other programmes and nonparticipants (ATE). Even without exact standard errors for this test, it is obvious that, if there is evidence at all, it suggests that caseworkers do not send those unemployed into the programme for which the highest return is expected.

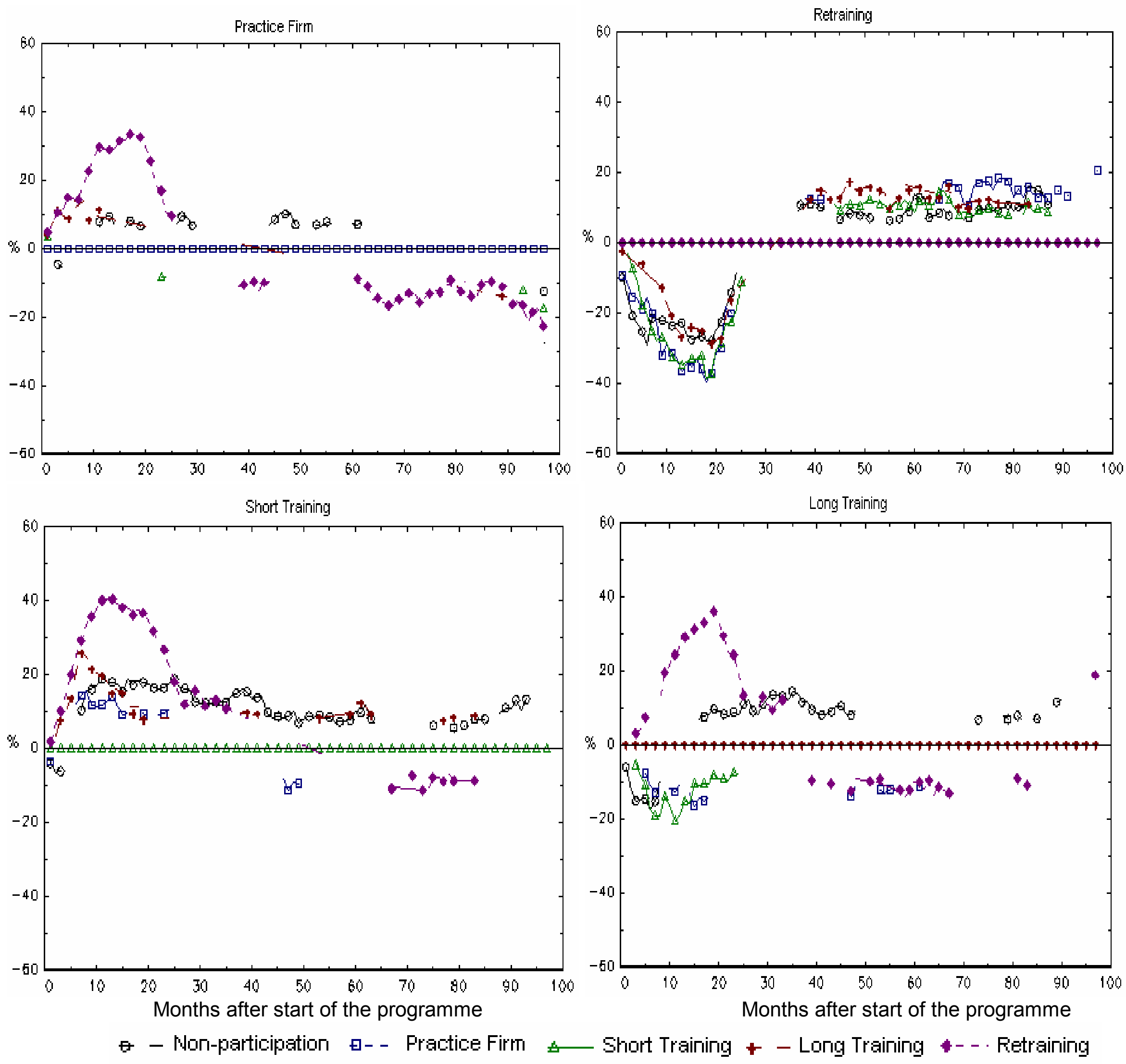
Figures 6.1 and 6.2 display the estimates of the effects of the different programmes (compared to the other states) for participants in the respective programme (ATET) for the two different outcome variables. A line above zero indicates that the programme has a positive effect relative to the programme (or nonparticipation) associated with that particular line. In other words, a line above zero is good news for the programme appearing in the header of the respective graph and bad news for the one associated with the particular line. Only effects significant at the 5 % level are displayed.

Figures 6.1 and 6.2 suggest that all programmes have some negative lock-in effect due to reduced job search or received job offers during participation in the programme. The length of this negative effect is very much tied to programme duration, being about two to three years for *retraining*, six months to one year for *long training*, three to six months for *short training* and *practice firms*.

After the lock-in effect, the reemployment chances for participants in *practice firms* are somewhat better than how they would have been in case of *nonparticipation*, but the effects are only mildly significant and appear not to be sustainable. Participating in *retraining* would have been superior in terms of employment chances.

After about three years there are substantial positive employment effects (Figure 6.1) of *retraining* for its participants compared to *nonparticipation* and the other types of training. A positive effect of similar size appears for *short training* compared to *nonparticipation* although it starts much earlier (nine months) and seems to decline slowly over time. The positive effect for *long training* compared to *nonparticipation* begins even later (18 months) and is not very incisive after about four years.

