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Michael Lechner, Ruth Miquel, and Conny Wunsch

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Department of Economics University of St. Gallen

Editor: Prof. Jörg Baumberger
University of St. Gallen
Department of Economics
Bodanstr. 1
CH-9000 St. Gallen
Phone ++41 71 224 22 41
Fax ++41 71 224 28 85
Email joerg.baumberger@unisg.ch

Publisher: Department of Economics
University of St. Gallen
Bodanstrasse 8
CH-9000 St. Gallen
Phone +41 71 224 23 25
Fax +41 71 224 22 98

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Michael Lechner

Ruth Miquel

Conny Wunsch

Authors address:

Professor Dr. Michael Lechner
SIAW-HSG
Bodanstr. 8
9000 St. Gallen
Tel. ++41 71 224 23 50
Fax ++41 71 224 22 98
Email michael.lechner@unisg.ch
Website www.siaw.unisg.ch

Abstract

We analyse the effects of government sponsored training for the unemployed conducted during East German transition. For the microeconometric analysis, we use a new, large and informative administrative database that allows us to use matching methods to reduce potential selection bias, to study different types of programmes, and to observe interesting labour market outcomes over 8 years. We find that, generally, all training programmes under investigation increase long-term employment prospects and earnings. However, as an important exception, the longer training programmes are on average not helpful for their male participants. At least part of the explanation for this negative result is that caseworkers severely misjudged the structure of the future demand for skills.

Keywords

Active labour market policy, nonparametric identification, matching estimation, causal effects, programme evaluation, panel data, gender differences

JEL Classification

J 68

1 Introduction*

In the course of the monetary, economic and social union in July 1990, East Germany almost immediately took over most of the rules and regulations that govern economic life from West Germany (currency, legal order, wage structure, social system, etc.). Transitional arrangements were rare, so, for example, a low wage policy by devaluating the currency, as used by other transitions countries, was not possible. As a result, the formerly centrally planned Soviet-style East German economy - almost over night - faced the conditions of a modern Western-style market economy. Until December 1990, production of goods dropped to only 50% of its 1989 level and unemployment rose rapidly (Akerlof, Rose, Yellen and Hessenius, 1991). To accommodate the dramatic developments in the East German labour market, the German government used active labour market policies (ALMP) on a massive scale. For example, at its peak in 1992, expenditures for ALMP summed up to more than 9% of the East German GDP.

From the early stages of transition on, government sponsored training played an important role in adjusting the skills of the East German labour force - which had become largely obsolete under the new system - to the requirements of a modern market economy. Training programmes were very heterogeneous but in many cases the human capital investment associated with these courses was substantial. Durations of one or two years were quite common and some courses even had durations of up to three years. In this study, we analyse the short and long-term effects of government sponsored training for the unemployed that has been conducted in the period 1993-1994. Our findings point out three issues. First, as has been emphasised for Western market economies by e.g. Couch (1991), Hotz,

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Imbens and Klerman (2000), Jacobson, LaLonde and Sullivan (2004), Jesperson, Munch and Skipper (2004), Lechner, Miquel and Wunsch (2004), and Winter-Ebmer (2001), it is important to study the long-run effects of training programmes that have a substantial human capital component. They all seem to agree that the appearance of positive long-run effects of training is likely (as opposed to the previous literature, which, based on shorter follow-up periods, could not agree on any likely sign of the training effects; see e.g. Martin and Grubb, 2001). We find that all programmes under investigation exhibit negative lock-in effects in the short run that are directly related to planned programme duration. In the long run, all programmes have positive employment effects. Thus, measuring long-run effects is crucial to determine whether or not employment rates recover from potentially severe lock-in effects of long training programmes. Second, find considerable effect heterogeneity among participants as well as across different types of programmes: In contrast to the overall averaged results, we find no or even negative employment effects of longer training programmes for men, but positive effects for women. The explanation for these gender differences is that in the period under investigation, men and women have been trained towards completely different occupations. However, caseworkers severely misjudged the future demand for skills for men. Many male participants have been trained towards occupations in the construction sector (about 70% of the participants of in the longest programme) which experienced a boom in the early 1990s but went into bust soon after the courses were completed, so that the blessings of having obtained funding for long vocational training became a curse for its participants. This also points to the third issue: It is important (but difficult) to correctly target training and other ALMP measures, especially in a rapidly changing economy.

Earlier studies that analyse government sponsored training in East Germany had not been able to address these issues. Although evidence for other transition countries like Poland (O'Leary, 1998b; O'Leary, Kolodziejczyk and Lázár, 1998; Puhani, 1999; Kluve, Lehmann and Schmidt, 1999, 2004)[1], the Slovak Republic (Lubyova and Van Ours, 1999) and Hungary (O'Leary, 1998a; O'Leary, Kolodziejczyk and Lázár, 1998) suggests that training programmes have succeeded in raising individual employment probabilities of participants, the evidence for East Germany was so far mixed.

[1] Earlier studies of Puhani (1996) and Puhani and Steiner (1997) find, however, no significant effects.

The earlier studies use survey data which are, however, rather limited with respect to length of the observation period, sample sizes and the availability of sufficiently detailed information to account for selectivity and programme heterogeneity.[2] Most of these studies use parametric models[3] and find negative or insignificant short- to medium-term employment effects (e.g. Pannenberg, 1995; Hübler, 1998, Hujer and Wellner, 2000), but there are also studies that obtain positive effects (e.g. Pannenberg and Helberger, 1997; Prey, 1999). The lack of robustness of the estimates has several reasons: sensitivity of the results to the different parametric assumptions, small sample sizes, and inability to measure long-run effects.

Recent evidence based on new administrative data shows that negative lock-in effects during and shortly after programme participation are important also in East Germany. Hujer, Thomsen and Zeiss (2004) estimate a multivariate mixed proportional hazard rate model to analyse the effects of public sector sponsored training conducted in the period 2000-2002 on the transition rate into regular employment. Since they estimate the effects at the beginning and shortly after the end of the programmes, the usual negative lock-in effects drive their results. Speckesser (2004) and Fitzenberger and Speckesser (2005) use the same database as we do but restrict their analysis to one special type of government sponsored training and follow individuals only up to 36 months after the beginning of the programme. Based on propensity score matching, they find negative lock-in effects up to 12-18 months after programme start. Speckesser (2004) who focuses on training conducted in the period 1993-1994 finds no significant effects thereafter, mainly because of strongly decreasing sample size towards the end of his observation period. Fitzenberger and Speckesser (2005) who cover a longer period of programme participation (1990-1997) obtain positive employment effects of about 5-10 percentage points about 20 months after programme start.

Altogether, we extend the current literature in several dimensions. First, using a newly available administrative database allows us to measure the effects of different training programmes over 8 years,

[2] Pannenberg (1995), Pannenberg and Helberger (1997), Fitzenberger and Prey (1998, 2000), Hübler (1997,1998), Staat (1997), Kraus, Puhani and Steiner (1999), Lechner (1999, 2000), Prey (1999), Hujer and Wellner (2000). These studies use the German Socioeconomic Panel or the Labour Market Monitor. For a recent survey of the existing econometric evaluation studies, see Wunsch (2005).

[3] Semi- or non-parametric approaches are used by Hujer and Wellner (2000) and Lechner (1999, 2000).

thus uncovering the potentially important long-run effects of substantial government sponsored training. Second, we check their homogeneity across programmes and groups of participants which enables us to add to the discussion of correct targeting of ALMP measures, a task which is particularly difficult in a rapidly changing economy. Third, the database is of good quality with respect to information concerning the selection process, so that this problem should not harm our study in a very substantial way. Finally, the sample used is large enough to allow non- or at least semi-parametric estimation of the effects using matching methods.

The remainder of the paper is organised as follows: In the next section, we describe the institutional settings of the labour market policies that are important to understand how we proceed in the empirical sections of the paper. Section 3 explains our definitions of the sample and the groups of programmes. Section 4 analyses the determinants of participating in the various programmes and Section 5 outlines our econometric strategy. Section 6 presents the main results as well as a sensitivity analysis. Section 7 concludes. Four appendices provide additional information on data, econometrics, and results.

2 Labour market policies in East Germany

2.1 Unemployment insurance system and active labour market policy

In Germany, the Federal Employment Agency (FEA) executes the passive and active labour market policy.[4] In case of unemployment, individuals can receive unemployment benefits (Arbeitslosengeld, UB) for at least six and up to about 28 months (depending on the duration of prior contribution to the unemployment insurance system, UI, and age) if they registered as unemployed and contributed sufficiently to UI.[5] Participation in ALMP measures has direct implications for UB entitlement. Not only is

[4] In the early 1990s the legal basis for the activities of the FEA was the Employment Promotion Act (Arbeitsförderungsgesetz, EPA) which regulates the policy measures available in the labour offices. The EPA, enacted in 1969 and since then repeatedly amended, was abolished on January 1st, 1998, and replaced by Social Code III. Since this paper analyses FEA supported training in 1993-1994, we refer to the EPA legislation effective in 1993-1994 everywhere in this paper.

[5] For a more detailed description of the German UI system see Wunsch (2005).

the willingness to participate in ALMP a requirement for UB eligibility,[6] but 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. 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. After exhaustion of their UB claim, the unemployed can receive unemployment assistance (Arbeitslosenhilfe, UA), which can be paid until retirement age if they pass a regularly repeated means test. In contrast to UB, paid out of UI contributions, UA funds come from the German Federal Government (tax revenue).

Table 2.1 shows the expenditure for different measures of passive and active labour market policies for the years 1991-2003. East Germany underwent dramatic economic and structural changes during the transition from a centrally planned to a market economy during the early nineties leading to a particular use of ALMP measures. On the one hand, the East German economy was contracting rapidly leading to dramatic reductions in labour demand. To cope with the immediate strongly adverse effects of this, short time work and early retirement schemes were used quite extensively. On the other hand, the skills of the labour force did not meet the requirements of a modern market economy, so that the introduction of different kinds of training programmes on a large scale was supposed to lessen these skill deficits. However, unemployment kept rising and became very persistent, so that employment programmes also became one of the most important ALMP measures. The increasing persistence of unemployment also led to rising expenditures for unemployment benefits and especially unemployment assistance (Table 2.1).

Short time work (Kurzarbeit, STW) traditionally aims at preventing layoffs due to temporary unanticipated reductions in a firm's labour demand. Workers in STW only work a few hours per week or month and receive income support to supplement their reduced earnings. In East Germany directly after Unification, however, STW was also used when it was clear that the reduction in labour demand

[6] UB recipients who refuse to participate in ALMP measures or drop out prematurely for no good reason (like having found a job) risk suspension of their benefits of up to eight weeks or even loosing their claim after repeated refusal.

was permanent. Especially in 1991, the main objective of STW was to delay the transition into unemployment to prevent the official unemployment rate from skyrocketing.

Table 2.1: Expenditure for passive and active labour market policy in East Germany 1991-2003

	1991	1993	1995	1997	1999	2001	2003
Total expenditure	15,426	27,724	21,232	24,055	25,416	24,645	25,769
	Shares of total expenditure for active and passive labour market policy in %						
Training	14	19	17	12	12	13	9
Temporary wage subsidies	2	<1	<1	<1	1	3	4
Short-time work	33	2	1	1	<1	<1	<1
Job creation schemes	10	17	20	15	11	9	5
Structural adjustment measures	-	-	-	-	6	3	2
Early retirement	9	25	5	<1	<1	<1	<1
Rehabilitation programmes	<1	1	2	3	2	3	3
Unemployment benefits	26	24	31	42	33	33	31
Unemployment assistance	1	7	14	18	21	21	28
Other expenditure	5	7	10	10	12	14	17
Unemployment rate in %	10.2	15.4	14.8	19.1	18.7	18.8	20.1

Sources: BA (1992-2004).

Notes: Expenditure in million Euro. Training: further training, retraining, short training. Temporary wage subsidies are subsidies during the phase of initial skill adaptation in a new job (*Einarbeitungs-/Einstellungszuschüsse*). Short time work: *Kurzarbeit*. Job creation schemes: *Arbeitsbeschaffungsmassnahmen*. Structural adjustment measures: *Strukturanpassungsmassnahmen*. Early retirement: *Vorruhestand/Altersteilzeit/Altersübergangsgeld*. 'Other expenditure' mainly includes counselling and job placement services as well as administrative costs of the FEA.

