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Evaluating the Employment Effects of Job Creation Schemes in Germany

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Preface

This book was written during my time as research assistant at the Department of Economics and Business Administration at Johann Wolfgang Goethe University in Frankfurt/Main and accepted as doctoral dissertation. It was undertaken within the joint research project "Eingliederungseffekte und weiterer Nutzen von ABM und SAM für die Geförderten unter besonderer Berücksichtigung von 'SAM für Ältere'" (Effects of Job Creation and Structural Adjustment Schemes) which was conducted by the Chair of Statistics and Econometrics and the Institute for Employment Research in Nuremberg.

I would like to thank Prof. Dr. Reinhard Hujer for initiating and supporting this thesis and supervising me with encouragement. It was his permanent lobbying for evaluation research in Germany that made the use of the administrative data of the Federal Employment Agency possible for the evaluation of the employment effects of job creation schemes. I am also very thankful to Prof. Bernd Fitzenberger, Ph.D. who did not hesitate to act as the second thesis supervisor. For valuable help with the data I am indebted to Christian Brinkmann, Elmar Kellner, Steffen Kaimer, Melanie Stamm and the team at the Institute for Employment Research. Warm thanks go to my former and current colleagues Dr. Marco Caliendo, Dr. Stefan Kokot, Dr. Dubravko Radić, Paulo Rodrigues, Sandra Vuletić and Christopher Zeiss. Thanks also to Birgit Andres-Kreiner, to all our student research assistants and to Romy Weiland for her proofreading.

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Mannheim, November 2006 Stephan Lothar Thomsen

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1

Introduction

Unemployment is a severe problem in Germany. In 2004, the unemployment rate amounted to 20.1 in East and 9.4 percent in West Germany (Bundesagentur für Arbeit, 2005). Unemployment causes major economic and social damage. On the macro level, unemployment represents an inefficient allocation of labour and human capital to the economy. Therefore, the economy's production capacity is not fully utilised. On the micro level, unemployment disables persons from earning a living on their own.[1] The Federal Employment Agency[2] (*Bundesagentur für Arbeit*, FEA) spends substantial amounts of the annual fiscal budget to overcome the unemployment problem. In particular, the use of active labour market policy (ALMP) programmes should help to balance labour supply and demand. There is a variety of programmes that aim at adjusting the human capital of workers and unemployed persons to the demands of the market, e.g., vocational training programmes and training courses, or are used to support the creation of new jobs, e.g., wage subsidies and job creation schemes.

Job creation schemes have been an important measure until the early 2000s. From 1997 to 2003, over 23 billion Euro were spent on job creation schemes, and approximately 1.6 million participants joined the programmes. Job creation schemes are a kind of subsidised employment and aim at improving the employability of unemployed persons with barriers to employment. Although efforts of the FEA were immense, scepticism about the effectiveness of the programmes in order to improve the employability of the participants increased due to a permanently bad labour market situation.

These doubts are not specific to Germany. For example, Heckman, LaLonde, and Smith (1999) point out that previous evaluations in OECD countries indicate that

[1] Although German social security generally prevents the unemployed from getting poor, the majority of them suffers from financial difficulties. In addition, unemployment is often followed by social exclusion and a decay of human capital. Furthermore, it may indicate a break in the professional career, induce psychosocial and physiological stress, and in some cases may heighten the crime rate and prevent the occupational socialisation in particular for younger unemployed persons. For that reason, unemployment is a heavy burden for the economy (see, e.g., German Council of Economic Experts, 1994).

[2] Until the end of 2003: *Bundesanstalt für Arbeit.*

ALMP programmes usually have at best a modest impact on participants' labour market prospects, but at the same time there is a considerable heterogeneity in the impact of these programmes. This is also a common finding in the recent evaluation literature of ALMP programmes in Europe (see, e.g., the overviews by Martin and Grubb, 2001, for OECD countries; Hagen and Steiner, 2000, for Germany; or Calmfors, Forslund, and Hemström, 2001, for Sweden). Whereas ALMP were seen as a reasonable opportunity to reduce and avoid unemployment for a long time, the international experiences with the implemented programmes show a mixed picture, and the majority of programmes seem to be ineffective in terms of their goals. For that reason, international evidence on the effectiveness of ALMP suggests that programmes should be well-targeted to the needs of the individual job-seekers and the labour market, and that treatment should start as early as possible in the unemployment spell (OECD, 1998). The aim of this study is to evaluate the employment effects of job creation schemes in Germany with respect to these two suggestions. My first question asks how programme effects differ with respect to the timing of treatment in the individual unemployment spell. The second question of my analysis considers a more adequate targeting of the programmes to the needs of the unemployed individuals.

Evaluation of programme effects is not an easy task. The individual causal effect of a programme is defined as the difference of the value of the participant's outcome in the current situation and the value of the outcome in a situation where the participant has not joined the programme. Since an individual cannot be in both states at the same time, one could never observe both states simultaneously for the same individual. Therefore, the outcome for the participants in the situation without training has to be estimated by using information of non-participating individuals, i.e. a comparison group. However, if the selected non-participants differ from the participants in relevant characteristics, treatment effects may be biased, and they could not be used as the comparison group. Thus, it is essential for evaluation that participants and non-participants are identical in all relevant observable characteristics that jointly determine programme participation and labour market outcomes. In addition, when using administrative data to evaluate the employment effects one has to apply a non-experimental evaluation approach. Since the selection process into programmes is non-random, ignoring the nature of the data may lead to selection bias.

I use propensity score matching to solve the selection problem according to Rosenbaum and Rubin (1983b). The basic idea of matching is to find, in a large group of non-participants, those individuals who are similar to the participants in all relevant observable pre-programme characteristics. These relevant characteristics are summarised in the estimated propensity to participate in the programme (propensity score). Thus, matching resembles an experimental control group in one key respect: The distribution of the counterfactual outcome of the participants is the same as the observed distribution of the outcome of the comparison group, conditional on the propensity score. Since matching methods concern themselves solely with selection on observable variables, they require very rich data in order to make the estimates credible. The main advantage of the method of matching is due to two properties of the approach. First, matching is non-parametric. Therefore, no particular distribution

has to be assumed. Second, matching is highly flexible. It may be combined with other methods or may be used to consider further aspects of evaluation, e.g., evaluation of the effects for sub-groups or with respect to the timing of treatment. Recent empirical studies on evaluation of ALMP programmes in comprehensive systems like Sweden (Sianesi, 2004), Switzerland (Steiger, 2004) or Germany (Fitzenberger and Speckesser, 2005) have emphasised the importance of the timing of treatment in the individual unemployment spell for the estimation of the treatment effects.

I do so by applying the approach by Sianesi (2004). She suggests discretising the unemployment duration and estimating the treatment effects by a series of matching estimators. For different durations of unemployment prior to the start of the programmes, treatment effects are estimated separately. Thus, the estimated effects provide a picture of the effects with respect to the timing of treatment. However, it has to be mentioned that this approach does not look at any interdependencies between the individual groups under analysis; and effects with respect to the timing of treatment can only be compared descriptively. Fortunately, I am able to use unique data derived from the final version of the programme participants master data set (MTG) of the Institute for Employment Research (IAB) that allow us to analyse the employment effects for entries in job creation schemes between July 2000 and May 2001 until 30 months after programmes have started. Moreover, with these rich data at hand, considering explicitly the timing of treatment in the individual unemployment spell is possible.

A number of empirical studies have been conducted to remove the uncertainties about the effectiveness of job creation schemes in Germany. The earlier studies are all based on survey data sets. Drawing policy-relevant conclusions from the results is problematic since those survey data have several shortcomings. First, the data cover a small number of observations only. Therefore, taking account of heterogeneity in the treatment effects is not possible in the estimations. Second, although the data are very informative due to a large number of attributes to describe the labour market situation of the individuals, inexact information on times of treatments as well as on the (un)employment histories of individuals makes the interpretation of the estimates difficult. Third, as they concentrate on East Germany, evidence for West Germany is missing in the earlier studies.

With the enaction of Social Code III (*Sozialgesetzbuch III*, SGB III) as the legal basis, output evaluation of all ALMP instruments became mandatory. Moreover, the legislator postulated the liberalisation of administrative data for scientific research. Subsequently, administrative data have been made available for researchers making it possible to evaluate the effects of job creation schemes (see, e.g., Hujer, Caliendo, and Thomsen, 2004, or Caliendo, Hujer, and Thomsen, 2004), but also of vocational training programmes (see, e.g., Lechner, Miquel, and Wunsch, 2005a; 2005b, Fitzenberger and Speckesser, 2005, and Hujer, Thomsen, and Zeiss, 2006b).[3] The major advantage of these administrative data is that they contain a large number of participants allowing effect heterogeneity to be considered. The studies using administrative data to evaluate the employment effects of job creation schemes in

[3] The studies evaluating vocational training focus on programmes carried out before 1998.

Germany are based on a prototype version of the MTG of the IAB that contain rich information to characterise the individuals' labour market situations. However, these data cover one single entry month of job creation schemes only (February 2000). Although different sources of effect heterogeneity are regarded, i.e. individual, sectoral and regional heterogeneity in the employment effects, possible differences in the allocation of unemployed persons to programmes due to the timing of treatment in the unemployment spell or changes in the economy (seasonal differences) could not be considered.[4]

Another aspect that has gained interest in the evaluation literature recently refers to the role of the allocation mechanisms for the programme effects. The results of, for example, Caliendo, Hujer, and Thomsen (2006a; 2006c) indicate that the average employment effects for the participating individuals of job creation schemes are negative. Possible reasons may be a poor quality of the programmes in association with often cited stigma and locking-in effects, but also inefficiencies in the allocation of unemployed persons to the programmes. Since programme effects are heterogeneous, negative mean impacts do not necessarily imply negative effects for all of the participating individuals. Therefore, identifying those individuals who gain from participation is an obvious opportunity to improve their future efficiency, i.e. target the programmes to those individuals who benefit.

Answering this question will be the second aspect I examine in this study. To do so, I use data on participants in job creation schemes who have started the programmes in February 2000. In the first step, treatment effects are estimated for a selection of special target groups of the labour market like long-term unemployed persons or individuals without professional training. After that, I construct a simple indicator called *target score* based on the individual's number of disadvantages on the labour market to analyse whether programme effects differ corresponding to the individual labour market obstacles. If programmes are tailored to the needs of the most disadvantaged, one would expect stronger effects for persons with a higher *target score*. Finally, I use the estimated participation probability to answer the question whether a higher participation probability correlates with a higher programme effect.

The study proceeds as follows. Chapter 2 presents some notes on the relevance of job creation schemes in Germany. Due to the clear differences of the labour market in West and East Germany, I start with a brief overview of the development since German Unification. Further topics of this chapter are the role of job creation schemes within the variety of ALMP programmes in Germany and the empirical and institutional arrangement. After summarising the findings of previous empirical studies evaluating the effects of job creation schemes in Germany, I discuss intended and possible impacts of job creation schemes with a distinction between the micro- and macroeconomic level. Chapter 3 presents the methodological framework for my evaluation. The evaluation approach in the static setting is used to discuss the fundamental evaluation problem, the parameter of interest, the problem of selection bias

[4] See Caliendo, Hujer, and Thomsen (2006a) for an analysis considering individual and regional heterogeneity, and Caliendo, Hujer, and Thomsen (2006c) for different aspects of sectoral heterogeneity in the employment effects of job creation schemes in Germany.

and different identifying assumptions invoked in the literature to deal with it. After that, the matching estimator and its identifying assumptions are discussed as well as the extension to the dynamic setting that allows to consider the timing of treatments. Moreover, several aspects to be considered in empirical implementation are discussed at the end of the chapter. Chapter 4 describes the preparation and content of the data used for the empirical analyses. The results for the estimated employment effects of job creation schemes are presented and discussed in chapter 5. The analysis considers the timing of treatment in the individual unemployment spell explicitly and takes account of regional differences by estimating the effects for West and East Germany separately. The results for the second evaluation question are given in chapter 6. It provides an approach identifying effect heterogeneity in the employment effects to improve the efficiency of job creation schemes in Germany. The last chapter concludes this study.

2

Some Notes on the Relevance of Job Creation Schemes in Germany

2.1 Overview

In this chapter, I will discuss the relevant empirical and institutional issues of job creation schemes in Germany. For a reasonable evaluation of the impacts, a careful characterisation of the programmes in analysis is needed. To do so, I will start with a brief characterisation of the German labour market since German Unification in 1990 in section 2.2. Reviewing the development of the labour market is necessary as on the one hand, the situation in East and West Germany is clearly separated, and on the other hand, relevance and efficiency of job creation schemes depend on the actual situation of the labour market. Section 2.3 provides an overview of German ALMP and a description of the legal basis and institutional framework of job creation schemes. To base my evaluation of programme impacts on an adequate economic model, it is important to know the main determinants of participation and outcomes. Here, a particular focus is on the admission criteria and the allocation mechanism that are essential for modelling the participation process and for the construction of the comparison group. Furthermore, the admission criteria are a constituent part for the participants' structure. As my empirical analyses in chapters 5 and 6 are based on programmes that have started during the years 2000 and 2001, I will focus on this time span in particular.

To improve the quality of my characterisation, a review of the experiences with job creation schemes in East and West Germany from previous empirical studies is given in section 2.4. Careful consideration of the results of these studies may help to obtain possible sources of heterogeneity and distinctive features of the programmes. Section 2.5 discusses the possible effects of job creation schemes taking account of the results from the previous sections of this chapter. The final section summarises the findings and implications.

2.2 A Brief Characterisation of the German Labour Market Since German Unification

The German Unification in 1990 reflects an incisive point for social, political and individual life in Germany. In consequence of the collapse of the Command Economy of the German Democratic Republic (GDR) two countries, which differed widely not only in their institutional and constitutional arrangements, but also in their monetary systems and real economic conditions, were unified (Siebert, 1991). In the last 15 years since German Unification, massive efforts have been made in social and labour market policy to smooth the differences of the labour markets between East and West Germany. However, the situation is still clearly separated, and talking of the 'German labour market' might be misleading. The substantial differences in the regional labour markets in eastern and western Germany are to some extent the legacy of the former countries, but also a result of labour market and economic policy of the past years. The following description will characterise the labour market development in East and West Germany since 1990.

To point up some of the differences, Tables 2.1 to 2.3 present some selected figures of the labour market for the years 1991 to 2003 with a distinction between West and East Germany. Table 2.1 contains information on population, unemployment, unemployment rates and GDP growth. The population figures cover the resident population, the labour force potential and the working population. Whereas the resident population gives an idea of the relative size of both parts, labour force potential and working population are indicators for the economic activity. The unemployment category comprises the number of openly unemployed persons, of long-term unemployed individuals, of hidden unemployed persons and the sum of the open and hidden unemployment. Open unemployment is defined as the sum of all registered unemployed persons at the FEA. Hidden unemployment refers to the concept of the German Council of Economic Experts (*Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung*). It contains all persons who participate in labour market programmes and would have been unemployed without those subsidies. Since they do neither receive unemployment benefits nor assistance, these persons are not registered as unemployed persons. The number of long-term unemployed persons is added to map the persistence of unemployment. Furthermore, the GDP growth in both parts as well as the productivity and gross wages per employee for East Germany in relation to the western level are displayed as indicators for the economic situation and development.

Table 2.2 provides a selection on the deployment of several (active) labour market policy programmes since German Unification. The choice was made according to the importance of the single programmes in terms of the number of promoted individuals and the corresponding expenditures. As unemployment also depends on the structure and development of the labour force potential, Table 2.3 tries to summarise the changes of the main determinants for the labour force potential following Bundesanstalt für Arbeit (2001) for three periods (1990 to 1993, 1994 to 1997, and 1998 to 2000). These determinants are inner German migration, commuters' balance, im-

Table 2.1: Selected Figures of the German Labour Market (1991-2003)

Year	1991	1993	1995	1997	1999	2001	2003
				West Germany[1]			
Population							
Resident Population[2]	64,485	65,740	66,342	66,688	66,946	65,353	65,619
Labour Force Potential[3]	34,013	34,824	35,147	35,562	35,936	35,580	35,830
Working Population	31,069	31,120	30,850	30,814	31,507	31,515	31,091
Unemployment							
Open Unemployment	1,689	2,270	2,565	3,021	2,756	2,321	2,753
Long-Term Unemployed	455	594	828	1,057	963	729	857
Hidden Unemployment[4]	786	1,006	964	1,027	1,030	1,019	1,039
Open and Hidden Unemployment	2,475	3,276	3,529	4,048	3,786	3,340	3,792
Unemployment Rates							
Open Unemployment	5.7	7.3	8.3	9.8	8.8	8.0	9.3
Open and Hidden Unemployment	7.9	10.3	11.2	12.9	*na*	*na*	*na*
GDP growth[5]	5.0	-2.6	1.4	1.5	2.1	1.1	-0.1
				East Germany[1]			
Population							
Resident Population[2]	15,790	15,598	15,476	15,369	15,217	17,118	16,913
Labour Force Potential[3]	9,025	8,781	8,615	8,493	8,333	9,520	9,485
Working Population	7,385	6,245	6,532	6,380	6,435	7,396	7,155
Unemployment							
Open Unemployment	913	1,149	1,047	1,364	1,344	1,532	1,624
Long-Term Unemployed	*na*	396	344	462	469	525	674
Hidden Unemployment[4]	1,810	1,573	1,215	928	931	748	599
Open and Hidden Unemployment	2,723	2,722	2,262	2,292	2,275	2,280	2,223
Unemployment Rates							
Open Unemployment	11.1	15.1	14.0	18.1	17.6	18.8	20.1
Open and Hidden Unemployment	29.5	30.5	26.2	27.1	*na*	*na*	*na*
GDP growth[5]	-19.2	8.7	3.5	0.5	1.8	-0.5	-0.2
Productivity[6]	32.9	39.0	42.5	44.6	67.7	69.1	na
Gross wages per employee[6]	57.5	74.2	79.1	79.8	80.9	81.2	81.2

na = not available

[1] West Germany refers to federal states that constituted the former Federal Republic of Germany (including West Berlin until 1999). East Germany refers to the federal states that constituted the former German Democratic Republic (including East Berlin until 1999, and Berlin since 1999).

[2] Resident population of West Germany including West Berlin until 2001; East Germany including East Berlin until 2001, and Berlin since 2001.

[3] Figures for the labour force potential are based on estimations by the IAB.

[4] The hidden unemployment covers all unemployed persons who participate in labour market programmes, do not receive unemployment benefits or assistance, and are therefore not registered as openly unemployed (German Council of Economic Experts).

[5] All figures are in percent. GDP growth based on GDP constant 1995 prices. Productivity in GDP per hours worked in 1995 prices.

[6] In relation to West Germany. Figures are taken from Wunsch (2005).

Sources: Bundesanstalt für Arbeit (2001), Bundesagentur für Arbeit (2004), German Council of Economic Experts (1999; 2000; 2002; 2003), Federal Statistical Office Germany.

migration from abroad, number of German resettlers from the Commonwealth of Independent States, and sum of demographic development and propensity to work.

I start the description of the development of the labour market with East Germany and describe the West German analogue afterwards. All numbers in the text refer to the figures of Tables 2.1 to 2.3 and the respective references.

2.2.1 Development of the East German Labour Market

For the characterisation of the East German labour market, it is useful to distinguish between three different periods of its development. The first period covers the years 1989/1990 to 1993 during which the East German labour market experienced an enormous employment reduction together with a strong increase of open and hidden unemployment. In the following years, 1994 to 1996, the labour market stabilised to some extent and the employment reduction of the first years after German Unification came to an end. However, since 1997 the development of the labour market has worsen again due to strong structural deficits of the East German economy.

Before German Unification the labour market of the former GDR was typical for the Command Economies in Eastern Europe at first sight. The characteristic elements were a full employment and a large labour market participation of women. However, it must be assumed that hidden unemployment amounted to 15 to 30 percent at a closer inspection (Bundesanstalt für Arbeit, 2001).[1] In 1989, the working population amounted to 9.8 million persons. As becomes obvious from the figures in Table 2.1, working population as well as labour force potential decreased dramatically until 1993. In that year, the working population was 6.25 million, which is (almost) 3.55 million less compared to 1989. In the same time, unemployment occurred for the first time. However, open unemployment increased only up to 1.15 million until 1993. The reason for this relatively small increase, compared to the employment reduction, was the massive deployment of labour market policy programmes and a strong East-West migration together with a large number of commuters to the West.

The employment reduction – and in consequence the rise of unemployment – was caused by several factors. The first factor was the obsolete capital and production stock. Siebert (1991) notes that 64 percent of the capital goods of the equipment in industry were older than 5 years and 21.1 percent were even older than 20 years. Furthermore, the capital stock was geared towards distorted environmental and energy costs. As the production and capital stock were oriented on the COMECON[2], many products were not able to compete internationally due to their poor quality, but also for environmental and safety reasons. Finally, 47.2 percent of the employment

[1] Hidden unemployment in the former GDR is not comparable to hidden unemployment as defined by the German Council of Economic Experts. Hidden unemployment contains all persons who would be unemployed if their occupations were not supported by governmental institutions. These are, for example, participants in ALMP programmes or persons in early retirement. As the majority of the former GDR occupations was public-sector sponsored, an explicit distinction between necessary and supported jobs is impossible.

[2] The Council for Mutual Economic Assistance (abbreviated COMECON or CMEA) was an international organisation of socialist countries for economic cooperation from 1949 to 1991. Members were the Soviet Union, the German Democratic Republic, Bulgaria, Cuba, Poland, Romania, Czechoslovakia, Hungary, Albania, Mongolia, Vietnam and Yugoslavia.

Table 2.2: Selected Figures on the Deployment of Labour Market Policy in Germany (from 1991 to 2003)

	1991	1993	1995	1997	1999	2001	2003
				West Germany[1]			
Early Retirement Schemes[2]	122,408	129,030	148,148	131,053	112,186	139,328	281,319
Short-Time Work[2]	145,009	766,935	128,059	133,363	91,608	96,146	160,496
Vocational Training Progr.[3]	593,904	338,211	391,552	266,193	307,479	261,199	153,975
Training Courses[3]	–	–	–	74,684	264,811	338,516	694,322
Job Creation Schemes[3]	108,983	62,783	87,548	74,041	85,003	61,890	31,495
Struct. Adjustment Schemes[3]	–	–	4,335	6,859	11,183	11,466	6,970
				East Germany[1]			
Early Retirement Schemes[2]	555,000	852,000	376,884	137,586	89,077	85,658	145,204
Short-Time Work[2]	1,616,224	181,428	70,521	49,490	27,039	26,729	34,876
Vocational Training Progr.[3]	892,145	294,153	237,103	155,448	183,317	188,423	92,270
Training Courses[3]	–	–	–	28,500	166,745	226,616	375,815
Job Creation Schemes[3]	422,349	243,094	222,488	141,865	210,496	130,147	109,398
Struct. Adjustment Schemes[3]	–	70,337	57,264	49,786	45,836	42,581	32,279

[1] West Germany refers to federal states that constituted the former Federal Republic of Germany (including West Berlin until 2001). East Germany refers to the federal states that constituted the former German Democratic Republic (including East Berlin until 2001, and Berlin for 2003).
[2] In yearly averages.
[3] In entries.
Sources: Bundesanstalt für Arbeit (1993; 1996; 2001), Bundesagentur für Arbeit (2004).

were in agriculture, manufacturing, and goods-producing crafts (West Germany: 37.0 percent). Therefore, severe structural problems had to be expected for the transition to a market economy.

The second factor, which fortified the problems, was the 1:1 conversion of wages, salaries and pensions that led to a heavy burden for the competitiveness of the Eastern German economy. One consequence was the triplication of the export prices for goods. This increase in price resulted in a loss of the main sales markets. In addition, the liquidation of the former economic structure[3] caused the losing of the domestic trade channels. As wages were set well above the full-employment, market-clearing level by collective bargain after conversion in the East, the advent of free trade placed the majority of firms in a severe price-cost squeeze (Akerlof, Rose, Yellen, and Hessenius, 1991). The wage settlements were not related to the economic conditions and productivity developments, but were simply set to catch up the pre-specified target of reaching parity with West Germany in 1994. Two main arguments were used for this policy. On the one hand, employment losses were viewed as inevitable and not related to wages at all, and on the other hand, East Germans would have migrated to the West on large scale and congest the already crowded labour and housing mar-

[3] 316 Kombinate have been transformed into 8,000 legally independent firms by law (Siebert, 1991).

ket (Franz and Steiner, 2000). However, Akerlof et al. found that only few workers would have migrated to the West for higher wages, but for the lack of Eastern jobs.

The third factor was the sharp drop in demand for Eastern German products. East German consumers and firms diverted their spending away from East German consumption and investment goods towards previously unavailable Western goods on a large scale. For that reason, in particular the industrial sector in East Germany suffered from this development. Industrial production declined to one third on a quantitative basis and to one fifth on a value basis (Bundesanstalt für Arbeit, 2001). Besides, the primary sector experienced a strong employment reduction as well. An exception was the construction business; due to a strong backlog in demand on the one hand and a massive subvention on the other hand, this sector expanded.

Table 2.3: Development of the Determinants for the Labour Force Potential (balances in million persons)

	1990 to 1993		1994 to 1997		1998 to 2000	
	West	**East**	**West**	**East**	**West**	**East**
Migration[1]	+0.50	-0.50	+0.10	-0.10	+0.05	-0.05
Commuters[2]	+0.33	-0.33	+0.36	-0.36	+0.42	-0.42
Immigration[3]	+0.60	n.r.	+0.40	n.r.	+0.08	n.r.
German Resettlers[4]	+0.50	n.r.	+0.40	n.r.	+0.14	+0.08
Demographic Development and Propensity to Work	+0.10	n.r.	+0.00	-0.20	+0.40	-0.25

n.r. = not relevant

[1] Migration refers to the balance of East-West and West-East migration.

[2] Commuters: Commuters' balance between East and West Germany.

[3] Immigration refers to the number of immigrants from abroad. As asylum seekers do not receive an employment permission since 1997, the numbers reduced significantly.

[4] German resettlers: With the opening of the borders in 1989/1990, resettlers from the Commonwealth of Independent States (CIS) were allowed to return to Germany.

Source: Bundesanstalt für Arbeit (2001), own view.

The changed situation affected the population in East Germany severely. To cushion the negative impacts of the German Unification (GDP growth in East Germany in 1991: -19.2 percent) and to preserve social peace, labour market policy programmes were implemented on a large scale. As becomes obvious from the figures of Table 2.2, especially early retirement schemes and short-time work were used to reduce open unemployment.[4] Thus, the stock of short-time workers amounted to more than 1.6 million persons in 1991, but was reduced rapidly (1993: 181,428). The number of persons entitled to early retirement measures reached its peak in 1993 with about 852,000. Further important programmes were full-time vocational training programmes with 892,145 entries in 1991 and job creation schemes with 422,349.

[4] The purpose of short-time work compensation is to avoid lay-offs due to temporary, unanticipated reductions in firms' labour demand. Until 1992, short-time work compensations were also paid if working hours were reduced to zero and even if it was clear that the reduction in labour demand was permanent (Wunsch, 2005).

Another aspect which was conducive to a relaxation of the tense situation of the labour market was the East-West migration in association with a large number of commuters. Particulary during the first years after German Unification, this migration reduced the labour force potential by 0.5 million in East Germany. The reduction was reinforced by the large number of commuters to West Germany that amounted to about 330,000 in 1993. The massive deployment of labour market policies together with the strong migration resulted in an open unemployment of about 913,000 (1991) to 1.15 million (1993). However, the hidden unemployment in East Germany amounted to 1.81 million in 1991 and 1.57 million in 1993 (Table 2.1). For that reason, the majority of East German workers experienced unemployment or labour market programmes. Bielenski, Brinkmann, and Kohler (1997) note that about three quarters of the East German labour force have been in a labour market programme between November 1989 and November 1994 at least once.

During the years 1994 to 1995, the labour market stabilised and recovered slightly. Since the East German economy had a weak export dependence only, there were no strong aftereffects of the global recession of the early 1990s determined by the collapse of the Warsaw Pact states and the oil-price shocks during and after the first Gulf War. During this period of stabilisation, employment increased up to 6.53 million in 1995. Reasons for this development were the ongoing expansion of the construction sector as well as an enlargement of the services sector. However, the industrial and public sector were still characterised by a continuing employment reduction. Due to a decrease in East-West migration compared to the first years after German Unification (0.1 million, Table 2.3), the labour force potential changed only slightly. Hence, the temporary release of the tense labour market situation is indicated by the reduced number of labour market programmes. Above all (see Table 2.2), the number of persons in short-time work (from 181,428 in 1993 to 70,521 in 1995), persons placed in early retirement (852,000 in 1993; 376,884 in 1995) and also the number of vocational training programmes (294,153 in 1993; 237,103 in 1995) decreased.

Due to the strong subvention to the construction sector in the early 1990s, the omission of these subventions resulted in a shrinkage of this oversized sector from 1996/97 onward. In consequence, employment decreased in the following years to 6.44 million in 1999. Although open unemployment increased (1999: 1.34 million), hidden unemployment remained constant at a level of about 0.9 million persons (Table 2.1). However, the structure of hidden unemployment differed compared to the past. Whereas the early retirement schemes were on the lowest level since German Unification (89,077 in 1999, Table 2.2), ALMP programmes experienced a particular emphasis (apart from short-time work). The most important programmes were vocational training programmes and training courses as well as public employment programmes, i.e. job creation schemes and structural adjustment schemes (see Table 2.2). Another reason for rising unemployment rates was the strong decrease of the labour force potential mainly due to the demographic development (apart from commuters and resettlers: -0.3 million between 1998 and 2000).

For the description of the development of the labour market after 2000 I have to rely on figures using the re-definition of the regions according to the geographic sit-

uation (figures in Tables 2.1 and 2.2 for the years 2001 and 2003). This re-definition accounts the former West Berlin (2.08 million residents, 2001) to East Germany. Therefore, the figures are not directly comparable and the only fact that could be established is a continuing decrease of hidden unemployment.

In summary, the development of the East German labour market since German Unification shows a mixed picture. On the one hand, there are positive aspects of the restructuring of the East German economy that should be mentioned. About 0.5 million new companies have been established with about 3.0 million jobs which are in line with the market (Bundesanstalt für Arbeit, 2001). In addition, the efficiency of labour increased significantly from 32.9 percent in 1991 to 69.1 percent in 2001 of the West German level. Due to that the strongest East German regions are comparable to the weakest West German ones, but there is still a large productivity gap between both parts. On the other hand, there are a number of apparent deficits as well. First, the relation of the gross-wages per employer in 2001 amounted to 81.2 percent of the West German level and was clearly above the productivity. Hence, there is still no self-contained economic basis in East Germany, and quick convergence to the western level is not expected. Second, the development of the number of long-term unemployed people shows that unemployment has become steadily more persistent. In 2003, 674,000 persons were long-term unemployed (Table 2.1). In relation to the number of 1.6 million unemployed people, this is a ratio of more than 40 percent. Finally, the difference in the labour force participation rate has to be mentioned: In contrast to the overall rate which is fairly equal in both parts (60 percent in East Germany, 61 percent in West Germany), about 72.2 percent of the East German women compared to only 62.1 percent of the West German women are willing to work (Bundesanstalt für Arbeit, 2001). The reader should bear this difference in mind in the empirical analyses below.

2.2.2 Development of the West German Labour Market

After having discussed the development of the East German economy and labour market since German Unification, I will review the West German analogue. Whereas East Germany experienced an economic slowdown and a massive employment reduction during the first years, the West German economy boomed (GDP growth in 1991: +5.0 percent, Table 2.1). This upswing was accompanied by an increase in employment in all sectors (except the primary one). The main reason for this development was the strong demand for consumption and industrial goods from East Germany, financed by massive West-East transfers which amounted to 200 billion Deutschmarks on an annual basis (Bundesanstalt für Arbeit, 2001). Although the labour force potential increased by about two million people during the years 1989 and 1993, mainly due to the strong East-West migration, the high level of commuters, but also due to immigration from abroad and the German resettlers from Commonwealth of Independent States (see Table 2.3), open unemployment decreased from 2.04 million (1989) to 1.69 million (1991). Open unemployment was higher in absolute numbers than in East Germany at that time, but hidden unemployment was clearly lower (786,000 in 1991). Thus, labour market policies were used more

sparsely than in the East. However, the rest of the world was affected by a strong recession during those years.

The aftereffects of this global recession reached the export-dependent West German economy in the years between 1993 (GDP growth: -2.6 percent) and 1997 when the demand for consumer durables from East Germany diminished. Moreover, the lower demand for German products from abroad together with an increase in price of the currency lead to new restrictions in monetary, budgetary, and foreign trade dependent policy. As a consequence, investments and economic growth in the following years were unsatisfactory – the end was a structural crisis that exceedingly affected the industrial sector. Thus, employment decreased from 31.12 million in 1993 to 30.81 million in 1997 while unemployment increased from 2.27 million to 3.02 million, which equals an unemployment rate of 9.8 percent. A further indicator for the crisis is the use of labour market policies. Whereas during the boom in 1991, about 145,000 persons were on short-time work; this figure amounted to almost 767,000 in 1993 (see Table 2.2). The number of job creation schemes supports this picture. As policy decisions for the use of ALMP programmes were highly centralised during those years, the effect of the boom as well as the recession were reflected in the number of programmes with a time lag. Whereas in 1991, the number of participants in job creation schemes amounted to 108,983, only 62,783 individuals were newly promoted in 1993. In 1995, the number increased again with 87,548 persons employed. A further consequence of the recession resulted in stricter rules for immigration.

A new temporary economic upswing characterises the end of the 1990s (1998 to 2001). The reasons for the bettering of the economic situation were a growing foreign and domestic demand as well as the boom of the New Economy. In particular, the services sector benefited from this development. Therefore, GDP growth exceeded 2 percent in 1999. Unemployment reduced to 2.32 million in 2001 (unemployment rate: 8.0 percent). With the introduction of SGB III as the legal basis for labour market policy in 1998, a stronger emphasis on active compared to passive labour market policies was postulated. The effects of this change become obvious by the figures of Table 2.2. In 1999, more individuals participated in active measures (vocational training programmes, training courses, job creation and structural adjustment schemes) compared to 1997, whereas the number of passive measures (early retirement schemes, short-time work) decreased. The effect of the stricter immigration rules was a clear decrease of immigrants (about 80,000 between 1998 and 2000).

However, economic growth decelerated following the collapse of the dot-com bubble and the slowdown of the world economy after September 11, 2001. The consequence was a new rise in unemployment to about 9.3 percent in 2003. To summarise the development, it has to be argued that the German economy suffers from the enormous costs of its high and persistent unemployment, which limits Germany's full participation in the recovery of the world economy as well (Wunsch, 2005).

2.3 Labour Market Policy in Germany and the Institutional Set-Up of Job Creation Schemes

2.3.1 Labour Market Policy in Germany

Labour market policy has a long tradition in Germany. Unemployment insurance (UI) was established in 1927 by the Job Placement and Unemployment Insurance Act (*Gesetz über die Arbeitsvermittlung und Arbeitslosenversicherung*). It is one of the main pillars of the German social insurance system besides health insurance, accident insurance, pension insurance, and the compulsory long-term care insurance. The legal basis for labour market policy has been reformed twice since that time, in 1969 with the introduction of the Work Support Act (AFG, *Arbeitsförderungsgesetz*), and in 1998 with the adoption of SGB III, the current legal basis. The most important innovation of the AFG was the introduction of ALMP programmes besides the pure provision of 'passive' income support during unemployment.

However, the AFG was adopted in a period of almost full employment. Due to the persistently high and rising unemployment, the law became more and more inadequate to achieve its main purposes (Lampert, 1989), even though is was amended repeatedly.[5] In particular, after German Unification and the adoption of the AFG to Eastern Germany, the set-up of labour market policy was not capable anymore to reach the main purposes, namely the achievement of a high level of employment, the enhancement of the employment structure, and the promotion of economic growth (§1 AFG). Hence, a reform of labour market policy was necessary. Fertig and Schmidt (2000) argue that one reason for the divergence between policy instruments and needs of the labour market was a high degree of centralisation. The overall budget for ALMP programmes allocated to the local employment agencies (LEAs) as well as the budget shares received by individual measures of employment promotion were determined by the central advisory board of the FEA. Cross-subsidisation between policy measures was impossible. Thus, the system was highly inflexible to be adjusted to the heterogeneous circumstances in the labour office districts. A further reason was the concern of the legislator that the widespread belief in ALMP programmes as a way to create many new jobs was quite unrealistic, but that, quite the contrary, there was the possibility of endangering existing jobs by those measures.

Therefore, SGB III as the new legal basis for labour market policy in Germany was enacted in 1998.[6] In contrast to the macroeconomic goals of the AFG, the law focusses on job-seekers who are unemployed or threatened by unemployment. The main emphasis lies on the prevention or reduction of unemployment or payment of income support during unemployment (§1 SGB III). To prevent the problems of the AFG, priority is given to job placement compared to other active and passive labour

[5] Sell (1998) notes 115 amendments.

[6] Sell (1998), Fitzenberger and Speckesser (2000) and Fertig and Schmidt (2000) discuss the relevant reforms of labour market policy and the consequences. Brinkmann (1999) deals with the introduction of decentralisation and regionalisation as well as the mandatory output evaluation of labour market policy. A more recent and comprehensive overview is given by Wunsch (2005).

market policy (§4 SBG III). A further important innovation is the substantial amount of self-responsibility of the employers and employees in creation of new jobs as well as in maintaining existing ones with tightened requirements for the acceptability of jobs (§2 SGB III). Regarding the organisation of labour market policy, a further innovation is a higher degree of decentralisation associated with a better flexibility of support. The responsibility for the implementation of labour market policy is delegated to the LEAs that directly determine the amount of money spent for each ALMP programme. Instruments should be efficiently used to improve the re-employment chances of priority groups of the labour market, e.g., long-term unemployed persons, disabled people, individuals who lack a professional training (§7 SGB III). Furthermore, the LEAs are allowed to allocate up to ten percent of their budget for innovative measures not defined in SGB III (§10 SGB III, *Freie Förderung*). In addition, the FEA defines general principles to be adhered and provides guidance to the local decision makers.

The catalogue of ALMP instruments maintained almost the same, but was partly modified and supplemented by new measures. The literature provides different classifications to categorise the set of instruments. For example, the OECD distinguishes five general types of ALMP programmes[7]: (1) Public employment services and administration, (2) labour market training, (3) youth measures, (4) subsidised employment, and (5) measures for the disabled. Fertig and Schmidt (2000) distinguish four classes of ALMP instruments: (1) Monetary and non-monetary assistance for finding jobs, (2) human capital formation, (3) incentives for employers and self-employed, and (4) active measures promoting the creation of jobs. Similar, but not congruent to Fitzenberger and Speckesser (2000) and Fitzenberger and Hujer (2002), I will use three complexes of ALMP in Germany and describe the main instruments in the following:

1. *Measures to Enhance and Adjust the Qualification of the Individuals:*
   The complex contains different measures aiming at human capital formation of unemployed individuals and those threatened by unemployment. One instrument are training courses (§§48-52 SGB III, *Trainingsmaßnahmen*) that consist of three different types. The first type are courses that are used to examine the ability of the unemployed for specific jobs lasting four weeks. Furthermore, two-week courses to improve the ability of the unemployed to apply for jobs are provided as well as eight-week courses that teach specific skills necessary for employment, e.g., computer courses. Another instrument is the so-called basic vocational training (§§59-76 SGB III, *Förderung der Berufsausbildung*). Financial support for a regular vocational training in the apprenticeship system could be granted covering course costs and a maintenance allowance if the individual lacks professional qualification. In addition, further vocational training (§§77-96, 153-159, 417 SGB, *Förderung der beruflichen Weiterbildung*) has the purpose to adjust human capital to the changed demands of the labour market. The assistance of the FEA is similar to that for basic vocational training, but promoted individuals have to own a professional qualification prior to the pro-

[7] See Martin and Grubb (2001).

grammes and meet tighter eligibility rules. Moreover, German language courses[8] (§419 SGB III) as well as granting of the service providers for vocational training (§§248-252) are provided by the FEA.