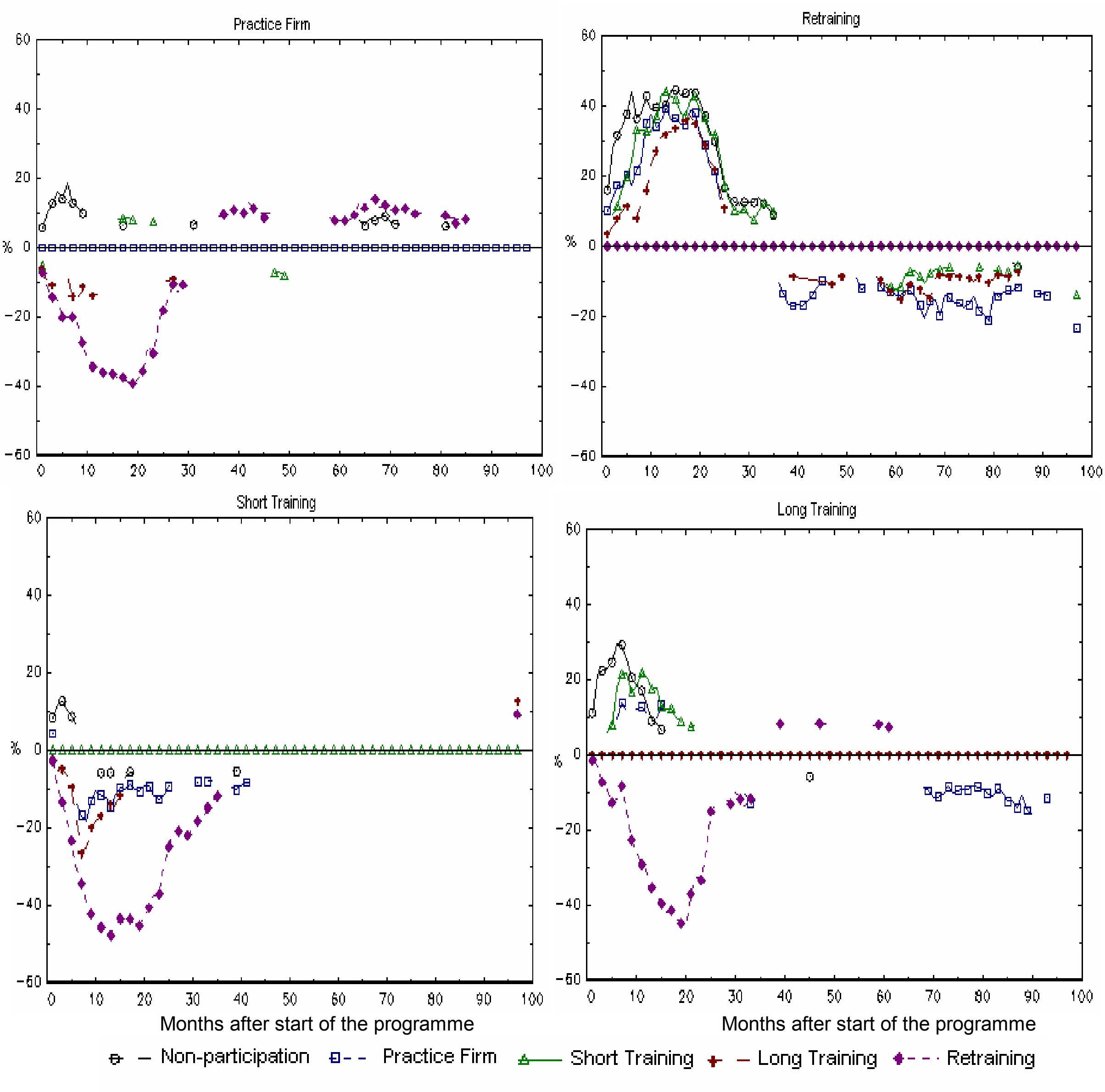
**Figure 6.1: Dynamics of the effects ($\hat{\theta}_t^{ml}$): Employment differences in %-points**




Note: Only effects that are significant at the 5% level (point wise) appear in the figures. Sample size declines quickly after 7 years (84 months).

To check whether jobs are (somewhat) stable, we use an outcome variable which requires at least seven months of continuous employment (six months is the usual probation period in Germany, within that period termination of a job is very easy for both sides). We obtained comparable results. In a similar vain we coded somebody as employed only if the person received at least 90% of the earnings of his last job prior to training. It does not change the conclusions (detailed results are available in the Internet Appendix).

**Figure 6.2: Dynamics of the effects ($\hat{\theta}_t^{ml}$): Unemployment (difference in %-points)**




Note: See note below Figure 6.1.

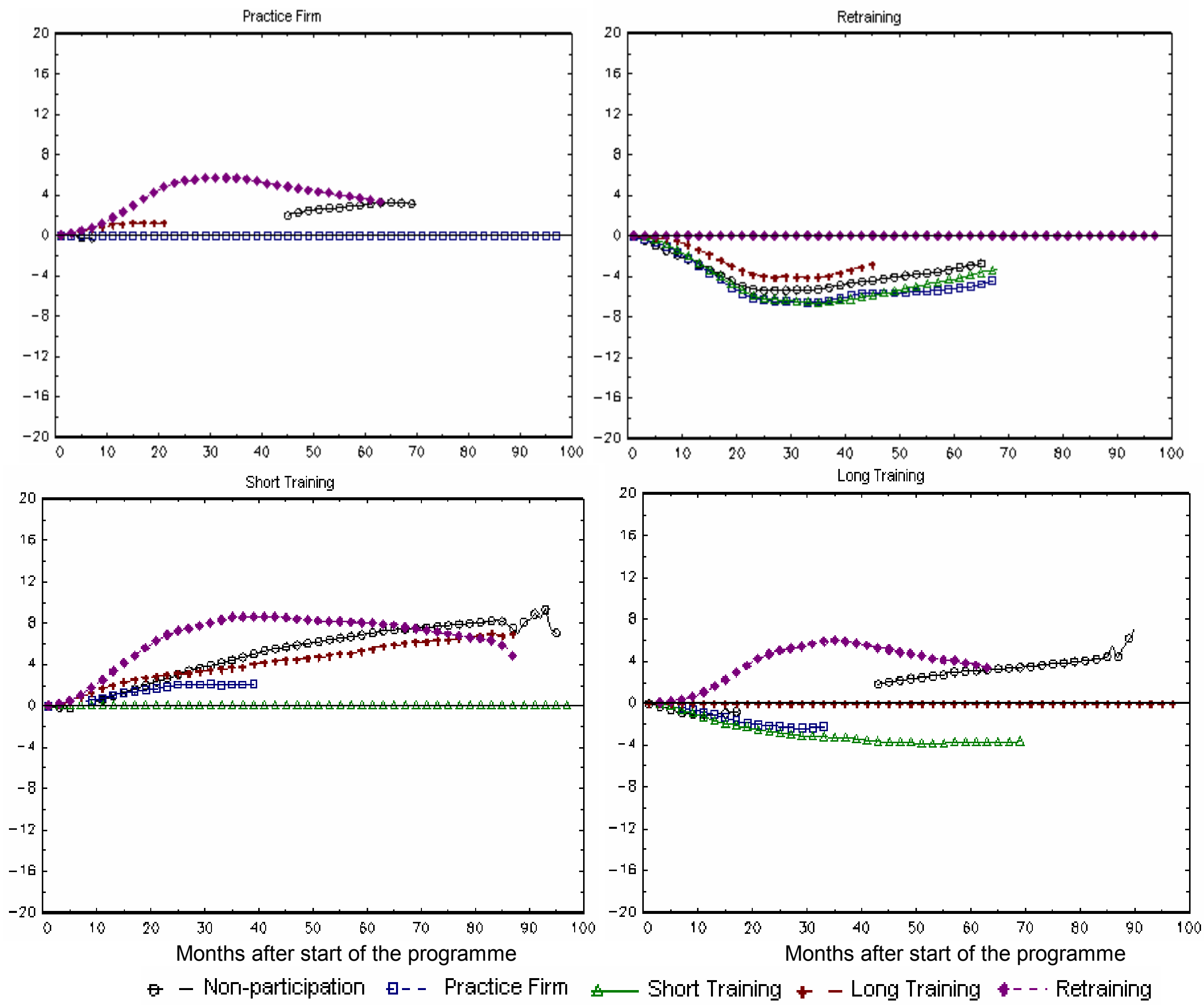
It is likely that a substantive programme like *retraining* may not only affect the employment probability, but the productivity of the new job as well. For the latter, earnings are a convenient summary measure. However, the earnings differences (zero earnings if not employed) are very much driven by the employment dynamics and hence, it is not surprising that they confirm the previous results: In year seven there is a gain in monthly earnings from *retraining* compared to *nonparticipation* of about 400 to 500 Euros (see Internet Appendix for details). The gains from *short* and *long training* compared to *nonparticipation* are positive as well, but about 250 Euros lower than for *retraining*.