Early retirement schemes seek to lower unemployment directly by reducing the labour supply of older individuals. To cope with the rapid drop in labour demand directly following Unification, the German Federal Government introduced a specific early retirement scheme for East Germany to encourage withdrawal of elderly workers from the labour market. It releases unemployed East Germans of age 55 and older from the obligation to search for a job while claiming benefits from the FEA. Instead of receiving UB/UA, these people receive so-called pension transition allowances (Altersübergangsgeld).

Training programmes - which are the subject of this study and are described in more detail in Section 2.2 - had the important task of adjusting the in many respects inadequate skills of the East German labour force to the requirements of a modern market economy.

Employment programmes provide subsidised jobs outside the regular labour market that have to be in the interest of the public. In East Germany there were two forms of employment programmes in the early 1990s: job creation schemes (Arbeitsbeschaffungsmassnahmen, JCS) which were also available in West Germany though with more restrictive eligibility criteria, and so-called productive wage subsidies (produktiver Lohnkostenzuschuss, §249h EPA). The latter had been introduced specifically for

East Germany and were characterised by less restrictive eligibility criteria than those of JCS. They have been replaced by so-called structural adjustment measures (Strukturanpassungsmassnahmen, SAM) in 1998. In times of high and persistent unemployment, employment programmes are an important measure to maintain the employability of unemployed and especially long-term unemployed persons, as well as to preserve social stability in regions with particularly high rates of unemployment.

Table 2.2: Participants in the quantitatively most important ALMP measures 1991-2003

	1991	1993	1995	1997	1999	2001	2003
Training (total) [a) c)]	760	263	237	155	350	415	468
Further training (in % of total)	58	69	78	83	NA	NA	NA
Short training (in % of total)	25	-	-	-	48	55	80
Retraining (in % of total)	17	31	22	17	NA	NA	NA
Temporary wage subsidy [a)]	133	31	20	11	65	99	107
Job creation schemes [b)]	183	237	206	154	168	123	70
Structural adjustment measures [b)]	-	-	-	-	180	67	40
Short-time work [b)]	1616	181	71	49	27	27	35
Pension transition allowance [b)]	189	639	341	58	-	-	-

Sources: BA (1992-2004).

Notes: [a)] Total number of inflows (1000 persons). [b)] Yearly average (1000 persons). [c)] Since 1998, further training and retraining can no longer be distinguished. Until 1992, short training included courses according to §41a AFG, since 1998 it consists of so-called training measures (*Trainingsmassnahmen*). Temporary wage subsidies are subsidies during the phase of initial skill adaptation in a new job (*Einarbeitungs-/Eingliederungszuschüsse*). Job creation scheme: *Arbeitsbeschaffungsmassnahmen*. Structural adjustment measures: *Strukturanpassungsmassnahmen* (since 1998). Short time work (STW): *Kurzarbeit*. Pension transition allowance (*Altersübergangsgeld)*. NA: not available.

In 1991 one third of the expenditure for East Germany were devoted to STW and more than 1.6 million people were directly absorbed into STW (see Table 2.2). Another 0.8 million individuals were referred to training measures. Among them, 25% participated in very short measures (§41a EPA). In addition, 183,000 individuals were absorbed by JCS. This pattern started to change already in 1992. The number of recipients of STW compensations declined substantially while utilisation of JCS and retraining were extended significantly. In addition, substantial funds were devoted towards payment of pension transition allowances (included in 'early retirement' in Table 2.1) in the period 1992-1994. While STW lost its importance after 1993, job creation schemes became increasingly important even though the number of participants declined over the years. In 1993, there were still 237,000 participants in JCS. Until 1996, the number varied around 200,000 participants. With the introduction of SAM in 1998, the number of participants in employment programmes increased temporarily but has now declined to only about 110,000 in 2003. The number of participants in government sponsored

training declined drastically in 1993 due to abolishment of the §41a programme. It decreased further until 1997 (with the exception of a temporary increase in 1996). Since 1998 the number of training participants is rising due to increasing use of short training (≤ 3 months) which has been reintroduced in that year under the label of so-called training measures (Trainingsmassnahmen).

2.2 The use of FEA supported training during East German transition

In Germany, training consists of heterogeneous instruments that differ largely in the form and the intensity of the human capital investment as well as in their respective duration. During East German transition the following three groups of training measures have been used extensively: (i) short training according to §41a EPA which has been abolished at the end of 1992,[7] (ii) further training, and (iii) retraining.[8] Since our analysis covers participation in FEA supported training between 1993 and 1994, short programmes are not considered. According to the EPA (§§ 41, 43), 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. In the East German transition process, however, the use of categories (c) and (d) was negligible since the main objective of training programmes was to adjust the skills of the East German labour force to the requirements of a modern market economy.

One form of further training, belonging to category (a) or (b), were courses in so-called *practice firms* that simulate - though under very realistic conditions - working in a specific field of profession. The other courses included in categories (a) and (b) had a mean duration of nine months in 1994. Dropout and failure rates were very low in these two categories: only 0.3% of participants failed and no more than 5% dropped out prematurely for reasons other than having found a job.

Retraining enables working in a different profession than the one currently held by qualifying for a new professional degree. The mean duration in 1994 was 22 months, 18% of participants spent more than two years in the programme. The dropout (3% in 1994) and failure rates (10% in 1994) were

[7] These courses 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.

[8] The FEA also supports two other forms of training: regular vocational training in the German apprenticeship system and German language courses. See Wunsch (2005) for details.

significantly higher than for further training but still low compared to West Germany (see Lechner, Miquel and Wunsch, 2004).

Table 2.3: Original and target professions by programme group in 1993-1994

Profession	Short training		Long training		Retraining		UE rate	
	Original	Target	Original	Target	Original	Target	1994	2002
			Men					
None*	13	-	8	-	14	-		
No information	-	22	-	4	-	4		
Agriculture, gardening, forestry, mining	8	3	5	2	10	2	24	31
Metal production/processing, mechanics	37	26	17	9	34	2	15	19
Textile/leather manufacturing	3	-	1	1	2	-	59	42
Food and nuitrition	1	-	2	2	3	1	33	30
Construction	6	12	4	12	6	18	6	33
Construction related handcraft	12	12	8	13	5	53	9	28
Technical profession, other manufacturing	12	11	27	23	15	4	16	19
Office work	2	3	6	25	1	7	16	20
Health/social services, education	3	2	7	2	2	3	9	9
Other services	5	11	6	8	10	7	22	28
Correlation(% target - % origin, UE rate 1994)	-0.21		-0.15		-0.24			
Correlation(% target - % origin, UE rate 2002)	0.15		0.15		0.18			
			Women					
None*	12	-	3	-	7	-		
No information	1	34	1	9	-	8		
Agriculture, gardening, forestry, mining	5	2	6	3	12	9	24	31
Metal production/processing, mechanics	4	1	1	0	5	1	15	19
Textile/leather manufacturing	8	2	5	0	16	-	59	42
Food and nutrition	5	3	6	3	3	4	33	30
Construction	-	-	<1	1	-	-	6	33
Construction related handcraft	2	1	2	0	3	5	9	28
Technical profession, other manufacturing	13	2	15	6	12	5	16	19
Office work	36	42	42	65	16	36	16	20
Health/social services, education	6	6	13	6	13	18	9	9
Other services	10	7	7	6	12	14	22	28
Correlation(% target - % origin, UE rate 1994)	-0.34		-0.15		-0.56			
Correlation(% target - % origin, UE rate 2002)	-0.19		-0.09		-0.51			
Overall UE rate							15	22

Source: Own calculations on the basis of our evaluation sample (see Section 3). UE rates: BA (1992-2004).
Note: Entries in %. UE rate: number of unemployed in the respective profession as a percentage of the sum of socially insured employees and registered unemployed in 1996/2002. * No formal professional degree.

Table 2.3 shows the shares in the original professions (before training) and the target professions (the training programme is planned to qualify for) for our evaluation sample of training participants in 1993 and 1994 for men (upper panel) and women (lower panel) and the different types of training. The last two columns give the profession specific unemployment rates in 1994 and 2002. In general, we

see that those men in the metal production/processing and mechanics sector are much more likely to receive either short training or retraining, whereas long training is somewhat concentrated among the technical professions. For women, long and short training is concentrated in office related occupations. Female retrainees are more evenly spread across occupations. Comparing the original and target professions for participants in short training is somewhat difficult because about the latter, information is missing for about 20% of participants. However, given the rules and regulations and the fact that courses have planned durations of up to 6 months it appears to be unlikely that participants plan to change profession using such courses. For long training for men, we find that certain professions decline to some extent, like the group of agriculture, mining, forestry, and metal related occupations as well as technical professions and the group of health, social services and education, whereas others increase like construction related as well as office related professions. For women in long training we see a considerable increase in office related occupations, and a decrease everywhere else.

The most interesting case is clearly retraining. For about 71% of the male unemployed the target profession of retraining was construction related (in particular craft related professions),[9] whereas this share was only 5% for women. The male share of this type of occupation in the original profession was only 11%. This change is clearly negatively related to the unemployment rate in 1994 and positively (!!) related to the unemployment rate in 2002. In fact, the construction sector went from boom to bust just while most of the retrainees attended their programmes. The inflow into construction comes from all occupations with the exception of the group health/social services, education (see Table IA.1 in the internet appendix). About one third of the women had some office related target profession, and about another 20% were directed towards health, social and education related professions, and another 14% towards other services that is negatively related to the respective unemployment rates in 1994 and 2002.

These choices of target professions by the caseworkers and unemployed may have been rational (given a shortsighted view) in the years 1993 and 1994 where the major destinations for male and female unemployed showed average or below average unemployment rates. However, the long-run prediction for the males turned out to be very bad, because of the bust in the construction sector. In 2002, the

unemployment rates in the construction related professions were around 30% compared to an already high East German average of about 22%. Women were luckier, since the unemployment rates in their main target professions are below average. The correlations between the difference in the shares in target and original profession and the unemployment rates in 1994 and 2002 confirm that view. For 1994, the correlation is negative for all types of training and both men and women indicating the rational strategy of directing jobseekers to professions with low unemployment rates. However, ex post the strategy for men turned out as counterproductive. Men were trained in professions with particularly high unemployment rates in 2002, which is resembled in the positive correlation in Table 2.3. In contrast, for women the correlation is still negative in 2002 indicating that the choices of the caseworkers had just been right for them. We come back to this issue when discussing the results.

The FEA can support participation in further training and retraining by paying a maintenance allowance (MA)[10] and by bearing the direct cost of the programme, as well as by covering parts of additional expenses for childcare, transportation and accommodation. In 1994, expenditure of the FEA for further training and retraining amounted to 4.6 bn DM for payment of MA plus 2.4 bn DM for programme costs (in total about 3.4 bn EUR) in East Germany (BA, 1995).[11] Most courses were full-time courses (more than 97% in 1994). 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 implicitly 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.[12] In addition, the potential participant had to be either unemployed, directly threatened with unemployment, or without any professional degree.

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[9] There was a construction boom after unification that bust in the second half of the decade.

[10] Until 1993, the amount of MA received was 73% of the previous net income with at least 1 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.

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

[12] 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.

An additional requirement was a minimum duration of insured employment (two years) or, alternatively, receipt of UB or UA before entering the programme.[13] Due to special regulations for East Germany regarding UB entitlement, virtually everyone was entitled to UB (and thus, MA) when becoming unemployed for the first time after German unification.

3 Definition of the estimation sample and the programmes

3.1 The data

We use the same administrative database as in Lechner, Miquel and Wunsch (2004) and refer the reader to that paper for more details. It stems from three different sources: the IAB Employment Subsample (ES), the benefit payment register (BPR), and the training participant data (TPD).[14] For East Germany, the database covers the period 1990-2002. It contains many, if not most, variables influencing the selection process into these programmes (see Appendix A for the variables used in our analysis and the discussion of the selection process below). It allows a precise measurement of interesting outcome variables, particularly those related to individual employment status. Furthermore, it contains information about different programme types and it has a sufficient number of observations for the major programme groups.

Of course, there are several drawbacks as well, five of those could be important: First, there are 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 this data. Second, employment that is not subject to social security contributions is unobserved, and it is possible to distinguish between subsidised and regular employment in the first labour market only from the year 2000 onwards. This problem is particularly severe for East Germany, because of a substantial part

[13] § 46 EPA. Individuals who did not meet these additional requirements could only apply for reimbursement of the cost of the programme (§§ 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.