2. *Subsidised Employment:*
   The second category of instruments could be differentiated into wage subsidies and employment programmes. Wage subsidies are offered to employers to hire unemployed persons with a reduced productivity. The idea is to give an incentive to employers by (partial) compensation for the reduced work capacity. Wage subsidies consist of integration subsidies (§§217-224 SGB III, *Eingliederungszuschüsse*), integration contracts (§§229-334 SGB III, *Eingliederungsvertrag*), bridging allowances (§§57-58 SGB III, *Überbrückungsgeld*), and recruitment subsidies (§§225-228 SGB III, *Eingliederungszuschüsse bei Neugründungen*).

   There are two types of employment programmes defined by SGB III: Job creation schemes (§§260-271, 416 SGB III, *Arbeitsbeschaffungsmaßnahmen*) and structural adjustment schemes (§§272-279, 415 SGB III, *Strukturanpassungsmaßnahmen*). The programmes aim at preserving the human capital of the unemployed and of the long-term unemployed persons in particular by offering occupations mainly in the public and non-profit sector. Further intentions are the relief of the labour market in regions with strong structural deficits and the maintenance of social peace. Job creation schemes and structural adjustment schemes found the so-called second labour market in Germany as the jobs are not allowed to compete with regular employment to avoid substitution effects and deadweight losses. I will discuss the institutional set-up and possible effects of job creation schemes in more detail below.
3. *Counselling and Assistance for Regional and Vocational Mobility:*
   The main programmes of this third complex of ALMP in Germany are counselling and placement assistance (§§29-44 SGB III, *Beratung und Vermittlung*) as well as mobility benefits (§§53-56 SGB III, *Mobilitätshilfen*). Counselling should be offered to any unemployed or threatened-by-unemployed person by providing information and advice that cover aspects like the individual's career options and employment prospects, the actual labour market situation and the availability of ALMP programmes. Mobility benefits should alleviate the take-up of employment in a different region by providing, among others, interest-free loans and assistance for travelling expenses.

This classification of instruments has to be completed by special measures for the disabled like vocational rehabilitation training and related measures to make the disabled employable as well as by several smaller measures that were mainly accomplished on a regional level. The most noteworthy measures have been the following. A programme for unemployed aged 25 or younger is the so-called JUMP (*Jugend mit Perspektive*). It combines a set of different aspects from several ALMP programmes like further vocational training, promotion of apprenticeship, intensified counselling, social assistance, wage subsidies as well as job creation schemes to qualify the un-

[8] German language courses were supported until the end of 2004 for German re-settlers and immigrants.

employed youth (Dietrich, 2001). Another programme intended to improve the incentives for low-qualified and long-term unemployed to take up employment was the so-called CAST (*Chancen und Anreize zur Aufnahme sozialversicherungspflichtiger Tätigkeiten*).[9] The idea was to subsidise the payment to social security of low-income earners who were unemployed or employed in marginal employment before. The programme was accomplished in two prototypical attempts of which one also proposed the opportunity of further qualification (Kaltenborn, 2001; Hollederer, Kaltenborn, and Rudolph, 2001). The so-called *Einstiegsgeld für Langzeitarbeitslose* was undertaken during 1999 and 2002 and allowed unemployed on social assistance to earn a higher share for their living to habituate the participating individuals to regular employment (Spermann, 2003). A last programme worth to mention is MozArT (*Modellvorhaben zur Verbesserung der Zusammenarbeit von Arbeitsämtern und Trägern der Sozialhilfe*). It was a prototype programme during the years 2001 and 2002/2004 to combine unemployment and social assistance by a joint administration and provision of qualification and employment programmes for both groups (Bundesministerium für Wirtschaft und Arbeit, 2001).

However, in spite of the new legislation the situation on the labour market did not advance. For that reason, two substantial amendments of the law have been enacted since 1998. In 2002, the *Job-AQTIV Gesetz* ('activate, qualify, train, invest, place') was adopted. The main emphasis was a change from active to activation measures. In addition, job search monitoring and placement were intensified, jobseekers were classified by a qualitative profiling, and labour market policy was made more flexible and preemptive. The second amendment are four laws called 'Modern Services on the Labour Market' (*Moderne Dienstleistungen am Arbeitsmarkt*). The first two laws provide foundations for faster and lasting (re-)integration of jobseekers into employment as well as new opportunities for temporary work, small jobs, self-employment and employment in private households (Wunsch, 2005). The third law rules the restructuring of the FEA (from an administration of unemployed to an agency for customers). The fourth law establishes a common basis for all jobseekers without unemployment benefit claims, pooling the former unemployment assistance (*Arbeitslosenhilfe*) and social assistance (*Sozialhilfe*) into the so-called unemployment benefits II (*Arbeitslosengeld II*).[10]

Besides the postulations of the legislator for the design of the labour market policy, assessment of the efforts for ALMP has become mandatory with the reform of the legal basis in 1998. The tight budgetary situation of the FEA and doubts about the efficiency of programmes are the main reasons for that. The induced necessity of monitoring the success of the programmes is arranged in two directions. On the one hand, each LEA has to publish the so-called *Eingliederungsbilanzen*[11] (§11 SGB III) that contain, inter alia, information on the number of participants who have left unem-

[9] CAST was finished in 2003 with the adoption of the second law of Modern Services on the Labour Market.

[10] These changes do not affect the empirical analyses as they are based on data on programmes that have started during 2000 and 2001, and the observation period has ended in 2003.

[11] Brinkmann (1999) translates *Eingliederungsbilanzen* as output evaluations, Fertig and Schmidt (2000) use the term balance sheets.

ployment or became regular employed six months after the end of the programmes. These outcome-based measures should provide a quick feedback to caseworkers and programme managers on the efficiency of programmes and help for a better control of ALMP. On the other hand, the legislator postulates the evaluation of the impacts for participants in terms of individual employability and integration chances into regular employment (§282 SGB III). The major drawback of the outcome-based measures is the lack of information on the individual utility of the programmes. They provide no information on how individuals would have performed without the programme. Therefore, measuring the performance of ALMP programmes using impact evaluations is necessary as it implies a great scientific and political value on how the programmes affect the employability of the participants.

Table 2.4: Absolute and Relative Spending of the FEA on Labour Market Policy in Germany (2000 to 2003)

	2000	**2001**	**2002**	**2003**	**2000**	**2001**	**2002**	**2003**
	Absolute Spending (bn Euro)				Relative to LMP (percent)			
				Passive Labour Market Policy				
Germany	37.80	38.77	44.12	47.34	63.20	63.46	66.33	69.07
West	24.09	24.91	28.13	31.58	66.33	66.73	69.84	72.00
East	13.71	13.86	16.00	15.76	58.39	58.36	60.95	63.86
				Active Labour Market Policy				
Germany	22.01	22.32	22.40	21.20	36.80	36.54	33.67	30.93
West	12.23	12.42	12.15	12.28	33.67	33.27	30.16	28.00
East	9.77	9.89	10.25	8.92	41.61	41.64	39.05	36.14
				Labour Market Policy				
Germany	59.81	61.09	66.52	68.54				
West	36.32	37.33	40.28	43.86				
East	23.48	23.75	26.25	24.68				
	Selected Programmes of Active Labour Market Policy							
	Absolute Spending (bn Euro)				Relative to ALMP (percent)			
				Job Creation Schemes				
Germany	3.67	2.98	2.33	1.68	16.68	13.33	10.42	7.90
West	1.02	0.86	0.55	0.37	8.35	6.95	4.56	3.02
East	2.66	2.11	1.78	1.31	27.21	21.36	17.36	14.63
				Further Vocational Training				
Germany	6.81	6.98	6.70	5.00	30.93	31.28	29.92	23.59
West	4.06	4.19	3.82	3.03	33.20	33.71	31.46	24.64
East	2.75	2.80	2.88	1.87	28.12	28.26	28.09	21.02

Source: Bundesanstalt für Arbeit (2001), Bundesagentur für Arbeit (2004).

The higher emphasis of ALMP after enaction of SGB III is also reflected in a higher number of participants (see Table 2.2) and increased expenditures (Table 2.4). This becomes obvious by taking a closer look on vocational training programmes and job creation schemes. Whereas in 1997 the number of newly promoted individuals in vocational training programmes was 266,193 (155,558) in West (East) Germany, it

increased by 15.51 (17.93) percent in 1999 to 307,479 (183,317). A similar tendency could be observed for job creation schemes, where in the same time the number of participants increased by 14.81 percent to 85,003 in West Germany, and clearly stronger by 48.38 percent to 210,496 in East Germany.

The spending on labour market policy (Table 2.4) during the time of the analysis (2000 to 2003) mirrors the relevance of ALMP. It also reflects the problematic situation of the labour market. Although overall spending on ALMP increased from 2000 to 2001 in West Germany and from 2000 to 2002 in East Germany, its ratio on the total spending on labour market policy decreased. The reason for this development is the legal claim of workers who fulfil the eligibility criteria for the reception of unemployment benefits which cannot be rejected by the FEA. Therefore, a rising unemployment is highly correlated with a higher spending on passive labour market policy. Thus, whereas the ratio of ALMP on the overall spending on labour market policy amounted to 33.76 (41.61) percent in West (East) Germany in 2000, it is decreased to 28.00 (36.14) percent in 2003. In contrast to that, the total spending on labour market policy increased from 59.81 bn Euro in 2000 to 68.54 bn Euro in 2003 for Germany.

With respect to the spending on the two most important ALMP programmes in the late 1990s and early 2000s, there are some notable findings. In terms of spending, job creation schemes and vocational training programmes almost experienced the same relevance in Eastern Germany in 2000 as 27.21 percent (2.66 bn Euro) of the spending on ALMP funded for job creation schemes and 28.12 percent (2.75 bn Euro) for vocational training attest. However, the importance of job creation schemes has been less emphasised in comparison to vocational training programmes in West Germany. Here, only 8.35 percent of the spending on ALMP (1.02 bn Euro) were allocated to job creation schemes in 2000, whereas 33.20 percent (4.06 bn Euro) were distributed for vocational training. Two characteristics describe the development of the spending in the following years: on the one hand, a decreasing relevance of job creation schemes, on the other hand, a short accentuation of vocational training programmes with a subsequent decrease after 2002. Job creation schemes were strongly reduced in West Germany. Only 31,495 individuals have been newly promoted in 2003 with a corresponding spending of 0.37 bn Euro, i.e. 3.02 percent of the spending on ALMP in that region. Although the reduction in East Germany was significant as well, programmes are still quite relevant as 109,398 promoted individuals in 2003 and a spending of 1.31 bn Euro, i.e. 14.63 percent of the ALMP expenditures, demonstrate.

2.3.2 Institutional Set-Up of Job Creation Schemes

As already mentioned, job creation schemes are a kind of subsidised employment in Germany. In association with structural adjustment schemes they establish the so-called second labour market in Germany. The legal basis for job creation schemes is defined in §§260 to 271 and 416 SGB III. As my analysis is based on programmes that have started during 2000 and 2001, I will concentrate the description of the institutional set-up on this time span. However, as there have been some important

amendments in 2002 and 2004, I will note the most relevant changes in brief at the end of this sub-section.

Financial assistance for job creation schemes could be granted to the implementing institutions by the FEA as wage subsidies or loans if the jobs fulfil several requirements. The primary condition is that occupations provided in job creation schemes must be additional in nature, of value for society, and carried out by unemployed persons in need of assistance. Priority should be given to projects that explicitly aim at improving the pre-conditions for permanent employment, provide occupations for unemployed persons with special barriers to employment, or improve the social and environmental infrastructure (§260 SGB III). Additional in nature means that, without the subsidy, the activities would not be undertaken now or in the near future. Occupations are of value for society if the outcome of the work is for the collective good. Job creation schemes could also be supported if the participants take part in qualification programmes (up to 20 percent of the time) or internships (up to 40 percent, in sum no more than 50 percent) to improve their employability (§261 SGB III). According to §7 SGB III, unemployed persons facing barriers to employment are defined as long-term unemployed, severely disabled persons, older unemployed with placement restrictions, as well as applicants for vocational rehabilitation programmes.

Eligibility of potential participants is generally approved if they are long-term unemployed (for more than one year, §6 SGB III) or unemployed for at least six out of the last twelve months prior to programme start and fulfil the eligibility criteria for reception of unemployment benefits or assistance, for vocational training programmes, or for vocational integration of the disabled.[12] Independently of these eligibility criteria, the LEAs are allowed to place younger unemployed (aged 25 or younger) without completed professional training, severely disabled persons, tutors

[12] Unemployment benefits (*Arbeitslosengeld*, UB) are paid for individuals who have contributed for at least twelve months to unemployment insurance (UI) during the last three years before unemployment (seasonally employed workers have a reduced contribution period of six months). UB amount to 60 (67) percent of the last average net earnings from insured employment (with at least one dependent child) and are paid from UI funds. The entitlement lasts for at least six months. The maximum duration is up to 32 months and depends on the contribution period and the individual's age. Payment to the UI is compulsory for all employees and amounts to 6.5 percent of employee's gross salary. However, persons with only a minor employment, civil servants, judges, clergymen, professional soldiers, and some other groups of persons are exempted from contributions. Minor employment are jobs with a salary of less than 325 Euro (400 Euro since 04/2003) as well as short-term and occasional jobs. The set-up of unemployment assistance (*Arbeitslosenhilfe*, UA) was changed within the Fourth Law 'Modern Services on the Labour Market' on January 1st, 2005. Until that time, UA was paid for persons who had exhausted their UB entitlement. UA amounted to 53 (57) percent of the last average net earnings from insured employment (with at least one dependent child). UA could have been paid potentially unlimited (until retirement age) if the individual satisfied the benefit conditions. UA was administered by the FEA, but funding was by tax. Since 2005, UA are pooled with social assistance (*Sozialhilfe*) in the so-called unemployment benefits II (*Arbeitslosengeld II*).

and up to five percent of the participants who do not meet the general eligibility criteria in the programmes (§263 SGB III).

Subsidies amount to 30 up to 75 percent of the worker's salary, but can be extended to up to 90 percent if the allocated individual is in special need of assistance and if the implementing institution is not able to cover the costs.[13] A further exception can be made with a subsidy of 100 percent for priority programmes (§264 SGB III). Furthermore, the LEAs could provide additional subsidies and loans if the programmes have an emphasised priority for labour market policy (increased support) and if the funding was not accomplished otherwise. These additional grants are not allowed to exceed 30 percent of the total spending on the particular programme (§266 SGB III).

The ordinary duration of support for occupations in job creation schemes is twelve months. This duration can be extended to up to 24 months for programmes with increased priority, and to up to 36 months if the implementing institution guarantees a permanent contract for the individual afterwards (§267 SGB III). Programmes could also be supported in the commercial sector if delegated to private businesses. To prevent substitution effects and windfall gains, the number of promoted jobs in a region and an economic sector is limited. Job creation schemes in the commercial sector could be accomplished by public institutions only for the following reasons. The participants have to achieve special educational assistance or fulfil a qualification or internship of at least 20 percent of the total programme duration. Participants employed have to be younger than 25 years and lack a completed professional training or have to be older than 50 years. Financial assistance should only be granted in case of missing interest or insufficient capabilities of private businesses to accomplish the tasks (§262 SGB III).

The allocation of unemployed individuals to places in job creation schemes results from decisions of the responsible caseworkers. Once an unemployed person has registered at the LEA, the responsible caseworker takes up the case and meets the unemployed individual at regular intervals. In these meetings, the caseworker evaluates the individual's efforts for finding a job. Furthermore, he or she conceives a plan for the integration into employment in cooperation with the unemployed person. By this procedure, the responsible caseworker possesses a large degree of discretion with respect to the allocation of unemployed persons into programmes. The caseworker decides to offer a specific occupation in a job creation scheme solely if his assessment of the individual's need of assistance implies that the unemployed person cannot be integrated into regular employment and does not meet the conditions for other ALMP programmes. The occupations can be accomplished in different economic sectors of which the most important are Agriculture, Construction and Industry, Community Services, and Office and Services. The caseworker chooses the occupation in consultation with the unemployed person and according to the individual's qualification and interest. Once decreed by a caseworker, the programme is compulsory for the

[13] It may be worth noting that the average monthly costs per participant for the FEA have been 1,419 Euro in West Germany and 1,518 Euro in East Germany in the year 2001 (Bundesanstalt für Arbeit, 2002).

individual and rejection will be sanctioned by benefit cancellation for up to twelve weeks. In repeated cases, the unemployed individual may lose his/her UI entitlement completely (§269 SGB III).[14] Since placement depends on the availability of programmes, unemployed persons may not be assigned to programmes due to a limited supply.

In the following, I will note some aspects of the important reforms of law for job creation schemes in 2002 and 2004. As these reforms do not fall into the observation period (with respect to the programme start), I refrain from discussing the changes in detail. With the 2002 amendment of SGB III (Job-AQTIV), the eligibility criteria for job creation schemes have been relaxed. Since 2002, all unemployed individuals could be placed in a job creation scheme independently of the preceding unemployment duration. Furthermore, support for the implementing institutions could be granted as lump sum payments, according to the level of the individual's qualification. In addition to that, the delegation of programmes to private businesses has been simplified. Since 2002, financial support for programmes in the commercial sector requires the agreement of the local advisory board in the LEA only.

With the adoption of the Third Law 'Modern Services on the Labour Market' in 2004, the former structural adjustment schemes have been pooled with job creation schemes into one homogeneous instrument.[15] The purpose of the new job creation schemes is adjusted with respect to the experiences from the past. From 2004 onward, job creation schemes can be promoted only if they are used to reduce high unemployment according to regional and vocational problems in the labour market, support activities which are additional in nature, are of value for society, and are undertaken by unemployed persons in need of assistance. An amendment in the law serves the purpose that any damage of the economy due to programmes must be excluded prior to allowance of financial support. Payment for jobs is arranged by lump sum grants only according to the level of qualification. Therefore, the traditional aim of job creation schemes to enhance the qualification of the participants has been abandoned, and job creation schemes are mainly used as an instrument to relieve the labour market.

2.4 Experiences with Job Creation Schemes in East and West Germany from Previous Microeconometric Empirical Studies

Due the enormous use of ALMP measures in the transition process from a centrally planned to a market economy in Eastern Germany, the corresponding high expenditures raised the interest of the public and scientific community about efficiency and effectivity. Thus, researchers started to make various attempts to uncover the effects of German ALMP since the mid 1990s, using microeconometric methods to account for selectivity in the assignment process to programmes. For that reason, there is a

[14] See §144 SGB III for the definitions regarding the exposure of income support.

[15] See Caliendo and Hujer (2004) for a discussion of the reform of job creation and structural adjustment schemes.

considerable number of studies analysing the effects of job creation schemes. However, the lack of appropriate data hampered evaluation efforts for a long time. The only data available were survey data sets like the German Socio-Economic Panel (GSOEP) and the Labour Market Monitor (LMM) for Eastern Germany and the state of Saxony-Anhalt (LMM-SA). Since the GSOEP does not contain information on job creation schemes, all of the earlier studies focus on programme effects in East Germany based upon data from the LMM and the LMM-SA (except the study of Reinowski, Schultz, and Wiemers, 2003). The LMM is a panel data set based upon a representative 0.1 percent sample of the working-age population (persons between 16 and 64) in East Germany. The information is derived by a survey based on questionnaires in eight waves between November 1990 and November 1994 (Bielenski, Brinkmann, Magvas, and Parmentier, 1997). It contains information on individual and socioeconomic aspects as well as the (un)employment history based on retrospective information. As the sample of the LMM is too small to allow analyses with regional differentiation, the ministry of labour of the state of Saxony-Anhalt decided to survey regional information since 1992 (Bielenski, Brinkmann, Plicht, and von Rosenbladt, 1997). In 1999, the survey was conducted for the last time. Both the LMM and the LMM-SA have two important drawbacks: First, they do not allow to identify job creation schemes separately from other kinds of subsidised employment in the so-called second labour market. Second, the small sample sizes for participants in ALMP measures limit the scope for the analysis of sub-groups considerably.[16] Within the project 'Effects of Job Creation and Structural Adjustment Schemes'[17], the large and informative administrative data of the FEA have been made accessible for scientific purposes (see chapter 4 for a description of the data) which enables the evaluation of programmes for West and East Germany as well as the consideration of specific sub-groups.

In this section, I review the major findings of the previous microeconometric studies which analyse the effects of job creation schemes in East and West Germany for the participating individuals.[18] I start with the studies employing survey data to study the effects of the programmes. After that, I review the results of the studies based on administrative data.

[16] To give an idea of the sample sizes, Steiner and Kraus (1995), for example, use 582 participants and 2,179 comparison individuals for their analysis from LMM. Eichler and Lechner (2002) are able to base their study on 1,123 participants and 12,565 non-participants from LMM-SA.

[17] The German title of the project is *Eingliederungseffekte und weiterer Nutzen von ABM und SAM unter besonderer Berücksichtigung von 'SAM für Ältere'* (IAB project number 10-535).

[18] Since I analyse the microeconometric effects of job creation schemes only, I refrain from reviewing the evidence from the macroeconom(etr)ic literature. However, for the sake of completeness, studies that analyse, among others, macroeconomic effects of job creation schemes in Germany should be mentioned: Büttner and Prey (1998), Hagen and Steiner (2000), Schmid, Speckesser, and Hilbert (2001), Hagen (2003), Hujer, Blien, Caliendo, and Zeiss (2006), and Hujer and Zeiss (2005).

2.4.1 Studies Based on Survey Data

Steiner and Kraus (1995) analyse the effects of public employment programmes (PEP) on re-employment probabilities compared to non-participation in East Germany. PEP consist of job creation schemes and structural adjustment schemes without differentiation. Their analysis is based on the first six waves of the LMM from November 1990 to November 1992. The authors apply a discrete hazard rate model with multiple destinations and consider unobserved heterogeneity. The results of the estimation of the participation probability show a high probability for women who have a high probability to find an unsubsidised job anyway. Therefore, the authors presume that women apply for PEP as a transition state for a later leave from the labour market. The results of the programmes in terms of the re-employment probabilities establish a short-term re-integration success for men; for the other groups, programme participation seems to have no effect.

The effects of PEP in East Germany based on the LMM are also analysed by Hübler (1997) using different econometric methods (multinomial logit model, random-effects probit model with and without pre-programme test-based control group selection). Based on the eight waves of the LMM from November 1990 to November 1994, he analyses the programme effects on the employment status after training in 1994 as well as the probability of becoming employed in 1993 or 1994. Although the results vary with the different methods applied, the author states that PEP do not achieve the (expected) positive impacts.

The study of Kraus, Puhani, and Steiner (2000) extends the analysis of Steiner and Kraus (1995). The authors evaluate the effects of PEP in East Germany by a discrete hazard rate model using the eight waves of the LMM. The observation window is decomposed into two sub-periods, covering the time between January 1989 and August 1992 and between September 1992 and November 1994. Effects are estimated separately for men and women. They analyse two outcomes of interest: the probability of finding stable employment and the probability of becoming non-employed after participation. The results of the effectiveness of bringing people back into stable employment are not encouraging; participants in PEP are on average worse off in terms of individual re-employment prospects than people who do not join a programme. For that reason, the authors conclude that policy makers should reconsider the role of PEP as an ALMP tool.

Eichler (1997) analyses the effects of PEP in the East German state of Saxony-Anhalt based on the information of the LMM-SA. He estimates the employment probabilities of regular employment for participating individuals using probit models for the single cross-sections of 1993 and 1996. The results of the probit models show clear negative impacts of programme participation. Due to the small number of variables available, unobserved heterogeneity may affect the estimates. Therefore, the author applies a difference-in-differences estimator covering the time between German Unification and autumn 1996. The information used stems from the retrospective calendar surveyed in the wave of 1996. The results of this estimator establish significant positive effects for a participation in PEP in Saxony-Anhalt. However, the results vary considerably with different model specifications.

Based on the waves of 1997 and 1998 of the LMM-SA, Bergemann and Schultz (2000) are able to use the employment history of individuals from 1990 onward to analyse the effects of vocational training programmes and PEP. A special emphasis is spent on the possible occurrence of *Ashenfelter's Dip* (see Ashenfelter, 1978), i.e. potential participants anticipate the upcoming treatment by reducing their search efforts. For that reason, the authors apply a conditional difference-in-differences estimator, where participants and non-participants are partly comparable conditional on observable covariates (Heckman, Ichimura, Smith, and Todd, 1998). In the first step, participants and non-participants are matched based on observable characteristics. In a second step, a difference-in-differences estimator is used based on the matched set of participants and non-participants. The results show that *Ashenfelter's Dip* amounts to about 20 percent lower employment rates for participants immediately before programmes start. Although the employment rate of participants is on average significantly lower than for non-participants after programmes end, participants of PEP gain from participation about one and a half to 2 years after the end of the programmes.

Bergemann, Fitzenberger, Schultz, and Speckesser (2000) use the same data as Bergemann and Schultz (2000) and analyse the effects of PEP and vocational training but allow individuals to participate more than once. Therefore the authors extent the conditional difference-in-differences estimator to the context of multiple treatments. Due to this, the identification of causal treatment effects becomes more difficult for two reasons. First, the identification of separate treatment effects for the single programmes is not straightforward. Second, problems may arise as a second treatment may be endogenous to a certain extent because the incentive structure of a programme could motivate further treatment. Programme effects are evaluated in terms of the employment rates. The authors estimate the effects for different time periods (programmes starting at the end of 1990, at the end of 1992, and at the end of 1994). They find negative impacts for the first programme participation until 3 years after the programmes have ended for persons starting at the end of 1990. The results for programmes starting at the end of 1992 and 1994 are significantly negative shortly after treatment, but become zero until three years after the start of the programmes. The results of a second participation are insignificantly different from zero.

The study of Eichler and Lechner (2002) extents the preliminary results of Eichler (1997) using data of the LMM-SA from 1992 to 1997. They use a conditional difference-in-differences estimator based on a matched sample to estimate the probability of becoming unemployed, employed, and of leaving the labour force. The results show a significant reduction of unemployment for participants. For male participants, this is due to an increased employment probability, for female participants due to a higher propensity to leave the labour market.

Based on two census surveys for the free state of Saxony from 2000 and 2001, Reinowski et al. (2003) analyse the effects of PEP for long-term unemployed participants on the probability of leaving unemployment. The retrospective calendar enables the authors to reproduce the individual employment histories until 1989 for a representative 0.5 percent sample of all Saxon households. Programme effects are

evaluated using a two-step procedure. In a first step, participants and comparable non-participants are matched. In a second step, the individual risk of leaving unemployment is estimated using a proportional hazards model. The results establish negative programme effects, i.e. participation prolongs the average duration of unemployment.

Bergemann, Fitzenberger, and Speckesser (2004)[19] use information of the surveys from the LMM-SA of the years 1997 to 1999 with the retrospective information until 1990 to evaluate the effects of public-sector-sponsored training possibly followed by a PEP. They apply an extension of the conditional difference-in-differences estimator in hazard rates (cDiDiHR) based on a matched sample to analyse the effects on the transition rates to employment. In the context of vocational training followed by a PEP they distinguish two possible sequences. In a first approach, they estimate the combined effect of the sequence of both treatments as a straightforward application of the single binary treatment case. In a second approach, they estimate the incremental effect of the sequence using a heuristic two-step procedure. Based on the timing of treatment, the effect is estimated using the outcome before and after the second treatment by the conditional difference-in-differences estimator in hazard rates, treating previous participation (the vocational training programme) as non-employment. However, the results do not show any significant effects on the transition rates to employment.[20]

Based on the same data, Bergemann (2005) evaluates the effects of PEP on the re-employment probabilities and the probability of remaining employed using the cDiDiHR estimator. Bergemann analyses the effects for men and women separately and distinguishes between five points of programme starts (December of the years 1990, 1992, 1994, 1996 and 1997). The results at the end of the periods indicate insignificant effects on the re-employment probability for women except for the group that starts the programme in December 1997. For male participants the re-employment probabilities do not differ from zero either. The results for the probability to remain employed show a slightly different picture. Two of the female groups have a positive probability, for the other three there are no significant differences between treated and non-treated women. Men who have started a programme in December 1992 or December 1997 have a higher probability to remain employed compared to the non-treated. However, men who have started at the other dates considered do not experience an increased probability to remain employed.

In summary, the evidence of the studies based on survey data is mixed. However, as the results of any programme evaluation depend on several aspects, like the data in use, the underlying model, the definition of the outcome of interest, this variety of aspects must be considered when comparing the results of the different studies. Therefore, possible reasons for the lack of robustness of the estimates may be the

[19] A preliminary version of this paper circulated as Bergemann, Fitzenberger, and Speckesser (2001).

[20] The authors find positive effects on the probability to remain employed after participation in a PEP following a participation in a vocational training.

sensitivity of the results to the different parametric assumptions, the small sample sizes, and the inability of the studies to measure long-run effects.

2.4.2 Studies Based on Administrative Data of the FEA

Based on information derived from administrative sources of the FEA (see chapter 4), Caliendo, Hujer, and Thomsen (2003; 2004; 2006a; 2006c) and Hujer, Caliendo, and Thomsen (2004) evaluate the effects of job creation schemes for Germany using matching estimators.[21] All studies are based on the population of participants in job creation schemes starting their programmes in February 2000 and a sample of eligible non-participants from the population of unemployed persons in January 2000. For that reason, the original participants' sample contains more than 12,000 individuals, the non-participants' sample more than 260,000 persons. The large number of observations, in association with the very rich and informative set of characteristics in the data set, enables the analysis of job creation schemes considering different sources of effect heterogeneity, like the individual diversity of the participants, the programme design, the types of providers and support, and the target-oriented allocation of individuals to programmes.

Common to all studies is the purpose to evaluate whether job creation schemes help unemployed persons to find regular (unsubsidised) employment. However, the two earlier studies (Caliendo et al., 2003; Hujer, Caliendo, and Thomsen, 2004) have to deal with an unsatisfying outcome variable that does not cover regular employment. To approximate the employment effects, the individual effects in terms of unemployed or job-seeking were estimated using two possible outcome scenarios. Unfortunately, this information only provides an imperfect approximation of the employment effects. Furthermore, the observation window is limited by March 2002, i.e. about 2 years after the start of programmes. With an average duration of job creation schemes of one year, programme effects are estimated for the majority of participants for a period of one year after programmes have ended. In the later studies, Caliendo et al. (2004; 2006a; 2006c) are able to use the information of the Employment Statistics Register on regular employment and estimate the effects until December 2002, i.e. for almost 3 years after the programmes have started. Although the results vary with respect to the different sources of effect heterogeneity considered as well as with respect to the different outcome variables, one finding is stable throughout all studies: Job creation schemes are associated with strong locking-in effects during the first months of the programmes. Hence, participants are worse off in terms of search for regular employment.

Caliendo, Hujer, and Thomsen (2003) analyse the effects of job creation schemes with respect to regional and individual differences on the rate of leaving unemployment and registered job-search. Regional differences are considered by separate estimation of treatment effects for East and West Germany as well as for Berlin. Separate evaluation of the effects of the capital city is necessary to take account of the special situation of the labour market. Moreover, effect heterogeneity resulting from

[21] See Brinkmann, Caliendo, Hujer, and Thomsen (2006) for a summary of the results.

individual characteristics is considered by analysing three different age and unemployment groups. As local labour market information is an important determinant of treatment effects, regional differences are considered by using the classification of Hirschenauer (1999).[22] The results – based on nearest-neighbour matching with replacement – show clear negative results for the East German groups, and the best results are insignificantly different from zero for some West German groups at the end of the observation period in March 2002.

Hujer, Caliendo, and Thomsen (2004) provide an extension of the preliminary results of the former study by additionally considering sectoral heterogeneity of the programmes. The outcome variables used are the same as in Caliendo et al. (2003). Sectoral heterogeneity of the programmes is due to the different economic sectors job creation schemes are accomplished in. The analysis focusses on the main four sectors (Agriculture, Construction and Industry, Office and Services, Community Services); the remaining sectors are summarised in another category (Other). Effects are estimated separately by gender and region based on a nearest-neighbour matching without replacement. The labour market of Berlin is excluded from analysis as the number of participants is too small for considerable evaluation of the programme impacts in the sub-groups. Instead of using the classification of Hirschenauer (1999) for the labour offices, the underemployment quota and the size of the labour office district are included as regional context variables. Furthermore, Hujer, Caliendo, and Thomsen conduct a sensitivity analysis to test the robustness of the estimates against unobserved heterogeneity. The results in March 2002 cannot establish significant positive effects in terms of leaving unemployment or registered job-search for any of the groups in analysis.

Caliendo et al. (2004) is the first study for Germany which is able to explicitly evaluate the effects of job creation schemes on regular (unsubsidised) employment for participating individuals. Information on regular employment is also available in the survey data sets for Eastern Germany. However, job creation schemes are not clearly identified as a single programme, but are part of the PEP recorded in those data. In this study, regional heterogeneity is analysed in more detail using the classification of similar and comparable labour office districts following Blien et al. (2004). The aspects of individual heterogeneity are considered by a selection of specific problem groups of the labour market. These groups are persons without professional training (with a further differentiation by age), persons with health restrictions, barriers to employment, vocational rehabilitation attendants, and persons with college or university degree. All effects are estimated separately by gender and region or rather by regional cluster. The results in December 2002 show significant positive effects on the employment rates for women and insignificant effects for men in West Germany; the employment chances of participants in East Germany are harmed by the programme as negative effects on the employment rates indicate. The results of the regional clusters establish positive effects for women in West Germany in office districts with advantageous labour market prospects. Furthermore, the analysis of the group-specific effects provides positive programme impacts for persons with health

[22] See, e.g., Heckman, LaLonde, and Smith (1999).

restrictions and for persons with barriers to employment at the end of the observation period (December 2002).

As the results of any evaluation depend on the underlying model and the chosen evaluation method, Caliendo et al. (2006a) consider these aspects in the evaluation of job creation schemes. They test and report several specifications for the model of the propensity scores and estimate the programme impacts in terms of employment rates for participants in East and West Germany with respect to gender using different matching algorithms.[23] These algorithms comprise nearest-neighbour matching estimators with and without replacement and with additional calliper levels of 0.01, 0.02, and 0.05. Furthermore, matching estimators with 2, 5 and 10 times oversampling are used. The results imply that, due to the rich data in use, the estimates are insensitive with respect to the varying algorithms. Furthermore, the treatment effects for selected sub-groups of the labour market are analysed, augmenting the selection of the former paper by different unemployment durations and applying a nearest-neighbour matching estimator with a calliper of 0.02. The results show positive effects in December 2002 for long-term unemployed men and women in West as well as for women in East Germany. Further groups that benefit from participation are higher qualified men and older women in West Germany. However, men and women in East Germany with only a short duration of unemployment before programme entry (up to 13 weeks) suffer from participation in terms of reduced employment rates. Women in East Germany, aged 26 to 50 as well as women with higher qualification are harmed in their employment chances by participation as well. For all other groups in the analysis, the programme effects are not significantly different from zero at the end of the observation period.

The last study I want to review is the one carried out by Caliendo et al. (2006c) that focusses on effect heterogeneity resulting from differences in the implementation of programmes. The evaluation considers the different economic sectors, the types of support, and the types of providers. The matching algorithm used is the same as in Caliendo et al. (2006a) and effects are estimated separately by gender and region. The employment effects at the end of the observation period in December 2002 for the different programme sectors are positive for men in Office and Services in West Germany and women in Community Services in East Germany. Participation in a programme in Construction and Industry harms the employment chances of male, in Office and Services of female, and in Other of all participants independently of gender in East Germany. For all other groups in consideration, the effects do not differ from zero. Referring to the type of promotion within sectors, men in Office and Services with regular promotion, women with enforced promotion in Community Services and with regular promotion in Other benefit in West Germany. In contrast to that, regularly promoted men in Agriculture and Construction and Industry as well as women with enforced promotion in Agriculture in East Germany suffer from participation. Last but not least, the analysis of the different types of providers does not provide much evidence for the effectiveness of programmes as most of the estimates are insignificantly different from zero; the only groups that are affected

[23] I will discuss the method of matching and the different algorithms in detail in chapter 3.

negatively by participation are persons in programmes provided by non-commercial institutions, namely men in Agriculture and women in Other in East Germany with reduced employment in December 2002.

2.5 Possible Effects of Job Creation Schemes

The description of the development of the German labour market has shown that job creation schemes have been used to a large extent especially in the first years after German Unification. Although the number of persons allocated to programmes has decreased during the last years, they are still an important tool of ALMP particularly in East Germany. Nevertheless, from the review of the existing microeconomic studies, questions about the effectiveness of the programmes become obvious. In addition, the recent change of the institutional set-up emphasises these questions as well. However, before assessing the value of job creation schemes too quickly, I want to discuss possible effects of the programmes.

Table 2.5: Possible Effects of Job Creation Schemes – Pros and Cons

Microeconomic Dimension	
Pros	**Cons**
• Prevention of human capital loss	• Discreation of human capital
• Adjustment of human capital	• Negative signal to potential employers
• Improve motivation	• Reduce one's own initiative
• Habituation to regular employment	• Discourage people
• Bridge to regular employment	• Imply negative incentives for job-search
• Reduce stigmatisation of long-term unemployed persons	• Locking-in effects
• Bridge to early retirement	
• Screening device for new workers	
• Social protection	
• 'Soft' human capital effects	
Macroeconomic Dimension	
Pros	**Cons**
• Relief of the labour market	• Misallocation of resources
• Assumed to be self-financing	• Compete with private production
• Investment in infrastructure	• Displacement and substitution of regular employment
	• Distort competition

Table 2.5 summarises the popular pros and cons for the use of job creation schemes from the empirical literature in Germany with a distinction of the economic dimension, i.e. microeconomic and macroeconomic arguments.[24] The list is ordered without valuing the content of the single arguments. As mentioned in the description

[24] I will reference the respective arguments from Table 2.5 in *italic* letters in the text.

of the institutional set-up (2.3.2), job creation schemes offer temporary employment in particular to unemployed persons with barriers to employment, like long-term unemployed, disabled or older aged people, and individuals who lack a completed professional training. The main purpose is to increase the employability of the individuals by providing a stable foundation and relevant qualifications for later (re-) integration into regular jobs. For that reason, the most important microeconomic effects are, on the one hand, the prevention of a professional descent associated with a loss of human capital and a downgrading of the employees (*prevention of human capital loss*). Professional descent is a usual consequence of unemployment. On the other hand, job creation schemes should increase the chances of the participating individuals for regular and permanent employment (*bridge to regular employment*).

Further intended positive aspects due to the offering of employment in such a programme are an increase of the personal motivation and self-respect (*improve motivation*). Being employed enhances the social cachet of the individual and prevents further negative effects of unemployment, like a strong psychosocial burden, health trouble, or crime (*social protection*, Spitznagel, 1992). Moreover, occupational stabilisation should be achieved by habituating unemployed workers to regular employment. It should help to reduce problems of an anew professional socialisation and enhance the (re-)integration chances (*habituation to regular employment*). Occupations offered in job creation schemes may also provide important contacts and references (e.g., private businesses) that can be helpful in finding permanent jobs (‘*soft’ human capital*, Gerfin, Lechner, and Steiger, 2005). Another argument that speaks for the use of job creation schemes relies on the belief that programmes may act as a bridge to early retirement and leaving the labour force (*bridge to early retirement*) and may therefore help to alleviate the psychosocial effects of unemployment in particular for older unemployed persons (Hübler, 1997). In addition, primarily long-term unemployed persons may experience a reduced stigmatisation if participation is seen as a positive signal for the willigness and productivity of the individual by potential employers (*reduce stigmatisation of long-term unemployed persons*). From an employer’s perspective, job creation schemes cheapen employment for firms and allow them to collect information on the individual’s abilities without concluding a permanent contract (*screening device for new workers*). A last point, mentioned by Eichler (1997), is the potential *adjustment of human capital* to changed demands of the labour market by participating in a job creation scheme. However, this argument seems to be of limited explanatory power for the following reason only. Although activities undertaken in job creation schemes should be of value for society, they must not compete with regular employment, i.e. in particular private production. Therefore, achieving both tasks simultaneously seems to be problematic, and if occupations are not in line with the market, it is questionable to what extent a competitive qualification could be transferred to the individuals.