At least from the point of view of the unemployment insurance system, considering the outcome variable registered unemployment is relevant (Figure 6.2). We find that none of the programmes dominates nonparticipation systematically, probably because programme participation increases the maximum unemployment benefit entitlement period, so that non-workers have an incentive of remaining registered. Thus, the major effect of the programmes compared to nonparticipation is that they bring those unemployed back to work that would otherwise leave the labour force. For inter-programme comparisons, Figure 6.2 by and large confirms the previous findings.

The previous figures showed that there are indirect costs of the programmes in terms of the initial negative effects most likely due to lock-in, i.e. a reduced job finding probability during programme participation. A first step of a cost benefit analysis is to compare the initial negative effects to the positive effects that may occur later. To do so, we accumulate the effects over time, starting with the first month of the programme. Figures 6.3 and 6.4 shows the respective total effects at any point in time during the seven year interval for which we have reliable data. Not surprisingly, the effects appear in a somewhat different light.

Considering employment first, Figure 6.3 shows that *short training* is now clearly the most attractive programme. Short programmes have by definition only a small lock-in effect, and thus their positive effect accumulates much longer, suggesting a gain of about seven to eight months of employment over the seven to eight years following programme start compared to *nonparticipation* and a corresponding gain of about four months compared to *retraining.* A similar shape shows up for *long training* compared to nonparticipation, but the level of the effects is somewhat different. There appear to be some positive accumulated effects for *practice firms* compared to *nonparticipation* as well, but they fade out after six years. For *retraining,* eight years are not sufficient to recover fully from the initial lock-in effect and to create an overall significantly positive effect (compared to all programmes and nonparticipation). Assuming a continuing trend, it seems likely that positive effects appear after ten years, but of course this projection remains a speculation. Nevertheless, after seven to eight years for participants in retraining it is impossible to conclude which of the available training schemes would be overall most effective for them.

**Figure 6.3: Accumulated employment effects ($\hat{\theta}_t^{ml} = \sum_{\tau=1}^{t} \hat{\theta}_\tau^{ml}$) in months**

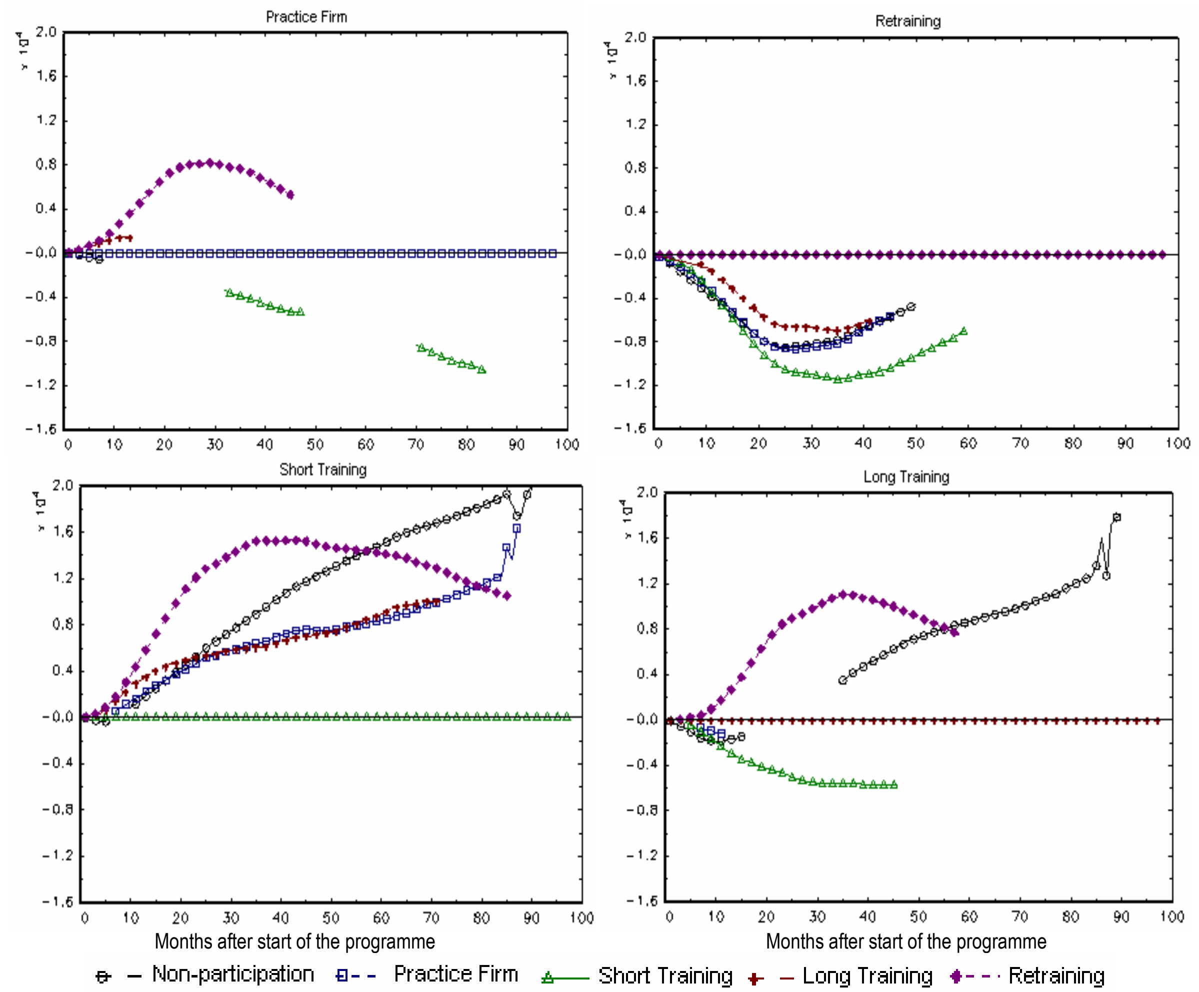



Note: See note below Figure 6.1. Read entry for $\hat{\theta}_t^{ml}$ as: "On average for participants in *m*, *t* months after beginning participation in *m*, it increased the total time in employment compared to *l* for by $\hat{\theta}_t^{ml}$ months."