[14] The common German abbreviations for these data sources are IABS, LED and FuU. A detailed description of the ES and the BPR is provided by Bender et al. (1996) and Bender, Haas and Klose (2000). For the TPD see Miquel, Wunsch and Lechner (2002). See also Bender et al. (2005) for how the data has been prepared for evaluation purposes.

of the labour force participated in job creation schemes and similar employment programmes during the 1990s (see Section 2.1).

Third, the unification process had a direct impact on the data gathering: The data collection system that depends to a considerable part on reports from employers was phased in after unification. Some employers provided information as early as 1991 whereas in most cases it took until 1992 until all employers were registered with the authorities. Therefore, implicitly our sample is selected on either having an initial employment or unemployment spell in the data. However, since later on we condition on having an (observable) employment spell prior to the unemployment spell leading to participation, this is no serious issue. The other drawback is that information about long-term employment histories is absent. However, since in the German Democratic Republic unemployment was (officially) absent and labour force participation was very high, the resulting additional unobserved heterogeneity should be very small. Furthermore, unification per se certainly discounted the value of human capital and experience obtained under the old centrally planned economic system.

Fourth, the training information for East Germany prior to 1993 is incomplete and not correctly coded. Fifth, 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 East Germany relied on much smaller survey data requiring substantial aggregation across programmes, this database is a substantial improvement in several dimensions, like sample size, selection and outcome information, as well as observable 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 distinguish reliably different types of programmes. Table 3.1 shows the resulting five groups of training programmes plus a residual category. Ignoring the programme types for which the number of observations is too small, we restrict our analysis to general further vocational training and retraining programmes (see the descriptions in the previous section). Since the first group of programmes is fairly large and heterogeneous, we split them into two groups based on the

planned duration of a particular programme. To characterise the programme and not its participants, *planned* instead of *actual* duration has the clear advantage that the behaviour of the individual during participation does not influence it. Figure 3.1 shows that the planned duration varies between one month for short training programmes and up to three years for some retraining courses. Generally, the German programmes are long on average compared to other OECD and transition countries (e.g. OECD, 1996a, b).

Table 3.1: Definition of programmes

Programme	Description
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; planned duration ≤ 6 months.
Long training	Same types as short training with a planned duration > 6 months.
Retraining	Training to obtain a new professional degree in a field other than the profession currently held.
Practice firm	Further training that simulates a job in a specific field of profession
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.

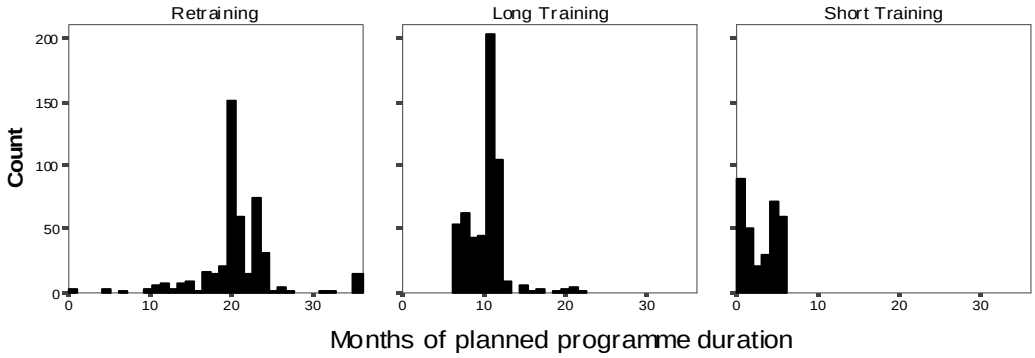
Note: *Other* is a residual category that comprises very heterogeneous, small programmes. Therefore, the latter is not evaluated. Due to insufficient sample size in the categories *practice firm* and *career improvement*, these are also not evaluated in this study.

Next, we define 'participation' and our population of interest. First, 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 recent programmes while at the same time still having an observation period that allows us to detect long-run effects.[15] Moreover, before 1993 training programmes had not been well targeted and were often very general compared to later programmes. The reason is that during the early stages of transition, the main objective of training and other ALMP programmes had been to keep people out of the official unemployment statistics and to avoid social hardship associated with long-term unemployment (Buttler and Emmerich, 1994; Kraus, Puhani and Steiner, 1999).

[15] 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 underrepresentation of long training spells.

Second, a person is included in our population of interest if she starts an unemployment spell between 1993 and 1994. The group of participants in training consists of all persons entering a programme between the beginning of the first unemployment spell after 1992 and the end of 1994. If there are multiple treatments over time, only the first one is included in the analysis (if it occurred 1993-1994).

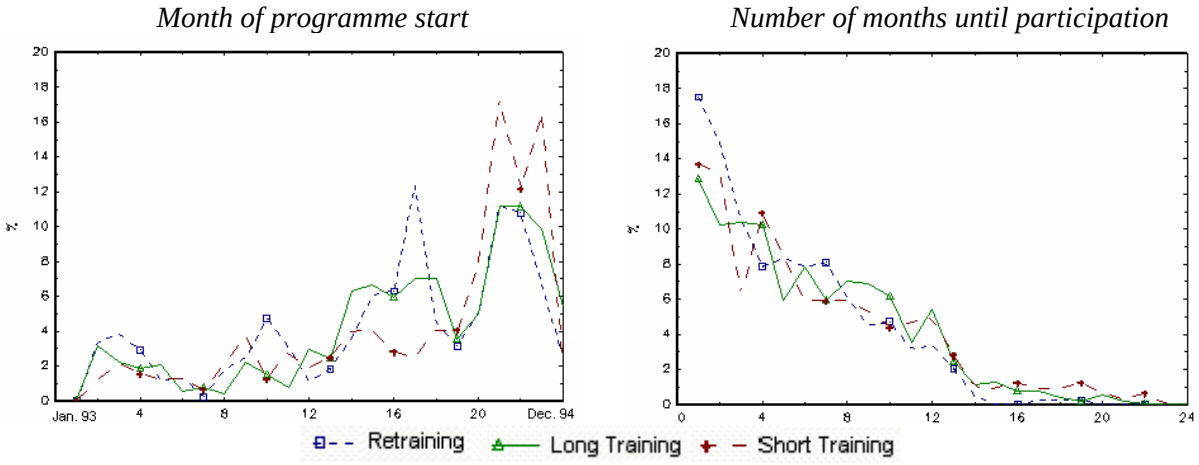
Figure 3.1: Distribution of the planned programme duration



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

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). From the latter chart it seems that a considerable group of trainees enter training fairly (surprisingly) early in the spell, particularly for retraining.

Figure 3.2: Distribution of start dates and duration until start of programmes (in %)



Note: Monthly measurement based on estimation sample for participants.

Given our definition of a small treatment window, it is particularly important for the interpretation of our results which share of the nonparticipants receives training later on. Table 3.3 shows that the problem does not appear to be too severe: About 17% of those classified as nonparticipants get some training between 1995 and 1997 (the participation data ends 1997) and less than 10% of the trainees participate a second time, mostly in the same programme type they already participated in.[16]

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	Non-participation	Short training	Long training	Retraining	Practice Firm	Career improvement	Other
Short training	8.8	8.7	6.7	0.7	8.0	12.5	3.4
Long training	3.7	2.8	2.8	1.3	4.3	0	0.8
Retraining	1.8	2.2	0.9	1.0	3.1	0	0
Practice firm	1.8	5.9	0.2	1.6	2.5	0	0.8
Career improvement	0.04	0	0	0	0	0	0
Other	0.8	0.3	1.1	0.5	0.02	12.5	0
Total other treatments than first treatment	16.9	11.2	8.9	4.0	15.5	25	5.1

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 of the TPD data base only training spells completed by the end of 1997 are observable.

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 (2001) and Lechner (2004), which is not feasible with our data without further aggregation of programme types, which, in turn, is undesirable for obvious reasons. However, as Table A.1 in the Appendix shows, only very few people in our evaluation sample had participated in a programme before the programme participation we are evaluating in this study or the assigned hypothetical starting date for nonparticipants.

3.3 Selection of population and sample

When choosing the appropriate subpopulation of our inflow sample into unemployment 1993 and 1994, we aim at having a homogenous group of people covering the prime age part of the population

[16] Overall, about 20% of registered unemployed enter government-sponsored training per year (BA, 1992-2004).

of East Germany that are eligible for participation in the programmes. Thus, we require that all individuals were employed[17] at least once prior to programme participation and that they were receiving UB or UA in the month before the programme start and in the month of the programme start for the non-participants.[18] This, however, requires the use of variables measured relatively to the programme start.[19] In this paper, we follow one of the approaches suggested by Lechner (1999). We simulate start dates for nonparticipants by drawing start dates from the empirical distribution for 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 observed in the data.[20] To avoid most influences coming from retirement, early retirement and primary education, we also impose an age restriction (20-53 years).[21] Concentrating on the main body of the active labour force for which we could also assume a homogenous selection process, we exclude unemployed who were - before the 'defining' UE spell - trainees, home workers, apprentices or without previous employment or whose previous employment was less intensive in terms of hours than half of a full-time equivalent. Furthermore, since the group of 'Non-Germans' is extremely heterogeneous in East Germany (there is no 'stable' and at least partly assimilated guest worker population as in West Germany), we drop them as well.

Table 3.4 shows how the sample shrinks imposing these criteria successively. The largest drop in the number of observations occurs for the group of nonparticipants to make them a priori comparable to the treated using the criteria above, which they have to fulfil at the simulated start date. However,

[17] 'Employed' means that we observe the person at least once in an insured employment spell in the ES (for the majority of our individuals, the first observation in the data is an employment spell).

[18] In fact, receipt of UB or UA directly before entering a programme is not entirely sufficient to ensure eligibility. 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, thus by also requiring individuals to be employed at least once before the programme, the remaining group of participant and nonparticipants is most likely to be eligible.

[19] All variables potentially influencing selection into programmes and outcomes are measured relatively to the start of the programme (or earlier).

[20] Nonparticipants who do not satisfy this criterion are excluded from the sample.

[21] Measured in the year of the beginning of the programme (simulated start date for nonparticipants).

since we still keep almost 5000 unemployed nonparticipants, the large reduction is of no serious concern. The reduction in the number of the participants is fairly small and about 20% on average.

Table 3.4: Sample selection rules

	Nonpar-ticipation	Short Training	Long Training	Retraining	Practice firm	Other
Persons entering unemployment between Jan. '93 and Dec. '94	17574	418	692	531	203	180
Simulated programme start after the entry in unemployment (UE) and before the end of the observation period						
Remaining observations	12904	418	692	531	203	180
Eligibility: Only individuals receiving UB or UA in the month before the programme start (also in the month of the programme start for the non-participants)						
Remaining observations	6599	400	656	496	198	152
Personal characteristics : a) 20 ≤ age ≤ 53; b) no trainees or apprentices; c) at least one observation of employment; d) no home workers; e) no part-time worker less than half of a full-time work; f) only German						
Final sample	4604	321	538	445	162	126

Note: All variables are measured before or in the same year as the start of the programme. Note that in this table the residual category *other* includes *career improvement*. Due to the small number of observations, *practice firms* and *other* are not evaluated.

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 unemployed end up in different programmes or in no programme at all. Instead of postulating a complete structural model for the selection process, we discuss the main determinants of selection and then explain which observable variables may capture them. The determinants fall 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.[22] 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 have been employed for at least two years within the three years prior to the programme. As discussed in Section 3.3 our selected sample fulfils the eligibility rules.

If the unemployed meets these conditions, she *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 to discuss the job search efforts of the unemployed, 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.[23] 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 she will receive support. There is no legal entitlement to FEA support, and caseworkers have a considerable amount of discretion 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 of which of the available measures 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 unemployed. Since the willingness to participate in labour market programmes is a precondition for receipt of UB and UA, unemployed who refuse to apply or, having applied, refuse to participate in a training measure, risk suspension of their benefits for up to eight weeks.[24]

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[22] The exact requirement is either 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.

[23] The caseworker can schedule a meeting at any time but at least every three months, e.g. 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.

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

In a typical selection process, the caseworker's decision about referral of applicants to specific programmes is guided by two objectives: efficiency or equity. Caseworkers pursuing efficiency goals assign those individuals to the programmes that they expect to benefit most. In contrast, equity goals require caseworkers to select the neediest individuals into the programmes, where neediness is defined for example by a high risk of becoming long-term unemployed. The factors relevant for pursuing the latter policy probably are best approximated in our data by the (short) employment and unemployment history after unification[25] as well as by the economic situation of the individual. The latter in turn is largely determined by the last job, educational attainment, nationality and family status since these variables govern chances in the labour market. These factors may be related to the effect maximising strategy. In addition, we expect that participation rates decline 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, however, may be supply constrained and not able to offer what he considers best. Yet this is not so important here, because it is likely 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, her 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 that reduces his leisure time (an important issue in a rapidly contracting East German economy). 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 supported by payment of MA: periods of receipt of MA can extend existing or renew exhausted UB entitlements. To control for this fact, we have constructed variables from the (un)employment histories indicating the UB claim at the beginning and at the end of a spell.