Thus, it is more likely that occupations in job creation schemes reduce the human capital (*discreation of human capital*), a possible negative effect. An empirical finding that supports this assumption is that activities in job creation schemes usually do not meet the individuals’ qualification. Spitznagel and Magvas (1997) point out that about 40 percent of the participants are allocated to jobs that are below the individ-

ual qualification level. Further arguments that speak against the use of job creation schemes are uncertainties about the possible signals. In contrast to the opinion that job creation schemes reduce the stigmatisation of long-term unemployed persons, the opposite may also be the case (*negative signal to potential employers*). The main reason is that programmes are targeted to persons with the a priori worst labour market perspectives (due to low discipline and productivity), and participation may be interpreted as a negative selection of low productive persons. In addition, participants may be seen to be passive in terms of job search as they have accepted the allocation to the programme by the LEA. The results of Spitznagel and Magvas highlight some aspects of the passivity of participants. Whereas the job searching behaviour of programme participants within their individual neighbourhood (i.e. their circle of friends) is fairly similar to that of other unemployed, participants do more often rely on the offers of the LEA. In addition, a large share of about 45 percent of the participants do not look for a job at all for different reasons. Spitznagel and Magvas speculate that this may be mainly due to unrealistic expectations of the participants concerning a permanent contract following the programmes and a consequently reduced own initiative to look for a job (*reduce one's own initiative*). Moreover, a strong discouragement with respect to the individual labour market chances may occur, in particular if unemployed persons are allocated to different occupations subsequently, which in addition results in discontinuities in the individual's labour market career (*discourage people*). Further arguments which point in the same direction are a discouragement of the participants to search for regular employment because programmes have to offer contract wages which often exceed wages for comparable work in the private sector (*imply negative incentives for job-search*, Kraus, Puhani, and Steiner, 2000), or just due to the involvement in the programmes (*locking-in effects*).

Besides those microeconomic effects, one can think of several macroeconomic effects of job creation schemes in Germany. The most important macroeconomic argument with common consent for the use of the programmes is their ability to relieve the tense situation on regional labour markets with high unemployment (*relief of the labour market*). For that reason, job creation schemes are used as a means to preserve social peace. In contrast, the validity of the other possible positive effects from the literature is more ambiguous. Proponents of job creation schemes have claimed programmes to be self-financing in the sense that programmes' costs would be compensated by savings on passive labour market policy and by higher tax receipts due to higher employment as well as a higher consumption level of the participants (*assumed to be self-financing*, Spitznagel, 1992). However, Kraus et al. (2000) note that costs on the programmes seem to exceed the sum of direct savings and additional income generated by any macroeconomic effects by a large margin. Especially with the introduction of large-scale programmes[25] (*Mega-ABM*) and Societies

[25] Each of the large scale programmes had a funding of at least three million Deutschmarks and more than 150 persons per programme with a focus towards infrastructure and economic development.

for Employment Promotion and Structural Adjustment[26] (*ABS-Gesellschaften*), programmes became more cost-intensive. A last aspect to be mentioned is suggested by Buttler and Emmerich (1995). It states that job creation schemes have been intended as investments in the East German infrastructure (*infrastructure investment*).

In contrast to these possible positive effects associated with the deployment of job creation schemes, there are strong disbeliefs in the effectiveness and efficiency for the following reasons. The most striking argument is that job creation schemes compete with private production since the purposes of being additional in nature and of value for society could not be achieved simultaneously. In consequence, job creation schemes result in deadweight losses, in substitution effects and in displacement effects (*displacement and substitution of regular employment*). According to Calmfors (1994), deadweight losses are defined as hirings from the target group of the programme that would have occurred also in the absence of the programme; substitution effects describe the extent to which jobs created for a certain category of workers simply replace jobs in other (regular) categories. Due to the high wage subsidies, displacement effects of regular employment are a further consequence and the implementation of programmes distorts the competition on markets for goods and factors (*distort competition*).[27] In particular, the large scale programmes in East Germany (during the 1990s) have not been without controversy regarding this point. Although competition with private production should have been avoided in the past and in the present, allocation of individuals was carried out with low respect to the targeting criteria only. Primarily during the first years after German Unification, many people have entered programmes directly from employment, e.g., in case of mass redundancies (Kraus et al., 2000). The results of Brinkmann, Caliendo, Hujer, Jahn, and Thomsen (2002) have shown that even ten years after German Unification the shares of long-term unemployed persons amounted to about 40 to 50 percent only; the share of disabled people was 20 percent. Therefore, if allocation of unemployed persons is accomplished with this small degree of target orientation only, it has to be questioned if there are no better alternatives for the individuals on the labour market that do not meet the target criteria (*misallocation of resources*, Spitznagel, 1992).

2.6 Summary

In this chapter, I have discussed the relevant aspects of job creation schemes in Germany. The discussion started with an overview of the development of the German labour market since German Unification. The current situation of East Germany is characterised by a high and persistent unemployment with an unemployment rate of about 20.1 percent and a share of long-term unemployed of about 41.5 percent in

[26] The task of these Societies for Employment Promotion and Structural Adjustment was to employ and qualify people within job creation and structural adjustment schemes. In the beginning of 1995, more than 150,000 persons were employed in these societies (Kraus et al., 2000).

[27] According to Calmfors (1994), displacement effects describe the possible reduction of jobs elsewhere in the economy because of competition in goods markets.

2003. Due to the lower productivity of about 70 percent together with gross wages per employee of about 81.2 percent compared to the West, the East German economy suffers from the lack of a self-contained economic basis. In consequence, active and passive labour market policy play an important role for the East German labour market. In contrast, the situation of the labour market in West Germany is less severe, but unemployment is a serious problem here as well. With an unemployment rate of about 9.3 percent and a ratio of long-term unemployed persons of about 31.1 percent in 2003, the persistence is not as strong as in the East. As the West German economy is export dependent to a high degree, a strong Euro and the still to-be-solved structural problems harm the expectation of a positive development of the labour market in the near future. Following the description of the development of the labour market, I have explained the set-up of ALMP in Germany in general and of job creation schemes in particular. I have also reviewed the results of previous microeconometric studies evaluating the effects of job creation schemes in Germany. The last section has provided a discussion of the possible effects of job creation schemes with a distinction between the micro- and macroeconomic dimension.

To conclude, I will discuss the findings of this chapter in light of the five features of programmes that should be kept in mind for evaluation stated by Heckman, LaLonde, and Smith (1999). The first feature is *decentralisation*. Due to decentralisation in the operation of programmes, differences may emerge with respect to design and implementation of the programmes. Furthermore, the actual practice of implementation can deviate from the explicit written policy in the law. As becomes obvious from my description, labour market policy in Germany has become more decentralised with the reform of the legal basis in 1998. Programmes are accomplished in the responsibility of the local employment agencies; therefore, regional differences should be considered in the evaluation. Moreover, since job creation schemes have been used on a large scale in East Germany during and after the transition process of the economy in association with a low target-oriented allocation of individuals, the question has been raised whether the purpose of the programmes to be additional in nature together with being of value for society could be achieved simultaneously.

The second aspect concerns *multiple services*. Since participants may receive services from more than one category, the various types of possible combinations constitute a source for heterogeneity. Isolation of the impact of one particular service may therefore be difficult or impossible. The description of the institutional set-up of job creation schemes has shown that participants are allowed to do practical training or vocational qualification during the programmes. Thus, if frequently used, this aspect raises a further question for the evaluation of the programmes.

The *influenced participation decision* is the third feature Heckman, LaLonde, and Smith (1999) note. In some countries, like Sweden[28] or Switzerland[29], participation in a programme is a condition for receiving unemployment benefits rather than less generous social assistance payments. For that reason, individuals' decisions to participate may be affected by features of ALMP programmes. The discussion clar-

[28] See, for example, Sianesi (2002).
[29] See, for example, Gerfin and Lechner (2002).

ified that participation is no general condition for receiving unemployment benefits in Germany. However, since job creation schemes have been accounted as regular employment until 2004, participation in a programme has prolonged the eligibility for unemployment benefits of the participants. This aspect should be regarded when interpreting the results.

Feature four refers to the *discretion of the caseworkers*. In most countries and for the majority of programmes, allocation of participants is carried out by caseworkers' discretion. This discretion results from the fact that the number of applicants usually exceeds the number of programme places by far. Therefore, Heckman, LaLonde, and Smith (1999) recommend a special emphasis on the allocation mechanism in modelling the participation decision, i.e. it has to be accounted not only for the individuals' incentives to participate, but also for those of the programme operators. I have started the discussion of the allocation process in this chapter relying on the institutional set-up and on information from interviews with caseworkers in the LEAs. Since caseworker discretion is an important determinant for the allocation of individuals to programmes, I will take up the discussion in chapter 3 when establishing the evaluation question. Moreover, in my empirical analysis of the effect heterogeneity to improve the efficiency of the programmes in chapter 6, I will analyse whether unsatisfying impacts of job creation schemes may be due to inefficiencies in the allocation process.

The last feature to be considered for evaluating programme impacts reflects the specificity of programmes. Hence, it could be called *different programmes – different economic models*. It is concerned with the modelling of the participation decision and the modelling of the impacts. Heckman, LaLonde, and Smith (1999) recommend a careful choice of the economic model that is suitable to the respective programme. From the discussion of the possible effects of job creation schemes it becomes obvious that a standard human capital model would provide little guidance to the programmes in analysis. Therefore, for a reasonable evaluation of programme impacts, the model of the participation decision has to be based on a careful description of the relevant determinants influencing participation. These determinants must be revealed from the institutional set-up, the assignment process of participants, the available data as well as from experiences from the empirical literature.

3

The Methodological Framework of Evaluation

3.1 Overview

The main purpose of any microeconometric evaluation is to answer the question whether the outcome variable of interest is affected by participation in the programme under study. This question is analysed using individual data. The empirical evaluation literature deals with different outcome variables, like the duration of individual unemployment, the probability of employment, or the future earnings. Whereas earnings have been more often analysed in US studies, European studies typically rely on the employment probability of the participants or their unemployment duration.

The individual causal effect of treatment is defined as the difference of the value of the participant's outcome in the current situation and the value of the outcome in a situation where the participant has not taken the treatment.[1] Both states are mutually exclusive, i.e. an individual cannot be in both states at the same time and thus, one could never observe both states simultaneously for the same individual. For that reason, one of the states is counterfactual. This is what Holland (1986) calls the *fundamental problem of causal inference*. Both experimental and non-experimental approaches require assumptions to construct the missing counterfactual that cannot be tested without collecting data specifically designed to test the assumptions of the model (see Heckman, Ichimura, Smith, and Todd, 1996; 1998).

Solving the missing data problem lies at the heart of the microeconometric evaluation problem. The literature provides a set of approaches that attempt to estimate the missing data. These approaches differ in the assumptions they make about how the missing data are related to the available data and what data are available.[2] Thus, constructing an adequate comparison group is necessary to make a comparison possible.

[1] Following the bio-statistical literature, programmes are referred to as 'treatments'. Both terms are used interchangeably throughout this thesis.

[2] See Heckman, LaLonde, and Smith (1999) for an overview on these approaches.

In this chapter, I will describe the methodological framework for the microeconometric evaluation of job creation schemes in Germany used in the empirical analyses in chapters 5 and 6. I will begin my discussion with a brief review of the evaluation approach for the static binary treatment case in section 3.2. To do so, I will present the standard framework for microeconometric evaluation (section 3.2.1) and the most common parameter of interest (section 3.2.2). After that, I will discuss the issue of selection or evaluation bias which occurs when using non-experimental data (section 3.2.3). The three basic principles of possible comparison in the evaluation approach will be presented by a description of three commonly used simple non-experimental estimators in section 3.2.4. As the empirical analyses are based on the matching estimator, I will present this estimator in section 3.3. The section contains a discussion of the identifying assumptions (section 3.3.2) as well as of the balancing property of the propensity score (section 3.3.3). In section 3.3.4, I will discuss the matching estimators used in the empirical analyses in detail.

The recent empirical literature on evaluation of social programmes has emphasised that the timing of treatment in the individual unemployment spell conveys important information on the parameter of interest. This aspect is considered in the context of matching estimators, for example, in the studies by Sianesi (2002; 2003; 2004) for Sweden, for Switzerland by Steiger (2004), and by Speckesser (2004) and Fitzenberger and Speckesser (2005) for Germany. In addition, the interested reader is referred to Abbring and van den Berg (2003) who discuss the identification of the treatment effect in the timing-of-events approach. An inchoate bridge of both approaches is provided by Fredriksson and Johansson (2003; 2004). I will analyse the differences in the effects of job creation schemes on the integration into regular employment with respect to the timing of treatment in the individual unemployment spell in chapter 5. Hence, I will present the extension of the matching estimator to the dynamic setting in section 3.4. To do so, I will discuss some considerations on the timing of treatment (section 3.4.1) as well as the changes in the identifying assumptions and the estimator (section 3.4.2). Section 3.5 will provide some important issues to be regarded when implementing the estimator. These issues comprise the estimation of the propensity score (section 3.5.1), the check of the common support assumption (section 3.5.2), the choice of an adequate matching algorithm (section 3.5.3) as well as the estimation of the standard errors (section 3.5.4), the assessment of the quality of matching (section 3.5.5) and some further methodological questions to be answered for the empirical analyses (section 3.5.6). The final section will summarise this chapter.

3.2 Evaluation Approach – The Static Binary Treatment Case

3.2.1 The Evaluation Problem

The standard model in the microeconometric evaluation literature is the so-called *potential outcome approach* which has been variously attributed to Neyman (1923;

1935), Fisher (1935), Roy (1951), Quandt (1972; 1988) or Rubin (1974). In the simple form, the model considers two possible states of the world.[3] An individual i is imagined to either participate in a programme or not. Let Y_i^1 and Y_i^0 denote the potential outcomes corresponding to the states, where 1 denotes treatment and 0 non-treatment. According to this definition, the individual causal effect of treatment is defined as the difference of the two potential outcomes, i.e.

$$\Delta_i = Y_i^1 - Y_i^0. \tag{3.1}$$

However, since the individual cannot be in both states of the world at the same time, the actual observed outcome for each individual i can be written as:

$$Y_i = Y_i^1 \cdot D_i + (1 - D_i) \cdot Y_i^0, \tag{3.2}$$

where $D_i \in \{0, 1\}$ is a binary treatment indicator, with $D = 1$ denoting participation and $D = 0$ denoting non-participation.[4] To complete the notation, let X denote variables that are unaffected by treatment – the so-called *attributes* by Holland (1986). Eq. (3.2) makes clear that one of the outcomes is unobservable for each individual, i.e. only Y_i^1 or Y_i^0 is observable. For that reason, there is no opportunity to calculate individual effects directly from the data, and Δ_i is not observed for anyone. In the words of Dawid (2000) this is that potential outcomes are complementary.

To render the model useful for causal analysis, one must take the stable unit treatment value assumption (SUTVA, see, e.g., Rubin, 1986). SUTVA rules out any cross-effects or general equilibrium effects that may occur among potential programme participants because of their participation decision (Lechner, 2001). In other words, the potential outcomes of an individual depend on the his or her participation decision only and are not affected by the treatment status of other individuals. Furthermore, whether an individual participates or not does not depend on the participation decision of other individuals. This additional feature excludes peer-effects (Sianesi, 2004). If one is willing to estimate the effect of the programme for a person drawn randomly from the participants sample, those effects are negligible and SUTVA could be assumed to be fulfilled.[5]

A further interesting issue to note in this context is mentioned by Heckman, LaLonde, and Smith (1999). They point out that microeconometric evaluation concentrates on direct effects only. A full evaluation of the programme of interest would

[3] The model has been extended for the case of multiple treatments by Lechner (2001) and Imbens (2000).

[4] Alternatively, in the case of J mutually exclusive treatments (e.g., for the case of evaluating different ALMP programmes), D could be an indicator for the $J + 1$ possible states the individual faces. D could also be $\mathbb{R}_+ := [0, \infty)$, representing a continuum of doses of some medication, for example (see Abbring, 2003).

[5] It should be noted that since job creation schemes have been used to a large extent especially in East Germany, assuming no spill-over effects on non-participants may be questionable. Thus, microeconometric evaluation can only analyse partial equilibrium effects of the programmes. Further macroeconometric analyses of programme effects are necessary for a full evaluation, see, e.g., Hujer and Zeiss (2005).

require an enumeration of all outcomes of interest for every person, both in the actual state of the world as well as in the alternative state(s). In the most general view, almost everyone in a modern economy participates in each social programme either directly or indirectly. Direct effects affect the situation of only those persons enrolled to the programmes. Effects that do not flow from participation directly are defined as indirect effects. The indirect effects could occur for participants and non-participants. For example, participants may pay taxes or UI contributions to support the programme just as persons who do not participate. Furthermore, indirect effects occur for persons with whom the participants compete in the labour market and for the firms that hire the participants. The problem of the indirect effects is ignored in the econometric and statistical evaluation literature, and treatment outcomes are equated with the direct outcome Y^1 in the programme state; no treatment outcomes are equated with the direct outcome Y^0 in the no-programme state. However, this is a crucial assumption in the traditional evaluation literature (Heckman and Smith, 1998) and should be kept in mind when drawing policy-relevant implications.

3.2.2 The Average Effect of Treatment on the Treated

As already mentioned, direct estimation of the individual effect of treatment in eq. (3.1) is impossible. Therefore, evaluation has to focus on population averages of gains from treatment. Under certain assumptions, it is possible to estimate group impact measures even though it may be impossible to measure the impacts of a programme on any particular individual (Heckman, LaLonde, and Smith, 1999). The most common parameter of interest in the empirical literature is the average effect of treatment on the treated (ATT).[6] The ATT is defined as

$$\begin{aligned} \Delta^{ATT} = E(\Delta|D=1) &= E(Y^1 - Y^0|D=1) \\ &= E(Y^1|D=1) - E(Y^0|D=1), \end{aligned} \tag{3.3}$$

which is the difference of the expected outcomes with and without treatment for participants. As it focusses directly on the actual participants, it determines the realised gross gain for this group (Heckman, LaLonde, and Smith). Thus, its importance for policy makers becomes obvious as programmes are generally targeted to

[6] Heckman, LaLonde, and Smith (1999) discuss further parameters that may be of interest: for example, the average effect of treatment (ATE) defined as:

$$\Delta^{ATE} = E(\Delta) = E(Y^1 - Y^0) = E(Y^1) - E(Y^0).$$

The ATE computes the difference of the expected outcomes after participation and non-participation. It answers the question what the impact of treatment would be if individuals are randomly assigned to treatment. However, for policy implications it is only of minor relevance as persons are included for whom the programme was never intended (Heckman, 1997). Further parameters of interest may be the proportion of people taking the programme who benefit from it, or the increase in the proportion of outcomes above a certain threshold outcome value due to a policy. As the empirical analyses are based on the ATT, I will concentrate the discussion on this parameter.

certain groups; and by comparing the programme effect with its costs, the ATT is a reasonable approach to measure the performance of the programme, i.e. deciding whether the programme is a success or not (see Heckman and Robb, 1985b, and Heckman, Ichimura, and Todd, 1997).[7]

The ATT can also be defined for a certain sub-population of the treated individuals defined by attributes X:

$$\begin{aligned}\Delta^{ATT}(X) = E(\Delta|X, D=1) &= E(Y^1 - Y^0|X, D=1) \\ &= E(Y^1|X, D=1) - E(Y^0|X, D=1). \quad (3.4)\end{aligned}$$

A requirement for the interpretation of this parameter is that the conditional distribution of X has to satisfy

$$F(X|Y^0, Y^1, D) = F(X|Y^0, Y^1), \quad (3.5)$$

i.e. that conditional on potential outcomes, the realised participation decision D does not predict X (Heckman, Ichimura, Smith, and Todd, 1998). If this requirement holds, conditioning on variables that are determined by the participation decision is ruled out. Otherwise, this would mask the total effect of D. The parameter allows different groups to have different programme impacts. If, for example, X determines the schooling of the individuals, one could define the expected impact of a job creation scheme for individuals with O-level.

In addition, Heckman, Ichimura, Smith, and Todd (1998) propose an averaged version for X in some region K,

$$\begin{aligned}\overline{\Delta}^{ATT}(X \in K) = E(\Delta|X \in K, D=1) &= E(Y^1 - Y^0|X \in K, D=1) \\ &= \frac{\int_K E(\Delta|X, D=1) dF(X|D=1)}{\int_K dF(X|D=1)}, \quad (3.6)\end{aligned}$$

where $F(X|D=1)$ is the distribution of the attribute(s) X for the participants. Whereas in the example, the estimator in eq. (3.4) allows to estimate the effect of a job creation scheme for a person with a completed O-level, the estimator in eq. (3.6) allows to estimate the effect of the programme for a specific subset of the schooling variable, e.g., the effect of a job creation scheme for all persons with a schooling of O-level or below.

To show some further properties of the ATT, I will follow Smith and Todd (2005a). Let the potential individual outcomes, Y_i^1 and Y_i^0, be represented by

[7] Another parameter that might be of interest is the average effect of treatment for the non-treated (ATU). It is symmetrically constructed to the ATT and is defined as:

$$\begin{aligned}\Delta^{ATU} = E(\Delta|D=0) &= E(Y^1 - Y^0|D=0) \\ &= E(Y^1|D=0) - E(Y^0|D=0).\end{aligned}$$

This parameter can be used to estimate the impact on the group that was excluded from treatment of what they would have gained if they had participated.

$$Y_i^1 = \phi^1(X_i) + U_i^1 \tag{3.7}$$

$$\text{and} \quad Y_i^0 = \phi^0(X_i) + U_i^0, \tag{3.8}$$

where U_i^1 and U_i^0 are distributed independently across persons and satisfy $E(U_i^1) = 0$ and $E(U_i^0) = 0$. The observed outcome (similar to eq. (3.2)) can be written then as

$$Y_i = \phi^0(X_i) + D_i\Delta(X_i) + U_i^0,^{8} \tag{3.9}$$

where the treatment effect $\Delta(X_i)$ is given by

$$\Delta(X_i) = \phi^1(X_i) - \phi^0(X_i) + U_i^1 - U_i^0. \tag{3.10}$$

Since the treatment effect varies across persons even conditional on X_i, this is a random coefficient model. This is an important feature because it allows the treatment effects to be heterogeneous. In the simplest case, the treatment effect is assumed to be constant across individuals, i.e. $\Delta = \Delta_i$. This is the fixed effects model, sometimes called 'common effect' assumption. In that case, eq. (3.10) would be

$$\Delta = \Delta(X_i) = \phi^1(X_i) - \phi^0(X_i), \tag{3.11}$$

for any i. This means that ϕ^1 and ϕ^0 are two parallel curves, only differing in the level, and the participation-specific error terms are not affected by the treatment since $U_i^1 = U_{it}^0 = U_i$. Eq. (3.9) denoting the outcome is then

$$Y_i = \phi^0(X_i) + D_i\Delta + U_i. \tag{3.12}$$

Although this common effect has been used in the earlier empirical literature (Smith, 2000a), it does provide a poor approximation of most real situations only. Therefore, the consideration of the evaluation problem in the context of heterogenous treatment effects points up that there is more than one parameter of interest. The feature of the estimator to capture heterogeneous effects is also important for the analysis in chapter 6. Since programme effects are heterogeneous (Manski, 1997; 2000), the average effects for the whole population must not apply to all strata of the population. Negative mean impacts may be acceptable if the majority of participants gain from the programme (Heckman, LaLonde, and Smith, 1999). Thus, abandoning the 'common effect' assumption of treatment effects provides an opportunity to identify individuals that benefit from the programme, and therefore, improve the future efficiency of the social intervention.

[8] This is derived by:

$$\begin{aligned} Y_i &= D(\phi^1(X_i) + U_i^1) + (1 - D)(\phi^0(X_i) + U_i^0) \\ &= \phi_i^0 + U_i^0 + D(\phi^1(X_i) - \phi^0(X_i) + U_i^1 - U_i^0) \\ &= \phi_i^0 + D\Delta(X_i) + U_i^0. \end{aligned}$$

3.2.3 Examining Selection Bias

It becomes obvious from eq. (3.3) that the second term on the right-hand side is unobservable. The term describes the hypothetical outcome of the participants if they had not participated in the programme. In an experimental evaluation, the missing counterfactual data for the treatments can be derived by using information from a control group. Here, the hypothetical outcome of participants if they had not participated would not differ from the expected outcome of the non-participants, i.e.

$$E(Y^0|D=1) = E(Y^0|D=0). \tag{3.13}$$

Therefore, it would be possible to approximate the counterfactual outcome of the participants by the non-participants' outcomes. The ATT can easily be computed since the data on programme participants identify the mean outcome in the treated state, $E(Y^1|D=1)$, and the randomised-out comparison provides the direct estimate for $E(Y^0|D=1)$ (Smith and Todd, 2005a).[9]

Social experiments have been seen as the ideal way to evaluate the impacts of programmes in particular for US programmes (Smith, 2000b). In his survey, Smith (2000a) notes a set of advantages of social experiments over standard non-experimental methods. First, they are easy to explain to policy makers because most educated persons understand the issue of random assignment. Second, as becomes obvious by eq. (3.13), social experiments produce consistent estimates of the impact of treatment on the treated and they are less controversial than non-experimental methods. Third, for conductors of experiments it is more difficult to cheat, i.e. to produce the impact they want, because the evaluators could not choose from a set of estimators. Fourth, experiments provide an opportunity to examine the efficacy of different alternative non-experimental estimators.[10]

However, social experiments also have some important drawbacks. On the one hand, they cannot address many questions of interest to researchers or policy makers for the following reason. As they generate choice-based, endogenously stratified samples that are difficult to use in any other economic question, they only allow the evaluation of the impact of treatment on the treated for one programme with one set of participants and eligibility rules (Heckman, LaLonde, and Smith, 1999). On the other hand, social experiments may be hard to accomplish as they entail high costs and ethical issues concerning the use of those experiments. Moreover, there are some practical problems with the implementation of social experiments mentioned in the literature: the problem of non-compliance, the problem of substitution and of randomisation bias. Non-compliance occurs if persons assigned to the treatment group do not participate or if members assigned to the control group participate in the programme. Selective non-compliance may lead to biased estimates of the programme effects (see Bijwaard and Ridder, 2000). Randomisation bias describes the phenomenon if persons selected for the programme differ from persons

[9] See Heckman, Ichimura, and Todd (1997), for a description of how randomisation solves the evaluation problem.

[10] The interested reader is referred to the paper of LaLonde (1986) and the responses and extensions by Dehejia and Wahba (1999; 2002) and Smith and Todd (2005a).

who would participate in the programme under usual conditions, i.e. the effects of the programme are estimated for an unrepresentative sample. Substitution bias occurs if members of the control group participate in similar programmes to the experimental treatment (see Heckman and Smith, 1995).

Whereas in the experimental situation the randomised-out control group provides a direct estimate of the non-treated outcome of the treated, there is no such group available in non-experimental data. Therefore, in a non-experimental evaluation, analysts must replace the missing data with data on non-participants along with additional assumptions invoked when using the method of social experiments since no direct estimate for this counterfactual mean is available and eq. (3.13) will usually not hold, i.e. $E(Y^0|D=1) \neq E(Y^0|D=0)$. Using the unadjusted outcomes of the non-participants to approximate the missing counterfactual in the ATT will lead to selection or evaluation bias:

$$\begin{aligned} \Delta^{ATT} &= E(Y^1|D=1) - E(Y^0|D=0) \\ &= E(Y^1 - Y^0|D=1) + \underbrace{\{E(Y^0|D=1) - E(Y^0|D=0)\}}_{=B}. \end{aligned} \tag{3.14}$$

The term in the curly brackets is the selection bias, B, i.e. the difference between the hypothetical and actual outcomes after non-participation. The reason why this selection bias could not be assumed to be zero with non-experimental data is that participants and non-participants would also have had different non-treatment outcomes even in the absence of the programme. As has been seen from the description of the allocation mechanism for job creation schemes in Germany (see section 2.3.2), participation in a programme depends on certain eligibility criteria, the individuals' characteristics, and to a large extent on caseworkers' discretion. Therefore, it is reasonable to assume that selection into programmes is not random, but is carried out according to some allocation rules.

To provide a more appropriate selection bias measure of B for the more narrowly defined ATT parameters in eq. (3.4) and (3.6), let $B(X)$ denote the selection bias for a particular value of X (Heckman, Ichimura, Smith, and Todd, 1996; 1998):

$$B(X) = E(Y^0|X, D=1) - E(Y^0|X, D=0). \tag{3.15}$$

This selection bias measure is limited to the set of X values common to the $D=1$ and $D=0$ populations. Following Heckman, Ichimura, Smith, and Todd (1998), let

$$S_{1X} = \{X|f(X|D=1) > 0\} \tag{3.16}$$

be the support of X for $D=1$. Analogously, let

$$S_{0X} = \{X|f(X|D=0) > 0\} \tag{3.17}$$

to be the support of X for $D=0$. $f(X|D=1)$ and $f(X|D=0)$ denote the conditional distributions of X for $D=1$ and $D=0$ respectively. For the region of the overlap between both groups then follows

$$S_X = S_{0X} \cap S_{1X} \quad \text{(common support)}. \tag{3.18}$$

The mean selection bias $\overline{B}_{S_X}$ that conditions on the set of common support can be constructed as an average version of $B(X)$ for $X \in S_X$ (Heckman, Ichimura, Smith, and Todd):

$$\overline{B}_{S_X} = \frac{\int\limits_{S_X} B(X) dF(X|D=1)}{\int\limits_{S_X} dF(X|D=1)}. \tag{3.19}$$

To show how the conventional measure of selection bias, B, from eq. (3.14) is related to $\overline{B}_{S_X}$ and $B(X)$ it is useful to decompose it into three terms. To do so, (Heckman, Ichimura, Smith, and Todd) note that it might help to rewrite B as

$$\begin{aligned} B &= E(Y^0|D=1) - E(Y^0|D=0) \\ &= \int\limits_{S_{1X}} E(Y^0|X, D=1) dF(X|D=1) - \int\limits_{S_{0X}} E(Y^0|X, D=0) dF(X|D=0) \\ &= B_1 + B_2 + B_3, \end{aligned} \tag{3.20}$$

where

$$\begin{aligned} B_1 = &\int\limits_{S_{1X} \setminus S_X} E(Y^0|X, D=1) dF(X|D=1) \\ &- \int\limits_{S_{0X} \setminus S_X} E(Y^0|X, D=0) dF(X|D=0), \end{aligned} \tag{3.21}$$

$$B_2 = \int\limits_{S_X} E(Y^0|X, D=0) \left[dF(X|D=1) - dF(X|D=0) \right] \text{ and} \tag{3.22}$$

$$B_3 = P_X \overline{B}_{S_X}. \tag{3.23}$$

The term $P_X = \int_{S_X} dF(X|D=1)$ is the proportion of the density of X given $D=1$ in the common support region S_X. The support of X given $D=1$ ($D=0$) that is not in the overlap is denoted by $S_{1X} \setminus S_X$ ($S_{0X} \setminus S_X$).

The term B_1 therefore defines the bias due to non-overlapping support of the treated and the comparison group. That is, for some treated individuals it is impossible to find comparable persons from the non-treated group and vice versa, i.e. to construct the counterfactuals to $E(Y^0|X, D=1)$ from $S_{0X} \setminus S_X$ or the counterfactuals to $E(Y^0|X, D=0)$ from $S_{1X} \setminus S_X$. The term B_2 describes the bias due to different weighting of $E(Y^0|X, D=0)$ by the densities of X given $D=1$ and $D=0$ within the common support. Differences in the outcomes that remain even after controlling for the observable differences are given by B_3. Thus, this bias is due to unobservable or unmeasured differences in the characteristics between participants and non-participants.[11]

[11] This is the true econometric selection bias resulting from 'selection on unobservables' (Blundell and Costa Dias, 2000).

3.2.4 Three Basic Principles of Evaluation Estimators

All evaluations are based on comparisons between treated and untreated individuals. To impute the counterfactual outcomes for non-experimental estimators, two types of data can be used: First, data on participants measured in the untreated state, and second, data on non-participants.[12] Heckman, LaLonde, and Smith (1999) note that the evaluation literature makes an artificial distinction between the task of creating a comparison group and the task of selecting an econometric estimator to apply to that comparison group although in truth all estimators define a comparison group, and the choice of the comparison group affects the properties of the estimator. In principle, there are three different ways to compare the treated with the untreated persons and to estimate the impact of treatment on the treated. These principles are (i) a comparison of the same person in the treated and the untreated state, (ii) a comparison of (different) treated and untreated persons at one point of time, and (iii) a hybrid of these two principles. In the following, I will briefly review three commonly used non-experimental estimators and their identifying assumptions that are in line with these principles: the before-after estimator, the cross-section estimator, and the difference-in-differences estimator. The estimators of the three classes differ in the way they adjust, condition or transform the data in order to construct the counterfactual $E(Y^0|D=1)$. As noted by Heckman, LaLonde, and Smith, these estimators would identify the same parameter only if there is no selection bias at all.

Before-After Estimator

The idea of the simple before-after estimator (BAE) is to compare a person with himself/herself. Based on longitudinal data, it exploits the intuitively appealing idea that a person can be in treatment and non-treatment states at different points of time. For that reason, the values of the outcome in one state at one time are good proxies for the value(s) of the outcomes in the same state at other times (at least for the non-treatment state). The estimator is valid if there is access to longitudinal data on outcomes measured before and after programme participation, or if repeated cross-section data from the same population with at least one cross-section before the treatment are available (see Heckman and Robb, 1985a; 1985b). The time t denotes the post-treatment period. Then, Y_{it}^1 denotes the post-treatment outcome of person i. Let t' denote the time before treatment and therefore $Y_{it'}^0$ is the pre-treatment outcome of the individual.

The underlying identifying assumption of the BAE is

$$E(Y_t^0 - Y_{t'}^0|D=1) = E(Y_t^0|D=1) - E(Y_{t'}^0|D=1) = 0. \tag{3.24}$$

If this assumption holds, the ATT can be estimated by the BAE as follows:

$$\Delta_{BAE}^{ATT} = E\left[(Y_t^1|D=1) - (Y_{t'}^0|D=1)\right]. \tag{3.25}$$

[12] This distinction primarily results from the US literature. As will be seen when I discuss the extension to the dynamic setting, for the analyses of the effects of job creation schemes, one has to be careful in defining the non-participation state.

The major advantage of this estimator is that it does not require information on non-participants and is therefore easy to implement. But two important drawbacks have to be noted. First, the underlying assumption implicitly assumes that there are no time-variant effects that influence the potential outcomes from one period to another. This is very restrictive since any change in the individual life-cycle position as well as in the overall state of the economy could violate this assumption (Heckman, LaLonde, and Smith, 1999). The second drawback follows from this first assumption: By excluding any time variation from the model the estimator attributes changes due to macro or life-cycle factors to the programme effect, too.

The standard example in the empirical literature where the assumption of eq. (3.24) is likely to be violated is provided by Ashenfelter (1978) and is known as the so-called *Ashenfelter's Dip*. In his analysis of the training effects on earnings, he observed that participants experienced a decline in earnings prior to the start of the programmes. This effect was also observable in other empirical analyses based on different outcome variables.[13] If this change is permanent, the BAE provides unbiased estimates for the ATT, but if this change is only transitory, the approximation error, $E(Y_{t'}^0 - Y_t^0)$, will not average out, and it is likely that the ATT is overestimated. One solution to overcome the problem of Ashenfelter's Dip is to collect more pre-programme periods and to choose a reference period for the BAE before the dip has occured.

Cross-Section Estimator

The second basic principle to estimate the parameter of interest is to compare treated and non-treated individuals at one point in time, i.e. after the programme took place in t. The corresponding estimator is the so-called cross-section estimator (CSE). From the discussion above, it is clear that the same person is never observable in different states at the same time. Therefore, the CSE compares different persons. Data are required for participants and non-participants, but only for the time after the programme has taken place. However, to be reliable, additional assumptions about the distributions of gains have to be invoked. The central identifying assumption of the CSE is given by

$$E(Y_t^0|D=1) - E(Y_t^0|D=0) = 0, \tag{3.26}$$

i.e. that the expected non-treatment outcomes of the participants equate to the outcomes of the non-participants. If this assumption is valid, the ATT can be estimated by the CSE as follows:

$$\Delta_{CSE}^{ATT} = E\left[(Y_t^1|D=1) - (Y_t^0|D=0)\right]. \tag{3.27}$$

As Heckman, LaLonde, and Smith (1999) mention, the estimated ATT by the CSE will be violated if persons go into the programme based on outcome-measures in

[13] See, e.g., for job creation schemes in Germany Bergemann and Schultz (2000) and Bergemann et al. (2000) who analyse the occurrence of Ashenfelter's Dip.

the post-programme state. However, the assumption holds if participation in the programme is unrelated to the outcomes in the no-programme state at time t. Therefore, Ashenfelter's Dip does not affect the estimates of the CSE. In addition, as long as participants and non-participants respond to changes of the macro-environment or the ageing process in the same way, the CSE is robust to the problems of the BAE.

Difference-in-Differences Estimator

If longitudinal data or repeated cross-section data on participants and non-participants are available, one can estimate the ATT by the difference-in-differences estimator (DiD). The DiD estimates the impact of the programme as the difference between participants and non-participants in the before-after difference in outcomes (Smith and Todd, 2005a). The underlying identifying assumption is that the mean change in the no-programme outcome measures would be the same for participants and non-participants, i.e. biases are on average the same before and after the programme,

$$E(Y_t^0 - Y_{t'}^0|D=1) - E(Y_t^0 - Y_{t'}^0|D=0) = 0. \tag{3.28}$$

If this assumption holds, the ATT is calculated by the DiD as

$$\Delta_{DiD}^{ATT} = E\left[(Y_t^1 - Y_{t'}^0|D=1) - (Y_t^0 - Y_{t'}^0|D=0)\right]. \tag{3.29}$$

By differencing the differences in the outcomes between participants and non-participants the estimator is able to capture selection by unobservable factors to some extent, but only when the underlying assumption of time-invariant linear selection effects holds (Heckman, Ichimura, Smith, and Todd, 1998). Therefore, the estimator overcomes one shortcoming of the BAE by allowing for time-specific intercepts that are common across groups (Smith and Todd, 2005a). But, the DiD cannot control for unobserved temporary individual-specific components either and the estimates are biased in presence of Ashenfelter's Dip.[14]

Summary

So far, I have reviewed the trilogy of conventional non-parametric evaluation estimators that consists of the BAE, the CSE and the DiD estimator. The estimators employ the three basic principles to make a comparison. It becomes obvious that the identifying assumptions of each method would in general not hold for the others. The three estimators could be extended by conditioning on observable covariates X. In that case (or by alternatively conditioning on additional instrumental variables), the modified versions of the identifying assumptions for the estimators (eq. (3.24), (3.26) and (3.28)) may be more likely to be satisfied, but this is not guaranteed (Heckman, LaLonde, and Smith, 1999). An enhancement of the estimators would be achieved, if the distributions of the attributes X differ between treated and non-treated individuals, and therefore an additional conditioning may eliminate systematic differences.

[14] See Blundell and Costa Dias (2002) for a further discussion.

But if these differences are due to unobservable factors a simple conditioning on the X may aggravate the differences. To give an example, imagine that the absolute value of the difference between the expected non-treatment outcome for participants and non-participants amounts to a constant c, i.e.

$$|E(Y^0|D=1) - E(Y^0|D=0)| = c. \tag{3.30}$$

Then, conditioning in addition on the attributes X is possible to reduce the difference,

$$|E(Y^0|X, D=1) - E(Y^0|X, D=0)| < c, \tag{3.31}$$

but this is not guaranteed. Instead, if unobservable factors determine the differences between the treated and non-treated group, it is possible that some set of X increases the difference (see Heckman, LaLonde, and Smith).