Figure 6.4 shows that the shapes of accumulated earnings and employment effects are fairly similar. After about seven to eight years, the accumulated earnings gains (not discounted) in *short training* are very similar to those in *retraining* and *long training*. Compared to *nonparticipation* the break even point (passing the zero line from negative to positive) for *short training* occurs in month eight, for *long training* in month 23, and for *re-*

*training* in month 69. The accumulated effect for *practice firms* becomes positive after month 15, but is always insignificant.

**Figure 6.4: Accumulated effects ( $\sum_{\tau=1}^{t} \hat{\theta}_{\tau}^{ml}$ ): Monthly earnings differences in 10.000 EUR**




Note: See notes below Figure 6.1 and 6.3.

## 6.3 Heterogeneity by types of unemployed

Now, we investigate whether groups defined by different exogenous socio-economic characteristics exhibit different effects by stratifying the sample along the dimensions unemployment duration, regional unemployment rate, type of occupation and education and match within the strata.

## Table 6.2: Effect heterogeneity (employment) seven years after the beginning of the programme (difference in %-points) ($\theta_0^{m,l}$)

| | Regional UE-rate | | Big town | | Sex | | Long-term UE | | Type of occupation | | | Education | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| m - l | ≤10% | >10% | <100,000 inhabit. | ≥100,000 inhabit. | Men | Women | < 12 months | ≥ 12 months | Un-skilled | Skilled (incl. master crafts-man) | Salaried | Without profess. degree | With profess. degree |
| Practice firm - nonparticipation | *9.1* | -4.3 | 6.3 | -1.1 | -1.3 | **22.8*** | *9.0* | -0.6 | -3.3 | 2.4 | 9.5 | | 4.1 |
| Practice firm - short training | -3.1 | 7.6 | -5.0 | -8.9 | -6.1 | -2.9 | -1.5 | -9.4 | -2.8 | -14.7 | -8.5 | | -1.9 |
| Practice firm - long training | 4.0 | 0.8 | 0.2 | 2.3 | -2.6 | 12.5 | 4.8 | 2.7 | -4.0 | | -2.4 | | -0.9 |
| Practice firm - retraining | **-13.2** | -4.6 | -9.9 | -7.9 | **-21.1*** | -11.2 | **-15.8** | 0.1 | -7.2 | | *-17.2* | | **-14.4** |
| Short training - nonparticipation | **14.6*** | -6.8 | **15.6*** | -0.4 | **11.8*** | **10.9*** | **13.3*** | -4.0 | -3.5 | **12.0** | **11.2** | 5.3 | 5.4 |
| Short training - practice firm | 0.3 | 4.5 | 4.1 | 0.1 | **13.6** | *-16.0* | -5.0 | 4.1 | 4.7 | **20.8** | -0.9 | | 2.8 |
| Short training - long training | **14.2** | -6.0 | 6.3 | 10.1 | **15.7** | 2.0 | 4.4 | 8.0 | 0.0 | | 2.0 | | 13.2 |
| Short training - retraining | -8.1 | -11.8 | -0.7 | *-13.9* | -3.6 | *-13.4* | **-12.7** | 5.1 | *-12.1* | | **-20.8** | -8.1 | *-9.0* |
| Long training - nonparticipation | 5.7 | 3.8 | **19.0*** | -7.8 | 6.1 | 9.4 | **10.6** | -3.3 | -5.1 | | *10.1* | | 4.0 |
| Long training - practice firm | -6.4 | **31.5*** | -4.9 | 3.4 | 10.7 | -17.0 | -3.9 | 6.2 | 1.0 | | -3.5 | | 1.1 |
| Long training - short training | -6.0 | 13.4 | -2.4 | -0.6 | -2.9 | -0.3 | -0.7 | 4.2 | -1.0 | | -5.6 | | -0.6 |
| Long training - retraining | *-11.0* | -14.7 | -2.4 | **-28.9*** | *-12.3* | **-19.9** | **-16.7*** | 11.4 | -10.2 | | **-23.4** | | **-12.6** |
| Retraining - nonparticipation | **15.5*** | 6.8 | **15.6*** | **13.0** | **12.6*** | **15.2*** | **18.6*** | 3.7 | **9.8** | | 12.9 | **17.4*** | **12.0*** |
| Retraining - practice firm | 2.5 | **25.8** | 6.9 | -0.2 | **17.5** | -1.8 | 3.3 | *16.7* | **23.9*** | | 2.9 | | **20.5*** |
| Retraining - short training | 9.2 | 4.4 | 6.3 | **16.7** | 3.9 | 8.2 | 7.3 | 6.3 | *12.3* | | 3.9 | 0.1 | *8.7* |
| Retraining - long training | 10.2 | 9.4 | 5.0 | **27.5*** | 12.3 | 14.9 | 8.4 | 9.6 | 11.3 | | 13.7 | | **16.6** |
| Sample size | | | | | | | | | | | | | |
| Nonparticipation | 4245 | 1641 | 3177 | 2179 | 3597 | 2119 | 4082 | 1736 | 2184 | 1001 | 1097 | 1119 | 4835 |
| Practice firm | 163 | 67 | 137 | 82 | 159 | 73 | 140 | 84 | 82 | 52 | 73 | 40 | 193 |
| Short training | 348 | 110 | 277 | 161 | 285 | 141 | 301 | 160 | 137 | 92 | 141 | 58 | 405 |
| Long training | 165 | 74 | 147 | 90 | 127 | 96 | 161 | 85 | 52 | 32 | 107 | 26 | 225 |
| Retraining | 234 | 122 | 244 | 106 | 224 | 112 | 247 | 113 | 186 | 46 | 54 | 89 | 269 |

Note: **Bold** numbers indicate significance at the 5% level, numbers in *italics* relate to the 10% level and * to the 1% level. Comparisons based on less than 50 observations are not reported in the table. Cells shaded in grey indicate that the difference of the two estimated effects is significant at the 5% level.