[25] Heckman and Smith (1999, 2004) point to the importance of this information when analysing participation in the Job Training Partnership Act in the United States.

Finally, since unemployment is rapidly rising during 1993 and 1994 and participations shows a varying pattern as well, we condition on the month of the start of the programmes thus netting out seasonal and timing effects.

Our data capture the factors determining participation mentioned so far by 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 unemployed, 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 four groups included in the econometric analysis, the results can be summarised as follows: (i) Participants in retraining are on average 32 years old and thus about five years younger on average than other unemployed are. This is in line with the idea that substantive human capital investments are more beneficial the longer the productive period of the new human capital is. (ii) Participants in retraining are less educated and skilled than the rest. (iii) The mirror image of this observation is that participants in short and long training are better educated and in higher job positions than the rest. This difference is particularly pronounced for those individuals in long training. Usually, in the West it would seem that participants in long training have the best a priori chances on the labour market. However, here the 'last job' is most likely the one that comes from continued employment before and after unification. Thus, the qualifications coming with these types of occupations may have been discounted heavily in the new economic order. (iv) For the two variables indicating remaining UB claims, Table 4.1 does not show much variation. (v) Concerning male-female differences, note that the share of women among the unemployed is much higher in East Germany than in the West which is reflected in the high share of women among nonparticipants and participants in long and short training, but not in the share of women observed in retraining, the latter

showing a 'male' bias. (vi) Finally, note that there are some regional differences that, however, do not appear to be related to the local unemployment rate (see Appendices A and B for a list of more disaggregated regional variables and the influence on participation).

Table 4.1: Selected descriptive statistics according to participation status (means or shares in %)

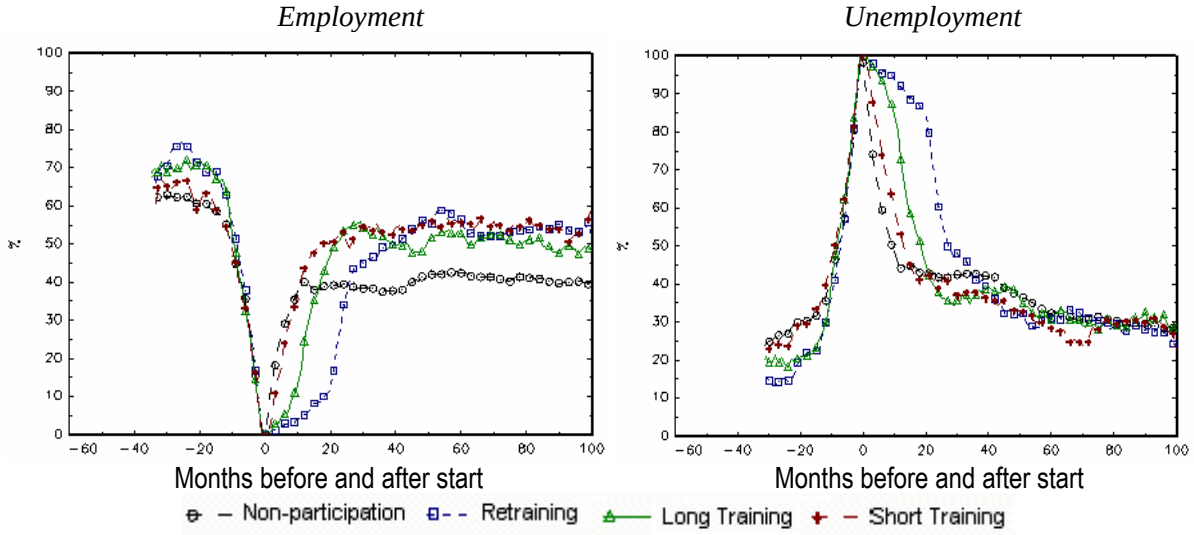
		Nonparticipation	Short training	Long training	Retraining
Number of observations		4604	321	538	445
		Personal characteristics			
Women		63	65	65	43
Age*		37	36	38	32
Education: No professional degree		12	7	4	14
With professional degree		78	87	77	81
University, polytechnical degree		4	3	15	3
		Position in last job			
Salaried employee		26	30	54	21
Part-time worker		26	20	17	10
Master craftsman		1	0	1	0
Unskilled worker		22	25	13	28
Skilled worker		26	26	16	40
		Last monthly earnings			
Salary in Euros*		947	1053	1204	1144
		Remaining UB claim (before entry into the programme)			
Remaining UB claim (months)*		2.2	2.1	2.3	2.3
		Legal UB claim at the beginning of the last unemployment spell before the programme			
Legal claim* (months)		5.7	5.2	5.8	5.5
		Programme information			
Planned programme duration* (months)		0	3.4	10.5	20.5
		Regional information			
Berlin, Postdam		11	7	7	9
Saxony, Thuringia		40	46	47	41
Mecklenburg-Western-Pomerania, Brandenburg		10	14	15	18
Saxony-Anhalt		17	10	14	13
Regional information missing		22	23	17	19
Local unemployment rate	≤ 15%	35	33	37	34
	15% < UE rate ≤ 20%	58	58	56	56
	UE rate > 20%	7	9	7	10
		Employed in			
January 1991	N = 3326	90.8	94.5	97.6	92.9
1992	5911	60.0	69.2	73.7	75.2
1993	6150	50.2	53.9	61.9	59.0
1994	6194	21.9	20.9	16.0	16.0
1996	6196	34.4	42.4	42.0	20.4
1998	6196	32.5	46.1	45.7	48.5
2000	6196	37.4	49.5	49.6	49.0
2002	6148	36.4	51.4	46.3	51.4
		Subsidised employment in			
January 2002	6148	5.8	7.3	6.5	4.2

Note: * Numbers are means rather than shares. ++ Measured in the year of the programme start. Subsidised employment includes temporary wage subsidies, job creation schemes and structural adjustment measures. 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. '*N* =' means the number of individuals for whom this information is available at that point in time. See Appendix A for the full set of descriptive statistics.

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. First, note that when we go back in time, the sample size decreases because of the effect of introducing the statistical system already discussed above. Second and related to the first point, before the programme (in 1991) employment mainly reflects the fact that this type of history is only available from employers who have sent employment information to the social security administration early, thus individuals with this early information are typically employed. Third, in 2002 subsidised employment is still a considerable part of employment (about 15% of all employment is subsidized; this information is unavailable before 2000 and incomplete for 2000-2001).

In Figure 4.1, we show the time path of employment (subsidised and unsubsidised) and unemployment rates for the different groups of participants and the nonparticipants relative to the actual or simulated beginning of the training. By construction of the sample, all members of these groups must be unemployed in the month before participation. Thus, the unemployment and employment rates show the usual dip/increase before participation (cf. Ashenfelter, 1978). Furthermore, note that prior to the unemployment spell in which participation happens, all individuals are in employment. Thus, there is a sharp increase in unemployment rates starting about one year before participation and a corresponding decline in employment rates.

Figure 4.1: Employment and unemployment rates by participation status



Note: The employment and unemployment information is given only up to 36 months prior to training. Due to the process of gathering administrative data after unification, many unemployed may be observed for shorter periods.

After the programme, all employment measures show some recovery. The immediate speed of the recovery is negatively correlated to programme duration, pointing to a lock-in effect of participation (cf. Van Ours, 2004). None of the groups reaches its previous level, but the rate of recovery for non-participants is particularly low, already foreshadowing the results of the econometric part below. Note the interesting difference that appears between unemployment and employment. Both measures of labour market attachment show the same shape over time. However, for registered unemployed (defined as receipt of some form of benefits or participation in training) there does not appear to be any difference between the different groups of participants in the long run, pointing to a significant group of people leaving the labour force (see the detailed statistics at the end of Table A.1 in Appendix A).

In addition to the simple statistics describing the differences of the participants in the different groups presented above, Appendix B contains the detailed results of a multivariate analysis based on modelling the selection into the different groups based on a multinomial probit model estimated by simulated maximum likelihood. Beyond providing useful descriptive statistics, the output from this selection model plays a key role in our selection correction mechanism explained below. The analyses revealed that gender, age, education, and last occupation are important individual characteristics that determine participation. Furthermore, although the observed employment and unemployment history is short, it is significantly correlated with participation choice. Regional information, such as the industrial, employment, population and wealth composition of the region as well as migration streams and tax revenues, which entered the probit in a highly disaggregated way to capture the specifics of supply and demand in the local labour market, are an important component of the selection process.

5 Econometrics

We base our analysis on the prototypical model of the microeconometric evaluation literature with multiple treatments (see Appendix C for the formal model): An individual chooses between several states, like participation in a specific training programme or nonparticipation in such a programme. The potential participant in a programme gets a hypothetical outcome (e.g. employment) in both states. If we can observe all factors that jointly influence outcomes and the participation decision, then

- conditional on these factors - the participation decision and the outcomes are independent like in an experiment. Thus, for the same values of these factors (call them *X*) a comparison of the outcomes for the different group of participants with the same value of *X* identifies the effects (like in an experiment). This is the intuition of the so-called matching estimators used here.

The parameters typically estimated by matching methods are the average treatment effect on the treated (ATET), which is the expected effect of the programme *m* compared to *l* for its participants, denoted as $\theta_0^{m,l}$, and the average treatment effect (ATE, $\gamma_0^{m,l}$). This is the expected effect of *m* compared to *l* for the population. It is a clear advantage of matching estimators that they are essentially nonparametric, thus independent of the kind of functional dependences between outcomes and *X* and that they allow arbitrary individual effect heterogeneity (see also Heckman, LaLonde and Smith, 1999) and Imbens (2000) and Lechner (2001) for the case of multiple treatments.

As regards identification, ATE's and ATET's are generally not identified if we do not observe all factors jointly determining outcomes and treatments. Thus, to make this assumption credible we need a very informative database. We already noted that our data, which are compiled from different administrative records, are so rich that it seems plausible to assume that we observe the important factors that jointly influence labour market outcomes and the process selecting people into the four different states. Therefore, we assume that treatment participation and treatment outcome are independent conditional on a set of (observable) attributes. In other words, there are no exogenous variables left out that are both correlated with potential outcomes and the participation decision.

If this assumption holds, then for the ATET for example the intuition is to find for every participant in one treatment a participant in a comparison state who has the same conditional on X choice probabilities between those two states. Having found this matched comparison sample, comparing the sample means between the matched comparisons and treated gives the desired results. This simple idea can be exploited and modified to obtain 'better' estimators. Here, we use the modified version suggested and applied by Lechner, Miquel and Wunsch (2004) which is detailed in that paper and partly in Appendix C. All checks of its operational characteristics and issues concerning the estimation

are contained in the internet appendix (see in particular Appendix IB for the implications of imposing common support, and match quality). The estimator performed reasonably well.

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 the probability of remaining unemployed. Therefore, important outcome variables are those relating to the employment status, like registered unemployment and different types of employment.[26] We use different definitions for measuring employment, sometimes requiring a certain quality of the job, approximated for example by the job's duration and earnings compared to the previous job. Furthermore, we consider gross earnings as well, being a crude measure for individual productivity. We present the results below for those variables that seem to us most interesting. For all the others, the reader may consult the internet appendix that contains all background material. Our employment related outcome variables have one severe problem, namely that with our data we cannot distinguish subsidised and non-subsidised employment (e.g. employment programmes, wages subsidies) before the year 2000. Beginning in 2000, this becomes possible, but for 2000 and 2001 there is some underreporting. Since in East Germany subsidised jobs were used on a large scale in the early and mid 1990s, all results presented below that relate to this period have to be interpreted with that caveat in mind. Nevertheless, we do have a clean measure of the long-run effects of the programmes.

Effects are measured monthly based on process time: *Month 1* in process time is 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 have a very positive effect (in the short run) if everybody would leave for 'good' employment immediately after starting participa-

[26] Here 'registered unemployment' is defined as receipt of some form of benefits according to the BPR or participation in training according to the TPD.

tion. Persons participating in any of the programmes are coded as registered unemployed (and not as employed) in the outcome variables.