In addition to these three simple estimators, the literature provides a set of more 'sophisticated' evaluation estimators that extend the simple mean differences by making a variety of adjustments to the mean. All of the estimators are based on one of the three basic principles described in this section. The most important ones are the matching estimator, the instrumental variable estimator, the Heckman selection estimator, the regression discontinuity estimator, the conditional DiD (or DiD matching), and duration models. As I use the matching estimator for the empirical analyses below, I discuss its idea, identifying assumptions and properties in detail in the next section. I refrain from presenting details of the other estimators. A comprehensive overview on instrumental variable methods, the Heckman selection and the regression discontinuity estimator can be found in (Heckman, LaLonde, and Smith, 1999).[15] For the conditional DiD the interested reader is referred to Heckman, Ichimura, and Todd (1997), Heckman, Ichimura, Smith, and Todd (1998) and the recent work of Smith and Todd (2005a). An overview on duration models for the evaluation of social programmes is provided by van den Berg (2001); in addition, see Abbring and van den Berg (2003) for the non-parametric identification of treatment effects in duration models.

3.3 The Matching Estimator

3.3.1 Some Introducing Remarks

An important share of the non-experimental evaluation literature deals with providing estimators for average treatment effects of receiving or not receiving a binary

[15] General surveys are provided by Angrist and Krueger (1999), Heckman, LaLonde, and Smith (1999) and Blundell and Costa Dias (2002). For instrumental variable methods see also Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996). Blundell and Costa Dias (2000) provide further information on the Heckman selection estimator in the evaluation context. A good example for an application of the regression discontinuity estimator is given by Angrist and Lavy (1999).

treatment under the assumption that the treatment satisfies some kind of exogeneity. This assumption, variously referred to as unconfoundedness by Rosenbaum and Rubin (1983b), selection on observables by Barnow, Cain, and Goldberger (1980) or conditional independence assumption by Lechner (1998) denotes that the receipt of treatment is independent of the potential outcomes with and without treatment if certain observable attributes are held constant. Throughout this work I will use the terms unconfoundedness and conditional independence interchangeably. In his review on non-parametric estimators that are based on this exogeneity assumption, Imbens (2004) distinguishes five classes of estimators that comprise regression, matching on covariates, methods based on the propensity score, combinations of these approaches, and Bayesian methods. However, in particular the matching estimator has become a popular approach to estimate causal treatment effects. The main reasons for the popularity of the matching estimator are its underlying idea as well as the simplicity of explanation (Heckman, LaLonde, and Smith, 1999). Therefore, matching estimators are frequently used for programme evaluation and in consulting business.

The basic idea of the matching approach is to find in a large group of non-participants those individuals who are similar to the participants in all relevant pre-treatment characteristics X ('statistical twins'). For that reason, the method appeals to the intuitive principle that it is possible to 'adjust away' differences between participants and non-participants using the available regressors (Heckman, LaLonde, and Smith, 1999). Originated in the statistical literature[16], matching thus generates a comparison group that resembles an experimental control group in one key respect: conditional on X, the distribution of the counterfactual outcome, Y^0, of the participants is the same as the observed distribution of the outcome Y^0 of the comparison group (Heckman, LaLonde, and Smith). In the method of matching, the construction of the correct sample counterpart (for the missing information on the treated outcomes had they not participated) consists in pairing each programme participant with one or more members of a comparison group (Blundell and Costa Dias, 2002). Therefore, the matching approach allows to compare treated and non-treated outcomes directly without having to impose structure on the problem. This is the analogy to random assignment in a (social) experiment.

An advantage of the method of matching is its generality due to the non-parametric nature of the approach. Therefore, no particular distribution has to be assumed. Furthermore, it is highly flexible as it may be combined with other methods to produce more accurate estimates of the treatment effects allowing for less restrictive assumptions.[17] However, since matching methods concern themselves solely with selection of observable variables to solve the selection problem, they require very rich data in order to make the estimates of the treatment effects credible (Smith, 2000a).

[16] See, e.g., Rubin (1974; 1977; 1979; 1991), Rosenbaum and Rubin (1983b; 1985), and the overview by Rosenbaum (2002). However, the idea of matching is not new. Heckman, Ichimura, Smith, and Todd (1998) note that the method of matching was first used by Fechner (1860).

[17] One example is the so-called conditional DiD suggested by Heckman, Ichimura, and Todd (1997) that combines matching and the DiD estimator.

3.3.2 Identifying Assumptions

The key assumption in the statistical matching literature for the construction of a valid comparison group is that conditional on all relevant pre-treatment characteristics, X, the potential outcomes, Y^1, Y^0, are independent of the treatment assignment, D (see Rubin, 1977). In the notation of Dawid (1979), this is

Assumption 1 *Conditional Independence Assumption:*

$$Y^0, Y^1 \amalg D|X, \tag{3.32}$$

where $\amalg$ denotes independence, and X are covariates that are unaffected by the treatment. As a consequence of ass. 1, the distributions of outcomes

$$F(Y^0|X, D = 1) = F(Y^0|X, D = 0) = F(Y^0|X) \tag{3.33}$$

and

$$F(Y^1|X, D = 1) = F(Y^1|X, D = 0) = F(Y^1|X) \tag{3.34}$$

are independent of the treatment assignment (Heckman, LaLonde, and Smith, 1999). Furthermore, to guarantee that a match can be found for all participants and non-participants, it has to be assumed that there are treated and untreated individuals for each relevant X, i.e. to ensure that ass. 1 has an empirical content.

Assumption 2 *Common Support Assumption:*

$$0 < Pr(D = 1|X) < 1. \tag{3.35}$$

Ass. 2 implies that there is an overlap in the distribution of X between the treated and the non-treated group. Furthermore, it prevents X to be a perfect predictor for treatment or non-treatment respectively. Failure to the common support assumption would lead to biased estimates of the treatment impact as it cannot be identified for all values of X (Heckman, LaLonde, and Smith, 1999). In that case, matching can only be performed within the common support of treated and non-treated individuals. In consequence, the estimated ATT has then to be re-defined for those treated falling in the common support (Blundell, Dearden, and Sianesi, 2004).[18]

Whether ass. 1 and 2 are plausible or not in economic settings has raised some discussion recently. The critical question is that the optimising behaviour of the decision makers, e.g., the individual or the caseworkers, precludes their choices being independent of the potential outcomes. Imbens (2004) presents three arguments concerning the reliability of the assumptions. These arguments comprise statistical, data-descriptive, and empirical questions as well as the occurrence of selection on unobservables. First, as the natural starting point for any evaluation is the comparison of average outcomes for treated and non-treated individuals, the quality of the

[18] It has to be noted that ass. 1 and 2 are not specific to the matching estimator, but apply to all non-experimental evaluation estimators that condition on exogenous covariates.

comparison may be enhanced by adjusting away any difference in outcomes for differences in exogenous attributes, where attributes are exogenous in the sense that they are not affected by treatment. Although this may not lead to the final word on efficacy of the treatment, its absence would seem difficult to rationalise if one seriously attempts to understand the evidence regarding the impact of the treatment (Imbens). Second, the empirical question of the evaluation asks which individuals should be compared. Therefore, economic theory on the decision process of treatment may provide some guidance in choosing the variables that need to be adjusted for versus those that do not need to be adjusted for. Ass. 1 is fulfilled if the researcher observes all variables that need to be adjusted for (relevant covariates). However, if variables that are needed to be adjusted for are not observed, strong assumptions will be required for the identification of the effects of interest. Third, even when agents optimally choose their treatment, two agents with the same values for observed characteristics may differ in their treatment choices. The unconfoundedness assumption must not be invalidated in this case if the differences in the choice are driven by unobserved factors that are themselves unrelated to the outcomes of interest. This may be the case if the objective of the potential participant to participate is distinct from the outcome that is of interest for the evaluator.

This third argument is in line with the discussion of Heckman, LaLonde, and Smith (1999) about the validation of the outcomes. Since different persons may value the same state of the world differently even if they experience the same 'objective' outcomes, this must be considered in the economic model. A good example is a programme that is in part due to paternalistic or altruistic preferences. In that case, allocation of individuals may be guided by equity concerns, whereas evaluation may focus on programme efficiency. While the efficiency criterion focusses on maximising the social return to a public programme investment, i.e. it concentrates on groups for whom the impact is largest, the equity criterion aims at groups who are most in 'need of services'. In chapter 6, I will analyse this aspect empirically.

Ass. 1 and 2 have been termed 'strong ignorability' by Rosenbaum and Rubin (1983b). However, if the interest is in average effects only, ass. 1 is overly strong and could be substituted by the conditional mean independence that suffices identification of the parameters, i.e.

Assumption 3 *Conditional Mean Independence Assumption:*

$$E(Y^0|X, D = 1) = E(Y^0|X, D = 0) = E(Y^0|X) \tag{3.36}$$

and

$$E(Y^1|X, D = 1) = E(Y^1|X, D = 0) = E(Y^1|X). \tag{3.37}$$

However, ass. 1 and 3 allow to identify all kinds of average treatment effects, including the ATT and the ATU. If interest is in the ATT only, the assumptions can be weakened. Since the aim is to generate the counterfactual term $E(Y^0|X, D = 1)$, no conditional independence has to be imposed between D and Y^1.[19] The conditional independence assumption for the ATT is then given by

[19] The term $E(Y^1|X, D = 0)$ would be required for the identification of the ATU.

Assumption 4 *Conditional Independence Assumption for ATT:*

$$Y^0 \amalg D|X. \tag{3.38}$$

That is, conditional on a set of observable variables X, the non-participation outcome, Y^0, is independent of the participation decision D. Since the sole parameter of interest is the mean impact of treatment on the treated and not the impact on the distribution, the analogue to ass. 3 for the ATT is defined as

Assumption 5 *Conditional Mean Independence Assumption for ATT:*

$$E(Y^0|X, D = 1) = E(Y^0|X, D = 0) = E(Y^0|X). \tag{3.39}$$

Furthermore, ass. 2 is also not required because this condition guarantees the possibility of a participant for each non-participant as well as a non-participant for each participant. All that is required for the ATT is the non-participant analogue for each participant (Smith and Todd, 2005a). This is

Assumption 6 *Common Support Assumption for ATT:*

$$Pr(D = 1|X) < 1. \tag{3.40}$$

If ass. 5 and 6 hold, the ATT (eq. (3.3)) can be rewritten for the matching estimator following Smith and Todd as

$$\begin{aligned} \Delta^{ATT}_{MAT} &= E(Y^1 - Y^0|D = 1) \\ &= E(Y^1|D = 1) - E_{X|D=1}\{E_Y(Y^0|X, D = 1)\} \\ &= E(Y^1|D = 1) - E_{X|D=1}\{E_Y(Y^0|X, D = 0)\}. \end{aligned} \tag{3.41}$$

The first term, $E(Y^1|D = 1)$, can be estimated from the observed outcomes of the treated individuals; the second term, $E_{X|D=1}\{E_Y(Y^0|X, D = 0)\}$, can be estimated from the observed outcomes of the (conditional on the X for the treated) matched non-treated.[20]

3.3.3 Balancing Property of the Propensity Score

It is well known that matching on X can become hazardous when X is of high dimension ('curse of dimensionality', see, e.g., Pagan and Ullah, 1999). To deal with this dimensionality problem, Rosenbaum and Rubin (1983b) suggest to use balancing scores $b(X)$. Balancing scores are functions of the relevant covariates X, such

[20] The idea of conditioning on X to eliminate selection bias may also justify linear regression. However, two drawbacks of this method relative to matching have to be noted. First, matching is a non-parametric method and therefore does not require any parametric assumption, like the linearity implicit in linear regression. Second, matching emphasises the common support problem, whereas in analyses that estimate impacts simply by running regressions on X, the issue is rarely even investigated (Smith, 2000a).

that the conditional distribution of X given $b(X)$ is independent of assignment to treatment, i.e. the same for the treated and the non-treated individuals

$$X \text{ II } D|b(X). \tag{3.42}$$

This means that for treated and non-treated individuals with the same balancing score the distributions of the covariates X are balanced across the treated and the non-treated group. One possible balancing score is the probability of participating in a programme, i.e. the propensity score $p(X) = E(D = 1|X)$ that summarises the information of the relevant covariates X into a single index function. Therefore, all biases due to observable covariates can be removed by conditioning solely on the propensity score. The proof of Rosenbaum and Rubin (1983b) is condensed by Smith and Todd (2005a) to:

$$E(D|Y, p(x)) = E(E(D|Y, X)|Y, p(X)), \tag{3.43}$$

so that

$$E(D|Y, X) = E(D|X) = p(X) \tag{3.44}$$

implies

$$E(D|Y, p(X)) = E(D|p(X)). \tag{3.45}$$

Eq. (3.43) to (3.45) show that if the non-participation outcomes Y^0 are independent of the participation decision conditional on X, they are also independent conditional on the propensity score, $p(X)$.[21] Therefore it is sufficient to rewrite ass. 5 as

Assumption 7 *Conditional Mean Independence Assumption for ATT using the propensity score:*

$$E(Y^0|p(X), D = 1) = E(Y^0|p(X), D = 0) = E(Y^0|p(X)). \tag{3.46}$$

In consequence, the matching estimator based on the propensity score is defined as

$$\begin{aligned} \Delta_{MAT}^{ATT} &= E(Y^1 - Y^0|D = 1) \\ &= E(Y^1|D = 1) - E_{p(X)|D=1}\{E_Y(Y^0|p(X), D = 1)\} \\ &= E(Y^1|D = 1) - E_{p(X)|D=1}\{E_Y(Y^0|p(X), D = 0)\}. \end{aligned} \tag{3.47}$$

The outer expectation of the second term is taken over the distribution of the propensity score in the treated population. When the propensity score, $p(X)$, is known, the curse of dimensionality for the X can be eliminated; and solving the fundamental evaluation problem requires only to pair treated and non-treated individuals who have the same $p(X)$ as this balances the distributions of X across groups.

When the propensity score is unknown, it could be estimated by parametric, semi-parametric or non-parametric methods. However, non-parametric estimation is not preferable since the curse of dimensionality will reappear in the estimation of

[21] That is, Y^0 II $D|p(X)$ in analogy to ass. 4.

the propensity score. Therefore, much of the empirical literature uses probit or logit models.[22] Smith and Todd (2005a) note that much of the recent focus on propensity score matching methods results from the potential for reducing the dimensionality of the problem.

Following from the decomposition of the selection bias in section 3.2.3 according to Heckman, Ichimura, Smith, and Todd (1996;1998), it becomes obvious that the matching estimator (whether conditional on X or on $p(X)$) is able to eliminate two of the three sources of selection bias. By forcing the estimation to the region of common support, the bias B_1 (eq. (3.21)) due to outlying observations is ruled out. Furthermore, the bias due to misweighting, B_2 (eq. (3.22)), is circumvented since matching re-weights the non-participant data according to the distribution of the participants' data. Only the bias B_3 (eq. (3.23)) cannot be controlled for with the matching estimator. However, if one is able to observe all relevant covariates, i.e. there are no unobservable or unmeasured factors that jointly determine participation and outcome, there should be no selection bias left.

3.3.4 Possible Matching Estimators

The literature provides a variety of alternative matching schemes to estimate the treatment effects. In this section, I will introduce the schemes applied in the empirical analyses in chapters 5 and 6. For a further detailed discussion the interested reader is referred to the overviews by Heckman, LaLonde, and Smith (1999) and Imbens (2004). Furthermore, see Heckman, Ichimura, and Todd (1997;1998) for some additional estimators and their properties respectively, e.g., kernel matching or local polynomial matching, and Smith and Todd (2005a).

As noted above, the idea of the matching estimator is to find for each treated individual i comparable persons j from the comparison group. Let N_1 denote the number of treated individuals $(D = 1)$ and N_0 the number of comparison individuals $(D = 0)$. Matches are constructed based on a neighbourhood $C(p(X_i))$, where $p(X_i)$ is the propensity score for individual i.[23] Possible matches (neighbours) to treated person i are persons j in the comparison sample whose propensity scores are in the neighbourhood $C(p(X_i))$, i.e. $p(X_j) \in C(p(X_i))$. The persons matched to individual i are those in the set A_i, where $A_i = \{j \in D = 0 | p(X_j) \in C(p(X_i))\}$ (Smith and Todd, 2005a). With $0 \leq W(i,j) \leq 1$ defining the weight placed on the non-treated observation j for forming a comparison with observation i, the general form of the matching estimator for the ATT is given by

[22] Rosenbaum (1986) uses a linear probability model (LPM) but states that logistic regression models are preferable for the well-known shortcomings of the LPM, especially the unlikeliness of the functional form when the response variable is highly skewed as well as predictions that are outside the $[0, 1]$ bound of probabilities.

[23] Alternatively, the neighbourhood can be defined as $C(X_i)$, where X_i is a vector of attributes for individual i. Since I use propensity score matching to evaluate the effects of job creation schemes on regular employment, I use the propensity score representation for the discussion.

$$\Delta_{MAT}^{ATT} = \frac{1}{N_1} \sum_{i \in \{D=1\}}^{N_1} \left[Y_i^1 - \sum_{j \in \{D=0\}}^{N_0} W(i,j) Y_j^0 \right]. \tag{3.48}$$

The weights always satisfy $\sum_{i \in \{D=0\}}^{N_0} W(i,j) = 1 \ \forall \ i$, i.e. the total weight of all comparisons sums up to one for each treated individual. The different matching estimators vary in the weights attached to the members of the comparison group (Heckman, Ichimura, Smith, and Todd, 1998).

Nearest-Neighbour Matching

The most popular matching scheme is the traditional pair-wise matching, i.e. the so-called single nearest-neighbour (NN) matching without replacement. It sets the neighbourhood as

$$C^{NN}(p(X_i)) = \min_j \|p(X_i) - p(X_j)\|, \text{ for } j \in D = 0, \tag{3.49}$$

where $\|(\cdot)\|$ is obtained through a distance metric. Doing so, the non-participant with the value of $p(X_j)$ that is closest to $p(X_i)$ is selected as the match and A_i is a singleton set. Therefore:

$$W^{NN}(i,j) = \begin{cases} 1 \text{ if } \|p(X_i) - p(X_j)\| = \min_j \|p(X_i) - p(X_j)\|, \\ 0 \text{ otherwise.} \end{cases} \tag{3.50}$$

Without imposing any common support condition, each non-treated observation $(D = 0)$ could serve as a match for at most one treated observation $(D = 1)$. Two possible problems of NN matching without replacement should be noted. First, if the distributions of the propensity scores differ across groups, e.g., if there are many participants with high values of $p(X_i)$ and only a few non-participants with a $p(X_j)$ similar to those values, this will result in bad matches. Second, the estimates of NN matching without replacement depend on the ordering of the individuals. Therefore, it has to be ensured that the ordering of the individuals is random before matching.

To overcome the problem of bad matches, another variant is NN matching with replacement. In that case, a non-treated observation could be used more than once as a match for a treated observation. However, although NN matching with replacement may reduce the bias of the estimator due to an increase of the average quality of the matches, it may also imply an increased variance of the estimator because the number of distinct non-treated individuals used to construct the counterfactual mean is reduced. Thus, there is a trade-off between bias and variance that should be considered when choosing NN matching with replacement (Smith and Todd, 2005a).

Alternatively, one can use multiple nearest neighbours to construct the counterfactual mean. Here, m distinct non-treated individuals j are matched as a comparison to individual i, i.e. the so-called m-NN matching or oversampling. In that case, one has to decide which weight to attach to each of the neighbours. The easiest way is to assume equal weights for the comparisons (see, e.g., Smith and Todd, 2005a). Then, each of the m comparison individuals in set A_i receives the weight $1/m$, i.e.

$$W^{m-NN}(i,j) = \begin{cases} \frac{1}{m} \text{ if } j \in A_i, \\ 0 \text{ otherwise.} \end{cases} \tag{3.51}$$

A drawback of this weighting scheme is that comparison individuals are weighted independently of the distance. Therefore, Davies and Kim (2004) suggest to use triangular weights where the neighbours are ordered according to the distance of the scores. Let $\rho = 1$ denote the closest neighbour and $\rho = m$ the neighbour that is farthest away, the weights are defined as

$$W^{m-NN}(i,j) = \begin{cases} \frac{2(m-\rho+1)}{m(m+1)} \text{ if } j \in A_i, \\ 0 \qquad\quad \text{otherwise.} \end{cases} \tag{3.52}$$

Since more information is used by m-NN matching to construct the counterfactual mean relative to NN matching, this reduces the variance of the estimates. However, as m-NN matching has to deal with, on average, poorer matches, the variance reduction is paid for by an increased bias of the estimates (Smith and Todd, 2005a).

If the propensity scores of participants and non-participants are far away, NN matching faces the risk of bad matches. Therefore, Cochran and Rubin (1973) propose a variant to circumvent the problem, the so-called calliper matching. The idea is to impose a tolerance on the distance metric of the propensity scores between treated and non-treated individuals, i.e.

$$\|p(X_i) - p(X_j)\| < \epsilon, j \in D = 0, \tag{3.53}$$

where ϵ is a pre-specified level of tolerance that determines the maximum distance for potential matches. Accordingly, the weights for calliper matching (CM) are

$$W^{CM}(i,j) = \begin{cases} 1 \text{ if } \|p(X_i) - p(X_j)\| \\ \qquad = \min\limits_j \|p(X_i) - p(X_j)\| \wedge \|p(X_i) - p(X_j)\| < \epsilon, \\ 0 \text{ otherwise,} \end{cases} \tag{3.54}$$

and the neighbourhood is defined as $C(p(X_i)) = \{p(X_j)|\ \|p(X_i) - p(X_j)\| < \epsilon\}$. As Smith and Todd (2005a) note, if there are treated observations for whom the neighbourhood is empty, these individuals can be excluded from analysis since calliper matching is one way of imposing a common support condition. However, as the calliper has to be pre-defined, a priori knowing what tolerance level is reasonable may be difficult. Calliper matching can be extended for the use of all non-treated observations within the calliper, i.e. the so-called radius matching suggested by Dehejia and Wahba (2002). Caliendo et al. (2006a) analyse the sensitivity of the estimated treatment effects to different variants of NN matching with a large sample of participants and non-participants (see chapter 2.4.2 for a summary). The results show that the estimates are relatively insensitive to the choice of the algorithm.

Stratification Matching

An alternative to the NN matching discussed above is the so-called stratification matching (SM) on the propensity score suggested by Rosenbaum and Rubin

(1983b).[24] In the stratified matching approach, the full sample is divided into M strata of units of approximately equal probability of treatment based on the (estimated) propensity score.[25] Within each stratum, the average treatment effect is calculated as the mean difference of treated and non-treated outcomes. That is, the procedure implicitly assumes that within the strata the situation is similar as if random assignment holds. I will use this approach in chapter 6 to analyse whether a higher propensity score, i.e. a higher probability of treatment, correlates with a higher impact of the programme. Following Imbens (2004), I define J_{im} as an indicator for unit i being in stratum m, with

$$J_{im} = 1\left\{\frac{m-1}{M} < p(X_i) \leq \frac{m}{M}\right\} \text{ for } m = 1, \ldots, M. \tag{3.55}$$

To do so, Imbens suggests to divide the unit interval into M strata with boundary values equal to m/M for $m = 1, \ldots, M-1$.[26] However, the distribution of the estimated propensity score could have its maximum below the unit value. Therefore, I will use the range of the distribution of the estimated propensity score for the participants as the base for the division into M strata. Stratification leaves me with N_{dm} observations with treatment equal to d, $N_{dm} = \sum_i 1\{D_i = d, J_{im} = 1\}$ with $d \in \{0, 1\}$. The within-stratum average treatment effects are given by

$$\Delta_{SM}^{m} = \frac{1}{N_{1m}} \sum_{i=1}^{N} J_{im} D_i Y_i - \frac{1}{N_{0m}} \sum_{i=1}^{N} J_{im}(1 - D_i) Y_i. \tag{3.56}$$

The ATT is estimated by weighting the within-stratum average treatment effects by the number of treated units:

$$\Delta_{SM}^{ATT} = \sum_{m=1}^{M} \Delta_{m}^{SM} \cdot \frac{N_{1m}}{N_1}. \tag{3.57}$$

Thus, stratification matching can be seen as a crude form of non-parametric regression where the unknown function is approximated by a step function with fixed jump points (Imbens, 2003). However, although the implementation of the approach is fairly simple, determining the number of strata to be used may be difficult in the empirical analysis. The example of Cochran (1968) with a single covariate and assuming normality has shown that five strata could be enough to remove 95 percent of the bias. Imbens (2004) notes that all bias is associated with the propensity score under the conditional independence assumption and therefore, under normality the use of five strata should remove most of the bias associated with all covariates. Five strata have also been proposed by Rosenbaum and Rubin (1983a).

[24] Stratification matching is also termed interval matching, blocking or subclassification (Rosenbaum and Rubin, 1983b).

[25] With an sufficiently large M, stratification matching is close to the weighting estimator. See Imbens (2004) for further details.

[26] By doing so, the boundaries of the strata are strictly greater than 0 and below 1.

Hence, the use of five strata provides a good starting point of the empirical analysis. What has to be checked is if the covariates within each stratum are balanced across groups. If the true propensity score per stratum is constant, the distribution of the covariates among treated and non-treated individuals should be identical (Imbens, 2004). The adequacy of the statistical model can be checked by comparing the covariate distributions among treated and non-treated individuals within strata. If the distributions of the covariates are unbalanced among groups within strata, there are two ways to solve the problem. First, assuming that strata are too large they have to be split. For example, Aakvik (2001) chooses ad hoc twelve sub-groups in his application. Second, one can generalise the propensity score. Imbens (2004) suggests an informal algorithm to be used: if within a stratum the propensity score itself is unbalanced, the number of strata has to be increased. If, conditional on the propensity score being balanced, the covariates are unbalanced, the propensity score model has to be re-considered as it is not adequate to balance the covariate distributions among treated and non-treated individuals within strata. Dehejia and Wahba (1999) propose adding higher order or interaction terms to re-specify the propensity score.

3.4 Evaluation Approach – Extension to the Dynamic Setting

3.4.1 Consideration of the Timing of Treatment

Up to now, I have discussed the evaluation approach for the static binary treatment case, i.e. treatment is exposed once and at one specific point of time only. In that case, those individuals who take the treatment are defined as the participants; all others are non-participants. Simplifying the evaluation problem that way may be reasonable for social experiments. In contrast, for most regular ALMP programmes this approach may concur rather poorly (cf. Fredriksson and Johansson, 2004). As I have described in chapter 2, there is a comprehensive system of ALMP programmes in Germany. This system is characterised by a wide array of different ongoing programmes which take place continuously over time and are open to job-seekers who meet the differing eligibility criteria. For that reason, job-seekers can participate in a programme at different points of time in the unemployment spell. Furthermore, for some programmes, such as the job creation schemes, unemployment is in general a pre-condition for participation. Therefore, the starting point of the programme within the individual unemployment spell may be an important determinant for the type of programme an individual is assigned to as well as for the selectivity of the participating individuals. Moreover, the calendar time of the treatment also affects the assignment process because of changing budget constraints within the calendar year or changes in the focus of the policy interventions from one year to another (cf. Speckesser, 2004).

Thus, considering the timing of events is important when evaluating the effects of ALMP programmes. This importance has been recently reflected in the empirical literature. For example, the timing of events methodology lies at the heart of the contemporary evaluation literature using duration models. Abbring and van den Berg

(2003) have shown that the timing of the programme conveys useful information that allows a non-parametric identification of the treatment effect within a multivariate mixed proportional hazards (MMPH) model.[27] Explicit consideration of the timing of treatment has become more important in studies utilising the method of matching to evaluate ALMP programmes as well. In Sianesi's studies on the efficiency of Swedish ALMP (see Sianesi, 2002; 2003; 2004) it is emphasised that within the Swedish system (similar to the German system) an unemployed person will join a programme at some time, provided the individual remains unemployed long enough. Consequently, the reason why an unemployed individual is not observed to participate in a programme is that the person has found a job before or that the time horizon of the analysis is too short. Hence, this has serious implications for the choice of the comparison group and the econometric evaluation estimator. If one chooses those individuals as the comparison group who have been observed never to participate in the data, there is an implicit conditioning on future outcomes which may invalidate the conditional independence assumption. The conditioning on future outcomes may furthermore bias the estimates. To give an example: If all individuals who have never been on a programme within the observation window and for whom a transition into employment is observed are selected as the comparison group, the true treatment effects may be underestimated because one can assume that this group contains a large number of individuals who were never intended to be treated because they have a per se higher probability to become regularly employed.

For that reason, participation and non-participation has to be defined dynamically, i.e. with respect to the point of time the comparison should be made. According to Sianesi (2004), I define persons who have neither entered a programme nor left unemployment up to a specific point in time as non-participants of interest or 'waiters' (in the sense that they are waiting to be allocated to a programme). Thus, non-participation can be interpreted as the default state for each individual, and everybody is a non-participant until entering a programme or leaving for employment. In this context it should be noted that the state to which programme participants are compared to is in fact none of being completely left on one's own to look for a regular job. It is rather the state of the baseline services provided by the LEA since being registered as a job-seeker gives access to the various employment services offered by the offices, e.g., counselling etc. (see Sianesi, 2002). Furthermore, individuals who are defined as non-participants in the moment I start my comparison may enter a programme at a later point of time.

[27] There is a growing literature applying this method to evaluate the effects of ALMP programmes. For example, Hujer, Thomsen, and Zeiss (2006b) apply a MMPH model to estimate the effects of vocational training programmes in Eastern Germany. Hujer, Thomsen, and Zeiss (2006a) analyse the effects of short-term training measures on the duration of unemployment in West Germany. Similar approaches have been applied in a number of studies for other countries, like Lalive, van Ours, and Zweimüller (2002) for Switzerland, Richardson and van den Berg (2001) for Sweden, Bonnal, Fougere, and Serandon (1997) for France, and van Ours (2001) for Slovakia. The interested reader is also referred to the comprehensive survey on the methodology by van den Berg (2001).

The approach has also been used in the study by Steiger (2004) which evaluates the effects of different ALMP programmes in Switzerland. Speckesser (2004) and Fitzenberger and Speckesser (2005) apply a matching approach that takes the timing of the programme into account to evaluate the effects of a programme called *provision of specific professional skills and techniques* in Germany. A similar definition of non-participation is used by Brodaty, Crépon, and Fougere (2001) who focus on the effects of youth employment programmes in France. Fredriksson and Johansson (2004) try to formalise this idea and to connect the matching approach to the concept of duration models.

In contrast, several studies only use individuals who have never participated within the observation window as the comparison group, for example, Gerfin and Lechner (2002) or Lechner et al. (2005a; 2005b). To overcome the problem of comparing participating individuals to non-participants who were never intended to be treated, they apply an approach suggested by Lechner (1999) first. In this approach, each comparison individual is assigned a random starting date by drawing from the discrete distribution of the estimated starting dates of the participants. All non-participants who are already employed at the time of the hypothetical starting date are excluded from the analysis. However, this approach adds additional noise to the data and does not take the timing of events seriously (Fitzenberger and Speckesser, 2005). Moreover, since the observation window is generally limited the observable distribution of the starting dates will be truncated. Thus, imposing the starting date distribution to the non-participants by random drawing may be biased (Fredriksson and Johansson, 2004). For these reasons, the approach is not feasible for this empirical analysis.

3.4.2 Evaluation Approach in the Dynamic Setting

To formalise the evaluation approach in the dynamic setting, i.e. when the timing of treatment is considered explicitly, I will introduce some additional notation. Let $U = \{0, \ldots, U_{\max}\}$ define the discrete elapsed unemployment duration of the individual since registration at the LEA. Furthermore, let u denote the point of time in the unemployment spell where the programme of interest starts and D_u the treatment indicator with the discrete time index. $D_u = 1$ if the individual starts a programme at time u of the unemployment spell, $D_u = 0$ if the individual remains unemployed at u. Programme effects are estimated for time t with $t \geq u$, i.e. the time after the programme has started. The hypothetical outcomes for time t given a treatment at time u are then defined as $Y^1_{t,u}$ for individuals who received the treatment at u and $Y^0_{t,u}$ for individuals, who have not received the treatment at least up to time u.

The parameter of interest for each u is the average effect in t for individuals starting a programme in the u^{th} month of their unemployment spell of joining the programme at u compared to not joining at u, i.e. waiting longer in open unemployment. This is (in analogy to eq. (3.3)):

$$\begin{aligned}\Delta^{ATT}_{t,u} &= E(Y^1_{t,u} - Y^0_{t,u}|D_u = 1, D_1 = \cdots D_{u-1} = 0) \\ &= E(Y^1_{t,u}|D_u = 1, D_1 = \cdots D_{u-1} = 0) \\ &\quad -E(Y^0_{t,u}|D_u = 1, D_1 = \cdots D_{u-1} = 0). \end{aligned} \tag{3.58}$$

Alike in the static approach, the first term is identified in the data by the observed outcome of the participants. For the second expectation to be identified, I can invoke an adjusted version of the conditional independence assumption. That is, the hypothetical outcome at time t after not participating up to time u is independent of a programme participation at time u, conditional on a set of observed characteristics X_u or the propensity score $p(X_u)$ measured at time u. By use of the propensity score, this dynamic version of the conditional independence assumption is defined as:

Assumption 8 *Dynamic Conditional Independence Assumption for ATT:*

$$Y^0_{t,u} \amalg D_u|p(X_u), D_1 = \cdots = D_{u-1} = 0. \tag{3.59}$$

Since the parameter of interest is the average effect only, all that is required for the ATT in the dynamic setting is the weaker version of this assumption, namely the:

Assumption 9 *Dynamic Conditional Mean Independence Assumption for ATT:*

$$\begin{aligned}&E(Y^0_{t,u}|p(X_u), D_u = 1, D_1 = \cdots = D_{u-1} = 0) \\ &= E(Y^0_{t,u}|p(X_u), D_1 = \cdots = D_u = 0). \end{aligned} \tag{3.60}$$

Ass. 9 states that treated and non-treated individuals are comparable in their non-treatment outcomes at time t conditional on $p(X_u)$, conditional on being unemployed up to time $u-1$, and conditional on not receiving treatment before u. If this assumption holds, the parameter of interest could be estimated by propensity score matching in the following way:

$$\begin{aligned}\Delta^{ATT}_{t,u_{(MAT)}} &= E(Y^1_{t,u}|p(X_u), D_u = 1, D_1 = \cdots = D_{u-1} = 0) \\ -E_{p(X_u)|D_u=1,D_1=\cdots=Du-1=0} &\left\{E_Y(Y^0_{t,u}|p(X_u), D_1 = \cdots = D_u = 0)\right\}. \end{aligned} \tag{3.61}$$

In analogy to the ATT in the static setting, the second term approximates the participants outcome in t of joining a programme in u by the outcome of the comparable non-participants in u. This approach has been also suggested by Li, Propert, and Rosenbaum (2001) as the so-called optimal balanced risk set matching. The term risk set matching is used since the definition of the comparison individuals is similar to the risk set that arises in the partial likelihood associated with Cox's proportional hazards model (see Cox, 1972, 1975). However, in contrast to the partial likelihood

that compares an individual who experiences an event at time u to all other individuals at risk of the event at time u, matching pairs a treated person at time u with a similar untreated person at time u, but at risk of treatment at time u.[28]

Considering the timing of treatment has also an implication for the interpretation of the ATT. Whereas the ATT in the static setting was defined as the difference of the average outcomes of the treated and the hypothetical average outcome had they not been treated, the adjusted version is defined as the ATT conditional that the unemployment spell has lasted at least until the start of the comparison. This parameter is of interest in a situation where caseworkers and individuals meet at regular intervals, e.g., on a monthly basis, and decide on each meeting whether to start a treatment in the next interval or whether to postpone it to the future (Sianesi, 2004). Therefore, by interpreting the results one has to bear in mind that the chosen comparison group does not reflect a no-programme state, but rather possibly postponed participation. In addition, Sianesi suggests to estimate a combined effect for a synthetic overview of the effectiveness of the programme. This estimator is the sum of the ATT for the different u, weighted according to the observed placement distribution of the treated at each u. However, whereas the effects for the single u can be interpreted as causal, the overall effect (or 'composite effect', see Steiger, 2004) cannot be interpreted this way.[29]

A last aspect to be noted in this section is that individuals are not allowed to anticipate future treatments as well as future labour market outcomes. Anticipatory effects of a treatment are present if, for example, those individuals who are informed about a future ALMP programme reduce their search activity in order to wait for the treatment. Anticipatory effects of future employment may occur if the individuals know that the former employer is going to call them back. In that case, the person is likely to have no or less incentives to participate in a programme at any given month in unemployment (Sianesi, 2004). However, as noted by Abbring and van den Berg (2003), the exclusion of anticipatory effects does not rule out that the individuals know and act on the determinants of the assignment to treatment or labour market outcomes, i.e. individuals are allowed to adjust their optimal behaviour to the determinants of the treatment process, but not to the realisations of the treatment. This is not a problem for the analysis as long as treated and non-treated individuals anticipate the chances of these events conditional on $p(X_u)$ and the elapsed unemployment duration in u in the same way (Fitzenberger and Speckesser, 2005).

3.5 Some Aspects of Implementation

In this section, I will briefly discuss some aspects of the implementation of the estimators for the empirical analyses. In particular, I will consider the choice of the

[28] Matching or sampling from a risk set for the time up to an outcome event has been discussed already by Prentice and Breslow (1978), Oakes (1981), and Prentice (1986), but not for the time up to treatment (see Li et al., 2001).

[29] See also the discussion in Fredriksson and Johansson (2004).

propensity score model and the selection of variables to be included, possible ways to check the common support assumption, the choice of the adequate matching algorithm, the estimation of the standard errors and some further methodological issues dealing, e.g., with the time when to compare treated and non-treated individuals or the possible occurrence of locking-in effects.

3.5.1 Estimation of the Propensity Score

The key choices to be made for the estimation of the propensity score refer to the econometric model and the selection of variables to be included in the model. These choices are to some extent independent of applying propensity score matching in the static or the dynamic setting. I have mentioned above that the propensity score could be estimated in a non-, semi- or parametric way. As one key aspect of using the propensity score is to prevent the curse of dimensionality, preference should be given to semi-parametric or parametric approaches. Thus, any discrete choice model, like the logit or probit models, could be used. Unfortunately, the literature provides little advice concerning which functional form to use.[30] In the binary treatment case, both models yield similar results and the choice is not too critical, even though the logit distribution has more density mass in its bounds.[31]

The selection of the variables to be included in the model should be adjusted to the main purpose, namely to achieve the conditional independence assumption. That is, all variables have to be considered that jointly influence the participation decision and the outcome variable.[32] Omitting important variables may result in a seriously increased bias of the estimates (Heckman, Ichimura, and Todd, 1997). The set-up of the model should be guided by economic theory, knowledge of previous empirical studies as well as of the institutional setting of the programme of interest (Smith and Todd, 2005a). Moreover, no variables that are affected by treatment themselves, like intermediate outcomes, should be included in the model. A simple way to ensure this is to use variables that are fixed over time or that are measured before the treatment. However, for the latter case it has to be ensured that the variables have not been influenced by anticipation of the treatment.

A further aspect to be regarded in this context is the reliability of the data. As Heckman, Ichimura, Smith, and Todd (1998) point out, data on the treated and non-treated individuals should originate from the same source, i.e. both groups should be

[30] See, for example, the discussion in Smith (1997).

[31] For the multiple treatment case, the choice of the model becomes more important. For example, whereas the multinomial logit model requires strong assumptions (independence of irrelevant alternatives), the more flexible multinomial probit is computationally burdensome. See the discussion in Lechner (2001).