Table 6.2 displays the results. The number of observations given in the lower part of this table indicates that in many cases the subsample estimates will be too imprecise to uncover significant differences.[37] Despite the problems of precision, some conclusions can be derived from this table. Compared to *nonparticipation*, all programmes are more effective in regions without serious unemployment problems and for short term unemployed (less than one year). The most significant male-female differences appear with respect to *practice firms*, which are ineffective for men, but highly effective for women. An explanation could be that the types of *practice firms* men and women mainly attend are different (men: mainly manufacturing; women: mainly commercial), so that we measure different effectiveness for the two types of programmes instead of a male-female difference. Finally, the results concerning education levels and type of occupation confirm the impression that effect heterogeneity does not appear to be present on a massive scale.

## 6.4 Sensitivity analysis

We performed several sensitivity tests to check whether choices about implementational issues are relevant for the results we obtain. For sake of brevity, we summarise the results and refer the interested reader to the Internet Appendix for any details.

First, the common support criterion is made stricter by defining the upper and lower bounds as 10$^{th}$ largest and smallest observation instead of the minimum or maximum, leading to a better match in the tails of the propensity score distribution. In addition to the condition used before, another 40 % of *nonparticipants* are deleted. The corresponding numbers for *practice firms* are 12 %, *short training* 36 %, *long training* 30 % and *retraining* 35 %. Due to the smaller number of observations some effects are no longer significant, but the conclusions do not change.

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[37] We use the MNP estimate from the joint model, but the remaining steps of the estimation are performed in the subsamples. Therefore, the observations do not add up to the number of the observations in the full sample, because the common support criterion must delete more observations if used in subsamples separately (a table with detailed numbers is available in the internet appendix).

Second, the additional matching variables other than sex used to define the distance metric in the matching algorithm are not used. Again, the results are qualitatively the same, but in particular for *retraining,* the effects are somewhat smaller and fewer of them are significant.

Third, since the effects for men and women based on the common estimation of the MNP model showed considerable effect heterogeneity, it might be suspected that more flexibility is required when estimating the decision to participate in a programme. Therefore, we estimate MNP's for men and women separately but do not find significant differences in the effects compared to the case with a common MNP model.

The next check concerned the question to smooth (and thus increase precision) the estimated effects by computing three month moving averages of the respective outcome variables. Not surprisingly, the results are a bit 'smoother', but the efficiency gains appear to be very small.

Fifth, for a selected outcome variable, namely accumulated employment, in the Internet Appendix we report the results for simple one-to-one matching as in Gerfin and Lechner (2002). The main change is that several positive effects for *practice firms* compared to *nonparticipation* are estimated more noisily and could not be detected with one-to-one matching.

Finally, the Internet Appendix reports in detail the estimated asymptotic bias of the weighted matching estimator used here (all results are adjusted for this bias). A large and volatile value of it would clearly raise concerns about the adjustment procedure. This is not the case, however, compared to the magnitude of the effects.

# 7 Conclusion

This paper presents evaluation results for different forms of West German public sector sponsored training programmes of the mid 1990s. The empirical analysis is based on a new administrative data base constructed for evaluation purposes that supports selectivity correction by microeconometric matching methods as well as the identification of effects over a horizon of more than seven years. We find that all programmes have negative effects in the short run and most of them positive effects over a horizon of about four years.

However, the results for the various programmes differ quit substantially when considering our key outcome variable, employment, seven years after programme start, which is the longest horizon for which we have reliable outcome information. Retraining, involving the most substantial investment in human capital dominates all other programmes as well as the state of nonparticipation. The gain in employment probability over nonparticipation is about 10-15 %-points. This does not only hold for participants in retraining, but also for participants in other programmes had they participated in retraining. Short and long training dominate nonparticipation with a somewhat smaller gain of about 5-9 % after seven years. Positive effects for practice firms, if any, appear to be too small to be detectible with our sample sizes.

Focussing on the overall performance over the seven year period, i.e. netting out positive and negative effects over time, the findings change somewhat, because the different programmes have very different lock-in effects that are directly related to their duration. In this comparison, shorter programmes (below six months) outperform the rest. Compared to nonparticipation, the gain after seven years would be about eight months of additional employment. It is about half for longer training courses other than retraining. For retraining, the initial lock-in effects are so large that a period of seven years is too short to allow significant positive effects to be detected, although there is a clear trend towards positive overall effects. Finally, no sustainable positive effects are visible for practice firms. Compared to nonparticipation even after seven years, all programmes increase the duration of benefit receipt. The increase due to retraining is about 10 months, for short training it is a few days, for long training 3 months and practice firms increase benefit receipt by about 4 months. These numbers point again to the fact that the positive and sustainable employment and earnings effects of retraining come at a considerable cost.

Our findings help to understand a puzzle that occurred in the previous literature, namely that for most training programmes significantly positive effects cannot be found. It may be a needle in a haystack problem, i.e. if we believe the kind of consensus of the education literature that returns in earnings of one year of full-time schooling are between five and ten percent, then it would be surprising if programmes for unemployed with a duration of a few months or much less can have effects large enough to be detectible by noisy data, even if the samples are large. Yet, even the group of 'shorter' German programmes are fairly long by international comparisons, and particularly retraining involves rather significant human capital investments that can be expected to be above the 'noise-threshold' level, and are thus detectible in our study.

When trying to relate our finding of positive long-run effects of the long-term retraining programme to the literature, we discovered that there are very few evaluation studies for these types of programmes. Part of the explanation for this is that only very few OECD countries use long programmes in ALMP, and even if those programmes are evaluated, these studies are either not using an implicit or explicit control group design or long-term outcomes are not available. An exception is the paper by Winter-Ebmer (2003) who investigates a special programme used to assist the restructuring of the Austrian steel industry. There was a substantial human capital enhancing component in this programme for which he finds positive effects five years after leaving the programme.

To conclude, the literature developed the consensus that it is most important for evaluation studies to obtain large and highly informative data to control for selective participation in different programmes. In this paper, we acknowledge this fact and use a large and informative database, but we also point out that successfully controlling for selection effects does not imply that we are estimating an interesting policy parameter. If we want to understand the differential effects of training programmes that substantially differ with respect to their human capital augmenting nature, data that cover more than two to three years after the programme are crucial. However, there is a price to pay, namely that the programmes under consideration have to be implemented at least about ten years before the study is conducted. Typically, politicians are fairly impatient and econometricians tend to deliver the information that the policy makers request. Recent studies based on large and informative administrative data which were induced by the respective governments, like Gerfin and Lechner (2002) for Switzerland or Sianesi (2004) for Sweden, are interesting for understanding the immediate effects of short programmes that are not expected to change the long-term prospects of the unemployed by adding substantial human capital. The reason is that their only long-term effect is indirect by bringing participants into employment more or less immediately after the programme. Thus, short-term employment effects might be informative about long-term employment effects. If an active labour market policy consists basically of these short programmes, like the Swiss one, or the Swedish components Sianesi (2004) looked at, then this approach is valuable. However, for the long German programmes with the clear intention of substantial human capital addition, short-term effects can rarely be positive because the lock-in effect is much more important. Therefore, observing the outcomes over a longer time horizon, e.g. seven to eight years like in this paper, is crucial to obtain some understanding of the overall effects of these programmes. As an interesting by-product we obtain information whether the short-term effects of the short programmes are good predictors of their long-term effects.