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 long-term outcome unsubsidised *employment eight years (96 months) after participation started*. Columns (3) and (4) give the exact sample sizes (after imposing common support). Columns (5) and (8) show the observed mean outcomes for the participants in programme *m* (5) and in programme *l* (8), respectively. 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*. 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.

Table 6.1: Estimated effects on unsubsidised employment 8 years after programme start

State *m*	State *l*	Sample size		$E(Y^m \mid S=m)$	$E(Y^m \mid S=l)$	$E(Y^l \mid S=m)$	$E(Y^l \mid S=l)$	$\theta_0^{m,l}$	$-\theta_0^{l,m}$
		m	l						
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Short training	nonparticipation	292	3542	47.3	51.8	34.3	34.4	13.0*	17.4*
Long training	nonparticipation	502	3542	41.4	35.0	37.6	34.4	3.8	0.6
Retraining	nonparticipation	429	3542	47.6	46.6	40.3	34.4	7.3	12.2
Short training	long training	292	502	47.3	44.6	43.0	41.4	4.3	3.2
Short training	retraining	292	429	47.3	56.0	52.5	47.6	-5.2	*8.4*
Long training	retraining	502	429	41.4	41.5	56.1	47.6	-14.7*	-6.1

Note: Bold numbers indicate significance at the 5% level, numbers in *italics* relate to the 10% level and * to the 1% level. See the internet appendix for the same table using the combination of subsidised and unsubsidised employment.

The estimates of the mean effects of participating in *m* compared to *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) shows the mean effect for participants in treatment m (difference between column (5) and (7)), while column (10) displays the results for participants in group *l* (difference between column (6) and (8)).

Comparing the effects of the programmes to nonparticipation using unsubsidised employment 8 years after programme start as outcome variable, we find that short training and retraining have substantial positive effects on average, at a magnitude of around 10%-points in employment gain. The effects of long training are not significantly different from zero, though. The estimators of the pair-wise comparisons of the different programmes are subject to more sampling error due to the much smaller comparison samples, thus requiring a larger magnitude of the effect to become significant. Therefore, it is not surprising that - with few exceptions like the dominance of retraining compared to long training for the participants in long training - the effects are hardly significant.

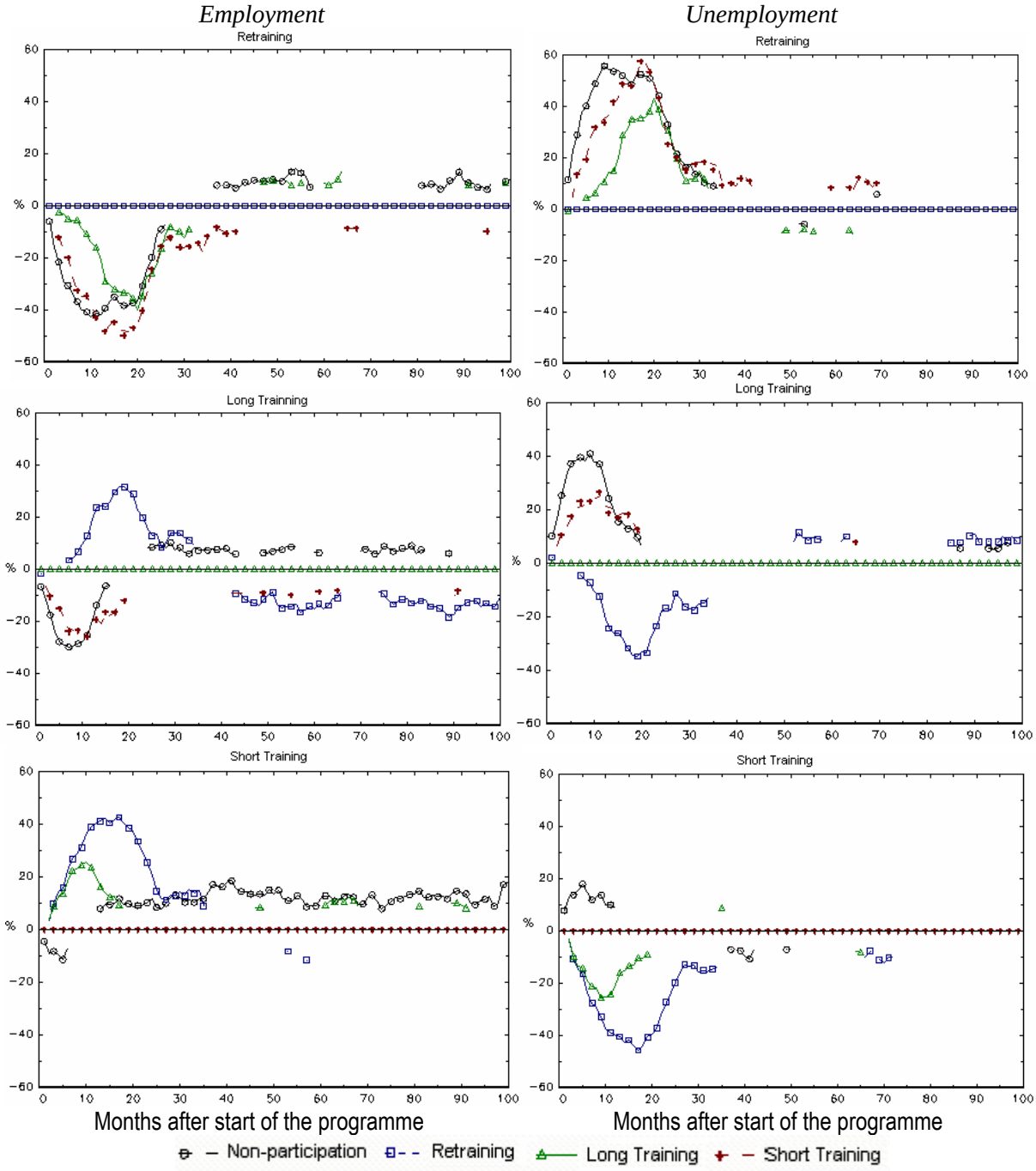
The following figures show how the effects evolve over time. Figure 6.1 displays the estimates of the effects of the different programmes (compared to the other states) for participants in the respective programme (ATET) for the outcome variables employment and unemployment. A line above zero indicates 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. To use a consistent definition of employment over time in these figures employment covers subsidised and unsubsidised employment. The difference between those two types is very small for about the last twenty months, but could be particularly important for the short and medium-run results (see Section 2).

The results for long training and retraining show negative effects in the short-run that are the larger the longer the programmes. However, since we are not able to distinguish subsidised employment from unsubsidised employment in the short run, the lock-in effects that are present in all figures may be just because nonparticipants and participants in short training move to employment programmes afterwards.[27] In the longer run and with respect to employment, Figure 6.1 confirms the results of Table 6.1 in that retraining and short training dominate nonparticipation on average. Long training seems to have positive effects as well, although they are not significant at the end of the observation window. However, the participants in long training would have benefited more had they participated in retraining

[27] Because of this ambiguity, we refrain from presenting accumulated effects, as we did for West Germany in Lechner, Miquel and Wunsch (2004).

instead. The effects with respect to unemployment are all zero in the long run, a finding that already has been suggested by Figure 4.1. The reason is probably related to the fact that (i) training programmes increase the period of receipt of unemployment benefits; and (ii) after remaining unemployed for such a long time, individuals are discouraged and leave the labour force.

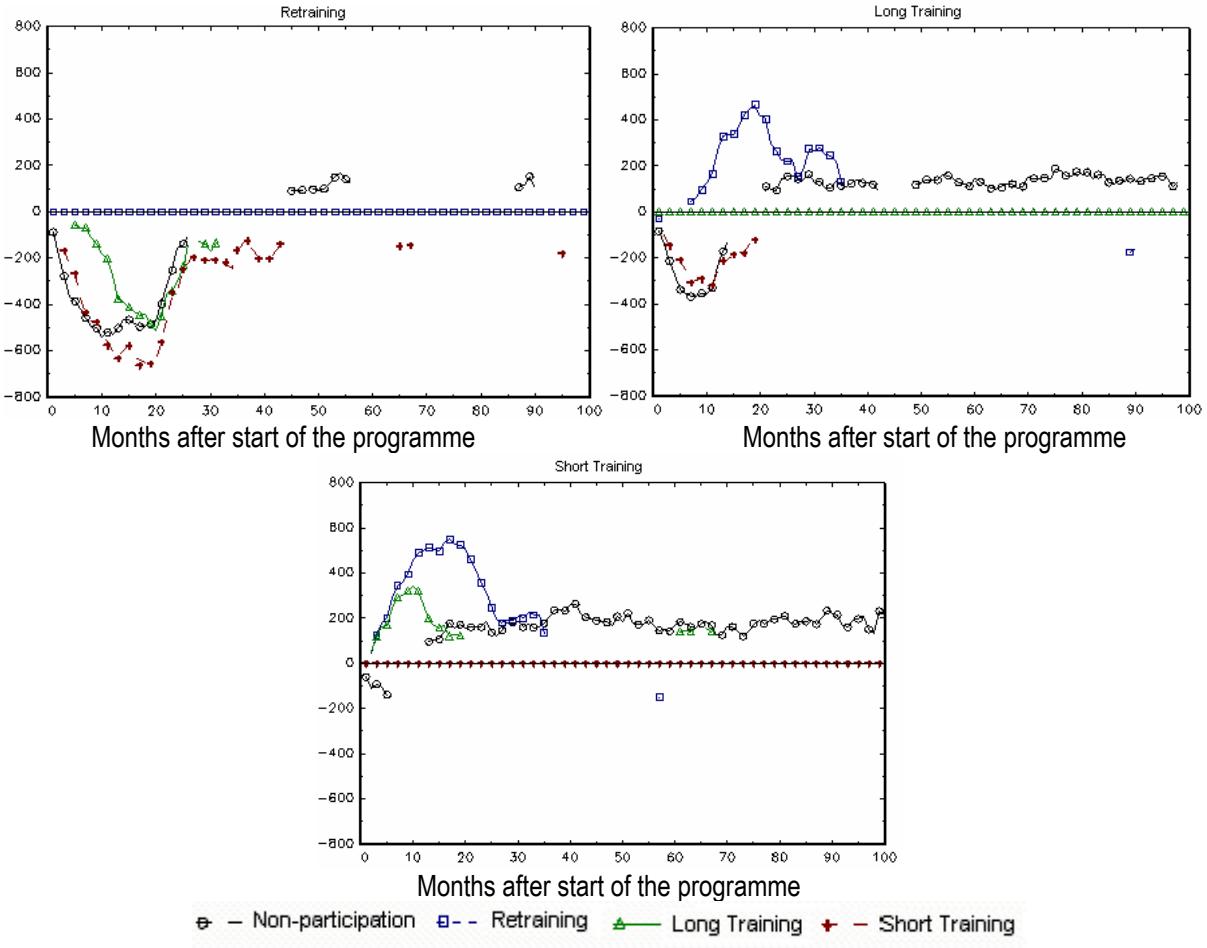
Figure 6.1: Dynamics of the effects ($\theta_0^{m,l}$) after the start of the programme (differences in %-points)



Note: Only effects that are significant at the 5% level (point wise) appear in the figures.

In Figure 6.2, we take account of job quality by using the information on monthly earnings. The results suggest that training consistently increases monthly earnings by about 100-200 EUR in the longer run for all training programmes, although the results are not always significant for retrainees.

Figure 6.2: Dynamics of the effects ($\theta_0^{m,l}$) monthly earnings (in EUR)



Note: Only effects that are significant at the 5% level (point wise) appear in the figures.

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 obtain comparable results. The results are confirmed as well when we use a definition of employment requiring that the new jobs pay at least 90% of the job held prior to training (the results are available in the internet appendix).

6.2 Heterogeneity by types of unemployed

So far, we have considered the average effects for the participants in the different programmes. However, since these groups are heterogeneous, we might expect that there may be differences in how the

programmes affect different types of participants. Therefore, we investigate whether groups defined by different exogenous socio-economic characteristics exhibit different effects by stratifying the sample along the dimensions unemployment duration, type of region, type of occupation and gender, and match within the strata. Note however, that the scope of this exercise, based on a subsample analysis, is limited by the size of our samples of trainees. We find significant differences of the effects with respect to the regional unemployment rate, the type of occupation before training and gender (Appendix D contains detailed results). The effects with respect to regional differences suggest that the overall positive effect of short training is driven by its high effectiveness in regions with a comparatively low unemployment rate, whereas in the other regions it seems to be ineffective. Furthermore, the subsample results for different types of occupations suggest that *short training* may be the most effective programme for skilled workers. However, the samples underlying these estimates may be too small to draw robust inferences.