[32] Some variables have reached a notable importance in the applied literature. From the accumulated evidence of the evaluation of labour market policy programmes, in particular the labour market history of the individual (see, e.g., Heckman, Ichimura, and Todd, 1997) and the regional labour market environment (see, e.g., Heckman, Ichimura, Smith, and Todd, 1998) are especially important. Furthermore, economic theory suggests to control for qualification level and work tenure as proxies for the reservation wage.

administered the same questionnaire. The value of rich and informative data is essential for credibly justifying the conditional independence assumption (Smith, 2000a). But, there must also be some randomness in the data that guarantees that persons with identical variables can be observed both in the treatment and the non-treatment state (Heckman, Ichimura, and Todd, 1998). If there would be information in the data that perfectly predicts treatment or non-treatment respectively, the common support assumption will not hold. Consequently, matching brakes down if there is too much information and other methods must be used to evaluate the programme, e.g., the regression discontinuity design estimator (Heckman, LaLonde, and Smith, 1999).

In cases of uncertainty of the proper specification, a particular concern may be about including too many than rather too few variables. The empirical literature is ambiguous about this point. On the one hand, inclusion of extraneous variables in the participation model may exacerbate the support problem. And although the inclusion of insignificant variables will not bias the estimates, it may increase the variance (see Bryson, Dorsett, and Purdon, 2002, and Augurzky and Schmidt, 2000). On the other hand, Rubin and Thomas (1996), for example, suggest to include the relevant variables in the propensity score estimation and that a variable should only be excluded from analysis if there is consensus that the variable is either unrelated to the outcome or not a proper covariate. By these criteria, there are both reasons for and against including all covariates available. Furthermore, there are some statistical tests suggested in the empirical literature that can be used to select the variables. With the 'hit or miss' method or prediction rate metric (suggested by Heckman, Ichimura, Smith, and Todd, 1998 and Heckman and Smith, 1999), variables are chosen to maximise the correct within-sample prediction rates, assuming that the costs of misclassification are equal for the two groups (Heckman, Ichimura, and Todd, 1997). An observation is valued as '1' if the estimated propensity score is larger than the sample proportion of persons taking treatment, i.e. $\hat{p}(X) > \overline{p}$, and else as '0'. An alternative approach is based on the statistical significance of the variables. Starting with a parsimonious specification of the model, variables are iteratively added and will be kept if they are statistically significant at conventional levels. This approach may also be combined with the 'hit or miss' method. In that case, variables are kept if they are statistically significant and increase the prediction rates by a substantial amount (Heckman, Ichimura, Smith, and Todd). However, the methods are based on goodness-of-fit considerations rather than on theory or evidence about the set of variables related to the participation decision and outcomes (Black and Smith, 2004).

In addition, some variables may be assumed to play a specifically important role in determining participation and outcome (Bryson et al., 2002). Accordingly, these variables should gain a greater emphasis in the analysis. This could be achieved by matching exactly on these variables in addition to the propensity score. In that case, the propensity score is used as a 'partial' balancing score which is complemented by the additional variables.[33] Alternatively, one can stratify the sample along the

[33] See, e.g., the study of Lechner (2002) who analyses the effects of ALMP programmes in Switzerland and complements the propensity score by sex, duration of unemployment and native language in the matching procedure.

variable that should be emphasised and estimate the effects for sub-populations. To give an example, if one assumes treatment effects to be heterogeneous by gender it is reasonable to estimate the effects for men and women separately. By doing so, one has to implement the complete matching procedure for each group.[34]

For the matching approach in the dynamic setting, the labour market history and the unemployment history of the individual in particular are assumed to be the most important explanations for the assignment to treatment. Similar to the considerations on matching on sub-populations, the sample is stratified along the unemployment duration into $U_{\max}$ discrete intervals, e.g., months. The propensity scores are estimated by a series of $U_{\max}$ binary probit or logit models, each one modelling the probability of joining a programme at time u, conditional on being unemployed up to $u-1$, conditional on X_u and on not having received a treatment up to time $u \in \{0,\ldots,U_{\max}\}$. Sianesi (2004) notes that this resembles a discrete hazard rate model with all estimated parameters allowed to be duration specific. It should be noted that u defines the unemployment duration until treatment. Therefore, the stratification may be applied to individuals who have the same duration of unemployment relative to the following treatment or non-treatment. Moreover, is is also possible to stratify the sample both on the specific unemployment duration and the calendar time to incorporate calendar time effects. However, in practice this will lead to very small samples (see, e.g., Speckesser, 2004, and Fitzenberger and Speckesser, 2005).

3.5.2 Checking the Common Support Assumption

As the ATT is only identified for the region of common support, an important issue of the implementation is to check the overlap between treated and non-treated individuals. A simple way to detect lack of overlap is to plot distributions of covariates in both groups. However, as this becomes difficult in high-dimensional cases, a more useful method is to inspect the distribution of the propensity score for the treated and non-treated group. Problems arise when the distributions in both groups do not overlap. To ensure that any combination of variables observed in the treatment group can also be observed in the comparison group, one has to impose a common support condition. A simple solution is the so-called 'minima and maxima comparison' that I will use for the empirical analyses.[35] The basic criterion of this approach is to delete all treated observations whose propensity score is smaller than the smallest minimum or larger than the largest maximum in the non-treated group. Treated individuals who fall outside the common support region have to be disregarded. For those individuals the treatment effect cannot be estimated. Hence, the 'minima and maxima comparison' is a simple way to ensure that for each participant a close non-participant can be found. It should be noted that, when no calliper is imposed, the common support condition is less important for NN-matching than, e.g., for kernel

[34] This approach has been used, e.g., by Heckman, Ichimura, and Todd (1997) and Heckman, Ichimura, Smith, and Todd (1998).

[35] Alternatively, Heckman, Ichimura, and Todd (1998) and Smith and Todd (2005a) suggest to use a 'trimming' procedure.

matching. Whereas with NN-matching only the closest neighbour is used to estimate the counterfactual outcome, with kernel matching all non-treated observations are used. In addition, imposing a calliper reduces the common support problem for NN- and kernel-matching as well.

If the proportion of individuals lost due to the common support is small, this poses few problems for the analysis (Bryson et al., 2002). However, if there are many individuals lost, there may be concerns whether the estimated effects on the remaining individuals can be viewed as representative. In such case, it is reasonable to analyse the characteristics of the discarded individuals as these may provide some important information to be considered when interpreting the treatment effects.

3.5.3 Choice of the Adequate Matching Algorithm

The choice of the adequate matching algorithm depends largely on the available data structure. As discussed above, the matching estimators contrast the outcome of a treated individual with the outcome of comparable individuals. However, the estimators differ not only in the definition of the neighbourhod for each treated individual and the handling of the common support problem, but also with respect to the weight given to the neighbours. These differences will be particularly important in small samples (see Heckman, Ichimura, and Todd, 1997) since the choice of the matching algorithm usually involves a trade-off between matching quality and variance. It should be noted that all approaches asymptotically yield the same results because with growing sample size all of them become closer to comparing only exact matches (Smith, 2000a).

The first decision to be made refers to the number of comparison individuals to match with each treated individual. On the one hand, single NN matching uses the participant and its closest neighbour only. Although it minimises the bias, it may involve an efficiency loss if a large number of close neighbours is disregarded. As noted above, NN matching faces the risk of bad matches if the closest neighbours are far away and if the ordering of the individuals is not random. The first problem could be circumvented by imposing an additional calliper, the latter by randomising the order of the individuals in the sample.[36] On the other hand, kernel-based matching uses all non-participants within the common support region as a match for the participants. Hence, it reduces the variance but possibly increases the bias. Finally, using the same non-treated individual more than once may possibly improve the matching quality, but increases the bias.

As will be seen below, the number of non-participants is clearly larger than the number of participants in the data used for the empirical analyses. In addition, Caliendo et al. (2006a) have analysed the properties of different matching algorithms applied to the data.[37] The results show that the estimates are not sensitive to the algorithm choice and that the improvement which comes from oversampling methods

[36] It has to be noted that effects estimated by NN-matching depend on the sort order the matches are drawn and therefore, the variance may not be reduced in any case.

[37] The data used in the analysis in chapter 6 is the same as in Caliendo et al. (2006a).

in terms of reduced variance is only marginal. Thus, the choice of the matching algorithm seems not to be too critical for the empirical analysis. For this reason, using a NN matching algorithm seems to be feasible for the further analysis. Furthermore, as I have a very large number of non-participants (see chapter 4), the probability of finding good matches without replacement is quite high. To avoid unnecessary inflation of the variance, matching is accomplished without replacement. Finally, to ensure a good matching quality, an additional maximum calliper of 0.02 is implemented.

3.5.4 Estimation of the Variance

A further aspect of implementation is concerned with testing the statistical significance of the estimated treatment effects, i.e. computing the standard errors. Unfortunately, there is no closed-form solution for estimating the variance of the treatment effects. This would require to include the variance of the parametrically estimated treatment effect, the imputation of the common support as well as possibly the order in which treated individuals are matched in the formula. So far, only approximations are suggested in the empirical literature as these estimation steps add variation beyond the normal sampling variation (see the discussions in Imbens, 2004, and Heckman, Ichimura, and Todd, 1998).

Hence, the most common approach used to estimate standard errors of the treatment effects is to use bootstrapping, see, e.g., Sianesi (2004), Fitzenberger and Speckesser (2005) and Caliendo et al. (2006a). As bootstrapping yields under mild regularity conditions an approximation to the distribution of an estimator that is at least as accurate as the approximation obtained from first-order asymptotic theory (see Horowitz, 2001), it is a popular way to estimate standard errors in case analytical estimates are unavailable or biased. The idea of bootstrapping is resampling one's data to treat the data as if they were the population for the purpose of evaluating the distribution of interest. Thus, each bootstrap draw has to repeat the full estimation sequence of the treatment effects including the (parametric) estimation of the propensity score imposing the common support condition as well as the matching. The resampling is repeated n times which leads to n bootstrapped samples and n estimated treatment effects. The distribution of these treatment effects approximates the sampling distribution and thus the standard error of the population mean. However, since bootstrapping the standard errors is a very time-consuming activity, it may be infeasible in some cases, e.g., in case of evaluating the effects of multiple treatments, if the propensity scores are estimated by a multinomial probit.

Although bootstrapping has been commonly used in empirical analyses applying nearest neighbour matching, Abadie and Imbens (2006b) show that standard bootstrapping may lead to invalid confidence intervals that can have over-coverage as well as under-coverage. In addition, Abadie and Imbens (2006a) propose analytical estimators of the asymptotic variance of matching estimators that are (together with sub-sampling) the only available methods of inference that are formally justified. Nevertheless, both approaches are even more computational burdensome than bootstrapping and do not consider propensity scores that are estimated parametrically.

Hence, standard bootstrapping may be inexact, but is better than doing nothing to take account of the variance component due to estimation of the propensity scores.

Alternatively, Lechner (2001) suggests to calculate the standard errors of the estimator in eq. (3.48) as the square root of the following variance:

$$\widehat{\mathrm{Var}}\left(\Delta^{ATT}_{t_{MAT}}\right) = \frac{1}{N_1}\widehat{\mathrm{Var}}_{i\in\{D=1\}}(Y_t) + \frac{1}{N_1^2}\sum_{j\in\{D=0\}}(w_j)^2 \cdot \widehat{\mathrm{Var}}_{j\in\{D=0\}}(Y_t), \tag{3.62}$$

where N_1 is the number of treated individuals. To take account of the increased variance through matching with replacement, w_j denotes the number of times individual j has been used as a match. However, for matching without replacement, the variance formula in eq. (3.62) coincides with the 'usual' variance formula. This approximation assumes weights and probabilities to be fixed and the observations to be independent (Lechner, 2001). Furthermore, it is assumed that the variances of the observable outcome variables do not depend on the values of the balancing scores and are the same within the particular state, i.e. treatment and non-treatment. Lechner (2002) justifies this approximation by comparison with bootstrapped variances. The results show only little differences between both.

The variance approximation of Lechner (2001) has been extended for the use in the dynamic setting by Steiger (2004). The variance for the single time effects is:

$$\begin{aligned}\widehat{\mathrm{Var}}\left(\Delta^{ATT}_{t,u_{(MAT)}}\right) = {} & \frac{1}{N_{1,u}}\widehat{\mathrm{Var}}_{i\in\{D_u=1\}}(Y_{t,u}) \\ & + \frac{1}{N_{1,u}^2}\sum_{j\in\{D_u=0\}}(w_j^u)^2 \cdot \widehat{\mathrm{Var}}_{j\in\{D_u=0\}}(Y_{t,u}).\end{aligned} \tag{3.63}$$

Here, $N_{1,u}$ denotes the number of individuals who receive treatment in month u of their unemployment spell. Accordingly, w_j^u is the number of times individual j has been used as a match at u.

3.5.5 Matching Quality

When applying propensity score matching instead of matching on covariates, one necessary step is to check whether the matching procedure is able to balance the distribution of the covariates between the treated and the non-treated group. The empirical literature provides a set of tests for this purpose. However, Smith and Todd (2005b) note that one limitation of the multiple versions of the different balancing tests is that little is known about the statistical properties of each one and of how they compare to one another given particular types of data.

A first suitable indicator is suggested by Rosenbaum and Rubin (1985) and relies on the examination of standardised differences (*st_dif*). This approach has been commonly used in the recent empirical literature, see, e.g., Lechner (1999; 2000), Sianesi (2004) or Hujer, Caliendo, and Thomsen (2004). Following Smith and Todd (2005b), in the general form it is defined as:[38]

[38] The version of Rosenbaum and Rubin (1985) is a bit simpler:

$$st\_dif(X_k) = 100 \cdot \frac{\frac{1}{N_1} \sum_{i \in \{D=1\}} \left[X_{ki} - \sum_{j \in \{D=0\}} W(i,j) X_{kj} \right]}{\sqrt{\frac{\mathrm{Var}_{i \in \{D=1\}}(X_{ki}) + \mathrm{Var}_{j \in \{D=0\}}(X_{kj})}{2}}}. \tag{3.64}$$

For each covariate X_k, the *st_dif* is the difference of the sample means in the treated and the matched comparison group, divided by the square root of the average of the variances of X_k in the unweighted treatment and non-treatment group. Therefore, it considers the size of the differences in means of a conditioning variable between the treated and matched comparison groups, scaled by the square root of the average variances in the original samples (Smith and Todd). In the empirical analysis, the *st_dif* should be computed for all of the variables included in the propensity score estimation. A common practice in the empirical literature is to estimate the mean (see Hujer, Caliendo, and Thomsen) or median (see Sianesi) of the *st_dif* of all single variables to abbreviate the documentation. One possible problem with the *st_dif* approach is that there is no clear indication for the success of the matching procedure, i.e. what level of the *st_dif* is acceptable. For example, Rosenbaum and Rubin (1985) suggest that a value of 20 is large. However, for most empirical studies a *st_dif* below 3 to 5 percent could be seen as sufficient (Caliendo and Kopeinig, 2006). In addition, a further disadvantage of the *st_dif* is that the level could be reduced by adding additional observations to the comparison group as long as these additional observations increase the second variance term in the denominator (Smith and Todd).

Alternatively, Rosenbaum and Rubin (1985) suggest to use a two-sample t-test to check if there are significant differences in covariate means between the treated and non-treated group. This approach has been applied, e.g., by Speckesser (2004) and Fitzenberger and Speckesser (2005). It is based on the idea that before matching there should be differences in both groups and if the covariates are balanced after matching there should be no significant differences left. The t-statistic is calculated as the ratio of the differences between the two means of the covariate in the treated and comparison group in the numerator to the square root of the sum of the variances of the sample means.

In addition, Sianesi (2004) suggests to repeat the propensity score estimation on the matched sample, i.e. on treated and matched comparison individuals, and to compare the coefficients of determination before and after matching. One possible coefficient is the pseudo-R^2 that assesses how well the variables X explain the participation probability. Successful matching should adjust away all systematic differences with respect to the observable covariates. Therefore, after matching there should be no systematic differences in the distribution of the covariates between the treated and the non-treated group. For that reason, the pseudo-R^2 should be fairly low. Alternatively, a F-test on the joint significance of the regressors could be per-

$$st\_dif = 100 \cdot (\overline{X}_1 - \overline{X}_{0M}) / [(\mathrm{Var}_1 + \mathrm{Var}_0)/2]^{1/2},$$

where $\overline{X}_1$ and $\overline{X}_{0M}$ are the sample means in the treated and matched comparison group, and Var_1 and Var_0 are the sample variances in the treated group and the comparison reservoir.

formed. The results of the test should indicate a joint significance of the regressors before, but no significance after matching.

3.5.6 Further Methodological Issues

Besides the discussed issues regarding the implementation of the matching estimator, there are some further methodological questions that should be answered for the empirical analyses. These questions comprise the definition of the relevant treatment, the time when to compare the programme effects, and how to deal with the possible occurrence of locking-in effects.

For the empirical analyses, I will consider the first participation in a job creation scheme in the unemployment spell only. Any later participation is viewed as an outcome of this first treatment. In contrast, if one wants to analyse the sequence of treatments, i.e. a series of programmes an unemployed individual participates in, a dynamic causal model has to be defined.[39] However, this study concentrates on the first programme only and does not consider possible 'programme careers' explicitly.

A further decision, which has to be made, relates to the point of time when the comparison of treated and non-treated individuals starts, i.e. how to deal with the duration of the programmes. The predominant approach in the empirical literature is to start the comparison at the beginning of the programmes (see, e.g., Gerfin and Lechner, 2002, Hujer, Caliendo, and Thomsen, 2004, or Sianesi, 2004). Whereas a possible occurrence of locking-in effects may be problematic for this approach, there are three main reasons that guide this decision. First, since an important task for the evaluation is to ensure that persons are compared in the same individual lifecycle position and in the same economic environment, comparing persons after the end of the treatment may impose a comparison of persons in very different situations. This problem is even aggravated if the exits of the participants from programmes are spread over a longer time period. Thus, starting the comparison at the begin of the programmes circumvents this problem. The second reason is to avoid the endogeneity problem of the programme exits (Gerfin and Lechner). Programme abortion could be caused by several factors that are, in general, unobservable to the analyst. Hence, it could not be identified whether the reasons depend on the programme or not.[40] The third argument for the decision to measure the effects from the start of the programmes onward relates to the policy-relevant question that should be answered. In chapter 5, I analyse whether an eligible person should be placed in a job creation scheme after a given number of months spent in open unemployment or not in order

[39] An approach that considers the possible influence of preceding treatments on the selection into further treatments and the resulting impacts based on the matching estimator is suggested by Lechner and Miquel (2002). Lechner (2004) proposes an estimation procedure and provides an application to Swiss data.

[40] As mentioned in the discussion of the institutional set-up of job creation schemes in Germany (section 2.3.2), participants are required to continue the job-search while in the programme. Furthermore, the caseworkers in the LEA should cancel a running programme if the participant could be placed into regular employment. Thus, participants are still at the labour market disposal during the programmes.

to improve the individual's chances for regular employment. The analysis in chapter 6 uses a slightly different version and asks if an individual should join a job creation scheme in February 2000 or not (conditional that he or she was unemployed up to that month).[41]

A drawback of comparing the effects from the beginning of the programmes is that possible locking-in effects cannot be disentangled from the employment effects of the programme for the participants. Participants, whilst in the programmes, have to be assumed to have a reduced search intensity for a new job compared to the non-participants. Following van Ours (2004), the net effect of a programme consists of two opposite effects. First, the (hopefully) increased employment probability of the participants caused by the programme and second, the reduced job-search intensity. When interpreting the results it should be considered that both effects are constituent parts of the treatment effect. Due to the reduce of the job-search activity, one should expect an initial negative effect from any kind of participation in a programme. However, after the end of the programmes, the increased employment probability due to the programmes should overcompensate for the initial fall in a successful programme.

3.6 Summary

In this chapter, I have discussed the necessary methodological issues for the evaluation of the effects of job creation schemes in Germany. The starting point was a review of the *potential outcome approach* to point out the fundamental problem of any microeconomic evaluation. As the individual causal effect of treatment is defined as the difference of the value of the participant's outcome in the current situation and the value of the outcome in a situation where the participant has not taken the treatment, this causal effect is not directly observable in the data as one state of the world at one point of time for each individual is unobservable. The purpose of microeconometric evaluation is to solve this evaluation problem by specifying an adequate comparison group of non-participants to approximate the counterfactual outcome of the participants. In contrast to social experiments, where the random assignment of individuals to the treatment and the non-treatment group should circumvent differences between both groups other than the treatment, with non-experimental data the problem of selection bias has to be considered. Selection bias occurs if treated and non-treated individuals would have different non-treatment outcomes even in the absence of the programme. In that case, the non-participants' outcomes provide a poor approximation of the non-treatment outcomes for the participants, and the estimated treatment effects would be biased. I have briefly discussed three commonly used simple evaluation approaches that are based on strong assumptions to solve the selection bias.

A more feasible approach is to use the matching estimator. The underlying idea is to resemble an experimental control group by conditioning on some set of relevant

[41] See chapter 4 for a description of the data sets used in the empirical analyses.

observable covariates to adjust away all differences between treated and non-treated individuals. The major advantage of the matching estimator is that the evaluation problem could be solved without imposing arbitrary parametric assumptions. Furthermore, due to its generality, it is highly flexible and may be combined with other methods. However, as matching can become infeasible if the number of relevant covariates is large, a popular choice uses the probability of participation to perform the matching (propensity score matching).

Recently, considering the timing of treatment in the evaluation of treatment effects has become important in the empirical literature. In countries like Sweden, Switzerland, or Germany that provide a comprehensive system of labour market policies for the unemployed, individuals are expected to receive a programme given they remain unemployed long enough. Thus, to participate in a programme or not is no non-recurring decision, but reflects a dynamical process between the unemployed individual and his or her responsible caseworker. To investigate the importance of the unemployment duration for the effectiveness of the programme, I have presented the extension of the evaluation approach to the dynamic setting in this chapter. The last section of this chapter was devoted to some practical issues concerning the implementation of the estimator for the empirical analyses.

What should have become clear is that three factors determine the appropriate methodology for non-experimental data: the type of the available information, the underlying model, and the parameter of interest. Using sufficiently large administrative data for the empirical analysis allows me to refrain from imposing strong parametrical assumptions (except those of the binary probit and logit models) to solve the selection bias. Thus, using the matching estimator seems to be most credible for evaluating the employment effects of job creation schemes.

4

The Database

4.1 Overview

As Angrist and Krueger (1999) point out, familiarity with data sets is as necessary in modern labour economics as familiarity with economic theory and econometrics. Therefore, I will describe the collection and preparation of the data used for the empirical analyses in this chapter. The data were compiled within the joint project 'Effects of Job Creation and Structural Adjustment Schemes' of the IAB, Nuremberg, and the Chair of Statistics and Econometrics, Frankfurt/Main. The project aimed at two main purposes: An *evaluation purpose* and a *data preparation purpose*.

The *evaluation purpose* of the project was to overcome the uncertainties with respect to the effects of subsidised employment programmes on the employment chances for participating individuals. Although the programmes were in focus by a series of studies during the late 1990s (see overview in section 2.4.1), all of these studies suffered from small numbers of observations as they were based on survey data sets, and difficulties in identifying the single measures and their corresponding causal effects. Furthermore, those small numbers did not allow to investigate programmes with respect to different sources of effect heterogeneity. The *data preparation purpose* of the project was to support the decisions of the Pallas project[1] of the IAB, which aimed at collecting, combining, transforming, and preparing the information of the different administrative sources of the FEA for scientific evaluation of all ALMP programmes accomplished by the FEA in the programme participants' master data set (*Maßnahme-Teilnehmer-Gesamtdatei*). Both purposes have been in line with the legal basis introduced in 1998 (SGB III) that postulates the evaluation of the activities of the FEA as well as the provision of administrative data to achieve this objective (see discussion in chapter 2).

[1] The objective of the Pallas project was to provide a general cross platform analysis and information system (*plattformübergreifendes allgemeines Analyse- und Informationssystem*). See Kellner (2002) or Passenberger and Reith (2002) for further details. In addition, the experiences of the preparation of the data have also been used for the data warehouse project of the FEA.

I will start with a short description of the different data sources of the FEA and their content. These sources have been used to derive the required information for evaluating the employment effects of job creation schemes in Germany. A special emphasis will be given to the available attributes, i.e. the observable characteristics to describe the individual labour market situation and the labour market outcomes over time. In section 4.3, I will present the construction of the samples used for the empirical analyses. In addition, I will note the necessary restrictions applied to the data and the relevant numbers of observations for the estimations. The chapter will conclude with a brief summary.

4.2 Data Sources of the Federal Employment Agency

The 181 LEAs in Germany collect information within the so-called *CoArb* system on all registered job-seekers.[2] These are persons who are registered unemployed, persons threatened by unemployment or in temporary employment as well as participants in the different ALMP programmes. The purpose of the data collection is to administer the job-seekers and to alleviate the decisions of the local caseworkers regarding the placement of job-seekers in regular employment or different ALMP programmes. Furthermore, the data are used to control the UI eligibility of the job-seekers. All data are collected detailed to a daily level, i.e. the day the unemployment spell starts as well as the day it ends are covered. These locally collected data are transferred to the FEA on a monthly basis.[3] The information for all job-seekers is consolidated in the so-called job-seekers database (*Bewerberangebotsdatei*, BewA) that is available from 1997 onward.[4] The BewA contains a rich set of attributes describing the individual's labour market situation. Three classes of characteristics could be distinguished: The first category, socio-demographic information, incorporates attributes like age, gender, marital status, citizenship, the number of children, birthday, and health restrictions. The second category, the qualification details, consists of, among others, the individual's graduation, information on a completed professional training, the occupational group, the last occupation, as well as the work experience. The last category, the labour market history[5], includes the date of registration at the LEA (and the duration of unemployment since that date), the duration of the last employment, the number of job offers received by the individual as well as information on preceding programme participation(s). Although most of the attributes are 'objective' facts, like age or gender, the BewA also contains subjective assessments of the individual's labour market prospects by the responsible caseworker. These subjective facts cover the assessment of the individual's employment chances with respect to health restrictions, but also the assessment of the actual qualification

[2] The term *CoArb* is an abbreviation for computer supported job placement (*computergestützte Arbeitsvermittlung*).

[3] A further purpose is the calculation of the official unemployment rate.

[4] See Wilke and Winterhager (2004) for an overview of the data sources of the FEA.

[5] See, e.g., Heckman and Smith (2004).

of the individual. Until 2004, information of the BewA was also available in an adjusted version for statistical purposes. This data set was referred to as *ST4*, with *ST* indexing data sets of the STADA system of the statistics department of the FEA.

The information on the different ALMP programmes is not included in BewA, but is collected separately in the LEAs within the so-called *CoSach* system[6] and transferred to the FEA on a monthly basis. As in BewA, data are detailed to a daily level. Information on the single ALMP programmes are stored in several different data sets. For example, data on vocational training programmes, training measures, and German language courses are stored in the so-called *ST35*. In contrast, data on subsidised employment programmes in Germany, like job creation and structural adjustment schemes, are consolidated in the *ST11*. This source contains all information necessary for the administration of the programmes. Among others, this is information on the employer who receives the wage subsidy, the economic sector of the activity, times of qualification and/or practical training of the individual during the programme, the beginning and end of the programme (payment of the subsidy), and the ex-ante planned as well as the ex post realised programme entry and leave dates of the individual. Besides those attributes, a small number of further individual characteristics is included, but these are redundant to those provided by BewA.

Table 4.1: Important Programme Information Comprised by MTG

FEA Source	Type of Programme
ST11	Job creation schemes, structural adjustment schemes, integration subsidies, integration contracts, bridging allowances etc.
ST35	Vocational training programmes, training measures, German language courses
ST11 FF	Free promotion
ST38	Programmes financed by European Social Fund
JUMP	Programmes for unemployed aged 25 or younger

Source: Wilke and Winterhager (2004).

During the years 2000 to 2004, information of BewA and the several sources for the different programmes were standardised and merged into one major source: the programme participants' master data set (*Maßnahme-Teilnehmer-Gesamtdatei*, MTG).[7] This data set includes information on all programme participations in FEA sponsored programmes from 2000 to the present. Table 4.1 provides an overview of the most important data sources and the corresponding ALMP programmes that are included in MTG.

Due to this, the MTG contains a large number of attributes to describe the individual's labour market situation on the one hand. On the other hand, it provides a reasonable basis for the construction of the comparison group as almost all individ-

[6] The term *CoSach* is an abbreviation for computer-supported processing (*computergestützte Sachbearbeitung*).

[7] At an earlier stage this data set was referred to as *Maßnahme-Teilnehmer-Grunddatei*.

ual characteristics are available for the participants as well as for the non-participants and originate from the same source. This same origin of the data is an essential building block for a valid estimation. The results of Heckman, Ichimura, Smith, and Todd (1998) who analyse the sources of potential biases of evaluation estimators show that having access to a geographically matched comparison group administered the same questionnaire as programme participants clearly matters in devising effective non-experimental estimators of programme impacts. Thus, equally derived data for participants and non-participants as in my case provide an excellent basis for the empirical analyses.

A further important determinant is the state of the local labour market (Heckman and Smith, 1999). For the description of the regional context I use the classification of the labour office districts by the FEA (see Blien et al., 2004). The aim of this classification is to enhance the comparability of the labour office districts for a more efficient allocation of funds. The 181 labour office districts are split into 12 types of office districts with similar labour market circumstances. The comparability of the office districts is built upon several labour market characteristics. The most important criteria are the underemployment rate and the corrected population density. The underemployment rate is defined as the ratio of the sum of unemployed individuals and participants in several ALMP programmes to the sum of all employed persons and these participants. The corrected population density is used to improve the comparability of rural labour office districts with metropolitan and city areas. In addition, the vacancy rate describing the relation of all reported vacancies at the LEA, the placement rate that contains the number of placements to the number of employments, and the rate of persons who achieve maintenance allowance in relation to the underemployment rate are used. Furthermore, an indicator for the tertiarisation level, built on the number of persons employed in agricultural occupations, and an indicator for the seasonal unemployment are considered.

These 12 types of comparable labour office districts can be aggregated by 5 types for strategic purposes. Since almost all labour office districts in East Germany belong to the first of these 5 strategic types (except the city of Dresden), I use the finer typing of the types with similar labour market circumstances here. Hence, four regional groups for East Germany can be distinguished. For West Germany, the remaining four types for strategic purposes are used. Table 4.2 presents the classification used in the analyses containing a short description of the clusters, the number and the names of the LEAs in each cluster. This leaves me with 7 clusters for the analysis. According to Blien et al. (2004), those clusters are defined in the following way. The first group (Ia) consists of five East German labour office districts with the worst labour market conditions of the whole country. The situation is characterised by the highest underemployment, a population density below average, and the slightest labour market dynamics. Cluster Ib contains the ‘typical’ East German labour office districts: In line with the description of the East German labour market in chapter 2, the LEAs of this type experience a high underemployment and minor labour market dynamics. 23 labour office districts are pooled in this cluster. Although the underemployment is above average and the dynamics are only moderate in the five LEAs of cluster Ic, they have the most promising labour market situation of East Germany. Cluster II

Table 4.2: Classification of Labour Office Districts in Germany

Cluster	Description	No.[1]
Ia	*East German labour office districts with worst labour market conditions*	5
	Altenburg, Bautzen, Merseburg, Neubrandenburg, Sangerhausen	
Ib	*East German labour office districts with bad labour market conditions*	23
	Annaberg, Chemnitz, Cottbus, Dessau, Eberswalde, Erfurt, Frankfurt/Oder, Gera, Halberstadt, Halle, Leipzig, Magdeburg, Neuruppin, Nordhausen, Oschatz, Pirna, Plauen, Riesa, Rostock, Stendal, Stralsund, Wittenberg, Zwickau	
Ic	*East German labour office districts with high unemployment*	5
	Gotha, Jena, Potsdam, Schwerin, Suhl	
II[2]	*Labour office districts dominated by large cities*	22
	Aachen, Bielefeld, Bochum, Bremen, Cologne, Dortmund, Dresden, Duisburg, Essen, Gelsenkirchen, Hagen, Hamburg, Hamm, Hannover, Krefeld, Mönchengladbach, Oberhausen, Recklinghausen, Saarbrücken, Solingen, Wuppertal	
III	*West German labour office districts with rural elements, medium-sized industry and average unemployment*	63
	Ahlen, Bad Hersfeld, Bad Kreuznach, Bad Oldesloe, Bamberg, Bayreuth, Bergisch-Gladbach, Braunschweig, Bremerhaven, Brühl, Celle, Coburg, Coesfeld, Detmold, Düren, Elmshorn, Emden, Flensburg, Fulda, Gießen, Goslar, Göttingen, Hameln, Hanau, Heide, Helmstedt, Herford, Hildesheim, Hof, Iserlohn, Kaiserslautern, Kassel, Kiel, Korbach, Landau, Leer, Limburg, Lübeck, Ludwigshafen, Lüneburg, Marburg, Mayen, Meschede, Neuenkirchen, Neumünster, Neuwied, Nienburg, Nordhorn, Oldenburg, Osnabrück, Paderborn, Pirmasens, Saarlouis, Schweinfurt, Siegen, Soest, Stade, Treves, Uelzen, Verden, Wesel, Wetzlar, Wilhelmshaven	
IV	*West German centers with good labour market prospects*	10
	Bonn, Düsseldorf, Frankfurt/Main, Mannheim, Munich, Münster, Nuremberg, Offenbach, Stuttgart, Wiesbaden	
V	*West German labour office districts with the best labour market prospects*	48
	Aalen, Ansbach, Aschaffenburg, Augsburg, Balingen, Constance, Darmstadt, Deggendorf, Donauwörth, Freiburg, Freising, Göppingen, Heidelberg, Heilbronn, Ingolstadt, Karlsruhe, Kempten, Koblenz, Landshut, Lörrach, Ludwigsburg, Mainz, Memmingen, Montabaur, Nagold, Offenburg, Passau, Pfarrkirchen, Pforzheim, Rastatt, Ravensburg, Regensburg, Reutlingen, Rheine, Rosenheim, Rottweil, Schwäbisch Hall, Schwandorf, Tauberbischofsheim, Traunstein, Ulm, Vechta, Villingen-Schwennigen, Waiblingen, Weiden, Weilheim, Weißenburg, Würzburg	

[1] No. describes the number of labour offices in cluster.
[2] The labour office districts of Berlin are counted as a single district only.
Source: Blien et al. (2004).

contains 22 labour office districts dominated by large cities.[8] Here, an above average to a high underemployment, a high population density, moderate labour market

[8] The labour office districts of Berlin are counted as one LEA only.

dynamics, a high number of welfare recipients and an above average tertiarisation of jobs describe the regional labour environment. Except the labour office of Dresden, all districts are in West Germany. The 'typical' West German labour office districts are pooled in cluster III. The labour market in these 63 districts is characterised by average to above average underemployment, little labour market dynamics and a low population density. Further attributes are rural elements and a medium-sized industry. Cluster IV pools West German labour office districts with advantageous labour market prospects. These are 10 big city districts with the highest labour market dynamics, an underemployment below average, a high tertiarisation of jobs, but also an above average number of welfare recipients. The labour office districts with the best labour market environment are pooled in cluster V. These are 48 districts were underemployment and also the number of welfare recipients is lowest in Germany. Finally, Table 4.3 summarises the available information. Altogether, I could base the evaluation of the employment effects of job creation schemes on the five different categories of variables described.

Table 4.3: Overview of Data Sources and Main Attributes

Data Source	Main Attributes
MTG[1] BewA[2]	a) **Socio-Demographic:** Age, gender, foreigner (citizenship), asylum seeker, marital status/cohabitation, number of children, health restrictions, placement restrictions b) **Qualification:** Graduation (schooling), professional training, occupational group, position in last occupation, work experience, work time of last occupation, appraisal of qualification by the caseworker, desired occupation, desired work time c) **Labour Market History**: Duration of unemployment, duration of last occupation, number of job offers, occupational rehabilitation, programme participation before unemployment, pension
ST11[3]	d) **Programme:** Institution receiving the subsidy, activity sector, time of qualification and/or practical training during programme, start and end of programme (payment of subsidy), entry and leave of the participation, duration of programme
	e) **Regional Information:** Types of comparable labour office districts by FEA

[1] MTG: Programme participants' master data set (*Maßnahme-Teilnehmer-Gesamtdatei*).
[2] BewA: Job-seekers' database (*Bewerberangebotsdatei*). For the empirical analysis in chapter 6, I also use the adjusted information of BewA provided by ST4.
[3] ST11: Progamme participants' database of subsidised employment.

For the outcome variable, I use information of the Employment Statistics Register (*Beschäftigtenstatistik*, BSt) that includes data on all persons registered in the German social security system since 1975. These are employees as well as participants in several ALMP programmes, but no self-employed or pensioners (Bender, 2002). Data on spells of employment being subject to social insurance contributions are collected for each employed person in account form based on yearly notifications by the

employers. However, due to delays in the sending of these notifications by the employers, the available data in the BSt have a time lag of up to 2 years. Therefore, the FEA forecasts the employment information. In consequence, assessing contemporary effects of ALMP programmes is possible, but the results may be problematic as they are based solely on forecasted employment information. As valuable evaluation of programme effects should be based on notified rather than forecasted information, the observation period ends in December 2003 for the data used in chapter 5.[9] However, as the time lag between the corresponding date of the information and the extraction of the information from the BSt for the analyses amounts to eight months only, the relation of notified and forecasted data has to be checked and should be considered when interpreting the estimated treatment effects.[10] Based on the results of Fröhlich, Kaimer, and Stamm (2004), the share of forecasted data used in the analyses amounts to 4 to 10 percent at maximum.

I define only regular employment as a success. All other kinds of subsidised employment or participations in ALMP programmes are defined as a failure. Although this definition may conflict with the institutional setting, it reflects the economic point of view to measure the integration ability of a job creation scheme into non-subsidised employment. To identify spells of regular employment without further promotion, I use the excerpted information of the final version of the MTG on the individual's time spent in ALMP programmes.

4.3 Preparation of the Database for the Empirical Analysis

In the empirical analyses in chapters 5 and 6, I will evaluate the employment effects of job creation schemes to answer two different research questions. In chapter 5 I will start with the evaluation of the employment effects for the participating individuals considering the timing of treatment. The analysis in chapter 6 will deal with the efficiency of job creation schemes explicitly considering certain targeting criteria. In particular, I will assess the value of stricter target rules for the allocation of individuals in that chapter. As my analysis in chapter 5 will be based on a different data set than the one used in chapter 6, I will describe both data sets in the following sections.

4.3.1 Data Used in Chapter 5

The central source of information on participants for the evaluation of the employment effects considering the timing of treatments is the MTG. For that purpose, random samples were drawn of individuals who have started a subsidised employment programme, i.e. a job creation or structural adjustment scheme, in six different months: July 2000, September 2000, November 2000, January 2001, March 2001,

[9] I use different data in chapter 6. The observation period of these data is limited to December 2002.

[10] It has to be noted that the data used in the analysis of chapter 5 were extracted from BSt in September 2004.

and May 2001. The data set contains the five categories of characteristics as described in the previous section (see Table 4.3). The construction of the comparison group has been accomplished in a similar way. Based on the information of the BewA population in the respective months before the participants started their programmes, six random samples were drawn. The proportions of these original samples have been 20:1, in other words, for each participant from MTG starting a job creation scheme in July 2000, there were 20 non-participants drawn from BewA of June 2000 as potential comparisons. By doing so, the same set of attributes for participants and potential comparison individuals is available except for the missing programme information.

The individual characteristics of the six cross-sections have been completed by the employment outcome of all individuals in the samples until December 2003. As described above, the relevant information has been taken from the BSt and was corrected by times spent in ALMP programmes based on information of an excerpt of the MTG for the same period of time.[11] However, a complete merge of the cross-section information (MTG/BewA) and the employment outcomes (BSt) was not possible for all observations since both data sets use different identifiers. Whereas MTG/BewA use FEA-specific customer numbers to identify job-seeking and participating individuals, the BSt refers to the social insurance policy number (*Sozialversicherungsnummer*). Therefore, only in cases where this information is available and valid, the data can be merged.