Future work will create a data base containing, in particular, caseworker information which is missing from the current data version. However, due to the administrative data collection process this new evaluation will only be possible for programmes having started after 2000. Therefore, we expect to report new long-term effects of German training programmes based on an improved selection correction not before 2010. Until then, the information provided in this paper is probably our best guess about long-term effectiveness of training programmes that substantially improve human capital.

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# Appendix A: Data

### Table: A.1: Descriptive statistics

| | Nonparticipation | Practice firm | Short training | Long training | Retraining | Career improvement | Other |
|---|---|---|---|---|---|---|---|
| Number of observations | 9197 | 273 | 572 | 329 | 413 | 110 | 74 |
| | | | | Proportions in % | | | |
| | | | | Personal characteristics | | | |
| Women | 41 | 33 | 37 | 39 | 38 | 24 | 27 |
| Older than 50 years | 15 | 7 | 6 | 5 | 1 | 3 | 1 |
| Younger than 26 years | 14 | 19 | 15 | 15 | 21 | 21 | 22 |
| Age* | 37 | 36 | 35 | 35 | 31 | 32 | 32 |
| Nationality: German | 81 | 87 | 91 | 92 | 89 | 92 | 92 |
| Western European | 12 | 7 | 6 | 5 | 7 | 3 | 5 |
| Eastern European | 4 | 3 | 2 | 1 | 2 | 3 | 1 |
| Other | 3 | 3 | 1 | 3 | 2 | 3 | 1 |
| Marital status: Single | 48 | 60 | 58 | 59 | 62 | 64 | 53 |
| Married | 52 | 40 | 42 | 41 | 38 | 36 | 47 |
| Children: No child | 62 | 73 | 64 | 65 | 61 | 72 | 58 |
| At least one child | 38 | 27 | 36 | 35 | 39 | 28 | 42 |
| | | | | Education | | | |
| No university entrance degree, no professional degree (PD) | 25 | 18 | 15 | 9 | 24 | 5 | 5 |
| No university entrance degree, with PD | 65 | 78 | 75 | 71 | 68 | 80 | 81 |
| University entrance degree, no PD | 1 | 1 | 1 | 2 | 1 | 1 | 1 |
| University entrance degree and PD | 3 | 2 | 3 | 5 | 3 | 5 | 8 |
| Polytechnical degree | 2 | 0 | 3 | 5 | 1 | 5 | 1 |
| University degree | 4 | 0 | 3 | 9 | 2 | 5 | 1 |
| | | | | Position in last job | | | |
| Salaried employee | 28 | 34 | 38 | 57 | 23 | 41 | 39 |
| Part-time worker | 10 | 5 | 8 | 7 | 8 | 4 | 4 |
| Master craftsman | 1 | 1 | 1 | 0 | 0 | 1 | 1 |
| Unskilled worker | 40 | 36 | 31 | 19 | 53 | 9 | 2 |
| Skilled worker | 21 | 23 | 23 | 16 | 16 | 45 | 35 |
| | | | | Industrial sector | | | |
| Construction | 8 | 5 | 6 | 5 | 5 | 14 | 9 |
| Commerce | 16 | 16 | 20 | 16 | 12 | 12 | 15 |
| Banking, insurance | 1 | 3 | 1 | 3 | 2 | 1 | 3 |
| Local, regional authorities, social insurance | 4 | 5 | 5 | 7 | 6 | 4 | 1 |
| Non-profit organisations, private households | 3 | 3 | 2 | 3 | 4 | 3 | 0 |
| Argiculture, forestry, fishing | 1 | 3 | 1 | 1 | 1 | 0 | 0 |
| Energy and supply industry, mining | 0 | 1 | 0 | 1 | 0 | 1 | 0 |
| Manufacturing (without construction) | 38 | 40 | 38 | 38 | 41 | 48 | 49 |
| Transportation, telecommunications | 5 | 4 | 5 | 4 | 7 | 3 | 7 |
| Other services | 23 | 20 | 21 | 22 | 22 | 15 | 16 |

Table A.1 to be continued

**Table: A.1: Descriptive statistics (continued-1)**

| | Nonpar-ticipa-tion | Practice firm | Short training | Long training | Retraining | Career improve-ment | Other |
|---|---|---|---|---|---|---|---|
| | Last monthly earnings | | | | | | |
| Salary in Euros* | 1680 | 1640 | 1773 | 1889 | 1640 | 2072 | 1781 |
| No information++ | 11 | 3 | 4 | 4 | 3 | 5 | 3 |
| | Regional information | | | | | | |
| Big city (at least 300,000 inhabitants) | 26 | 18 | 24 | 29 | 20 | 26 | 7 |
| North (Hamburg, Bremen, Schlesw.-H.) | 22 | 30 | 19 | 25 | 26 | 18 | 15 |
| North-Rhine-Westphalia | 31 | 21 | 28 | 36 | 35 | 23 | 28 |
| Rhineland-Palatinate, Hesse, Saarl. | 17 | 23 | 19 | 18 | 17 | 17 | 19 |
| Baden-Wuerttemberg, Bavaria | 30 | 26 | 34 | 21 | 22 | 42 | 38 |
| Local UE rate ≤ 5 % ** | 1 | 2 | 2 | 0 | 1 | 2 | 4 |
| Local 5 % < UE rate ≤ 10 % ** | 64 | 67 | 70 | 62 | 61 | 72 | 76 |
| Local UE rate > 10 % ** | 35 | 31 | 28 | 38 | 38 | 26 | 20 |
| | Last occupation in/as | | | | | | |
| Agriculture, forestry, fishing | 2 | 2 | 2 | 3 | 3 | 0 | 0 |
| Plumbing, metal construction technology | 9 | 15 | 11 | 7 | 9 | 19 | 7 |
| Food and nutrition | 4 | 2 | 4 | 1 | 4 | 2 | 4 |
| Construction, woodworking | 10 | 8 | 8 | 5 | 9 | 15 | 15 |
| Merchant (goods and services) | 9 | 12 | 12 | 9 | 6 | 5 | 7 |
| Transportation, storage | 13 | 12 | 14 | 9 | 15 | 3 | 15 |
| Administration, office work, business, social scienc. | 14 | 22 | 22 | 31 | 13 | 17 | 16 |
| Health services | 2 | 1 | 1 | 1 | 3 | 2 | 3 |
| Hairdressing,guest assist., housekeeping, cleaning | 8 | 6 | 3 | 2 | 7 | 3 | 1 |
| Chemical worker, polymer processing | 3 | 1 | 1 | 2 | 6 | 1 | 0 |
| Unskilled worker | 2 | 4 | 2 | 1 | 2 | 0 | 1 |
| Metal production and processing | 4 | 5 | 4 | 2 | 6 | 2 | 8 |
| Textile, leather, clothing | 3 | 1 | 1 | 2 | 2 | 0 | 1 |
| Security services | 1 | 1 | 1 | 1 | 1 | 4 | 0 |
| Paper manufacture and processing, printing | 2 | 2 | 2 | 2 | 2 | 4 | 1 |
| Social services, education, counselling | 3 | 0 | 0 | 2 | 4 | 2 | 1 |
| Media, humanities, arts | 1 | 0 | 1 | 1 | 0 | 0 | 1 |
| Mining | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Technology, natural sciences | 4 | 1 | 6 | 16 | 2 | 15 | 14 |
| Machinist | 0 | 0 | 0 | 0 | 0 | 1 | 0 |
| Electronics | 4 | 2 | 3 | 4 | 4 | 7 | 4 |
| Stone, ceramics, glass making and/or processing | 1 | 1 | 1 | 0 | 1 | 0 | 0 |