Clearly, the most substantial differences occur with respect to gender and the two longer training programmes (see Table 6.2).[28] First, note that in the comparison of retraining to nonparticipation, retraining increases the employment rate of participating women by about 25%-points, as well as it decreases unemployment by about 8% points, and increases monthly earnings by about 400 EUR. Retraining is, however, completely ineffective for participating men. Compared to short training, men do much worse than women do. The reason for these stark differences appears to be the different types of retraining obtained by women compared to men. In Section 2 we already showed that the majority of women were retrained towards occupations with average or below average unemployment rates. However, more than 70% of the men were trained towards construction related occupations, which were in high demand in 1993 and 1994, but were severely hurt when the construction boom started to bust in 1995/1996, just when these unemployed completed their retraining and began searching for a job.

[28] Since the effects for men and women based on the common estimation of the MNP model show considerable effect heterogeneity, it appears to be possible that more flexibility is required when estimating the decision to participate in a programme. Therefore, we estimate MNP's for men and women separately (see Table 6.2), but we do not find significant differences in the effects compared to the case with a common MNP model.

The gender differences that appeared for retraining are of a similar magnitude for long training, but now participating in long training really hurts men in reducing their employment probabilities and increasing their unemployment probabilities. However, with respect to the negative effect for men, month 96, which is given the table, really is an exception. For almost all other months, a zero effect for long training compared to nonparticipation cannot be rejected and the male-female difference is smaller than for retraining. Short training courses appear to be effective for men as well as for women.

Table 6.2: Effects 8 years after the beginning of the programme according to gender (ATET; difference in %-points and differences in EUR)

Treated	Obs.		Comparison	Obs.		Unsubsidised employment		Unemployment		Earnings	
	M	W		M	W	M	W	M	W	M	W
Short training	98	160	nonpart.	1252	1888	20.7*	11.7	-9.2	2.6	355*	190
Long training	159	292	nonpart.			-11.2	11.0*	16.7*	1.7	-96	228*
Retraining	232	161	nonpart.			0.8	25.5*	2.3	*-8.3*	4	386*
Short training	98	160	long train.	159	292	13.7	-2.8	-11.1	4.2	142	-79
Short training			retraining	232	161	-0.8	-17.5	0.1	3.5	-84	-275
Long training	159	292	retraining			1.5	-19.5*	12.4	8.4	227	*-199*
Long training			short train.	98	160	0.7	3.2	7.2	-9	16	136
Retraining	232	161	short train.			-11.1	*13.7*	5.6	-23.3*	-154	198
Retraining			long train.	159	292	7.9	3.4	-5.7	-10.9	80	-16

Note: **Bold** numbers indicate significance at the 5% level, numbers in *italics* relate to the 10% level and * to the 1% level. Cells shaded in grey indicate that the difference of the two estimated effects is significant at the 5% level. Employment is the combination of subsidised and nonsubsidised employment. Results are based on estimates in the different subsamples (including the MNP estimation) for men and women. M: Men, W: Women.

Significant gender differences in programme effects have been found in several other evaluation studies that analyse ALMP measures, e.g. Puhani (1999) and Kluve, Lehmann and Schmidt (1999, 2004) for Poland as well as the surveys by Friedlander, Greenberg and Robins (1997) and Heckman, LaLonde and Smith (1999) for Western market economies. Our findings suggest that rather than indicating that a programme has different effects, these differences may occur because women attend different programmes than men, and that it may be impossible to disentangle pure gender differences and programme heterogeneity if detailed data on different types of programmes is not available.

6.4 Sensitivity analysis

We performed several sensitivity tests to check whether choices about issues of implementation are relevant for the robustness of our results. For the sake of brevity, we summarise the results and refer the interested reader to the internet appendix for any details.

First, we checked whether our sample selection and matching algorithm succeeds in balancing participants' and non-participants' elapsed unemployment duration before the actual or simulated programme start. On the one hand, we used a different procedure for simulating programme start dates for nonparticipants. We randomly draw starting dates from the empirical distribution of elapsed UE durations before programme start of participants and look for a nonparticipant with at least the same UE duration. As hypothetical starting date we assign the month after the randomly drawn UE duration.[29] Our results are almost unchanged by this new procedure. On the other hand, we include the elapsed UE duration before programme start as additional matching variable to make sure that we match correctly on that variable (for both ways of simulating starting dates for non-participants). Again, our results remain largely unaffected.

Second, the common support criterion is tightened by defining the upper and lower bounds as the 10^{th} largest and smallest observation instead of the minimum or maximum. This leads to a better match in the tails of the propensity score distribution. Although a considerable number of observations is dropped, the effects hardly change in magnitude, though there is some change in the significance levels. Nevertheless, the overall conclusions do not change.

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

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

Finally, the region Berlin could be a special case because it combines East Berlin (former capital of the GDR) with West Berlin, which experienced the West German economic system even before unification. Therefore, we redo all estimations excluding all inhabitants of Berlin (the distinction between who was living in East and West Berlin before unification is not reliably possible with our data). Again, the results are qualitatively the same.

[29] We thank Barabara Sianesi for suggesting this to us.

7 Conclusion

We analyse the effects of government sponsored training for the unemployed in the beginning of the transition process in East Germany. Our microeconometric analysis is based on new, large and informative administrative data that allows us to use robust nonparametric estimators, like matching in a multiple treatment framework, to control for potential selection bias, and we observe interesting labour market outcomes over 8 years.

Generally, training increases long-term employment prospects and earnings, but does not change registered unemployment. The positive long-run effects need some years to materialise because for all programmes there are initial negative (lock-in) effects. The magnitude of the lock-in effects, and thus the time needed until positive effects appear, is related to the programme duration. On average, positive effects occur about one to three years after the programmes are completed.

This general finding is however not at all true for men participating in long training and retraining. At least part of the explanation for the negative results for men is that caseworkers in East Germany tended to retrain many of the male unemployed towards occupations in the construction sector (about 70% of the participants in retraining) that experienced a boom during those years. Unfortunately, soon after the courses were completed, the boom went into a bust and the blessings of having obtained funding for long vocational training course became a curse for its participants.

A major limitation of our approach and the data available from administrative sources is that prior to the year 2000 it is impossible to distinguish between subsidised employment, for example in an employment programme, and unsubsidised employment in the first labour market. Therefore, the first six to seven years of the dynamics of the effects have to be interpreted with that caveat in mind. However, since for 2002 this data is available, our long-term findings are not subject to this problem. Moreover, the availability of data about the individual costs of the different programmes would greatly improve the value of any evaluation study.

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Appendix A: Data

Table: A.1: Descriptive statistics

	Non-participation	Short training	Long training	Retraining	Practice firm	Other
Number of observations	4604	321	538	445	162	118
	Proportions in %					
	Personal characteristics					
Women	63	65	65	43	58	36
Older than 50 years	8	7	6	1	9	1
Younger than 26 years	12	12	9	23	12	19
Age*	36.82	36.18	37.62	31.64	37.16	34.17
Marital status: Single	41	40	32	48	33	44
Married	59	60	68	52	67	56
Children: No child	40	35	32	40	43	41
At least one child	60	65	68	60	57	59
	Education					
No professional degree	12	7	4	14	8	5
With professional degree	78	87	77	81	86	92
University degree, Polytechnical degree	4	3	15	3	4	4
	Position in last job					
Salaried employee	26	30	54	21	31	31
Part-time worker	26	20	17	10	12	9
Master craftsman	1	0	1	0	0	0
Unskilled worker	22	25	13	28	23	14
Skilled worker	26	26	16	40	34	45
	Last occupation					
Agriculture, forestry, fishing	6	7	4	9	7	8
Plumbing, metal construction technology, technology, natural sciences, machinist, electronics	9	11	14	14	14	19
Food and nutrition, merchant (goods and services), transportation, storage, administration, office work, business and social sciences, health services, hairdressing, guest assistance, housekeeping, cleaning, security services, social services, education, counselling, media, humanities, arts, unskilled worker	55	59	66	47	51	45
Construction, woodworking	9	8	4	11	17	14
Chemical worker, polymer processing, metal production and processing, textile, leather, clothing, paper manufacture and processing, printing, mining, stone, ceramics and glass making and/or processing	5	5	2	13	6	8
	Industrial sector					
Construction	8	6	5	8	9	11
Commerce, banking, insurance, local and regional authorities, social insurance, non-profit organisations, private household, transportation, telecommunications, other services	58	64	60	54	59	42
Agriculture, forestry, fishing	2	2	1	2	1	3
Energy and supply industry, mining, manufacturing (without construction)	27	25	30	32	27	37

Table A.1 to be continued

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

	Non-participation	Short training	Long training	Retraining	Practice firm	Other
Last monthly earnings						
Salary in Euros*	947	1053	1204	1144	1137	1132
No information	20	12	13	9	9	8
1278 > salary	53	55	41	53	54	55
1278 ≤ salary < 2556	25	33	42	39	35	36
2556 ≥ salary	2	0	3	0	1	1
Remaining unemployment (UE) benefits claim at the end of the last unemployment spell before entry in the programme						
Remaining UE benefits claim (in months)*	2.24	2.08	2.25	2.34	2.08	3.42
No information or no claim	56	59	55	53	56	36
≤ 6 months	31	30	32	33	32	42
≤ 1 year	43	40	44	47	43	63
≤ 18	44	41	45	47	43	64
≤ 2 years	44	41	45	47	44	64
Legal UE benefits claim at the beginning of the last unemployment spell before the programme						
Legally claim* (months)	5.73	5.15	5.78	5.52	5.52	6.27
No information	25	27	21	22	20	16
No claim	8	10	9	7	10	5
≤ 6 months	15	16	19	20	22	23
≤ 1 year	63	61	65	70	68	78
≤ 18 months	66	62	69	70	69	79
Up to 2 years	67	63	69	71	70	79
Unemployment benefits or assistance in the month before beginning of the programme						
Unemployment benefits	74	73	79	78	80	84
Unemployment assistance	26	27	21	22	20	16
Various historical un/ out-of/employment information before the "first unemployment period"						
Months of last employment spell*	15.74	14.96	16.58	16.35	15.15	17.16
Proportion of employment months (in %)*	63.38	72.13	77.10	77.54	75.01	79.29
Proportion of out-of-labour months (in %)*	11.63	9.37	8.85	9.86	9.60	5.77
Proportion of UE months (in %)*	13.14	9.98	7.84	7.69	11.31	7.75
# of programs up to 2 years before the UE period*	0.18	0.17	0.11	0.11	0.11	0.14
# of programs up to 5 years before the UE period*	0.22	0.19	0.12	0.12	0.12	0.18
# of programs from entry in the data up to the UE period (UEP)*	0.23	0.19	0.12	0.12	0.14	0.18
Mean duration of UE spells up to 2 years before the UEP*	2.06	1.94	1.37	1.37	1.74	1.23
Mean duration of UE spells up to 5 years before UEP*	3.08	2.24	1.79	1.73	2.16	1.75
Mean duration of UE spells from entry in data up to UEP*	3.12	2.24	1.79	1.72	2.14	1.75
Mean duration of employment spells up to 2 years before UEP*	7.28	9.40	9.93	9.80	10.62	10.35
Mean duration of employment spells up to 5 years before UEP*	13.06	13.42	15.21	14.73	13.29	15.51
Mean duration of employment spells from entry in data up to UEP*	14.78	14.11	15.64	15.74	14.10	15.51
Mean duration of out-of-labour spells up to 2 years before UEP*	1.88	1.60	1.63	1.70	1.52	0.76
Mean duration of out-of-labour spells up to 5 years before UEP*	3.00	2.30	2.10	2.27	2.31	1.64
Mean duration of out-of-labour spells from entry in the data up to the UEP*	3.34	2.33	2.26	2.32	2.31	1.64