In the empirical analysis, only the effects of job creation schemes on regular employment are evaluated. The effects of other programmes are not considered. Thus, I have restricted the participants' data to job creation schemes. By doing so, information on other subsidised employment programmes is excluded. This dropping is in line with the estimator in the dynamic setting as defined in chapter 3. The underlying intuition is to compare a person starting a programme at a specific point of time compared to staying unemployed at this point of time. As the participants of other programmes leave unemployment at that point of time, they provide no comparison group to approximate the counterfactual outcome of the participants.[12] Furthermore, to avoid issues related to education or basic vocational training, I have restricted the data to persons of 25 years or older. In addition, as early retirement may induce some trouble to the results, persons older than 55 years are excluded, too. Moreover, the labour market of the capital city is not considered in the analysis. With respect to the arguments of Brinkmann et al. (2002) and Caliendo et al. (2003), the special situation of Berlin would require a separate estimation and interpretation of the effects of job creation schemes on regular employment for participating individuals. Hence, East Germany will be the federal states of Mecklenburg-Western Pomerania, Brandenburg, Saxony, Saxony-Anhalt, and Thuringia for the rest of this work. West Germany refers to all West German Laender. For homogeneity reasons, I have also

[11] This excerpt solely contains the entry and leave dates of individuals in the different programmes sponsored by the FEA. Further attributes are not available.

[12] I should recall in this context that persons who start other programmes at later points of time should not be excluded from the analysis as this would imply a conditioning on future outcomes and therefore a violation of the conditional independence assumption.

excluded those persons whose unemployment duration and/or employment duration before the start of the programmes was above the 99th percentile of the individuals in order to reduce the problem of possible errors in the data.[13]

Table 4.4: Number of Observations

	Men				Women				Total	
	West		East		West		East			
Month	Part.	N-Part.	Part.	N-Part.	Part.	N-Part.	Part.	N-Part.	Part.	N-Part.
Data used for chapter 5										
Jul 00	917	64,992	2,259	33,009	419	58,525	2,423	40,523	6,018	197,049
Sep 00	963	71,615	2,403	36,907	628	66,246	2,838	46,756	6,832	221,524
Nov 00	752	48,265	1,493	25,384	482	43,911	1,936	31,390	4,663	148,950
Jan 01	658	43,117	539	23,495	410	34,313	895	24,186	2,502	125,111
Mar 01	882	61,457	1,684	34,743	416	46,561	2,481	32,434	5,463	175,195
May 01	1,009	70,344	2,453	39,971	419	58,394	2,682	40,649	6,563	209,358
Total	5,181	359,790	10,831	193,509	2,774	307,950	13,255	215,938	32,041	1,077,187
Data used for chapter 6										
Feb 00	2,140	44,095	2,924	64,788	1,052	34,227	5,035	76,512	11,151	219,622

Note: N-Part. denotes ‘non-participants’, Part. denotes ‘participants’.

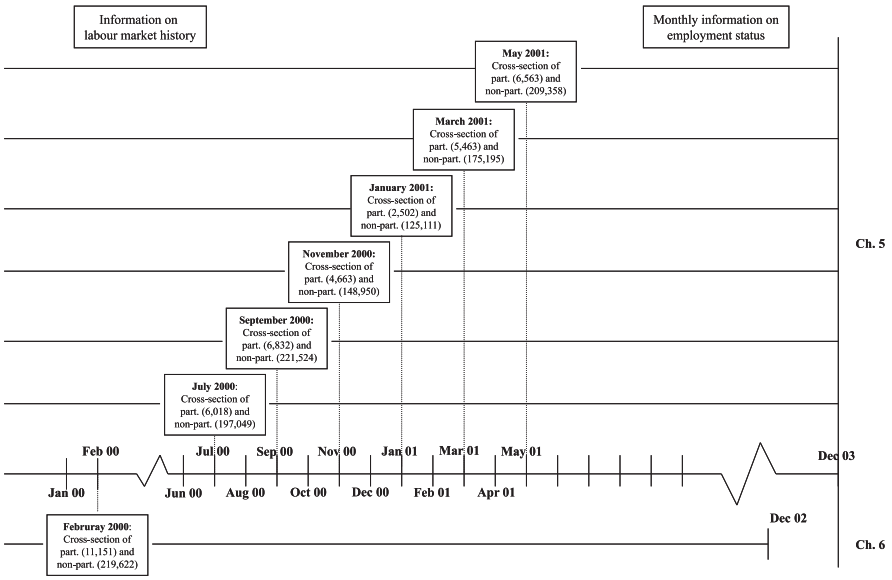
Table 4.4 presents the resulting number of observations for participants and non-participants differentiated by gender, region, and month of entry. In addition, Fig. 4.1 summarises the timeline of the available information and presents the resulting numbers for participants and non-participants. Due to the random sampling from the BewA and the differing use of job creation schemes in West and East Germany, the proportions between participants and non-participants differ in both regions. The numbers of entries in the months differ as well. Whereas the data set contains 6,832 participants starting a job creation scheme in September 2000, the corresponding number for January 2001 is 2,502 only. As expected from the description of job creation schemes in chapter 2, the number of programmes in East Germany outnumbers those in West Germany. Except for January 2001, the numbers of participating men are about twice as high in East as in West Germany; for women the relation is between 4:1 and 6:1. Altogether, information on 32,041 participants starting a job creation scheme between July 2000 and May 2001 and on 1,077,187 non-participants can be used.

4.3.2 Data Used in Chapter 6

The data used for the empirical analysis in chapter 6 contain information on all participants who were placed in a job creation scheme in February 2000 and on a comparison group of non-participants who were eligible for participation in January 2000,

[13] The limit for unemployment duration is 1,597 days which equals more than 4.3 years of unemployment. The limit for the duration of the last employment is 13,820 days which is approximately equal to 37.9 years of employment.

Fig. 4.1: Available Data for Analyses of Chapters 5 and 6



but did not enter those schemes in February. This data set is based on a prototype version of the MTG. It includes not only the information on individuals of BewA and on programmes of ST11, but also that of the adjusted data set for statistical purposes (ST4). The information on the comparison group is derived from BewA with the additional attributes of ST4. Therefore, almost all characteristics in the analysis for the comparison as well as for the participating group originate from the same data sources.

The observation period is shorter than in the larger data set used for chapter 5, and the individuals' outcomes are observed only until December 2002. I do not restrict the data to certain age groups since the analysis will include the effects for certain target groups, like younger unemployed without professional training or the effects for the older (see chapter 6). The labour market of the capital city Berlin is excluded for the reasons mentioned above. The last row of Table 4.4 presents the number of observations with respect to gender, region and participation status. The final sample consists of 11,151 participants and 219,622 non-participants. Thus, the comparison group is almost 20 times larger than the participants' group. Moreover, the set-up of information is also summarised in Fig. 4.1 (below the axis of the calendar time).

4.4 Summary

In this chapter, I have described the data collection and preparation for the empirical analyses in the following chapters. Data of different administrative sources of the FEA were merged. The major advantage of these data is the ability to use the whole

population of programme participants of selected months and a large sample of non-participants as potential comparisons. In addition, merging the data allows taking account of many attributes characterising the individual's situation.

In the resulting data sets, five categories of variables could be distinguished to describe the individual labour market situation and to address the problem of selection bias. The categories comprise socio-demographic information, qualification details, the labour market history, details of the programme, as well as a description of the regional context. Therefore, the data set contains many, if not most, variables influencing the selection process into the programmes. Due to this, the data at hand seem to justify the conditional independence assumption, and causal treatment effects can be identified based on observable variables.

Moreover, by merging information of the BSt, the individual employment could be used as the outcome of interest. For that reason, I am able to evaluate the net impacts of the programmes on regular employment for the participating individuals for up to 30 months (chapter 5) and up to 35 months (chapter 6) after the programmes have started.

5

The Employment Effects of Job Creation Schemes for the Participating Individuals

5.1 Overview

As already mentioned, consideration of the timing of treatment in the unemployment spell conveys important information on the effects of social programmes. The discussion of the institutional set-up of job creation schemes has shown (see section 2.3.2) that programmes are ongoing and unemployed persons may join the programmes at different points of time during their unemployment spells. Thus, in contrast to the typical US literature that assumes the treatment to be exposed once and at one specific point of time only, the set-up of job creation schemes requires carefulness in defining the comparison group (see chapter 3). Simply using all persons who are observed to never participate in a programme would eventually violate the conditional independence assumption due to a conditioning on future outcomes. Therefore, I define participation and non-participation dynamically, i.e. the comparison group to individuals starting at a specific point of time are all persons who have neither entered a programme nor left unemployment up to that point of time. For this reason, I explicitly consider the timing of treatment in the individual unemployment spell in the estimation of the employment effects for the participating individuals of job creation schemes in Germany and present the results of these estimations in this chapter.

I use the matching estimator in the dynamic setting as described in section 3.4 to evaluate the employment effects. The decision to apply a matching estimator is based on two main arguments. First, due to the high flexibility together with the non-parametric nature of the estimator, the matching approach allows to estimate the treatment effects of job creation schemes without imposing any arbitrary parametric functions. Second, I could use a unique data set that is remarkably rich by international standards. Hence, the characterisation of the individual likelihood of participation in a programme as well as of employment could be based on five different categories of attributes. Having access to these informative data is particularly important for achieving the identifying assumptions of the estimator.

Besides the timing of treatment, a further important issue to be regarded in the empirical analysis has become obvious from the description of the development of the labour market in West and East Germany (see section 2.2). Both parts clearly dif-

fer even 15 years after German Unification. For this reason, pooling the information of West and East Germany may veil some important effect heterogeneity. To account for these differences, I estimate the effects for both parts separately.

I will start the analysis with some descriptive results for selected attributes to characterise ex ante differences between participants and non-participants in section 5.2. Following that, I will discuss the plausibility of the conditional independence assumption in section 5.3. Since the conditional independence assumption is non-testable, one has to be careful in deciding whether the assumption is achieved. Section 5.4 will provide some details on the implementation of the estimator comprising the estimation of the propensity scores (section 5.4.1) as well as the quality of the matches (section 5.4.2). After that, I will present the estimates of the employment effects for participating individuals of job creation schemes with respect to the timing of treatment for West (section 5.5.1) and East Germany (section 5.5.2). In the last section of this chapter, I will summarise the findings of this analysis.

5.2 Descriptive Results

Since I use non-experimental data for the evaluation of programme effects, particular differences between participants and non-participants may indicate a possible source of selection bias. Hence, reviewing the characteristics of participants and non-participants may help to avoid lacking some important observable differences that are necessary to achieve the conditional independence assumption. Based on the extensive set of attributes (see section 4.2), I have selected a number of variables that will be used to analyse ex ante observable differences between participants and non-participants by descriptive statistics. The statistics are provided in Tables A.1 and A.2 for West Germany and in Tables A.3 and A.4 for East Germany in the appendix to this chapter. The tables distinguish four different time intervals of unemployment duration (until treatment starts): Up to 6 months, 6 to 12 months, 12 to 18 months, and 18 to 24 months.

Starting with the results for West Germany, a first interesting point to note refers to the age of the participants compared to the non-participants. The age of the individuals is subdivided into six categories.[1] Younger unemployed persons (25 to 29 and 30 to 34 years) with a duration of up to 12 months of unemployment are underrepresented in the participants' group. For an unemployment duration of up to 6 months (6 to 12 months), 10 (16) percent of the participants and 15 (20) percent of the non-participants are between 25 and 29 years old. The analogue figures for individuals aged 30 to 34 are 8 (15) percent for participants and 11 (18) percent for non-participants. In contrast, the shares of participants and non-participants with a longer unemployment experience (12 to 18 months as well as 18 to 24 months) are almost equal. A possible explanation for this finding may be given by the institutional set-up. An occupation in a job creation scheme should only be offered to the

[1] As mentioned in section 4.3.1, individuals younger than 25 and older than 55 years are excluded from analysis.

individual if no other ALMP programme is available and if the person cannot be integrated into regular employment. However, younger unemployed persons with only a short to medium duration of unemployment could be expected to have other opportunities on the labour market. With increasing unemployment duration, the number of these opportunities is assumed to be reduced ('negative duration dependence') and participation in a job creation scheme may be more likely. Persons aged 35 to 39 and 40 to 44 are placed in job creation schemes more often than younger individuals. The share of participants aged 35 to 39 (40 to 44) amounts to 20 (19) to 23 (22) percent. Therefore, these mid-aged persons are overrepresented in the programmes in West Germany independently of the preceding unemployment duration. This finding is in line with the purpose of the programme: to conserve the employability of the unemployed and to keep persons in touch with the labour market. The picture for older unemployed persons (45 to 49, 50 to 55 years) is the other way round. Whereas the share of participants (17 percent) with an unemployment duration of up to six months exceeds the share of this age class in the non-participating group (14 percent), in particular persons aged between 50 and 55 are less often placed in a programme with a longer unemployment duration. To give an example, 21 (26) percent of the participants (non-participants) are persons who are aged between 50 and 55 and who have an unemployment experience of 12 to 18 months before programme start. However, as mentioned in the description of the data (see section 4.3.1), I have excluded persons older than 55 years from the analysis to avoid problems related to early retirement issues. Therefore, one possible explanation for this underrepresentation may be the lack of available slots and a possible priority to the middle-aged groups in the placement process.

Independently of the preceding unemployment duration, there are some further notable differences between the participating and non-participating groups. It becomes obvious from the results in Tables A.1 and A.2 that foreigners and asylum seekers are less often regarded as participants for job creation schemes. A possible reason may be the availability of other programmes, e.g., language courses, that fit better to the needs of these groups. A further point to mention refers to the share of women in the participating group. Whereas between 45 (up to 6 months unemployed) and 49 percent (between 12 and 18 months unemployed) of the non-participants are women, the corresponding ratios in the participating groups amount to 33 to 37 percent only. Hence, women are clearly underrepresented in the participating groups in West Germany.

The number of placement propositions is, on average, higher in the group of participants. This indicates that persons are selected who are most in need of assistance. Moreover, it is in line with the legal postulation of particularly allocating persons facing barriers to employment. Whereas the non-participants have received about 4 placement propositions on average, the average number of job offers for the participants is 9 to 10. Clearly, this may be an important determinant that drives the participation decision. Considering the participation in a programme before unemployment indicates that participants in job creation schemes are more likely to be so-called programme careerists. Whereas only about three to six percent of the non-

participants have participated in a similar programme somewhen in the past, about one third of the participants did so.

Regarding the qualification of the individuals, there are also some ex ante observable differences between participants and non-participants. Except for persons starting the treatment between the 12th and the 18th month of the unemployment spell, participants do more often lack a completed professional training. From the figures of Tables A.1 and A.2 it becomes obvious that between 42 (unemployment duration between 12 and 18 months) and 50 percent (unemployment duration between 18 and 24 months) do not own a completed professional training. Consequently, caseworkers assess the individuals' qualification as skilled employee more rarely in the treatment than in the non-treatment group. Hence, between 27 to 32 percent of the participants are assessed as skilled employees. In contrast, for the non-participants these shares amount to 35-42 percent. These results indicate that participants are on average less educated than non-participants. The last point I want to mention for the West German groups is associated with the desired occupation of the individuals. Independently of the preceding unemployment duration, treated individuals seek more often for an occupation in farming. Whereas persons who look for a manufacturing profession are overrepresented in the group with up to six months of unemployment, persons with a service profession are underrepresented. However, for the longer unemployment durations these differences are not observable.

For East Germany (Tables A.3 and A.4) the findings are similar for most characteristics, but there are also some differences I want to emphasise. Younger unemployed persons (25 to 29, 30 to 34 years) are underrepresented in the participants' samples, too. Only 5 (10) to 6 (12) percent of the participants are 25 to 29 years (30 to 34 years) old, whereas in the non-participants' samples the corresponding shares are 7 (13) to 12 (17) percent. In contrast to West Germany, however, mid-aged persons (35 to 39, 40 to 44 years) are underrepresented or at best equal represented in the participating groups. Older unemployed aged 45 to 49 and 50 to 55 years are overrepresented in the programmes. Moreover, the largest group of participants is between 50 to 55 years old with shares of 27 to 29 percent. This result may be seen as an indication for the slightly different purpose in the allocation of participants to the programmes. The discussion in chapter 2 has highlighted the differences in the use of job creation schemes in both parts. Particularly in East Germany, job creation schemes have been used to a large extent, and a strict orientation on the targeting criteria could not be expected. The findings of the descriptive statistics support this expectation.

The findings for West Germany imply an allocation of individuals that is oriented on the individual employment chances since particularly mid-aged unemployed persons are potential participants. In contrast, in East Germany it is more likely that job creation schemes are used as a means to relieve the tense situation of the labour market and to conserve social peace. For that reason, the likelihood of participation is higher for older unemployed individuals.

The number of placement propositions differs similar to the picture for West Germany as well. On average, participants in East Germany have received a higher number of placement propositions compared to the non-participants. However, whereas

the ratio is almost 3:1 for West Germany, in the East it is less than 2:1. Since the number of placement propositions is no indicator for the number of available jobs, but only for the activity of the LEAs, two implications could be derived from this finding. First, the outside options in the East German labour market are worse compared to West Germany. Hence, there are more job applicants per vacancy and the probability of becoming regular employed is lower. Second, the stronger emphasis on ALMP programmes (see section 2.3.1) is reflected by the lower number of placement propositions in the participating groups, i.e. whereas in West Germany an unemployed individual receives between nine and ten placement propositions on average before the caseworker offers an occupation in a job creation scheme, in East Germany the number of required placement propositions amounts to six to eight on average only.

The shares of participating women are clearly higher in East Germany than in West Germany. About one half of the participants are females with unemployment durations until treatment of less than 12 months, and even about 59 to 62 percent with longer unemployment durations. A further difference refers to the qualification of the individuals. Whereas the majority of the participants in West Germany owns a Certificate of Secondary Education (CSE, *Hauptschulabschluss*) only, in East Germany more than half, i.e. between 50 to 53 percent of the treated individuals own an O-level degree (*Realschulabschluss*). This may be an important reason why caseworkers assess the individuals' qualification as skilled employees in 52 to 57 percent of the cases.

With respect to the desired occupation, the picture in East Germany is more similar to the West. Persons who seek for an occupation in farming are overrepresented in the programme groups compared to the share in the non-participating samples. A notable difference could be established for the service professions. Whereas there is an underrepresentation in the participants' groups with short to medium preceding unemployment duration in the West, the shares in the participating and non-participating groups differ hardly in East Germany. A last difference to be mentioned for East Germany refers to the number of UI recipients. In contrast to about 86 to 91 percent of the non-participants who receive UI, in the participating group these figures amount to 58-62 percent only. For West Germany, the figures in both groups are fairly similar.

5.3 Plausibility of the Conditional Independence Assumption in the Dynamic Setting

An important issue to be considered in the context of evaluating the treatment effects of job creation schemes using the matching estimator is the plausibility of the conditional (mean) independence assumption. As mentioned in section 3.4.2, for this assumption to be fulfilled in the dynamic setting, one has to observe all covariates that, conditional on having spent a given amount of time in unemployment u, jointly influence the participation decision at that time, D_u, and the outcome variable where such decision to be further postponed, $Y^0_{t,u}$ (Sianesi, 2004). If this assumption holds,

the observed probability distribution of subsequently finding a job or of later joining a programme for the non-participants in months u of the unemployment spell is the same as the counterfactual distribution for the treated individuals in u had they decided to wait longer. However, the choice of the relevant variables is not straightforward. Therefore, I relate the discussion on plausibility to the institutional set-up of the assignment process to job creation schemes (see section 2.3.2) as well as the rich set of variables available in the data set (see section 4.3.1).

To start with, let me briefly recapitulate the relevant aspects of the assignment process to be considered in the model. Allocation of an unemployed individual to a programme depends to a large extent on the caseworker's assessment of the individual's need of assistance. This need of assistance is assessed based on regular interviews of the unemployed individual to evaluate his or her efforts to find a job. In particular, groups with barriers to employment, e.g., long-term unemployed, severely disabled or older unemployed persons, are in need of assistance. Furthermore, to become eligible for participation in a job creation scheme, a person should be unemployed for at least 6 out of the last 12 months before the start of the programme and meet the criteria for the reception of UI benefits. Moreover, the need of assistance – as assessed by the caseworker – should imply that the potential participant cannot be integrated into regular employment or another ALMP programme at that time. In addition, a place in a programme has to be available. If these three preconditions are fulfilled, the caseworker may offer the unemployed individual a specific occupation in a job creation scheme. For the conditional independence assumption to be achieved it is crucial to identify enough information apt to capture these determinants of allocation.

The description of the data set has shown (section 4.3.1) that I can control for a large set of variables characterising the individual's past and current (at the start of the treatment) labour market situation. I assume the employment and unemployment experience of the individuals to be the most important determinant of the participation decision. Following Sianesi (2004), the elapsed unemployment duration of the individuals can be used to capture possible unobservable influences for the participation decision. These influences occur, for example, due to changes in the motivation, loss of hope, or perceived or actual deterioration of human capital. Moreover, in the presence of duration dependence the outflow to employment will differ between individuals with an unemployment duration of less than u for reasons unrelated to the programme. Thus, it is crucial to ensure that comparison individuals have spent in unemployment at least the time it took the participants to join the programme (Sianesi). For that reason, I condition on previous unemployment experience by stratifying the samples for East and West Germany by the discretised unemployment duration $U = 1, 2, \ldots, U_{\max}$ with $U_{\max} = 24$ and month as unit. Hence, I will analyse the employment effects of a job creation scheme for groups of individuals who join within the first two years of the current unemployment spell, i.e. whether there is a differential programme impact according to U. In the samples, more than 89 percent of all treated persons in job creation schemes are observed to enter a programme within that time span. However, I have to note that it is identified only whether the programme effect for persons joining in month k is better or worse than the effect

for persons joining in month l with $k \neq l$. The effects for participants of month k if they had decided to wait longer and started a treatment in month l are not evaluated.

In addition to the individual unemployment experience, programme effects may also differ with calendar time. For that reason, Fitzenberger and Speckesser (2005) note that an ideal approach would consider the different starting dates of unemployment as well as the different starting dates of the programmes.[2] However, this would lead to very small numbers of observations for the estimation of the treatment effects. Due to practical limitations, one has to rely on some kind of aggregation. The numbers of persons starting a programme do not differ too much (except the cohort of January 2001, see section 4.3.1). Moreover, as I use data on programmes that have started during one year and the discussion of the set-up of job creation schemes (see section 2.3.2) has shown that persons are not allocated to programmes at specific points of time, I assume the calendar date of unemployment entry to be of minor importance for the evaluation of the programme effects. Therefore, the six programme cohorts are aggregated into one sample considering the time the individuals have spent in unemployment before. However, to take account of possible seasonal differences, seasonal dummies for the different programme starts are included in the estimation. By doing so, the start of the unemployment spell in the estimation is implicitly regarded as well.

For the consideration of the employment experience and qualification of the individuals, the information that caseworkers survey to evaluate the unemployed person's likelihood of employment is used. The attributes comprise the duration of the last employment and a dummy for work experience, schooling and professional training of the individuals, and the work time of the last occupation. The duration of the last employment in combination with work experience are good proxies for the individual's familiarity with work. I consider employment duration in four different categories, i.e. up to 180 days, between 180 and 365 days, 366 to 730 days, and more than 730 days. To some extent, this distinction allows to proxy different levels of specific human capital accumulation during the jobs. Whereas one could expect persons who have worked for more than 2 years to own a relevant amount of specific human capital, this expectation would not hold for persons who have worked for less than 180 days. Unfortunately, as there is no information concerning the nature of the contract, i.e. whether the unemployed individual worked within a permanent or temporary contract before, the employment duration could only be used as a proxy. Schooling and professional training are regarded to assess the general human capital the person owns. Both variables are good indicators for the individual qualification. The work time of the last occupation is used to denote the past labour market involvement of the individuals. The characterisation of the individual's qualification is completed by a subjective assessment of the caseworkers. This subjective assessment seems particularly important to be considered in the model as it refers to observed and unobserved differences in the characteristics of individuals. It can therefore be viewed as a summary statistic of the amount as well as the transferability, effective-

[2] Fitzenberger and Speckesser (2005) further argue that not only participants' outcomes, but non-participants' outcomes as well may differ over time.

ness, and obsolescence of previous human capital accumulation. The desired occupation together with the desired work time of the individuals provides information on the economic sector of the job and the work time the individual seeks for. Furthermore, as there is no strong occupational mobility between economic sectors in Germany, the desired occupation can be used as a proxy for the past occupation of the individuals. In addition, consideration of the occupation type of the individual together with the implicit consideration of the unemployment spell in the estimation is necessary to capture possible anticipatory effects in terms of future employment. To give an example, seasonal unemployed workers may know in advance that their past employer will call them back. In that case, they would have no incentive to participate in a job creation scheme. By balancing the occupation as well as the month of unemployment start between treated and non-treated individuals the problem of this type of anticipatory effects should be ruled out.

There are also some socio-demographic attributes that are important determinants for the individual labour market prospects, like gender (women), citizenship (foreigner, asylum seeker), the age of the individuals (measured in six categories at the start of the treatment), the number of children and marriage/cohabitation. For example, the number of children and marriage/cohabitation are indicators for the social background, mobility and responsibility of the individual for other persons. Moreover, the characterisation of the labour market prospects is supported by a number of further variables. These variables comprise the application for vocational rehabilitation, whether or not the individual has joined an ALMP programme somewhen in the past, the number of placement propositions, the reception of UI, and the caseworker's assessment of the placement restrictions due to health restraints. The number of placement propositions is a direct indicator for the placement restraints and the employment chances of the individual. A higher number of unsuccessful placement propositions refers to a higher need of assistance in the placement process including an adjustment of the unemployed person's human capital to the demands of the labour market. Information on participation in an ALMP programme before may be used to identify potential 'programme careerists'. The description of the development of the labour market in East Germany has pointed out (see section 2.2) that the majority of the unemployed persons have at least once been allocated to a programme. This is also supported by the descriptives in Tables A.3 and A.4. Between 57 and 62 percent of the participants and between 24 and 36 percent of the non-participants have participated in a programme in East Germany before. Hence, the variable may be used as a proxy to capture the willingness of the participants to participate in an ALMP programme.

It is quite obvious that the caseworkers play an important role in the allocation process to the programmes. Since denying a job offer in a job creation scheme by the unemployed individual could be sanctioned by benefit cancellation, the caseworkers are expected to have the final word in the participation decision. If the caseworkers act upon unobservable information that is correlated with the individual's potential labour market outcomes, the conditional independence assumption would be violated. However, the information in use is surveyed by the caseworkers and is extended by their subjective assessment of the individuals qualification and placement

restrictions. Thus, I assume that caseworkers act idiosyncratically given the observable characteristics of the unemployed individual and the subjective assessments, i.e. the information available. The large degree of freedom of the caseworkers in the allocation process has implications for possible anticipatory effects in terms of future participation for the unemployed individuals. Unemployed individuals are unlikely to turn down an offered occupation in a job creation scheme in order to wait for a place in another ALMP programme since this would imply the cancellation of unemployment benefits or assistance. Therefore, a possible bias of the estimations due to anticipation effects of a future participation could be excluded.

The attributes considered so far concentrate on supply side aspects of the labour market. But, demand side aspects should be considered as well. They are relevant factors that influence the participation decisions and the labour market outcomes of the individuals due to the availability of places in ALMP programmes or regular jobs. These demand side aspects characterise the local labour market conditions (see Heckman, Ichimura, and Todd, 1997). On the one hand, the situation of the labour market differs between West and East Germany (see section 2.2). On the other hand, the enaction of SGB III in 1998 has provided a larger degree of self-responsibility and flexibility to the LEAs, i.e. the single agencies decide on the set of labour market policy interventions they offer. For that reason, it is quite possible that different local labour market conditions in the LEAs lead to a different mix of policy interventions. Explicit consideration of the 181 labour office districts is not feasible for estimation. Therefore, I use the classification of the FEA as described in section 4.2 to take account of the differing local labour market conditions in a parsimonious way.

In summary, the discussion of the conditional independence assumption has shown that, given the detailed and comprehensive data set at hand, I am able to consider all factors that determine participation and labour market outcomes. For that reason, it could be argued that the conditional independence assumption holds. Hence, the matching estimator in the dynamic setting could be used to evaluate the employment effects of job creation schemes in Germany.

5.4 Implementation

5.4.1 Estimation of the Propensity Scores

The propensity scores have been estimated by two series of 24 probit models (for West and East Germany), each one modelling the probability of starting a programme in month u, conditional on X, conditional on having reached the unemployment duration of $u \in \{1, \ldots, 24\}$ months, and conditional on not having received a treatment before u. The final model used for the propensity score estimations has been chosen based on the above discussion of the plausibility of the conditional independence assumption and extensive specification testing.[3] The results of these estimations are

[3] With respect to the findings of the discussion in section 3.5.1, I have selected the variables in order to minimise the differences between the treated and non-treated samples after matching, i.e. to maximise the balance between the covariate distributions.

given in Tables A.5 to A.10 for West Germany and Tables A.11 to A.16 for East Germany in the appendix to this chapter.

The results of the estimates differ by months until start of the treatments and by regions. I will give a brief summary of the characteristic finding of the estimations. The parameters for the age effects are not significant in most cases. As expected from the descriptives (section 5.2), foreigners and/or asylum seekers have reduced participation probabilities compared to Germans. Gender differences in West Germany are only observable for months $u = 1, 2, 8, 9$ and 10, where women have a reduced participation probability. A similar picture can be revealed for East Germany. Here, females are less often regarded for participation in months $u = 6, 7$ and 9 of the unemployment spell. However, women are more likely to start a programme in the first month ($u = 1$). The number of placement propositions as well as having participated in a programme before have a positive influence on the treatment decision in both regions. A difference can be observed with respect to the number of children. Whereas in West Germany persons with children are more likely to participate at almost any time of the unemployment spell, in East Germany this is only true for persons starting a programme early (between months $u = 2$ and 4). The chances for married persons or persons who live in cohabitation differ between the different points of times and regions. In West Germany, there is a tendency that those persons are less often regarded by the caseworkers for participation. In contrast, for East Germany this aspect makes no difference in terms of participation. However, it should be noted that living with a partner or being married increases the participation probability for months $u = 7$ and $u = 22$ and decreases it for month $u = 23$. With respect to the preceding duration of employment, persons with only a short duration (up to 180 days) are more likely to receive the treatments compared to persons with a work experience of more than 2 years. Unfortunately, the parameters for the qualification details (schooling, professional training, assessment of individual's qualification by the caseworker) are not significant in the majority of cases. Nevertheless, as the discussion in section 3.5.1 has emphasised, these variables should be regarded for balancing purposes. The parameters for the seasonal dummies show a mixed picture. For West Germany, there is an observable tendency that in the winter half of the year (between November 2000 and March 2001) persons are more likely to participate in a job creation scheme compared to July 2000. However, it should be noted that the parameters are not significant for all points of time. For persons who start a programme after only one month of unemployment, there is no seasonal influence in the participation decision. The same result with only a few exceptions could be established for persons who have an unemployment experience of more than 13 months. In contrast, the parameters for East Germany reveal the opposite picture. Here, participation seems to be more likely in the summer half of the year. Taking a look at the parameters of the regional context variables, the estimates indicate that participation is less likely in labour office districts with better labour market opportunities independently of region.

5.4.2 Common Support and Matching Quality

The treatment effects of job creation schemes have been estimated using NN matching on the propensity score without replacement as described in section 3.3.4. However, to check the common support, Fig. A.1 to A.3 in the appendix to this chapter visualise the density distributions of the estimated propensity scores at each u for West Germany. The analogues for East Germany are given in Fig. A.4 to A.6. The left hand side of the graphs provide the distributions for the non-treated samples ($D_u = 0$), the distributions of the estimated propensity scores for the treated samples ($D_u = 1$) are on the right hand side. It becomes obvious that for most of the non-treated groups the distribution is highly skewed to the left. Problems with the overlap could be expected in particular for the East German groups with an unemployment duration of more than twelve months. The graphs indicate that the participants' distribution has more density mass at the right. To overcome problems due to a lack of overlap in the distributions, I impose the 'minima and maxima comparison' condition with an additional calliper of 0.02 (see section 3.5.2).

Since I use propensity score matching to estimate the treatment effects, the ability of the procedure to balance the relevant covariates between treated and non-treated individuals has to be checked. Tables 5.1 and 5.2 provide some matching indicators for West and East Germany. In addition, the number of treated (column 2) and non-treated individuals (column 3) are presented.

The first indicator is the standardised difference in percent (*st_dif*) as described in section 3.5.5. To abbreviate the documentation, I present the median of the statistic before (column 4) and after matching (column 5). Moreover, the tables contain the calculated pseudo-R^2 of the propensity score estimation for the full (column 6) and the matched sample (column 7). The last column denotes the number of individuals lost due to the common support condition.

Starting with the median of the *st_dif* for West Germany, it becomes obvious that the treated and non-treated samples differ by between 6.23 ($u = 10$) and 14.55 percent ($u = 24$) before matching. After matching, the covariate distributions of both samples are far more balanced. The median of the *st_dif* is reduced to between 2.02 ($u = 1$) and 7.09 percent ($u = 20$). It should be noted that the remaining imbalance is larger for treated groups with only a small number of observations. For all groups with more than 150 treated observations, the matching procedure reduces the bias to below 4.51 percent. Therefore, it seems that the matching procedure is able to reduce the bias between treated and non-treated groups satisfactorily.

It has to be kept in mind that the median of the *st_dif* in percent only allows a crude approximation of the bias reduction in the single covariates. Whereas some of the covariates differ enormously between treated and non-treated groups before matching, others are more similar. Particularly for variables that are statistically significant in the propensity score estimations on a high level, e.g., the number of placement propositions or the fact of having participated in a programme before, the matching procedure strongly reduces the imbalances between treated and non-treated individuals. To give an example, whereas for persons starting a programme in West Germany in month $u = 1$ the *st_dif* for the number of placement proposi-

Table 5.1: Indicators of Covariate Balancing Before and After Matching (West Germany, by Month of Treatment Start)

Month[1]	No. of Treated Before	No. of Non-Treated Before	Median Bias Before[2]	Median Bias After[2]	Probit ps-R^2 Before[3]	Probit ps-R^2 After[3]	No. lost due to CS[4]
1	698	124,732	8.07	2.67	0.13	0.01	0
2	506	92,971	8.09	2.02	0.15	0.02	0
3	474	69,395	7.36	4.04	0.16	0.03	0
4	405	54,953	11.16	3.74	0.13	0.02	0
5	354	37,737	8.57	3.70	0.15	0.03	3
6	389	35,471	8.80	2.47	0.16	0.03	2
7	419	26,928	9.27	3.19	0.15	0.03	0
8	368	23,234	6.81	3.57	0.12	0.04	0
9	331	18,991	8.95	4.34	0.14	0.04	1
10	295	16,355	6.23	3.03	0.15	0.03	3
11	267	13,911	8.55	3.19	0.14	0.03	2
12	476	12,996	8.47	2.06	0.16	0.02	4
13	609	10,856	9.46	2.43	0.26	0.02	110
14	228	9,335	7.05	4.51	0.13	0.06	1
15	198	8,631	6.86	3.80	0.15	0.05	2
16	131	7,290	11.59	6.32	0.16	0.10	3
17	149	6,863	11.31	6.11	0.20	0.08	2
18	128	6,358	7.19	7.70	0.18	0.08	0
19	151	5,889	7.43	4.13	0.15	0.06	4
20	95	5,417	8.39	7.09	0.22	0.13	0
21	109	5,185	7.71	5.95	0.19	0.10	2
22	95	4,452	12.48	6.90	0.18	0.10	0
23	105	4,258	7.83	4.38	0.15	0.08	1
24	96	3,858	14.55	4.81	0.21	0.11	0

[1] Month refers to the month the treatment starts in the individual unemployment spell, u.
[2] Median bias denotes the median of the standardised difference in percent following Rosenbaum and Rubin (1985) before and after matching.
[3] Probit ps-R^2 refers to the pseudo R^2 computed for the full sample (before) and the matched sample (after).
[4] Number of treated individuals lost after imposing the common support condition.

tions (programme before unemployment) amounts to 60.77 (78.48) percent before, it is reduced to 5.61 (9.60) percent after matching.

The comparison of the pseudo-R^2 between the full and the matched sample supports the results of the *st_dif*. In particular for the larger groups, the systematic differences in the distributions of the covariates between the treated and non-treated groups are adjusted away. Unfortunately, this finding does not hold for all of the groups. Although the R^2 for persons starting a treatment in months $u = 16, 20, 21, 22$ and

$u = 24$ are clearly lower after matching than before, the figures after matching indicate some remaining differences. This should be kept in mind when interpreting the results. Considering the number of individuals lost due to the common support condition, only for persons starting a treatment in month $u = 13$ I lose about 18.06 percent of the treated individuals. Thus, the results indicate that adequate comparison individuals in the non-treated group that have similar propensity scores could be found for all points of time except $u = 13$.

Table 5.2: Indicators of Covariate Balancing Before and After Matching (East Germany, by Month of Treatment Start)

Month[1]	No. of Treated Before	No. of Non-Treated Before	Median Bias Before[2]	Median Bias After[2]	Probit ps-R^2 Before[3]	Probit ps-R^2 After[3]	No. lost due to CS[4]
1	1,589	62,952	5.83	1.79	0.16	0.01	1
2	1,203	45,520	8.85	1.39	0.18	0.01	2
3	1,225	38,938	7.85	1.88	0.15	0.01	4
4	1,085	32,414	5.27	2.09	0.13	0.01	0
5	938	23,656	5.40	1.99	0.12	0.01	5
6	1,143	24,450	6.25	2.10	0.12	0.01	4
7	1,382	20,214	5.81	1.74	0.13	0.01	8
8	1,139	17,406	6.48	1.73	0.12	0.01	1
9	1,014	15,452	5.21	1.70	0.12	0.01	3
10	906	13,150	7.18	1.47	0.11	0.01	3
11	924	12,247	7.28	1.65	0.12	0.00	1
12	1,222	11,150	5.63	1.90	0.16	0.01	36
13	1,672	9,547	6.48	1.34	0.20	0.00	208
14	961	7,031	5.01	2.52	0.22	0.01	178
15	910	6,862	4.68	1.68	0.20	0.01	152
16	741	5,760	5.11	2.59	0.20	0.02	113
17	690	5,402	7.00	2.98	0.20	0.02	102
18	585	4,720	6.34	2.86	0.20	0.02	123
19	533	4,406	5.68	2.76	0.23	0.03	126
20	402	3,907	7.23	3.65	0.18	0.03	70
21	422	3,910	6.00	2.62	0.22	0.02	86
22	336	3,212	7.22	4.36	0.21	0.03	52
23	328	3,193	6.50	2.86	0.20	0.03	26
24	260	2,721	9.10	3.77	0.21	0.03	27

[1] Month refers to the month the treatment starts in the individual unemployment spell, u.
[2] Median bias denotes the median of the standardised difference in percent following Rosenbaum and Rubin (1985) before and after matching.
[3] Probit ps-R^2 refers to the pseudo R^2 computed for the full sample (before) and the matched sample (after).
[4] Number of treated individuals lost after imposing the common support condition.

The quality indicators for East Germany show a better picture. Although the median of the *st_dif* before matching is lower compared to West Germany and amounts to 5.01 ($u = 14$) to 9.10 percent ($u = 24$), the remaining median *st_dif* after matching is only between 1.39 ($u = 2$) and 4.36 percent ($u = 22$). Thus, I could expect the covariate distributions to be balanced between the treated and matched non-treated groups. Again, the median of the *st_dif* should be seen as a rough approximation of the bias reduction only. In analogy to the example for West Germany regarding the number of placement propositions and having participated in a programme before for persons starting a programme in month $u = 1$, the results for East Germany are the following. Whereas the *st_dif* for the number of placement propositions (programme before) is 53.64 (87.87) percent before, it is reduced to 3.15 (0.84) percent after matching. The pseudo-R^2 point in the same direction. For no group, significant differences can be observed after matching. Unfortunately, as expected from the visual analysis of Fig. A.4 to A.6 in the appendix to this chapter, the common support condition poses a few problems particularly for groups starting a treatment after month $u = 12$. Hence, I lose between 7.93 ($u = 23$) and 23.64 percent ($u = 19$) of the treated individuals.