Table A.1 to be continued

## Table: A.1: Descriptive statistics (continued-2)

| | Nonparticipation | Practice firm | Short training | Long training | Retraining | Career improvement | Other |
|---|---|---|---|---|---|---|---|
| Remaining unemployment (UE) benefits claim at the end of the last unemployment spell before entry in the programme | | | | | | | |
| Remaining UE benefits claim (in months)* | 8.1 | 7.4 | 7.8 | 7.2 | 5.8 | 6.8 | 6.5 |
| No information or no claim | 44 | 48 | 44 | 42 | 38 | 27 | 31 |
| Legal UE benefits claim at the beginning of the last unemployment spell before the programme | | | | | | | |
| Legal claim* (months) | 12.6 | 11.0 | 11.5 | 10.6 | 9.3 | 10.1 | 9.6 |
| No information | 26 | 29 | 28 | 24 | 25 | 17 | 20 |
| No claim | 4 | 5 | 3 | 4 | 3 | 3 | 3 |
| Unemployment benefits or assistance in the month before the beginning of the programme | | | | | | | |
| UE benefits | 73 | 71 | 72 | 76 | 75 | 83 | 80 |
| UE assistance | 27 | 29 | 28 | 24 | 25 | 17 | 20 |
| Various historical un/ out-of/employment information before the "first unemployment period" | | | | | | | |
| Months of last employment spell* | 50 | 44 | 51 | 49 | 40 | 48 | 45 |
| Proportion of employment months (in %)* | 72 | 70 | 72 | 72 | 70 | 80 | 75 |
| Proportion of out-of-labour months (in %)* | 13 | 12 | 13 | 13 | 16 | 11 | 10 |
| Proportion of UE months (in %)* | 11 | 12 | 10 | 9 | 9.03 | 43 | 8 |
| # of programs up to 2 years before the UE period (UEP)* | 0.09 | 0.11 | 0.09 | 0.12 | 0.12 | 0.15 | 0.08 |
| # of programs up to 5 years before the UEP* | 0.18 | 0.25 | 0.23 | 0.22 | 0.26 | 0.19 | 0.22 |
| # of programs from entry in the data up to UEP* | 0.28 | 0.45 | 0.39 | 0.40 | 0.34 | 0.25 | 0.41 |
| Mean duration of UE spells up to 2 years before UEP* | 1.48 | 1.74 | 1.30 | 1.19 | 1.23 | 0.69 | 0.87 |
| up to 5 years before UEP* | 2.77 | 3.05 | 2.31 | 2.03 | 2.30 | 1.16 | 1.77 |
| from entry in data to UEP* | 3.95 | 4.09 | 3.5 | 3.3 | 2.95 | 2.13 | 3.04 |
| Mean duration of employment spells | 3.7 | 3.5 | 3.3 | 4.6 | 4.8 | 4.8 | 3.8 |
| up to 2 years before UEP* | | | | | | | |
| up to 5 years before UEP* | 12.1 | 12 | 12.4 | 14.8 | 13.2 | 13 | 16.5 |
| from entry in data to UEP* | 48.6 | 42.0 | 48.6 | 47.5 | 35.7 | 47.3 | 41.5 |
| Mean duration of out-of-labour spells | 2.20 | 1.91 | 1.70 | 1.70 | 1.39 | 1.11 | 1.62 |
| up to 2 years before UEP* | | | | | | | |
| up to 5 years before UEP* | 3.61 | 3.22 | 2.95 | 2.94 | 3.60 | 3.19 | 3.86 |
| from entry in data to UEP* | 7.58 | 5.64 | 7.66 | 9.25 | 8.82 | 6.72 | 6.58 |
| Total months in all programmes up to | 0.73 | 0.52 | 0.47 | 0.76 | 0.70 | 0.63 | 0.46 |
| 2 years before the UEP* | | | | | | | |
| 5 years before the UE P* | 1.32 | 1.54 | 1.55 | 1.53 | 1.48 | 0.95 | 1.54 |
| before entry in the sample* | 2.07 | 2.89 | 2.85 | 2.73 | 2.04 | 1.65 | 2.58 |
| Programme information | | | | | | | |
| Planned programme duration* (months) | | 5.95 | 3.82 | 9.74 | 20.43 | 9.91 | 5.09 |