Table A.1 to be continued

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

	Non-participation	Short training	Long training	Retraining	Practice firm	Other
Total months in all prog. up to 2 years before the UEP*	2.10	1.46	0.89	0.71	0.69	1.19
Total months in all prog. up to 5 years before the UEP*	2.42	1.66	0.95	0.77	0.75	1.51
Total months in all prog. before entry in sample*	2.47	1.66	0.95	0.77	0.85	1.51
Various un/employment information from the "first unemployment period"						
Duration of the "first UE spell"*	7.59	8.04	7.39	6.41	6.65	4.91
Duration of last UE spell before programme*	6.66	6.44	6.23	5.12	5.72	3.98
Time since beginning of last UE spell (before the prog.) even if other state between UE and prog.*	6.68	8.17	7.46	6.58	6.56	4.89
Time between the prog. and last job*	14.60	12.79	11.65	10.20	9.77	8.23
3 months ≥ time between prog. and last job	15	17	15	19	19	31
6 months ≥ time between prog. and last job	34	35	34	40	43	58
12 months ≥ time between prog. and last job	57	61	68	71	78	80
24 months ≥ time between prog. and the job)	78	86	87	93	94	92
Transition in 6 months before programme:						
UE. → UE	59	65	65	60	63	43
Empl. → UE	26	28	29	31	31	49
Out → UE	9	4	5	6	5	5
Prog. → UE	5	3	1	3	1	3
# of prog. in year before actual prog.*	0.11	0.05	0.02	0.05	0.02	0.08
# of prog.'s in 6 months before actual prog.*	0.05	0.03	0.01	0.03	0.01	0.03
Regional Information (level of regional administrative unit: town, local district)						
Big City	17	17	14	13	10	3
Net migration per 1000 inhabitants	14.16	13.41	9.75	9.84	6.38	3.76
Inhabitants below age 15 in % of the 15-65 year olds	19.42	20.19	21.00	21.48	21.40	23.39
Inhabitants above age 65 in % of the 15-65 year olds	18.30	19.12	19.69	20.10	20.85	21.82
Inhabitants of age 15-65 in % of all inhabitants	59.19	61.63	63.23	63.60	63.63	67.98
Inhabitants above age 75 in % of all inhabitants	4.23	4.68	4.94	5.07	5.23	5.82
Recipients of social assistance per 1000 inhabitants	15.07	15.94	15.95	16.43	16.68	16.39
Fraction of non-German nationals among recipients of social assistance[a]	10.51	8.10	9.71	9.98	14.75	11.51
Net migration per 1000 insured employees*	77.25	66.92	37.52	0.48	5.36	-83.82
Gross value added - fraction agriculture and forestry*	0.65	0.78	1.14	1.36	1.54	2.22
Gross value added - fraction other services*	42.08	44.10	45.44	44.90	45.91	47.07
Gross value added - fraction manufacturing*	30.55	31.63	32.41	33.39	32.32	37.65
Gross value added - fraction commerce and transportations*	10.00	10.78	11.16	11.49	11.83	11.90
Gross value added in DM per inhabitant*	21290	22407	22540	22184	22067	21844
Tax revenue in DM per inhabitant	159	164	161	174	164	160
Rural areas	20	15	22	24	28	30
Core cities and highly concentrated districts in agglomerations	13	14	14	12	9	5
Concentrated and rural districts in agglomerations	19	21	20	21	14	23
Urbanised areas	36	40	37	36	42	42
Berlin, Postdam	11	7	7	9	4	1
Saxony, Thuringia	40	46	47	41	46	56
Mecklenburg-Western-Pomerania, Brandenburg	10	14	15	18	12	10
Saxony-Anhalt	17	10	14	13	27	19

Table A.1 to be continued

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

		Non-participation	Short training	Long training	Retraining	Practice firm	Other
UE rate ≤ 15%		35	33	37	34	27	29
15% < UE rate ≤ 20%		58	58	56	56	58	57
UE rate > 20%		7	9	7	10	15	14
		Firms size of the last employer					
No information		19	14	14	7	9	12
1 to 9 employees		15	12	14	14	10	26
10 to 99 employees		29	28	28	34	37	30
100 to 499 employees		21	25	23	24	20	18
500 employees or more		16	21	21	20	24	14
		Date of entry and exit from the sample					
Date of entry in the data*		Dec. 90	Jun. 91	Jun. 91	Apr. 91	Apr. 91	Jun. 91
Date of entry in the sample*		Nov. 93	Aug. 93	Sep. 93	Aug. 93	Jul. 93	Aug. 93
Date of beginning of prog.*		Jul. 94	Jun. 94	Mai 94	Apr. 94	Fev. 94	Fev. 94
		Outcome+					
Unemployment in Jan. 1991	3326	6.1	2.7	1.6	3.5	6.6	3.3
1992	5911	31.2	23.9	20.1	16.8	19.4	25.9
1993	6150	40.0	38.8	30.6	32.2	26.5	32.5
1994	6194	71.2	76.6	80.7	80.7	86.4	51.7
1995	6196	62.9	68.9	83.6	93.3	63.6	12.7
1997	6196	46.2	43.0	37.9	51.2	44.4	21.2
1999	6196	39.3	34.0	33.6	34.8	40.1	17.8
2001	6196	33.7	33.0	30.0	30.3	35.2	15.3
2002	6148	32.2	31.2	31.7	30.0	36.9	19.8
Employment in Jan. 1991	3326	90.8	94.5	97.6	92.9	93.4	95.0
1992	5911	60.0	69.2	73.7	75.2	70.3	71.6
1993	6150	50.2	53.9	61.9	59.0	63.0	62.4
1994	6194	21.9	20.9	16.0	16.0	12.4	47.5
1995	6196	26.2	28.4	14.1	4.3	33.3	85.6
1997	6196	34.4	48.6	51.5	40.2	46.3	72.0
1999	6196	39.1	52.3	51.7	54.6	49.4	72.9
2001	6196	37.3	49.5	49.3	52.6	47.5	71.2
2002	6148	36.4	51.4	46.3	51.4	43.3	65.3
Out-of-Labour in Jan. 1991	3326	3.3	2.7	0.8	3.5	0.0	1.7
1992	5911	8.8	6.9	6.2	7.8	10.3	2.6
1993	6150	9.8	7.3	7.5	8.6	10.5	5.1
1994	6194	7.1	2.5	3.4	3.4	1.2	1.0
1995	6196	10.8	2.8	2.2	2.5	3.1	1.7
1997	6196	19.3	8.4	10.6	8.5	9.3	6.8
1999	6196	21.7	13.7	14.5	10.6	10.5	9.3
2001	6196	29.0	17.5	20.8	17.1	17.3	13.6
2002	6148	31.4	17.4	22.1	18.6	19.7	14.9
Subsidised empl. Jan. 2002	6148	5.8	7.3	6.5	4.2	7.6	4.1

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. Entries that do not add up to 100% within a group of dummy variables are due to observations with missing information, and rounding. *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. Career improvement is omitted because it contains only 8 individuals.

Appendix B: Detailed estimation results from the multinomial probit model

Table B.1 shows the estimation results of a multinomial probit model (MNP) using simulated maximum likelihood with the GHK simulator.[30] Although being fully parametric, the MNP is a flexible version of a discrete choice model, because it does not require the Independence of Irrelevant Alternatives assumption to hold. With respect to the specification of the covariance structure of choice specific error terms, we impose the normalisation that all correlations with nonparticipation are zero. Furthermore, the correlation between *short training* and *retraining* is set to zero for reasons of numerical stability (this should not be very restrictive since those types of programme cannot not be considered as close substitutes). Furthermore, to increase the numerical stability of the results we exclude some regional variables and some interaction terms from the choice indices related to long training and retraining (these variables are insignificant in a binary probit of this group against nonparticipation). As shown in Table B.2, it turns out that covariance terms are however not significant. The number of draws per equation and observation is another choice parameter. We choose 1000 draws, which is very large by usual standards.

Table B.1: Estimated coefficients of a multinomial probit model for participation in a programme

	Short Training		Long Training		Retraining	
	Coeff.	Std.	Coeff.	Std.	Coeff.	Std.
Constant	-4.40	2.78	*-3.32*	1.81	-1.71	1.68
Women	0.01	0.17	-0.13	0.11	-0.46*	0.11
Age/10	-0.26	0.85	*1.19*	0.61	0.92	0.59
(Age/10)^2	0.01	0.11	-0.17	-2.08	-0.22*	0.08
Marital status: Single	0.03	0.15	-0.08	0.10	-0.15	0.11
Children: No child	-0.08	0.16	-0.04	0.10	-0.01	0.11
Education (reference category: polytechnical degree, University degree)						
No professional degree	-0.12	0.36	-1.16*	0.21	-0.23	0.26
Professional degree	0.39	0.34	-0.58*	0.14	-0.24	0.21
Position in last job (reference category: master craftsman, skilled and unskilled worker)						
Salaried employee	-0.28	0.23	0.53*	0.12	-0.12	0.14
Part-time worker	0.11	0.22	0.26	0.16	-0.24	0.17
Last occupation (reference category: Chemical worker, polymer processing, metal production and processing, textile, leather, clothing, paper manufacture and processing, printing, mining, stone, ceramics and glass making and/or processing)						
Plumbing, metal construction technology, technology, natural sciences, machinist, electronics	0.10	0.28	0.57*	0.22	-0.30	0.19

Table B.1 to be continued

[30] See for example Börsch-Supan, Hajivassiliou (1993) and Geweke, Keane and Runkle (1994).

Table B.1: Estimated coefficients of a multinomial probit model for participation (continued-1)

	Short Training		Long Training		Retraining	
	Coeff.	Std.	Coeff.	Std.	Coeff.	Std.
Food and nutrition, merchant (goods and services), transportation, storage, health services, hairdressing, guest assistance, housekeeping, cleaning, security services, social services, education, counselling, media, humanities, arts, unskilled worker	-0.11	0.25	0.17	0.19	-0.48*	0.15
Construction, woodworking	0.04	0.33	0.18	0.27	*-0.39*	0.21
Agriculture, forestry, fishing	0.02	0.37	0.31	0.28	-0.27	0.21
Administration, office work, business and social sciences	0.24	0.31	0.71*	0.22	-0.28	0.22
Industrial sector (reference category: Energy and supply industry, mining, manufacturing (without construction))						
Commerce, banking, insurance, local and regional authorities, social insurance, non-profit organisations, private household, transportation, telecommunications, other services	0.19	0.17	-0.12	0.10	0.08	0.11
Agriculture, forestry, fishing	-0.43	0.62	-0.38	0.36	-0.05	0.29
Construction	-0.25	0.30	*-0.38*	0.20	*-0.34*	0.19
Last monthly earnings						
log(last monthly earnings)	0.48	0.33	0.33	0.32	0.35	0.29
No information	1.07	1.08	0.96	1.12	0.78	0.99
2500 > salary	-0.09	0.16	*-0.21*	0.11	*-0.20*	0.11
Remaining unemployment (UE) benefits claim at the end of the last unemployment spell before entry in the programme						
Remaining UE benefits claim (in months)	0.20	0.09	0.06	0.04	0.11	0.05
No information	2.71*	0.67	2.00*	0.21	1.91*	0.18
≤ 6 months	0.23	0.34	-0.08	0.22	-0.07	0.22
≤ 2 years (and > 0)	2.06*	0.69	1.85*	0.35	1.41*	0.36
Legal UE benefits claim at the beginning of the last unemployment spell before the programme						
Legally claim* (in months)	-0.08	0.06	-0.04	0.04	-0.06	0.04
No claim	-0.91	0.36	-0.79*	0.21	-0.74*	0.22
≤ 6 months	-0.23	0.29	0.10	0.17	-0.18	0.17
≤ 18 months	-0.52	0.52	-0.72	0.30	0.04	0.32
Various historical un/ out-of/employment information before the "first unemployment period"						
Months of last employment spell[a]	-1.09	1.51	-1.00	1.11	*-1.87*	1.01
Months of last employment spell[a] ^2	0.22	0.98	0.01	0.68	0.97	0.62
Proportion of employment months (in %) before the UE spell[a]	0.04	0.04	0.09*	0.03	0.09*	0.03
Proportion of UE months (in %) before the UE spell[a]	0.09	0.06	0.07	0.04	0.11*	0.04
Mean duration of employment spells up to 2 years before UEP[a]	-3.39	3.72	-5.46	2.49	-2.28	2.48
Mean duration of employment spells up to 2 years before UEP[a] ^2	20.63	16.47	30.15*	11.01	14.89	10.83
Duration of last UE spell before programme[a]	-0.51	0.85	-0.71	0.64	*-1.24*	0.71
Duration of last UE spell before programme[a] ^2	*-0.73*	0.42	-0.66	0.33	*-0.75*	0.39
Duration of the "first UE spell"[a]	-1.61*	0.38	-1.54*	0.25	-1.76*	0.23
Time since beginning of last UE spell (before the prog.) even if other state between UE and prog. [a]	5.30*	0.98	3.59*	0.52	4.30*	0.48
Log(time since beginning of last UE spell (before the prog.) even if other state between UE and prog. [a])	*-1.31*	0.77	0.07	0.52	-0.16	0.52
12 months ≥ time between prog. and last job	0.61	0.28	0.41*	0.15	0.35	0.16
Transition in 6 months before the programme: empl. → UE	*0.55*	0.31	*0.30*	0.16	-0.12	0.16
Number of prog. in year before actual programme	0.11	0.35	*-0.47*	0.26	0.05	0.22
Unemployed and employment status before programme (reference categories: out-of-labour, missing)						
Unemployed the 6th. month before prog.	-0.44	0.37	*-0.35*	0.21	-0.14	0.22
Unemployed the 24th. month before prog.	-0.30	0.25	-0.37	0.18	-0.63*	0.18
Unemployed the 36th. month before prog.	-0.74	0.33	-0.13	0.19	-0.49	0.22
Employed the 6th. month before prog.	-0.33	0.35	-0.53	0.22	-0.16	0.24
Employed the 24th. month before prog.	-0.08	0.19	-0.13	0.13	-0.09	0.12
Employed the 36th. month before prog.	-0.27	0.17	-0.02	0.10	-0.16	0.11