A final point to be mentioned refers to the number of potential comparison individuals at each point of time. For persons starting a treatment early in the unemployment spell, there is a large number of non-participants that could be used as potential matches, e.g., for month $u = 1$ the number of non-participants before matching amounts to 124,732 individuals in West Germany and 62,952 individuals in East Germany. Thus, it is more likely for the matching procedure to find adequate matches. However, for persons starting a treatment later in the unemployment spell, the number of potential comparisons decreases because the non-participants have left the unemployment for regular employment or other programmes yet. Therefore, for month $u = 24$ the number of non-participants before matching amounts to only 3,858 individuals in West Germany and 2,721 individuals in East Germany. Due to this, it is harder for the matching procedure to find adequate comparison individuals to approximate the counterfactual outcome of the participants.

5.5 Employment Effects of Job Creation Schemes

In the following, the results of the estimations for the employment effects of job creation schemes are to be discussed with a distinction between West and East Germany. As mentioned in section 3.5.6, the outcomes of the treated and matched non-treated individuals are compared from the start of the programmes onward. By doing so, a possible occurrence of locking-in effects has to be considered in particular shortly after programmes have started. Therefore, taking a look at the exit rates of the participants from the programmes is helpful to assess the possible magnitude of these effects. Tables A.17 and A.18 (West Germany) as well as A.19 and A.20 (East Germany) in the appendix to this chapter present the cumulated exit rates by month of treatment start. The figures of the tables show that the majority of participants leave the programme within one year after the start of the programmes, i.e. between 89.47

($u = 20$) and 98.17 percent ($u = 21$) of the participants in West Germany have left the programmes 12 months after programmes have started. The corresponding numbers for East Germany amount to 93.46 ($u = 24$) to 97.26 percent ($u = 21$).[4] Since employment effects are measured until 30 months after the start of the job creation schemes, successful programmes should overcompensate for the expected initial fall.

5.5.1 Effects for West Germany

The employment effects of job creation schemes with respect to the timing of treatment for West Germany are presented in Fig. 5.1 to 5.3. The graphs plot the development of the effects from the first month after start of the job creation schemes to month 30. The solid line describes the monthly employment effect, i.e. the difference in the employment rates between treated and matched non-treated individuals. The dotted lines are the lower and upper 95 percent confidence limits. In addition, to allow a more accurate discussion, Table 5.3 presents the results for five selected months.

The first thing to note, common to all groups independently of the preceding unemployment duration, is a large drop in the employment effects during the first months after programmes have started. For the majority of groups this decline in the difference of the employment rates reaches its peak around 6 months after programmes have started. It is reasonable to interpret the drop in the effects as the expected locking-in effects the participants experience whilst in the programmes. These locking-in effects are particularly articulated for groups starting a job creation scheme early in the unemployment spell.

To give an example, 6 months after the start of the programmes for persons starting in months $u = 1$ ($u = 6$) of the unemployment spell, the employment rate is -22.6 (-21.2) percentage points lower compared to the matched non-participants. For groups starting the programmes later in the unemployment spell, the locking-in effects are still observable but not as strong as for the earlier starting groups. Thus, participants who have started a programme after $u = 19$ ($u = 24$) months of unemployment have an employment rate that is -13.6 (-9.4) percentage points lower compared to a situation where they had decided to wait longer. The different magnitude of the locking-in effects for different starting points of the programmes during the unemployment spell reflects the different labour market situation of the individuals. Persons with only a short duration of unemployment could be expected to have better outside options on the labour market, i.e. finding jobs earlier, compared to individuals with a longer unemployment experience ('negative duration dependence', see above). Therefore, the higher employment probabilities of individuals who join a programme early in the unemployment spell lead to a stronger locking-in effect whilst on the programme.

The tables providing the exit rates of participants from the programmes have shown (Tables A.17, A.18) that most of the participants have left 12 months after

[4] In addition, it should be noted that in each region about one half to two thirds of the participants leave the programme between month 10 to 12 after programmes have started.

Fig. 5.1: Employment Effects for West Germany (Treatment Start Between Months $u = 1$ and $u = 8$)

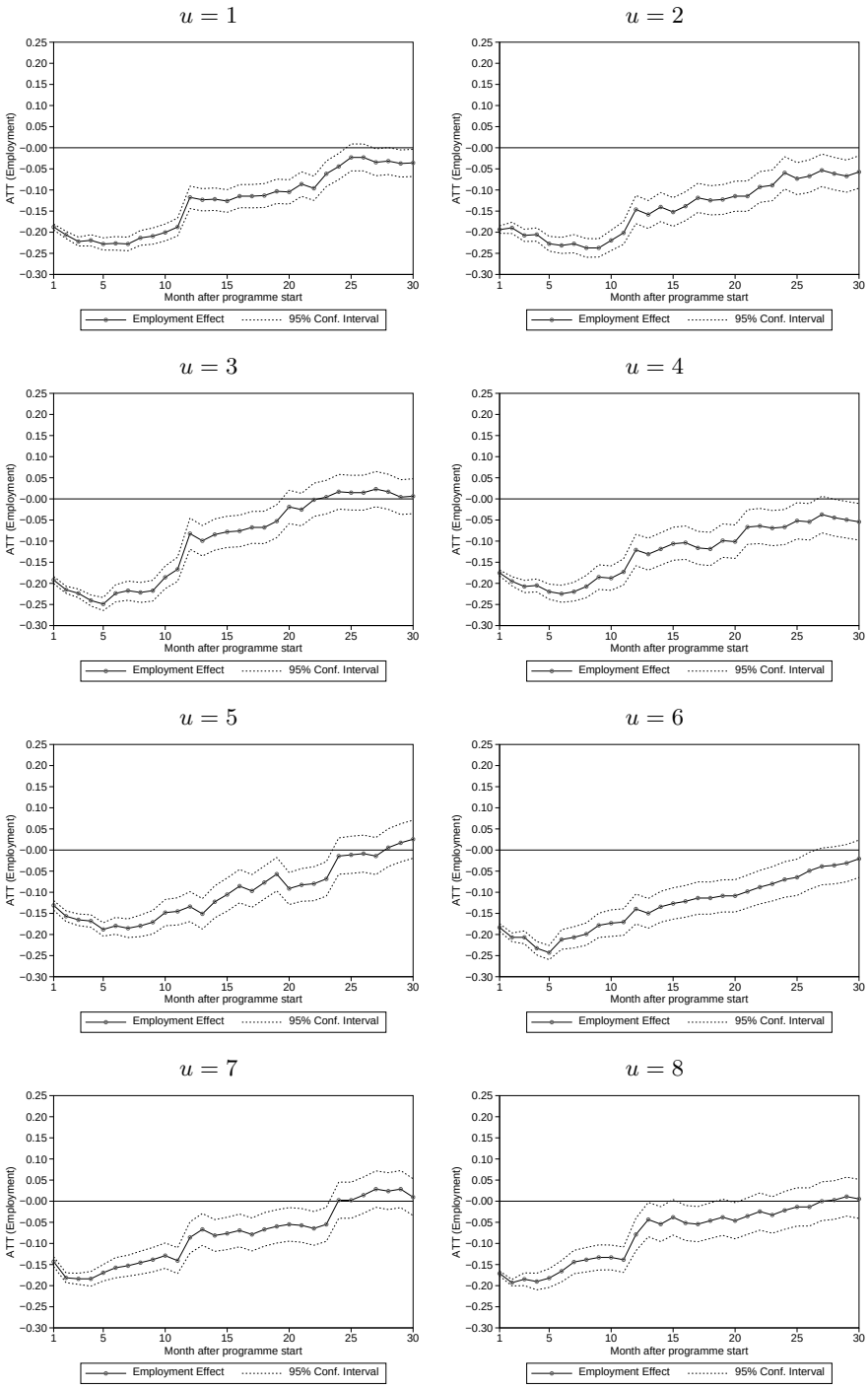


Fig. 5.2: Employment Effects for West Germany (Treatment Start Between Months $u = 9$ and $u = 16$)

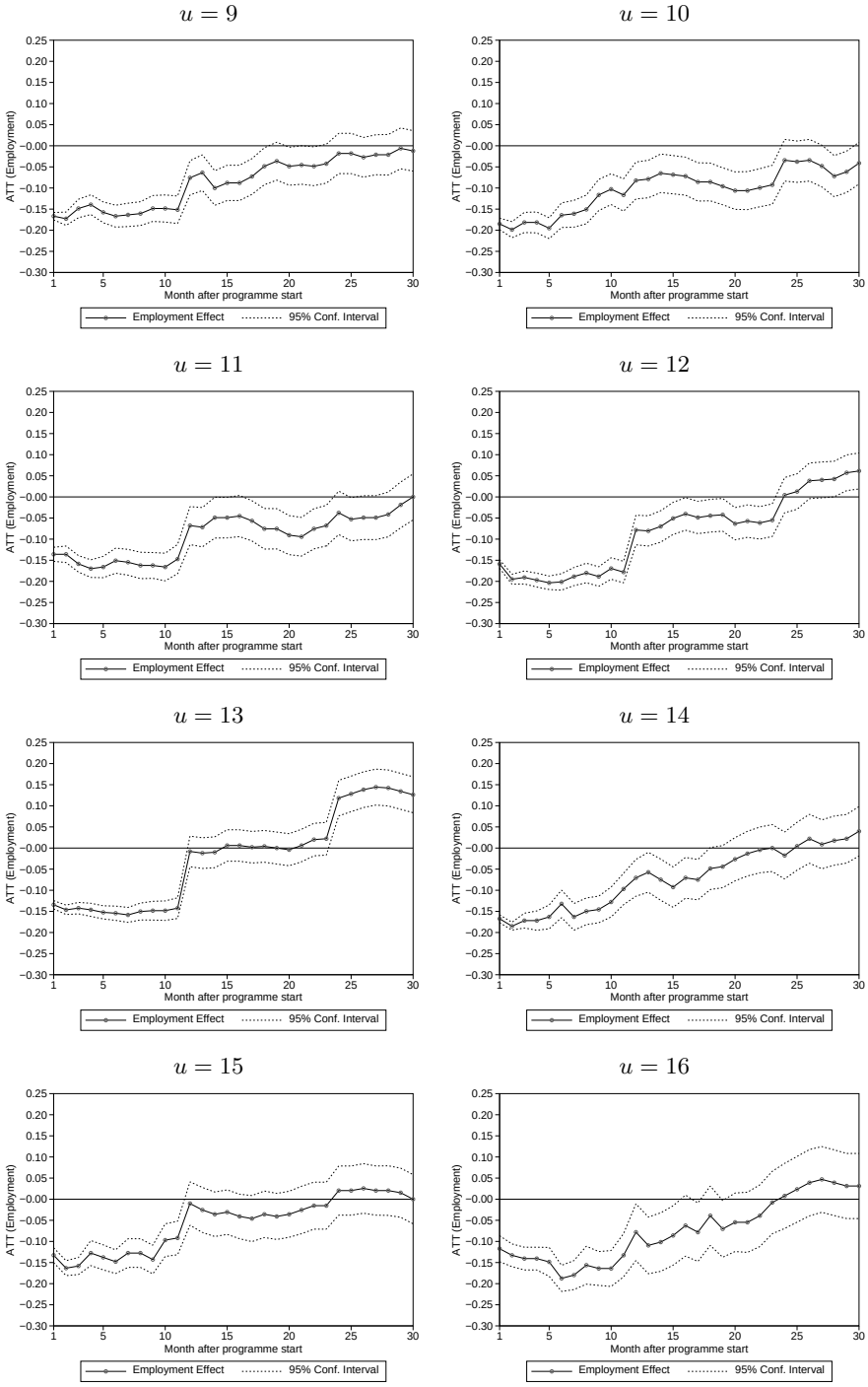


Fig. 5.3: Employment Effects for West Germany (Treatment Start Between Months $u = 17$ and $u = 24$)

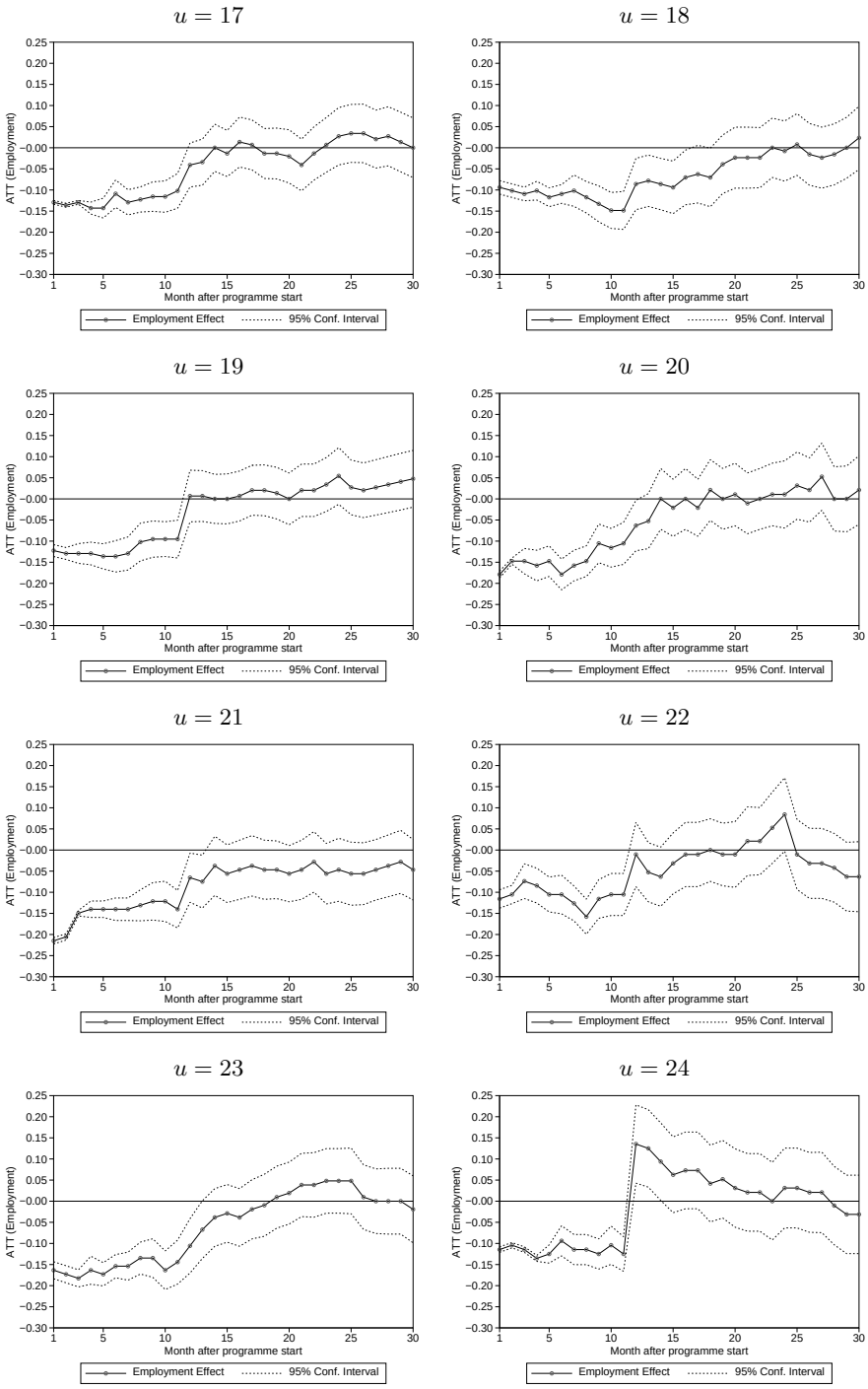


Table 5.3: Employment Effects for Selected Months (West Germany, by Month of Treatment Start)

u[1]	Obs.[2]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$
1	698	**-0.226**	**-0.117**	**-0.113**	**-0.044**	*-0.036*
2	506	**-0.231**	**-0.146**	**-0.125**	**-0.059**	**-0.057**
3	474	**-0.224**	**-0.082**	**-0.068**	0.017	0.006
4	405	**-0.225**	**-0.121**	**-0.119**	**-0.067**	*-0.054*
5	351	**-0.179**	**-0.134**	**-0.077**	-0.014	0.026
6	387	**-0.212**	**-0.140**	**-0.114**	**-0.070**	-0.021
7	419	**-0.158**	**-0.086**	**-0.067**	0.002	0.010
8	368	**-0.166**	**-0.079**	*-0.046*	-0.022	0.005
9	330	**-0.167**	**-0.076**	*-0.048*	-0.018	-0.012
10	292	**-0.164**	**-0.082**	**-0.086**	-0.034	-0.041
11	265	**-0.151**	**-0.068**	**-0.075**	-0.038	0.000
12	472	**-0.201**	**-0.078**	*-0.044*	0.004	**0.061**
13	499	**-0.154**	-0.008	0.004	**0.118**	**0.126**
14	227	**-0.132**	**-0.070**	-0.048	-0.018	0.040
15	196	**-0.148**	-0.010	-0.036	0.020	0.000
16	128	**-0.188**	*-0.078*	-0.039	0.008	0.031
17	147	**-0.109**	-0.041	-0.014	0.027	0.000
18	128	**-0.109**	**-0.086**	*-0.070*	-0.008	0.023
19	147	**-0.136**	0.007	0.020	0.054	0.048
20	95	**-0.179**	*-0.063*	0.021	0.011	0.021
21	107	**-0.140**	*-0.065*	-0.047	-0.047	-0.047
22	95	**-0.105**	-0.011	0.000	0.084	-0.063
23	104	**-0.154**	**-0.106**	-0.010	0.048	-0.019
24	96	**-0.094**	**0.135**	0.042	0.031	-0.031

Bold letters indicate significance on a 1% level, *italic* letters refer to the 5% level. $\Delta_{t,u}$ denotes the employment effect in month t after treatment start in month u of the unemployment spell.

[1] u denotes the months spent in open unemployment.

[2] Obs. refers to the number of treated observations when using nearest-neigbour matching without replacement and an additional calliper of 0.02. Common support is imposed by the minimum-maximum comparison.

the programmes have started. For this reason, locking-in effects should decrease at that time. The empirical picture confirms this expectation. For most of the groups, a jump in the employment effects could be observed between the months 12 and 14 after the start of the job creation scheme. However, some differences between the groups should be noted. On the one hand, for individuals starting a programme between months $u = 1$ to $u = 3$ of their unemployment spells, the jump in the employment effects in accordance with the exit rates is clearly observable. On the other hand, it is less pronounced for groups starting the treatment during months $u = 4$ to $u = 6$. For the groups starting the programme later than month $u = 7$,

the abrupt rise in the employment effects is again more emphasised analogous to the cumulated exit rates.

The picture of the development of the employment effects during the following months is mixed. Although the employment effects increase for the majority of the groups until the end of the observation period, for most of the groups the effects are at best insignificantly different from zero 30 months after programmes have started. For individuals who have started a programme in months $u = 1, 2$ and 4 of the unemployment spell, the employment rates are still -3.6, -5.7, and -5.4 percentage points below that of the matched non-participants at that time. However, for persons who have started the programmes after month $u = 12$ and 13 of unemployment, the results provide a more optimistic picture. Here, the employment rates are about 6.1 ($u = 12$) and 12.6 ($u = 13$) percentage points higher compared to the non-participants 30 months after programmes have started.

It should be noted that nine of the groups ($u = 1, 7, 10, 11, 12, 13, 15, 19, 21$) experience almost constant employment effects during the second year after programmes have started. Most of those groups have an employment rate that remains almost unchanged during that time and is below or equal to that of the matched non-participants. In particular, for the groups starting the treatment after 12 months of unemployment, there is a second jump of the employment effects at the start of the third year after the job creation schemes have started. For the smaller groups in analysis ($u = 20, 22, 24$), the employment effects reveal an erratic course. In this context, let me recall the results of the quality indicators (see section 5.4.2). For almost all of the groups with less than 150 treated individuals (except the cohorts starting a treatment in month $u = 17$ and $u = 23$), those quality indicators suggest some remaining imbalances in the covariate distributions.[5] Therefore, the estimated treatment effects should be interpreted with some caution.

To summarise the findings for West Germany, it becomes obvious that independently of the preceding unemployment duration participants in job creation schemes experience strong locking-in effects whilst in the programmes. Moreover, although I consider 24 distinct points of time in the individual unemployment spells when to start a programme, the results indicate that for most of the groups the effects do not differ significantly from zero even 30 months after programmes have started. For this reason, job creation schemes do not improve the employability of the participants in the short and medium run. In addition, individuals who start a programme early in the unemployment spell suffer from participation as the estimated negative employment effect 2.5 years after the start of the programmes imply. Thus, job creation schemes should be avoided early in the unemployment spell in the direction of searching longer in open unemployment. However, for the specific target group of job creation schemes, unemployed persons who exactly meet the eligibility criteria for long-term unemployment, the picture is not so bad at all. Here, the programme seems to work in terms of an improved employability of the participants as becomes

[5] For the groups starting a treatment in months $u = 16, 20, 21, 22$ and 24 of the unemployment spell, this is given by the R^2 statistics after matching, for the group starting in month $u = 18$ by the median of the *st_dif*.

obvious from the positive employment effects at the end of the observation period. Unfortunately, except for this group, the overall picture of the efficiency of job creation schemes with respect to its purpose – improving the employment chances of the participating individuals – is rather disappointing for West Germany.

5.5.2 Effects for East Germany

The development of the employment effects of job creation schemes in East Germany are given by Fig. 5.4 to 5.6 with respect to the timing of treatment in the unemployment spell. In analogy to the graphs for West Germany, the solid lines denote the employment effects in terms of the difference between treated and non-treated individuals for month 1 to month 30 after treatment has started. The dotted lines are the lower and upper 95 percent confidence limits. In addition, Table 5.4 provides more detailed information for five selected months.

Similar to the findings for West Germany, participants in job creation schemes in East Germany suffer from the locking-in effects of the programmes during the first months after programmes have started. However, in contrast to the West, the magnitude of these effects is less strong. I have described the tense situation of the East German labour market in detail in section 2.2. From this discussion, the smaller magnitude of the locking-in effects is not surprising because vacancies are rare. Therefore, there are not many outside options for non-participants. Consequently, the number of individuals leaving unemployment for regular jobs is lower than in West Germany, even if they have experienced only a short duration of unemployment. Thus, the employment rates for the majority of the participating groups 6 months after programmes have started are about 9 to 13 percentage points lower than for the matched non-participants. One exception are persons starting a job creation scheme in month $u = 23$ of the unemployment spell who suffer from participation by an employment effect of -19.2 percentage points.

Similar to West Germany, most of the participants have left the programmes about one year after programmes have started. However, a clear increase in the employment effects could not be established at that time, but several groups experience a modest increase. Unfortunately, in contrast to West Germany, where a rising development in the employment effects could be observed until the end of the observation period for most of the groups, this encouraging tendency could not be found for East Germany. In particular for groups with an unemployment duration until treatment of less than 12 months, the effects remain on an almost constant level until 30 months after treatments have started. Due to that, I find significant negative employment effects for all of the groups who have started the programmes between month $u = 1$ and $u = 12$ of the unemployment spell. The corresponding employment rates of these participants are between -7.3 ($u = 11$) and -2.8 ($u = 6$) percentage points lower at the end of the observation period compared to the situation where they had decided to wait longer in open unemployment.

For the groups starting the treatment between months $u = 13$ and $u = 23$ of the unemployment spell, the employment effects tend to increase over time. Unfortunately, no positive employment effects until 30 months after programmes have

Fig. 5.4: Employment Effects for East Germany (Treatment Start Between Months $u = 1$ and $u = 8$)

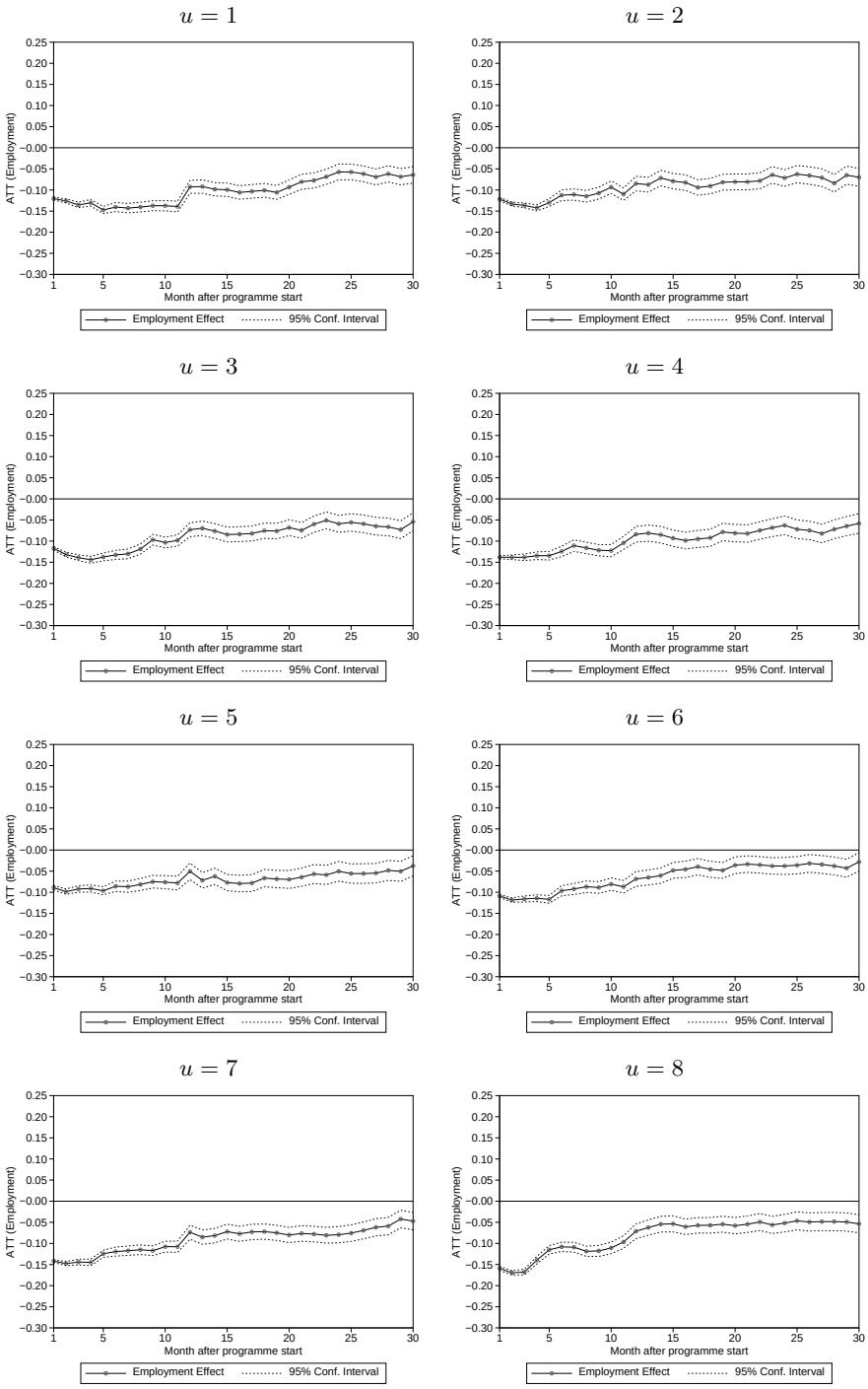


Fig. 5.5: Employment Effects for East Germany (Treatment Start Between Months $u = 9$ and $u = 16$)

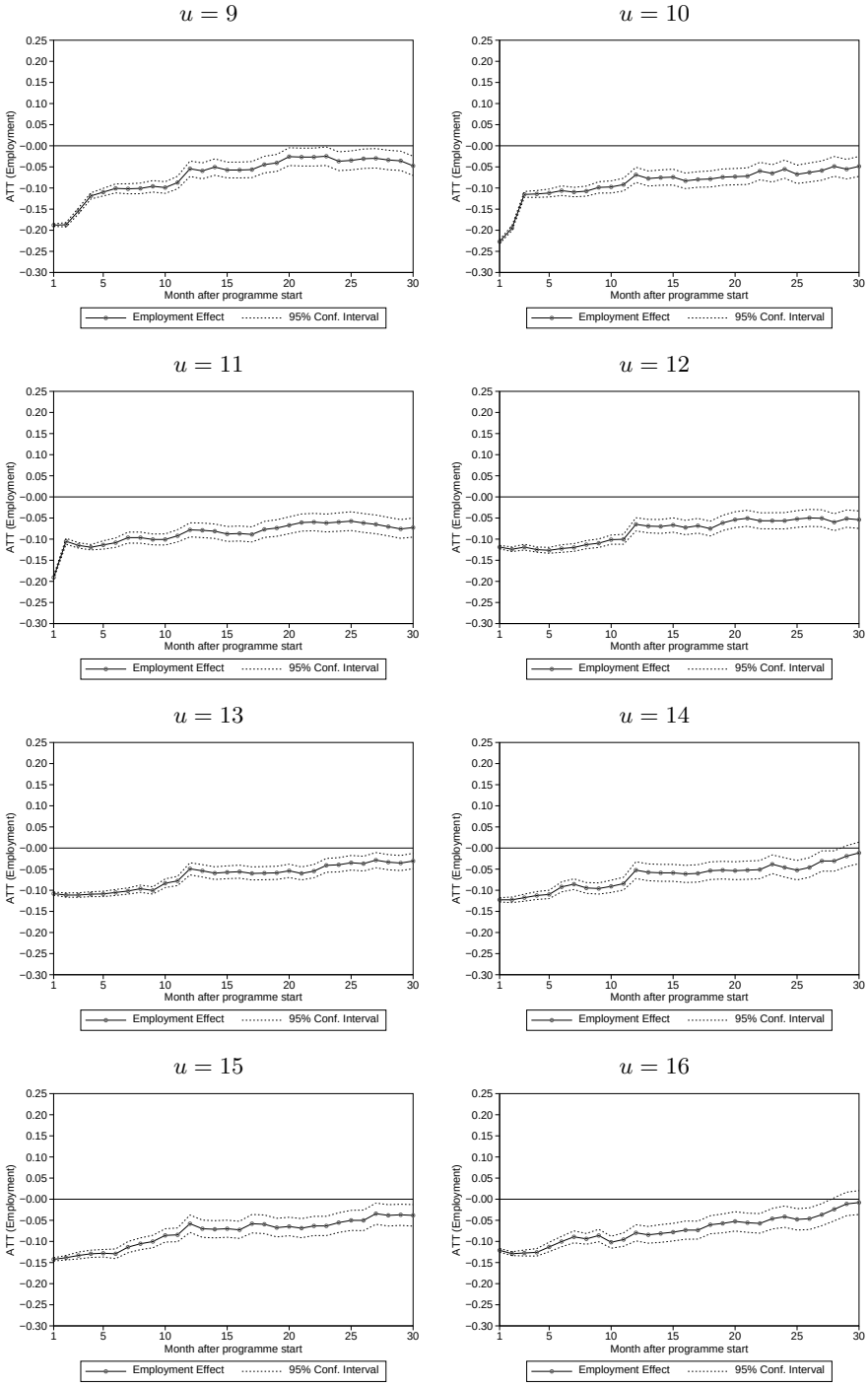


Fig. 5.6: Employment Effects for East Germany (Treatment Start Between Months $u = 17$ and $u = 24$)

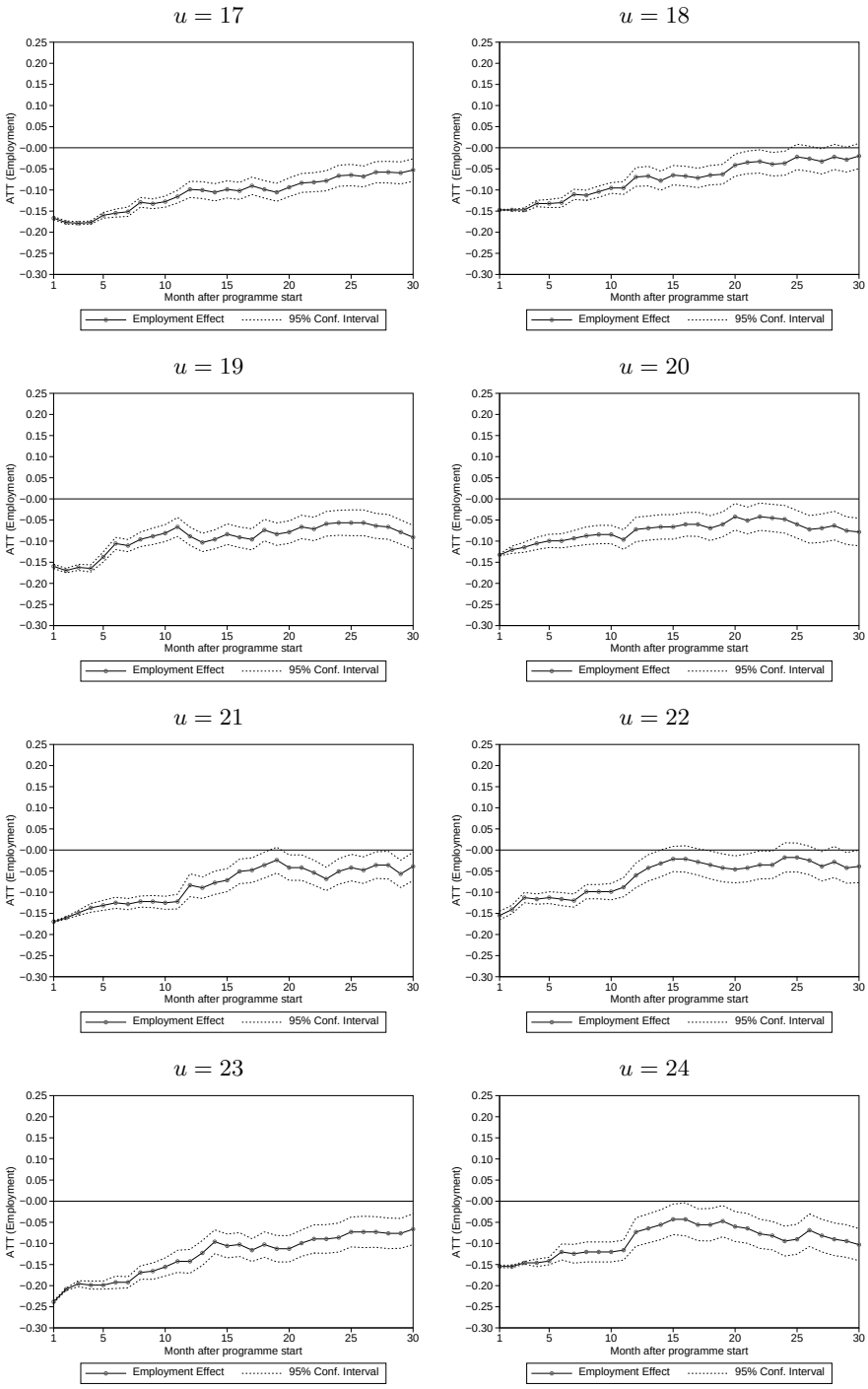


Table 5.4: Employment Effects for Selected Months (East Germany, by Month of Treatment Start)

u[1]	Obs.[2]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$
1	1,588	**-0.140**	**-0.093**	**-0.101**	**-0.057**	**-0.064**
2	1,201	**-0.112**	**-0.085**	**-0.091**	**-0.072**	**-0.070**
3	1,221	**-0.133**	**-0.073**	**-0.075**	**-0.059**	**-0.054**
4	1,085	**-0.124**	**-0.084**	**-0.092**	**-0.063**	**-0.058**
5	933	**-0.086**	**-0.050**	**-0.066**	**-0.050**	**-0.038**
6	1,139	**-0.097**	**-0.068**	**-0.046**	**-0.038**	*-0.028*
7	1,374	**-0.119**	**-0.074**	**-0.072**	**-0.079**	**-0.047**
8	1,138	**-0.108**	**-0.071**	**-0.057**	**-0.052**	**-0.054**
9	1,011	**-0.101**	**-0.054**	**-0.045**	**-0.037**	**-0.047**
10	903	**-0.106**	**-0.069**	**-0.079**	**-0.055**	**-0.049**
11	923	**-0.108**	**-0.078**	**-0.077**	**-0.060**	**-0.073**
12	1,186	**-0.122**	**-0.065**	**-0.075**	**-0.056**	**-0.054**
13	1,464	**-0.105**	**-0.049**	**-0.059**	**-0.040**	**-0.031**
14	783	**-0.092**	**-0.052**	**-0.054**	**-0.046**	-0.011
15	758	**-0.129**	**-0.058**	**-0.059**	**-0.055**	**-0.038**
16	628	**-0.100**	**-0.080**	**-0.061**	**-0.041**	-0.008
17	588	**-0.155**	**-0.099**	**-0.099**	**-0.066**	**-0.053**
18	462	**-0.130**	**-0.069**	**-0.065**	*-0.037*	-0.019
19	407	**-0.106**	**-0.088**	**-0.074**	**-0.057**	**-0.091**
20	332	**-0.099**	**-0.072**	**-0.069**	**-0.048**	**-0.078**
21	336	**-0.125**	**-0.083**	*-0.036*	**-0.051**	*-0.039*
22	284	**-0.116**	**-0.060**	*-0.035*	-0.018	*-0.039*
23	302	**-0.192**	**-0.142**	**-0.103**	**-0.086**	**-0.066**
24	233	**-0.120**	**-0.073**	**-0.056**	**-0.094**	**-0.103**

Bold letters indicate significance on a 1% level, *italic* letters refer to the 5% level. $\Delta_{t,u}$ denotes the employment effect in month t after treatment start in month u of the unemployment spell.

[1] u denotes the months spent in open unemployment.

[2] Obs. refers to the number of treated observations when using nearest-neigbour matching without replacement and an additional calliper of 0.02. Common support is imposed by the minimum-maximum comparison.

started could be established either. For persons who have started a programme in months $u = 14, 16$ and 18, the employment rates do not differ significantly from those of the matched non-participants at the end of the observation period. All other groups experience reduced employment chances due to participation in a job creation scheme. The worst effects are found for persons starting a programme in month $u = 24$: For those the employment effect in month 30 after programmes have started is -10.3 percentage points. Less severe, but still negative are the effects for persons starting the treatment in month $u = 13$. Here, the employment rates of the participants are -3.1 percentage points below those of the matched non-participants. For all

other groups except the three mentioned above, the employment effects at the end of the observation period are between those values (see Table 5.4).

In summary, the findings of the empirical analysis for East Germany indicate that job creation schemes are not able to improve the employment chances for participating individuals within the first 30 months after programmes have started. Although I find some effect heterogeneity with respect to the timing of treatment in the individual unemployment spell, positive treatment effects for any of the groups in analysis could not be established. Participants in job creation schemes suffer from strong locking-in effects during the first months after programmes have started. However, in contrast to the results for West Germany, the employment effects do not tend to rise after the majority of the participants have left the programmes. For that reason, the overall picture of the efficiency of job creation schemes in East Germany in terms of improved employment chances is rather unsatisfying. The results of the analysis are in line with the findings of previous empirical studies evaluating the effects of job creation schemes in Germany (see, e.g., Caliendo et al. (2006a; 2006c) and the review in section 2.4). On average, participation in the programmes in East Germany does not improve the integration chances of the individuals for regular (unsubsidised) employment for up to 30 months after programmes have started.

5.6 Summary

In this chapter, I have evaluated the effects of job creation schemes on the individual re-integration chances into regular (unsubsidised) employment for the participating individuals. A particular focus of the analysis has been on the timing of treatment in the individual unemployment spell. As emphasised in the recent empirical literature on evaluation of social programmes in comprehensive ALMP systems like Sweden, Switzerland and Germany (see, e.g., Sianesi, 2004, Steiger, 2004, and Fitzenberger and Speckesser, 2005), this timing conveys useful information to assess the efficiency of the programmes. Moreover, it allows to define participation and non-participation more dynamically, i.e. unemployed persons are non-participants as long as they do not join a programme or leave for regular employment.