Table A.1 to be continued

**Table: A.1: Descriptive statistics (continued-3)**

| | Nonpar-ticipa-tion | Practice firm | Short training | Long training | Retraining | Career improve-ment | Other |
|---|---|---|---|---|---|---|---|
| Various un/employment information from the "first unemployment period" | | | | | | | |
| Duration of the "first UE spell"* | 7.60 | 6.55 | 6.91 | 6.59 | 5.56 | 5.26 | 4.42 |
| Duration of last UE spell before programme* | 6.75 | 5.49 | 5.57 | 5.30 | 4.49 | 4.25 | 3.95 |
| Time since beginning of last UE spell (before prog.) even if other state between UE and prog.* | 6.77 | 6.60 | 7.08 | 6.60 | 5.61 | 5.60 | 4.85 |
| Time between the prog. and last job* | 15.0 | 13.4 | 13.8 | 12.3 | 11.0 | 9.7 | 10.8 |
| time between programme - last job ≤ 3 months | 16 | 11 | 16 | 17 | 22 | 26 | 20 |
| ≤ 6 months | 35 | 37 | 39 | 43 | 43 | 46 | 59 |
| ≤ 12 months | 62 | 67 | 67 | 67 | 74 | 75 | 78 |
| ≤ 24 months | 84 | 88 | 91 | 90 | 92 | 95 | 92 |
| Transition in 6 months before programme: UE. → UE | 58 | 56 | 61 | 56 | 54 | 44 | 42 |
| empl. → UE | 26 | 27 | 27 | 33 | 32 | 39 | 45 |
| out → UE | 14 | 16 | 10 | 10 | 10 | 8 | 11 |
| prog. → UE | 2 | 1 | 1 | 1 | 4 | 9 | 3 |
| Number of prog. in year before actual progr.* | 0.04 | 0.03 | 0.02 | 0.03 | 0.07 | 0.14 | 0.05 |
| Number of prog.'s in 6 months before actual pr.* | 0.02 | 0.01 | 0.01 | 0.02 | 0.04 | 0.10 | 0.03 |
| Firms size of the last employer | | | | | | | |
| No information | 4 | 4 | 4 | 3 | 3 | 4 | 5 |
| 1 to 9 employees | 21 | 21 | 21 | 19 | 18 | 27 | 27 |
| 10 to 99 employees | 36 | 36 | 38 | 36 | 35 | 31 | 30 |
| 100 to 499 employees | 21 | 25 | 22 | 23 | 23 | 24 | 26 |
| 500 employees or more | 19 | 14 | 15 | 18 | 21 | 15 | 12 |
| Timing of programme and appearance in data on average (average month) | | | | | | | |
| Date of entry in the data* | Oct. 83 | Dec. 83 | Jun. 83 | Aug. 83 | May 85 | Jul. 84 | Apr. 84 |
| Date of UE spell defining treat. status* | Oct. 93 | Aug. 93 | Sep. 93 | Sep. 93 | Sep. 93 | Jul. 93 | Sep. 93 |
| Date of begin of prog.* | Jul. 94 | Mar. 94 | May 94 | May 94 | Mar. 94 | Jan. 94 | Mar. 94 |
| Date of exit from the data* | May 00 | Dec. 00 | Feb. 01 | Jan. 01 | May 01 | Jan. 01 | Apr. 01 |
| Outcome+ | | | | | | | |
| Unemployment in | | | | | | | |
| Jan. 1990 9559 | 17.7 | 21.5 | 20.4 | 16.6 | 15.3 | 5.9 | 18.0 |
| 1992 10609 | 19.1 | 21.3 | 17.9 | 15.5 | 15.9 | 9.3 | 17.4 |
| 1993 10870 | 24.7 | 30.8 | 27.9 | 21.9 | 31.5 | 30.0 | 27.4 |
| 1994 10960 | 66.2 | 77.7 | 72.9 | 70.5 | 74.8 | 78.2 | 52.7 |
| 1995 10940 | 65.1 | 60.4 | 64.2 | 73.6 | 86.4 | 60.9 | 16.2 |
| 1997 10872 | 37.1 | 39.9 | 32.2 | 30.4 | 40.8 | 18.4 | 14.9 |
| 1999 10670 | 28.4 | 30.1 | 28.5 | 23.8 | 24.6 | 15.6 | 20.3 |
| 2001 10670 | 19.2 | 24.1 | 21.4 | 15.7 | 17.0 | 10.1 | 12.2 |

Table A.1 to be continued

**Table: A.1: Descriptive statistics (continued-4)**

| | | Non-partici-pation | Practice firm | Short training | Long training | Retrain-ing | Career improve-ment | Other |
|---|---|---|---|---|---|---|---|---|
| Employment in | | | | | | | | |
| Jan. 1990 | 9559 | 69.5 | 68.0 | 67.0 | 71.3 | 66.9 | 81.4 | 70.5 |
| 1992 | 10609 | 69.1 | 67.5 | 70.0 | 73.3 | 68.5 | 80.6 | 68.1 |
| 1993 | 10870 | 60.9 | 55.7 | 60.5 | 64.7 | 57.8 | 62.7 | 63.0 |
| 1994 | 10960 | 24.1 | 17.2 | 22.7 | 22.5 | 20.3 | 14.6 | 41.9 |
| 1995 | 10940 | 17.8 | 28.6 | 29.9 | 19.8 | 9.4 | 32.7 | 73.0 |
| 1997 | 10872 | 30.1 | 43.9 | 49.4 | 52.6 | 46.1 | 60.6 | 70.3 |
| 1999 | 10670 | 33.6 | 47.4 | 49.4 | 51.2 | 56.8 | 56.9 | 59.5 |
| 2001 | 10670 | 36.3 | 47.0 | 52.8 | 54.9 | 60.0 | 61.5 | 59.5 |
| Out-of-Labour in | | | | | | | | |
| Jan. 1990 | 9559 | 12.7 | 10.5 | 12.7 | 12.2 | 17.9 | 12.8 | 11.5 |
| 1992 | 10609 | 11.8 | 11.2 | 11.9 | 10.9 | 15.6 | 10.2 | 14.5 |
| 1993 | 10870 | 14.4 | 13.6 | 11.6 | 13.4 | 10.7 | 7.3 | 9.6 |
| 1994 | 10960 | 9.7 | 5.1 | 4.4 | 7.0 | 4.8 | 7.3 | 5.4 |
| 1995 | 10940 | 16.9 | 11.0 | 5.9 | 6.7 | 3.9 | 6.4 | 10.8 |
| 1997 | 10872 | 32.6 | 16.2 | 18.3 | 17.0 | 12.9 | 19.3 | 14.9 |
| 1999 | 10670 | 37.9 | 22.6 | 22.1 | 25.0 | 18.7 | 26.6 | 20.3 |
| 2001 | 10670 | 44.6 | 29.0 | 25.9 | 29.3 | 23.1 | 28.4 | 28.4 |

Note: The sample used for the table is the one after all selection steps described in Section 3, but before imposing the common support requirement. *The results for variables marked with an asterisk are means rather than proportions. **Local unemployment rates for each of the 141 local labour office districts. +The different outcomes do not add up to 100% because of some missing values. ++ The category 'No information' includes both cases with missing earnings information and with the entry '0'. Zero entries are made for so-called inactive employment which includes women on maternity leave, men in the military or civil service, as well as employees having been ill for more than six weeks. The first column gives the number of observations used to compute the proportions. The sample size decreases due to different entry dates into the sample (first UE spell in 93/94) and exit dates from the sample. Results for subpopulations with less than 50 observations are not reported.

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## Impressum

IABDiscussionPaper
**No. 3 / 2005**

**Herausgeber**
Institut für Arbeitsmarkt- und Berufsforschung
der Bundesagentur für Arbeit
Weddigenstr. 20-22
D-90478 Nürnberg

**Redaktion**
Regina Stoll, Jutta Palm-Nowak

**Technische Herstellung**
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