Table B.1 to be continued

Table B.1: Estimated coefficients of a multinomial probit model for participation (continued-2)

	Short Training		Long Training		Retraining	
	Coeff.	Std.	Coeff.	Std.	Coeff.	Std.
Regional information						
Big City	0.24	0.32	-0.27	0.18	-0.31	0.20
Net migration per 1000 inhabitants.[a] (if local district not missing)	0.12	0.06	0.02	0.03	0.04	0.04
Inhabitants below age 15 in % of the 15-65 year olds.[a,b]	11.94	5.36			7.02*	2.60
Inhabitants above age 65 in % of the 15-65 year olds.[a,b]	11.84	5.22			6.21	2.49
Inhabitants of age 15-65 in % of all inhabitants.[a,b]	25.30	11.17	-0.11	0.16	13.92*	5.40
Inhabitants above age 75 in % of all inhabitants.[a,b]	0.77	2.66	0.32	0.53	*3.16*	1.79
Recipients of social assistance per 1000 inhabitants.[a,b]	0.03	0.12	-0.06	0.08	-0.09	0.08
Fraction of non-German nationals among recipients of social assistance.[a,b]	-0.01	0.16	-0.10	0.11	0.06	0.10
Gross value added - fraction agriculture and forestry.[a,b]	-23.13	10.13			-13.6*	4.87
Gross value added - fraction other services.[a,b]	-22.96	10.13			-12.78*	4.89
Gross value added - fraction manufacturing.[a,b]	-22.95	10.11			-12.74*	4.88
Gross value added - fraction commerce and transportations.[a,b]	-23.01	10.13			-12.59*	4.88
Log(Gross value added in DM per inhabitant).[a,b]	0.42	1.21	0.73	0.73	-0.01	0.83
Log(Tax revenue in DM per inhabitant).[a,b]	-0.28	0.44	0.16	0.24	-0.27	0.28
Net migration per 1000 insured employees. [a,b]	0.01	0.01	0.00	0.00	-0.00	0.01
Rural area[b]	*-0.43*	0.23	-0.11	0.12		
(reference category: Saxony-Anhalt)						
No regional information	0.15	0.70	-0.42	0.49	0.44	0.48
Berlin, Potsdam	-0.52	0.76	-0.48	0.51	0.79	0.50
Saxony, Thuringia	0.23	0.71	-0.24	0.49	0.47	0.48
Mecklenburg-Western-Pomerania, Brandenburg	0.66	0.73	0.26	0.51	1.13	0.51
(reference category: UE rate > 20%)						
UE rate ≤ 15%	*-0.51*	0.28	-0.07	0.18	-0.37	0.19
15% < UE rate ≤ 20% (E)	-0.51	0.25	-0.22	0.18	-0.51*	0.17
Firms size of the last employer (reference category: 100 employees or more)						
No information	0.18	0.31	0.24	0.18	-0.22	0.22
1 to 9 employees	-0.24	0.19	-0.03	0.13	-0.16	0.13
10 to 99 employees	*-0.26*	0.15	-0.09	0.10	-0.16	0.10
Date of entry in the sample[a]	-0.49*	0.17	-0.52*	0.11	-0.61*	0.10
Age < 26 * time since beginning of last UE spell (before the prog. even if other state between UE and prog.) [a]	-0.21	0.32	0.27	0.23	-0.04	0.21
Age ≥ 50 * months of last employment spells[a]	-0.75	1.74	-1.94	1.23		
Empl. → UE in 6 months before prog * claim[a]	*-0.10*	0.06				
Polytechnical degree *time since beginning of last UE spell (before the prog. even if other state between UE and prog.) [a]	-0.61	0.65	-0.36	0.32	-0.43	0.52

Note: Simulated maximum likelihood estimates using the GHK simulator (1000 draws in simulator for each observation and choice equation). Coefficients of the category NONPARTICIPATION are normalised to zero. Inference is based on the outer product of the gradient estimate of the covariance matrix of the coefficients ignoring simulation error. N = 5908. Value of the log-likelihood function: -0.570502. **Bold** numbers indicate significance at the 5% level, numbers in *italics* relate to the 10 % level and * to the 1 % level. [a] The variable is scaled. [b] If local district not missing.

Table B.2: Estimated covariance and correlation matrices of the error terms in the multinomial probit

	Nonparticipation		Short Training		Long Training		Retraining	
	Coef	t-val	Coef	t-val	Coef	t-val	Coef	t-val
NONP	1	-	0	-	0	-	0	-
ST	0		1	-	0.25	0.53	0	-
LT	0		0.24		1.06	4.59	-1.00	-1.22
RT	0		0		-0.69		2.01	1.21

Note: Diagonal and upper triangular matrix shows covariance terms. Correlations are shown in the part below the main diagonal. In the estimation Cholesky factors are used for parameterisation to ensure that the estimated covariance matrix of the error terms is positive definite.

Appendix C: Technical details of the matching estimator used

Here, we consider outcomes of five different states denoted by $\{Y^0, Y^1, Y^2, Y^3, Y^4\}$ [31]. The different *states* are called *treatments* in the econometric evaluation literature. For any individual, only one component of $\{Y^0, Y^1, Y^2, Y^3, Y^4\}$ is observable. Participation in a particular treatment *m* is indicated by the realisation of the random variable *S*, $S \in \{0,1,2,3,4\}$. This notation allows us under the usual assumptions (see Rubin, 1974) to define average treatment effects for pair wise comparisons of the effects of different states (Lechner, 2001, 2002):

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

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

$\gamma_0^{m,l}$ denotes the expected effect of treatment *m* relative to treatment *l* for a participant drawn randomly from the population (average treatment effect, ATE) ($\gamma_0^{m,l} = -\gamma_0^{l,m}$).[32] $\theta_0^{m,l}$ is the expected effect for an individual randomly drawn from the population of participants in treatment *m* only (ATET). ATET's are not necessarily symmetric. Note that we are only interested in states 0 to 3.

ATE's and ATET's are generally not identified so that additional assumptions are needed. As stated in the main body of the text we assume that treatment participation and treatment outcome is independent conditional on a set of (observable) attributes (conditional independence assumption, CIA). CIA defined to be valid in a subspace χ of the attribute space is formalised in expression (C.3):

$$Y^0, Y^1, \ldots, Y^M \coprod S | X = x, \forall x \in \chi. \tag{C.3}$$

CIA requires that all individuals that are part of the evaluation could participate in all states (i.e. $0 < P(S = m \mid X = x)$, $\forall m = 0,\ldots,3$, $\forall x \in \chi$).

[31] The last state contains a residual group of programmes and is omitted in the estimation part.

[32] 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*.

Lechner (2001) showed that the famous result of Rosenbaum and Rubin (1983) that CIA conditional on *X* implies CIA conditional on the choice probabilities conditional on *X* (the so-called propensity score property) also applies to the case of multiple treatments. This property is the basis for the so-called matching estimators, a particular version of which is proposed by Lechner, Miquel and Wunsch (2004), which is detailed in Table C.1. The interested reader is referred to that paper for the motivation of the different choices implicit in the various estimation steps.

Table C.1: A matching protocol for the estimation of $\theta_0^{m,l}$

Step 1	Specify and estimate a multinomial probit model to obtain the marginal choice probabilities (see Appendix B): $[\hat{P}_N^0(x), \hat{P}_N^1(x), \hat{P}_N^2(x), \hat{P}_N^3(x)]$; $\hat{P}_N^l(x) := \hat{P}_N(S = l \mid X = x)$
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. For a given value of m and l the following steps are performed: a-1) Choose one observation in the subsample defined by participation in m and delete it from that pool. 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. c-1) Repeat a-1) and b-1) until no participant in *m* is left. d-1) Compute the maximum distance (d) obtained for any comparison between treated and matched comparison observations. a-2) Repeat a-1). 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. c-2) Repeat a-2) and b-2) until no participant in m is left. d-2) For any potential comparison observation, add the weights obtained in a-2) and b-2). 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). f-1) Predict the potential outcome $y^l(x_i)$ of every observation in *l* and *m* using the coefficients of this regression: $\hat{y}^l(x_i)$. f-2) Estimate the bias of the matching estimator for $E(Y^l \mid S = m)$ as: $\sum_{i=1}^{N} \frac{1(S = m)\hat{y}^l(x_i)}{N^m} - \frac{1(S = l)w_i\hat{y}^l(x_i)}{N^m}$. 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. 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 $\hat{\theta}_N^{m,l}$ 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 (and observed) 12, 24 and 36 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 this application. Note that once we estimate all $E(Y^l \mid S = m)$ for all *m*, they can be directly used to obtain $E(Y^l)$.

Appendix D: Subgroup analysis

Table D.1 displays the estimation results for unsubsidised employment in different subgroups of unemployed. We use the MNP estimates from the joint model, but the remaining steps of the estimation are performed in the subsamples. Thus, 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). The number of observations given in the lower part of the table indicates that in many cases the subsample estimates will be too imprecise to uncover significant differences.

Table D.1: Effect heterogeneity (unsubsidised employment) eight years after the beginning of the programme (difference in %-points) ($\theta_0^{m,l}$)

	Regional UE rate		Big town		Gender		Long-term UE		Type of occupation[a]		
m - l	≤ 15%	> 15%	<100,000 inhab.	≥100,000 inhab.	Men	Women	<12 months	≥12 months	Unskilled	Skilled[b]	Salaried
Short training - nonparticipation	29.7*	3.2	6.1	17.6	15.0	13.6*	21.9*	9.1	7.1	20.1*	8.6
Short training - long training	17.4	2.3	5.0	12.7	15.6	2.7	15.4	0.9	-14.9	26.7*	-3.3
Short training - retraining	9.6	-10.9	-3.2	5.6	2.7	*-12.4*	*11.1*	1.3	-20.6	7.7	-28.0
Long training - nonparticipation	6.3	1.9	5.3	-6.7	-12.9*	10.2	5.7	1.5	3.4	-4.3	2.6
Long training - short training	-23.5*	8.5	1.7	-14.4	*-18.0*	5.4	1.9	-8.9	5.4	-25.2*	-4.5
Long training - retraining	*-16.8*	-15.3	-12.8	*-24.3*	-1.1	-19.8*	-20.5*	-7.9	-13.6	-3.1	-20.6
Retraining - nonparticipation	6.8	3.7	*5.6*	1.8	-4.5	17.3*	14.6*	10.2	-1.6	6.1	17.4
Retraining - short training	-24.5*	0.8	-5.7	-25.2	-15.9	-1.4	-7.9	-12.4	0.5	-24.8*	1.8
Retraining - long training	0.8	*10.6*	5.8	15.4	8.7	5.3	12.1	-0.9	7.9	23.8	1.2
Sample size											
Nonparticipation	1156	2232	2696	639	1367	2013	1856	1502	680	810	946
Short training	91	187	211	61	101	164	149	130	55	68	80
Long training	174	309	347	122	170	305	291	186	58	74	263
Retraining	136	279	351	63	242	174	261	156	101	166	88

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. [a] For the comparison short training - retraining, only the pairwise differences unskilled-skilled and skilled-salaried are significant. [b] Incl. master craftsman.