With the exceptionally rich data set at hand (described in chapter 4), the problem of selection bias could be solved using the non-parametrical method of matching. Conditional on having the same distributions of the relevant covariates that determine programme participation and labour market outcomes, conditional on having the same unemployment experience and on not having joined a programme during this unemployment spell, I assume the non-participants' outcomes to be a reasonable approximation of the participants' outcomes if had they not participated yet and decided to wait longer in open unemployment.

Due to the large number of available covariates in the data set, exact matching has been infeasible for the estimation of the employment effects of job creation schemes. Instead, I have used propensity score matching that overcomes the curse of dimensionality. However, the application of propensity score matching requires a careful specification of the model as well as investigation whether the balancing property

is fulfilled for the single covariates. For that reason, I have presented a detailed discussion of the plausibility of the conditional independence assumption. In addition, I have calculated some indicators common in the empirical literature to assess the quality of the matches. The overall findings are quite satisfying. Whereas I could establish a good quality of the matches for the East German groups as well as for the larger West German groups, i.e. the matching procedure has balanced the covariate distributions accurately, some minor differences remained for the smaller groups in West Germany.

The differences in the labour market conditions and in the use of ALMP programmes in West and East Germany (as described in chapter 2) have been considered by separate estimations of the treatment effects for both regions. Treatment effects have been estimated for persons starting a treatment within the first 24 months of unemployment until 30 months after programmes have started. Since the programmes are in general promoted for 12 months and the majority of participants remain in the programmes for almost this duration, the results indicate strong locking-in effects during the first months of participation. These locking-in effects are more emphasised in West than in East Germany. One reason, among others, may be better labour market opportunities for non-participating individuals in West Germany.

At the end of the observation period (30 months after the start of the programmes) there are positive treatment effects for only two groups in West Germany. Namely, participants who have started the programmes after $u = 12$ and 13 months of unemployment experience 6.1 and 12.6 percentage point increased employment rates compared to the situation where they had decided to wait longer in open unemployment. Participants who have started the programmes early in the unemployment spell (month $u = 1, 2$ and 4) in West Germany suffer in terms of reduced employment rates compared to non-participation even 30 months after programmes have started. For the remaining groups in West Germany, the employment effects do not differ significantly from zero at the end of the observation period. Therefore, the results indicate that the intended positive aspects of job creation schemes are not able to overcompensate the initial locking-in effects during 2.5 years after programmes have started for most of the groups. The positive findings for persons who exactly meet the conditions for long-term unemployment indicate that the programmes work for this problem group of the labour market. Unfortunately, since the estimates of the employment effects for groups with slightly longer unemployment durations until treatment are insignificant, the findings for long-term unemployed persons could not be confirmed in general. However, this may also be due to the smaller numbers of treated observations in these groups.

Considering the employment effects at the end of the observation period in East Germany reveals a disappointing picture. No positive employment effects for any of the groups could be found. For persons starting a programme in months $u = 14, 16$ and 18 of the unemployment spell, the employment effects are at best insignificantly different from zero. All other groups experience negative employment effects even 30 months after treatments have started. The worst effects are found for persons starting a programme in month $u = 24$ with -10.3 percentage points.

In summary, the findings of the estimations for West and East Germany indicate that job creation schemes perform poorly in improving the employment chances of the participating individuals. To be more explicit, participation in the programmes does not help the individuals to re-integrate into regular (unsubsidised) employment. The only notable exception are long-term unemployed persons who start the programmes after 12 and 13 months of unemployment in West Germany. Long-term unemployed persons are one group that is most in need of assistance and re-integrating those persons into regular employment is difficult. Hence, the positive results are promising and show that job creation schemes may work for this target group although the findings are not confirmed for long-term unemployed persons in general.

6

Identifying Effect Heterogeneity to Improve the Efficiency of Job Creation Schemes in Germany*

6.1 Overview

In the last chapter, I have analysed the effects of job creation schemes on the re-integration into regular (unsubsidised) employment with respect to the timing of treatment in the individual unemployment spell. The results show that the average effects of these programmes (except two groups in West Germany) are negative or at best insignificantly different from zero. Although this is a common finding in the recent evaluation literature of ALMP programmes in Germany and in Europe, there is only little evidence on the reasons. ALMP programmes were seen as a reasonable opportunity to reduce and avoid unemployment for a long time, but the international experiences with the implemented programmes show a mixed picture. The majority of the programmes seem to be ineffective in terms of their aimed goals. As the overviews by Martin and Grubb (2001) for OECD countries and Calmfors et al. (2001) for Sweden clarify, most ALMP programmes are, in their present design and implementation, not able to achieve a lasting reduction of unemployment.

The aim of this chapter is to analyse reasons for the disappointing picture of job creation schemes in Germany. One possible explanation may be the poor quality of the programmes in conjunction with often cited stigma and locking-in effects. But leaving this argument aside for a moment, the results may also come from inefficient allocation mechanisms. The central motivation in this context is that programme impacts are heterogeneous (Manski, 1997 and 2000), and therefore negative average effects may not apply for all strata of the population. As Heckman, LaLonde, and Smith (1999) point out, negative mean impacts may be acceptable if most participants benefit from participation. Abandoning the 'common effect' assumption of treatment effects and identifying the individuals who gain from the programmes is an obvious opportunity to improve their future efficiency. If those personal characteristics could be identified which are responsible for the effect heterogeneity in individual impacts, this knowledge can be used for a better future allocation of individuals

* The results presented in this chapter are published in Caliendo, Hujer, and Thomsen (2006b).

to programmes. A good example is a situation where a certain programme works for older participants, but does not work for younger participants at all. If younger individuals have been more allocated to the programme in the past, the average effect may have been negative. Knowing the sources of effect heterogeneity would have helped to achieve a better allocation of unemployed persons to programmes in the future, i.e. assigning only older people in the example.

The analysis in this chapter is based on data on participants who have started a job creation scheme in February 2000 and on a comparison group of non-participants who were eligible for participation in a programme at the end of January 2000, but did not participate in February. The available information and set-up of the data set have been discussed in section 4.3.2. Since the data differ from that used for the analysis in chapter 5, I will consider two main issues in the empirical evaluation: First, I analyse whether individuals who started a programme in February 2000 gain on average from participation. Thereby, I take gender-specific and regional differences into account. Since the average effects may not apply to all strata of the population, I examine different sources of effect heterogeneity in a second step. I start with a selection of special problem groups of the labour market, like long-term unemployed or individuals without professional training, and estimate their treatment effects separately. After that, a simple indicator is constructed, called target score, based on the individual's number of disadvantages on the labour market, to analyse whether programme effects differ corresponding to the individual labour market obstacles. If programmes are tailored to the needs of the most disadvantaged, one would expect stronger effects for persons with a higher target score. Finally, I use the estimated participation probability to answer the question, whether a higher participation probability correlates with a higher programme effect.

All estimated employment effects in the later sections of this chapter correspond to December 2002 that is 35 months after programmes have started and the last month of the observation period. I am aware of the fact that consideration of only this month bears some shortcomings for a valuable interpretation of the programme effects. However, to give an idea of the development of the employment effects over time, I present the results during the observation period in the first step of the analysis. The estimates for the second step of the analysis focus on the mid-term effects of job creation schemes.

The treatment effects for the target groups and the target score are estimated using NN propensity score matching without replacement and a calliper of 0.02 (see section 3.3 for details). Since I use information on participants in job creation schemes who have started the programmes in one month only and analyse the effects for further sub-groups, explicit consideration of the timing of treatment in the individual unemployment spell, as in the last chapter, is not possible. Instead, I balance the distribution of the unemployment duration between treated and non-treated individuals in February 2000 within the propensity score. By doing so, I ensure comparison of persons with similar unemployment durations. In addition, as the only restriction for the non-treated is no participation in February 2000, I avoid a conditioning on future outcomes. Thus, the definition of the non-participants is similar to that in chapter 5. For the analysis of the third question – whether a higher participation probability

correlates with a higher programme effect – I use a stratification matching approach (see section 3.3.4) based on the estimated propensity score.

The chapter is organised as follows: In the next section I will present some estimation details concerning the estimation of the propensity scores as well as the matching quality. After that, the results for the reference groups in analysis will be presented, i.e. men and women in East and West Germany. In section 6.4, I will briefly review the different allocation mechanisms of ALMP. Having done so, the second issue of the analysis will be discussed in section 6.5. The final section will provide a summary of the findings of this chapter.

6.2 Some Estimation Details

6.2.1 Estimating the Propensity Score

I have estimated the propensity scores using binary logit models with participation as dependent variable. To take account of regional heterogeneity and to allow for gender-specific interaction effects, I have estimated separate models for men and women in East and West Germany.[1] Several model specifications have been tested for the selection of variables to be included in the model. The final specification contains explanatory variables, e.g., age, marital status, the number of children, nationality, and health restrictions, that describe the socio-demographic background of individuals. Furthermore, qualification is included by characteristics, like professional training, the occupational group, the professional rank, and previous work experience. The influence of the individual labour market history is given by the unemployment duration, the number of (successless) placement propositions, the duration of the last occupation, the last contact to the personal caseworker, whether the person is an aspirant to vocational rehabilitation, existing placement restraints due to health restrictions, and information on an ALMP programme participation in the past. The regional context is considered by using the classification of the FEA for comparable labour office districts (see section 4.2). Table 6.1 presents the estimation results for the participation probability in job creation schemes for the four main groups (men and women in West and East Germany). Additionally, the number of observations in those four participating and non-participating groups are included.

[1] I have also estimated the propensity scores for the two regions using dummy variables for sex. However, using the results of the two estimations ignores possible gender-specific interaction effects and the fact that the coefficients in the estimation differ in their significance and magnitude. This leads to a worse matching quality in the sense that the balancing of covariates after matching is reduced, i.e. the *st_dif* (see below) is higher.

Table 6.1: Estimation Results of the Logit-Models for the Propensity Score

	West Germany				East Germany			
	Men		Women		Men		Women	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.1739**	0.2731	**-3.1254**	0.4533	**-5.7880**	0.3659	**-8.0021**	0.3944
Age	**-0.0599**	0.0145	-0.0067	0.0235	**0.0901**	0.0141	**0.1702**	0.0136
Age (squared)	*0.0004*	0.0002	-0.0003	0.0003	**-0.0008**	0.0002	**-0.0019**	0.0002
Married	**-0.1676**	0.0612	**-0.4483**	0.0761	**0.2683**	0.0506	**0.1145**	0.0344
Number of children	*0.0653*	0.0281	-0.0183	0.0439	-0.0335	0.0266	-0.0238	0.0184
German	**0.4402**	0.0683	*0.2825*	0.1211	**0.6284**	0.1966	**0.7082**	0.2432
Health restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**0.9160**	0.1826	**1.3404**	0.2578	*0.5491*	0.2758	**1.1375**	0.2442
50% to under 80%	**0.8052**	0.1267	**0.6433**	0.1978	**0.4991**	0.1270	**0.6032**	0.1242
30% to under 50%	**1.1190**	0.3658	**1.9871**	0.4246	**0.5691**	0.1925	**0.7999**	0.1954
30% to under 50%, no equalis.[2]	0.2757	0.1570	0.0651	0.2685	-0.0708	0.1721	-0.0725	0.1826
Other health restrictions	-0.0472	0.0892	-0.0751	0.1390	**-0.1918**	0.0716	*-0.1422*	0.0608
Professional training								
None, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
None, with CSE	**-0.3364**	0.0622	0.2294	0.1334	0.1015	0.0823	**0.3428**	0.0865
Industrial training	**-0.6738**	0.0692	-0.0808	0.1399	*-0.1777*	0.0748	**0.3315**	0.0820
Full-time vocational school	**-0.7639**	0.2685	-0.0734	0.2432	-0.3223	0.2594	**0.8588**	0.1384
Technical school	-0.0987	0.1756	**0.7183**	0.1927	0.2227	0.1231	**1.0166**	0.0977
Polytechnic	0.3534	0.2009	**1.4983**	0.2144	-0.0135	0.2058	**1.0388**	0.1794
College, University	0.2399	0.1577	**1.0221**	0.1869	0.0810	0.1354	**0.9004**	0.1272
Occupational group								
Farming[3]	*0.2222*	0.0927	0.2628	0.2501	0.0092	0.0828	**0.2370**	0.0670
Mining, mineral extraction	-0.5605	0.4657	–	–	-0.7494	0.5154	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical occupations	**-0.5810**	0.1544	-0.1609	0.2605	-0.1954	0.0999	**0.2149**	0.0819
Service occupations	**-0.3077**	0.0544	**0.3167**	0.0995	**-0.1739**	0.0478	0.0127	0.0406
Other occupations	0.1023	0.1533	0.3933	0.2628	**-1.1891**	0.2170	**-1.2092**	0.2860
Professional rank[4]								
BC, unskilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	**-0.5499**	0.0982	-0.1637	0.1944	**-0.1811**	0.0597	0.0657	0.0525
WC, simple occupations	0.0163	0.1152	0.1490	0.1256	0.1809	0.1067	**0.2197**	0.0605
WC, advanced occupations	0.0877	0.1536	**0.5131**	0.1624	-0.2838	0.1662	-0.0404	0.1215
Other	-0.0112	0.0563	0.1512	0.1054	0.0345	0.0528	*0.1004*	0.0437
Work experience	**-0.3397**	0.0745	**-0.3139**	0.1017	**-0.2279**	0.0695	*-0.1175*	0.0527
Employment (months)[5]	**-0.0046**	0.0005	**-0.0033**	0.0007	**-0.0038**	0.0004	**-0.0028**	0.0003
Duration of unemployment (weeks)								
$<$13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
13-52 weeks	**0.2055**	0.0616	0.0698	0.0889	**0.4673**	0.0561	**0.2509**	0.0511
$>$52 weeks	**0.3087**	0.0678	0.0888	0.0974	**0.4498**	0.0599	**0.1694**	0.0509
Placement propositions	**0.0494**	0.0028	**0.0530**	0.0042	**0.0610**	0.0030	**0.0919**	0.0031
Last contact[6]	-0.0013	0.0125	**0.0520**	0.0177	**-0.1204**	0.0114	**-0.0644**	0.0085
Vocational rehabilitation[7]	-0.1533	0.1185	0.0696	0.2039	**0.2958**	0.0939	0.1535	0.1024
Placement restrictions	**-0.3396**	0.0989	-0.2654	0.1546	**-0.3164**	0.0870	**-0.3000**	0.0825

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Table 6.1: (continued)

	West Germany				East Germany			
	Men		Women		Men		Women	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Programme before unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], education	**0.2292**	0.0801	**0.5301**	0.1043	**0.4830**	0.0628	**0.5263**	0.0422
VT, adjustment	**0.6479**	0.2286	0.4613	0.4466	**0.6545**	0.0893	**0.5634**	0.0746
Job-prep. measure	-0.4764	1.0285	**2.6387**	0.5245	**1.1431**	0.4289	0.3364	0.5250
Job creation schemes	**2.1463**	0.0777	**3.0671**	0.1141	**1.7272**	0.0546	**1.5382**	0.0418
Rehabilitation measure	-0.0929	0.2706	**0.9368**	0.3406	0.4232	0.2273	0.3780	0.2720
Regional context variables								
Cluster Ia	–	–	–	–	-0.1040	0.1291	0.1421	0.1238
Cluster Ib	–	–	–	–	*-0.3077*	0.1248	-0.0242	0.1210
Cluster Ic	–	–	–	–	*-0.2838*	0.1361	-0.1841	0.1292
Cluster II	**-0.2225**	0.0730	**-0.5666**	0.0960	*Reference*		*Reference*	
Cluster III	*-0.1841*	0.0722	**-0.4601**	0.0917	–	–	–	–
Cluster IV	-0.0080	0.1002	**-0.4530**	0.1423	–	–	–	–
Cluster V	*Reference*		*Reference*		–	–	–	–
No. of Participants	2,140		1,052		2,924		5,035	
No. of Non-participants	44,095		34,227		64,788		76,512	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction (DoR).
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

It becomes obvious that allocation differs by regions. The coefficients of the socio-demographic variables show that the participation probability of men in West Germany decreases with age while in East Germany older men and women are more likely to participate. This indicates the slightly different purpose of the programmes in West and East Germany. Particularly in East Germany, job creation schemes function as a relief for the labour market and are used as a bridge to retirement. Furthermore, it has to be noted that German nationals are more likely to participate than foreigners. This may be due to the fact that other measures of ALMP (e.g., language courses) are preferred by foreigners. Regardless of region, health restrictions increase the individual participation probability. This finding indicates an allocation according to the legal basis.

The coefficients for the characteristics describing the qualification of the individuals emphasise gender-specific differences in the allocation. A higher qualification increases the participation probability in both regions for women, whereas the coefficients are insignificant for higher qualified men. The positive coefficients may be seen as an indication that for higher qualified women it is even harder to return to

regular employment; so they are willing to participate in job creation schemes to finish unemployment. As expected, work experience reduces the participation probability of all groups. Work experience is generally an important criterion for placement into regular employment. The finding indicates that experienced workers have other opportunities on the labour market. Since unemployment duration is an eligibility criterion for participation, its influence is of major importance. I included unemployment duration in three categories, up to 13 weeks, between 13 weeks and one year, and for more than one year. As expected, participation probability increases with unemployment duration.

The number of (successless) placement propositions is an indicator for bad labour market opportunities, and the coefficient affirms allocation according to the law. A last interesting point to note is that placement restrictions as evaluated by the caseworker harm the participation probability. This is somewhat surprising because job creation schemes should even be offered to these groups.

The coefficients for the regional context variables are in reference to the labour office districts with the best labour market environment. More severe labour market conditions correlate with a decrease in the participation probabilities in both parts. For men in East Germany, living in labour office districts with average labour market opportunities bears the clearest reduction of participation probability while analogously for West German women and men, living in labour office districts dominated by large cities with an above average unemployment shows the strongest decrease. The better the labour market conditions in the respective labour office district, the more likely are the unemployed persons to participate.

6.2.2 Matching Quality

Before I present the results, the quality of the propensity score estimation and the success of the matching procedure in balancing the covariates between treatment and comparison group should be checked. The model specification for the propensity score estimation is based on specification tests to identify the relevant variables. A simple method to validate the ability of a good prediction is the computation of hit-rates ('hit or miss' method, see section 3.5.1), i.e. the proportion of persons with a correct prediction of their status (participation and non-participation). As becomes obvious from Table 6.2, these hit-rates lie between 70.6 percent for men and 75.7 percent for women in West Germany. For East Germany, the hit-rates are 74.2 for men and 72.2 percent for women. This implies a quite accurate underlying model. However, the aim of propensity score matching is not to maximise the hit-rate, but to balance the covariates between treatment and comparison groups.

I do so by comparing the difference in percent between the respective participating and non-participating groups before and after matching took place. To abbreviate the documentation, I present only the means of the *st_dif* before and after matching for the four main groups (Table 6.2). While the mean *st_dif* lies between 10.83 and 14.62 percent before matching, it reduces to 1.60 to 3.20 percent after matching.

In addition, the results of the pseudo-R^2 from Table 6.2 show that the statistics are fairly low, and there are no systematic differences in the distributions of the co-

Table 6.2: Some Quality Indicators

	West Germany		East Germany	
	Men	**Women**	**Men**	**Women**
Before Matching				
Observations[1]	46,235	35,271	67,712	81,505
Hit-Rate[2]	70.6	75.7	74.2	72.2
Pseudo R^2	0.1389	0.1775	0.1225	0.1144
F-Test	2,406.8 (41)	1,679.4 (40)	2,951.3 (41)	4,323.3 (40)
Mean of *st_dif* (in percent)[3]	14.62	16.08	12.01	10.83
After Matching				
Observations[4]	4,246	1,960	5,846	10,054
Pseudo-R^2	0.006	0.009	0.004	0.003
F-Test	38.0 (41)	23.4 (40)	35.3 (41)	39.2 (40)
Mean of *st_dif* (in percent)[3]	2.51	3.20	1.78	1.60

[1] Observations are the sum of participating and nonparticipating individuals.

[2] *Hit-rates* are computed as follows: If the estimated propensity score is larger than the sample proportion of persons taking treatment, i.e. $\hat{P}(X) > \bar{P}$, observations are classified as '1'. If $\hat{P}(X) \leq \bar{P}$, observations are classified as '0'.

[3] *Mean of* st_dif is calculated as mean of the single characteristics' standardised differences in percent (see section 3.5.5 for details).

[4] Since I apply NN-matching without replacement and a calliper of 0.02, the number of treated individuals is reduced after matching by observations falling out of the region of common support. The numbers of the treated individuals can be calculated by dividing the number of observations by 2.

variates between both groups left after matching. The results of the F-tests (with degrees of freedom in brackets) point in the same direction indicating a joint influence before, and no joint influence after matching.

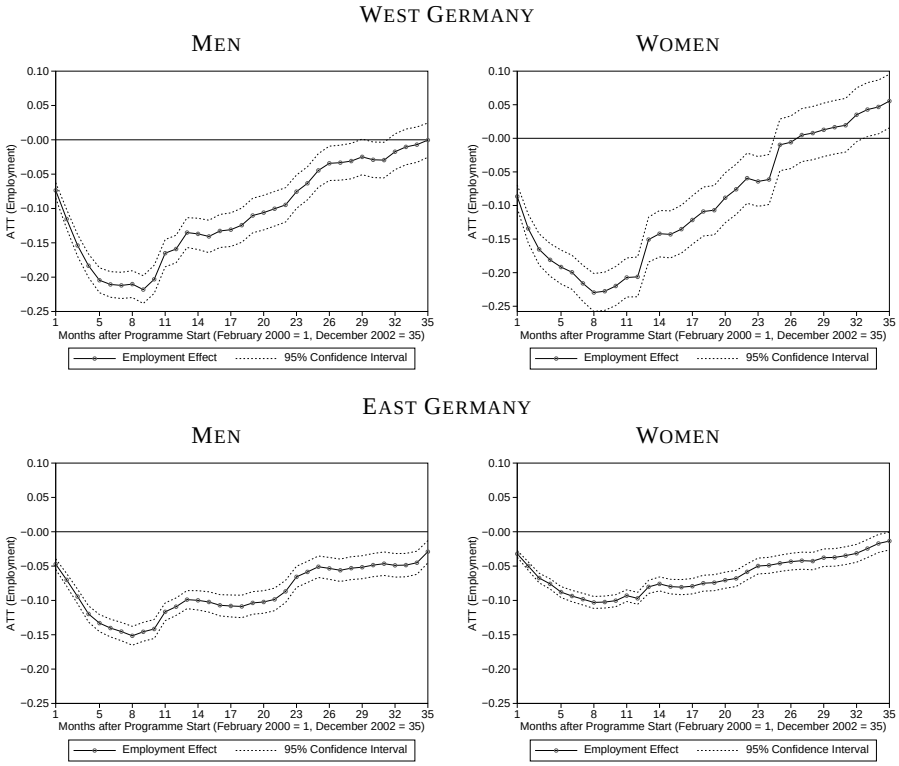
6.3 Employment Effects of the Reference Groups

The employment effects of job creation schemes for the reference groups of the analysis, men and women in West and East Germany, in the time between February 2000 and December 2002 are given in Fig. 6.1.[2] To some extent the development of the effects is comparable to the findings of chapter 5. In the first months after programmes have started, participants suffer from being locked into the programmes by negative employment effects. These locking-in effects have been expected as participating individuals have a reduced search intensity whilst in the programmes compared to non-participants. Due to this, a reasonable interpretation of the programme effects

[2] In analogy to the figures in chapter 5, the bold line denotes the treatment effect of the programmes as the difference between the employment rates of the treated and matched non-treated groups. The dotted lines refer to the 95 percent confidence limits.

on the employment rates should start after most of the participants have left the programmes, i.e. after 12 months. Since the purpose of job creation schemes is to stabilise and qualify unemployed persons for the re-integration into regular employment, I would expect increasing employment rates after the programmes have ended.

Fig. 6.1: ATT (Employment) Between February 2000 and December 2002



After an initial fall during the first months, there is a clear rising tendency of the employment effects for the groups in West Germany and a moderate rising tendency for the groups in East Germany. For the smallest group, women in West Germany, there is the strongest increase in the employment rates with significant positive effects at the end of the observation period in December 2002. The effects for men in West Germany are increasing, too, but the effects are insignificant at the end of the observation period, i.e. an increase in the employability due to participation cannot be established. While the effects in West Germany are clearly rising, I find a stepwise increase with relatively constant levels over one-year-periods in East Germany. Be-

sides that, the locking-in effects during the first year after the start of the programmes are not as strong as in the West. As mentioned in chapter 5, this finding may result from the worse outside options on the labour market for the non-participants.

Although the effects show a rising tendency for all groups, a significant increase of the employment rates due to participation can only be stated for women in West Germany, who have a significant positive effect of 4.6 percentage points in December 2002. For men in West Germany, I do not find any significant effects in December 2002, whereas men in East Germany have a significant negative effect of -2.9 percentage points. For women in East Germany, the effect is slightly better but still significant negative at -1.4 percentage points. It seems that job creation schemes rather decrease than increase the employment prospects of participants.

The results confirm the findings of the empirical analysis in chapter 5. Also when estimating the treatment effects until 35 months after programmes have started, the results do not reveal successful programmes in terms of increased employment chances. Except the group of women in West Germany, none of the reference groups in consideration experiences a significant increase of their employability. Of course, due to the strong locking-in effects participants are on average worse off after the programmes have ended compared to non-participants. However, as mentioned already, successful programmes should overcompensate for this initial fall.

6.4 Allocation Mechanisms

Obviously, one possible explanation for the discouraging results in terms of employment effects may be the poor quality of the programmes in association with stigma and locking-in effects. Another possible cause may be an inefficient allocation of participants. Since programme effects are heterogeneous (Manski, 1997 and 2000), the average effects depicted in the above section must not apply to all strata of the population. Negative mean impacts may be acceptable if most of the participants benefit from the programme (Heckman, LaLonde, and Smith, 1999). Abandoning the ‘common effect’ assumption of treatment effects and identifying the individuals that benefit from the programmes are obvious opportunities to improve the future efficiency of job creation schemes and ALMP. If individual characteristics could be identified that are responsible for the effect heterogeneity in individual impacts, one can use this knowledge to suggest allocation rules for a better future allocation of programme participants.

The potential improvement of allocation mechanisms is a much discussed topic in the recent evaluation literature (see, e.g., Frölich, 2001; Frölich, Lechner, and Steiger, 2003; Lechner and Smith, 2006). An optimal allocation should guarantee the best results according to the underlying programme goal where two goals – efficiency and equity – can be distinguished. If the goal is efficiency, programmes target at the maximisation of the impacts of the outcome of interest. If the goal is equity, treatments are administered to those individuals identified as ‘neediest’, i.e., for example, those individuals with the lowest predicted re-employment probabilities

(Plesca and Smith, 2002). Frölich et al. (2003) distinguish between non-statistical and statistical allocation mechanisms.

6.4.1 Non-Statistical Allocation Mechanisms

Caseworker discretion is the most common non-statistical allocation mechanism.[3] Potential programme participants are interviewed by their personal caseworker, and allocation to programmes is accomplished in accordance to the caseworker's evaluation of the unemployed person's capabilities, the individual's interests, and the availability of slots in the particular programmes. The crucial feature of the caseworker's mechanism for an optimal allocation of unemployed persons to programmes is the knowledge of the characteristics of the unemployed person, the situation on the local labour market, and the programme providers as well as the professional expertise of the caseworker (Lechner and Smith, 2006). There are only a few studies that examine the quality of caseworker allocation in Europe. Frölich (2001) analyses the effects of caseworker allocation in Sweden. Lechner and Smith and Frölich et al. (2003) evaluate the effectiveness of Swiss caseworkers in comparison to a simulated targeting system. The results indicate that caseworker allocation lacks the ability to achieve the expected programme goals. Reasons for the ineffectiveness of the caseworker allocation may be lack of knowledge of caseworkers regarding the effectiveness of certain programmes. Caseworkers have to build expectations about impacts of programmes on a very uncertain basis. In addition, the description of labour market policy in Germany has shown (see section 2.3.1) that there is a large number of different ALMP programmes. This broad variety of available programmes makes it difficult to select an optimal strategy for a specific person (Frölich et al.). Another issue concerns possible 'cream-skimming'. The experiences from the Job Training Partnership Act (JTPA) show that tying the funding to the performance of local programmes measured by job placement rates creates the incentive to serve the most able applicants without regarding how much different groups may have benefited from programmes (see, e.g., Bell and Orr, 2002).

Two other non-statistical allocation mechanisms should be noted. In addition to or as a substitute for caseworker allocation, participants can be assigned to programmes by deterministic or random assignment mechanisms. While random assignment avoids a selectivity bias in allocation, it is not able to control for effective placement without further restrictions.[4] Random assignment mechanisms are used in experimental design where a sample, based on a population of eligible persons, is allocated to services while another is not. For North American employment and training programmes in particular, experimental designs have been increasingly used to evaluate the treatment effects during the late 1980s and 1990s. They are less common in Europe, but experiments have been conducted in Germany (see Dann, Kirchmann, Spermann, and Volkert, 2001 and 2002), Britain, Norway and Sweden (see Heckman, LaLonde, and Smith, 1999). The limited number of social experiments reflects

[3] Unemployed persons in Germany are allocated to places in job creation schemes by this mechanism (see section 2.3.2).

[4] See also the discussion in section 3.2.3.

the ethical and financial restrictions that prevent implementation in many countries. A prominent example for the combination of caseworker allocation and random assignment is the National JTPA study of Bloom, Orr, Cave, Bell, and Doolittle (1993). Here, caseworkers determined the eligibility of the applicants for the programmes in a first step. In a second step, two thirds of this selected population were allocated to the treatment group and one third to the control group.

Deterministic allocation provides only a small potential to improve the effectiveness of placement since programme effects are heterogeneous with respect to individual characteristics and also for persons with the same set of (observable) characteristics. The virtues of deterministic rules are simplicity and equity in the sense of treating observationally equivalent cases in the same way (Plesca and Smith, 2002). An example for a deterministic allocation mechanism in Germany is that every first-time job-seeker receives an invitation to an individual counselling at the LEA.

6.4.2 Statistical Allocation Mechanisms

Statistical allocation mechanisms should avoid the possible problems by relying on some model indicating the individual gains of participation in a specific programme.[5] That is, the individual utility of participation in a programme is estimated by using a statistical method or an econometric model. Statistical allocation mechanisms are sometimes called *profiling* or *targeting*. Unfortunately, there is no consistent classification up to now. OECD (2002) only uses the term *profiling*. It defines *profiling* as 'a procedure where a numerical score, calculated on the basis of multivariate information, determines the referral of a job-seeker to further employment services'. In contrast, Frölich et al. (2003) distinguish between both terms. In their definition, *targeting* systems deal with a variety of programmes and with the hypothetical outcomes after participation in those programmes. Allocation of individuals is accomplished in order to maximise the labour market outcomes. *Profiling* is defined similar to the OECD definition; here, a single score is used to allocate the individuals to the programmes. The score is supposed to reflect the need of a person for intensive assistance in order to get back to work (Frölich et al.). In North American literature, both terms have been used interchangeably (see, e.g., Plesca and Smith, 2002), but *targeting* has recently become more relevant.[6] Hence, I use this term.

The starting point for the implementation of statistical allocation mechanisms in several countries has been evidence on the effectiveness of ALMP programmes. This evidence has suggested that programmes should be well-targeted to the needs of the individual job-seekers and the labour market, and that treatments should start

[5] The interested reader is referred to OECD (1998; 2002) for overviews of the experiences with statistical allocation mechanisms in member countries of the OECD. In addition, the studies by Berger, Black, and Smith (2001) and Black, Smith, Plesca, and Shannon (2003) analyse the value of statistical allocation in the US, and by Frölich et al. (2003) and Lechner and Smith (2006) for Switzerland.

[6] One reason is that the term *profiling* is used in the context of racial profiling in the US.

as early as possible in the unemployment spell (OECD, 1998).[7] Therefore, most systems aim at predicting the individual probability of becoming long-term unemployed. According to that prediction unemployed individuals are allocated to ALMP programmes. Examples for such systems are the Jobseeker Classification Instrument (JSCI) in Australia that started in 1994 and the Worker Profiling and Re-employment System (WPRS) in the US (since 1994). Similar systems are used in the Netherlands ('chance meter') and New Zealand (Service Group Indicator, SGI).

The Canadian system was different to those noted so far. During the years 1994 to 1999, the Service and Outcome Measurement System (SOMS) was used. This system was no formal profiling system to identify the job-seekers' risk of becoming long-term unemployed, but a combination of 'characteristics screening' and judgement by counsellors. The idea of the system was to assess the best possible (in terms of labour market outcomes) and cost-effective treatment (in terms of unemployment insurance saving) to the eligible individuals (OECD, 1998). However, since the system was very data demanding, violations of the unemployed persons' privacy rules were expected. Moreover, SOMS coincided with a lay-off of 5,000 service delivery staff. Thus, the remaining staff caused systematic disregard and refusal of the system. For these reasons, SOMS was shut down and the database was deleted in 2002 (Frölich et al., 2003). A similar system to SOMS is the Frontline Decision Support System (FDSS) in the US that uses an estimated employability score to determine the programmes unemployed persons are allocated to. FDSS started with a pilot-testing phase in 2002.

Other countries, like Germany (see Rudolph and Müntnich, 2001), Korea, Ireland and the UK (see OECD, 2002), conducted pilot projects for statistical allocation mechanisms, too. Denmark, Finland, Mexico, and the Slovak Republic plan or consider the implementation of statistical allocation in the near future.

6.5 Targeting

Based on the OECD definition (see above), I will present three approaches to identify potential sources of effect heterogeneity which could be used – if successful – for a better targeting in future. In a first step, I will select target groups with disadvantages on the labour market, e.g., long-term unemployed persons. In a second step, I will use these definitions and build a simple index that I call target score. The target score simply sums up the number of individual disadvantages. If programmes are tailored to the needs of the most disadvantaged on the labour market, I would expect higher impacts for persons with higher target scores. For the evaluation of the effects in the target groups and for the target scores, I estimate separate propensity scores for

[7] In this context, it should be noted that the recommendation to start programmes early in the unemployment spell has no general meaning. For example, the evidence on the employment effects of job creation schemes (chapter 5) has shown that if programmes are offered too early in the unemployment spell, they may have more harmful effects compared to when offered later.

each group and category considered.[8] Finally, I test whether the effects differ corresponding to different participation probabilities. To do so, I stratify the sample in 20 sub-samples along the propensity score of the participants and apply a stratification matching estimator.

6.5.1 Effects for Selected Target Groups

Identifying groups of participants who benefit from programmes is a central purpose of programme evaluation. Recent evaluation studies of job creation schemes in Germany (see, e.g., Caliendo et al., 2006a; 2006c) and experiences from abroad (Martin and Grubb, 2001) recommend a tighter targeting of programmes to individuals with disadvantages on the labour market. Selecting persons that are supposed to have a below average employability is a reasonable first approach to identify possible effect heterogeneity due to personal characteristics. The discussion of the legal basis for ALMP in Germany (section 2.3) has highlighted that there are several groups of individuals who should be promoted predominantly. These are long-term unemployed persons, individuals with health restrictions, or persons who apply for vocational rehabilitation.[9] Further target groups are younger and older unemployed as well as workers without a completed professional training. In addition, job creation schemes should be particularly applied to individuals with special placement restrictions.

The selection is oriented on these legal definitions. I estimate the effects for participants younger than 25 years and for participants older than 50 years respectively. Further groups are long-term unemployed persons who are unemployed for more than one year at the start of the programmes, individuals with special placement restrictions due to health restrictions, and aspirants to vocational rehabilitation. In addition, four groups with other barriers to employment are selected. The first group contains individuals with more than five (unsuccessful) placement propositions by the local labour offices, the second group are persons who have already participated in an ALMP programme before unemployment. Group three comprises individuals without professional training, and the last group are people without any work experience.

Table 6.3 contains the shares of individuals in each of the selected groups differentiated by treatment status. For most of the groups, the results show significant differences of the shares between treatment and comparison group. Thus, one can assume that these characteristics affect the allocation decision to some extent. Surprisingly, long-term unemployment (more than 52 weeks), which is expected to be an important selection criterion (in accordance to the law), differs only for men in East Germany. Additionally, the share of aspirants to vocational rehabilitation of this group and the proportions of men and women without work experience in the region

[8] The results of the propensity score estimations are provided in Tables B.1 to B.16 in the appendix to this chapter. Moreover, Table B.17 contains the results of the mean *st_dif* before and after matching for the target groups and target scores.

[9] These are especially persons who are not able to work in their profession anymore due to health restrictions and therefore should receive a promotion for vocational rehabilitation.

Table 6.3: Descriptive Statistics for the Selected Target Groups (Participants and Non-Participants)

West Germany	**Men**		**Women**	
	Part.	Non-Part.	Part.	Non-Part.
Target Group	Shares in percent[1]			
Age $<$ 25 years	21.40	9.30	17.30	7.14
Age $>$ 50 years	16.12	37.27	15.30	35.21
Without professional training	62.62	49.12	45.25	49.94
Without work experience	12.76	7.44	15.11	7.44
Long-term unemployed[2]	39.16*	40.79*	39.16*	42.16*
$>$ 5 placement propositions	49.21	21.21	42.49	17.05
Vocational rehabilitation[3]	5.19	6.27	4.18	3.11
Placement restrictions[4]	16.54	21.58	14.07	17.51
Participation in ALMP before unemployment	28.55	10.05	33.17	8.86

East Germany	**Men**		**Women**	
	Part.	Non-Part.	Part.	Non-Part.
Target Group	Shares in percent[1]			
Age $<$ 25 years	8.21	13.49	2.94	6.36
Age $>$ 50 years	38.06	31.05	30.69	35.71
Without professional training	28.63	23.10	22.26	25.85
Without work experience	10.02*	10.84*	9.89*	10.38*
Long-term unemployed[2]	37.55	30.75	49.45	48.89
$>$ 5 placement propositions	41.24	17.87	37.28	15.32
Vocational rehabilitation[3]	7.46*	7.48*	3.10	4.60
Placement restrictions[4]	13.47	16.16	7.47	11.92
Participation in ALMP before unemployment	47.16	17.08	57.28	27.85

* Denotes approximate equality of shares between treatment and comparison group (5% significance level).

[1] Shares are computed with respect to the number of participating/nonparticipating individuals in the according main group.

[2] Unemployment duration for participants and nonparticipants at end of January 2000.

[3] Persons in vocational rehabilitation are unable to work in their profession any longer and have to be qualified for a new profession.

[4] Placement restrictions refer to the assessment of the caseworker that health restrictions of the job-seeker reduce the number the job opportunities.

are approximately equal for participants and non-participants. This shows once again the different purpose of job creation schemes in East and West Germany.

Further notable findings are the different proportions of participants between the regions. While the ratio of younger unemployed (below 25 years) in West Germany is clearly larger in the participants' group, the situation in East Germany is the other way round. Older unemployed are more likely to participate here. These differences have to be interpreted in light of the different labour market situation in East and West

Germany and the consequently different purpose of job creation schemes in both regions. Placing a larger share of young unemployed into programmes in West Germany complies to the law that postulates stabilising efforts for later re-integration. In East Germany, job creation schemes are used to relieve the labour market, and therefore older unemployed are more likely to participate than younger ones.

Besides the age differences, it has to be mentioned that persons with a larger number of placement propositions or who have participated in an ALMP programme before unemployment are more frequent in the participating group. This confirms the expectation that the number of successless placement propositions directly indicates placement difficulties (see also the discussion in chapter 5). Furthermore, earlier participation may identify so-called 'programme careerists'. These are persons assigned to ALMP programmes subsequently with short spells of unemployment between the single measures.

The employment effects for these nine target groups in December 2002 are provided in Table 6.4. As above, I distinguish between gender and regions. It becomes obvious that programme effects are heterogeneous across the selected groups. Whereas the analysis of the four main groups has shown that on average men and women in East Germany suffer from participation, men in West Germany experience insignificant employment effects, and women in the same region benefit from participation, the effects for the target groups are not identical with those findings. Consideration of the effects for the selected groups of male participants in West Germany shows that the effects are insignificant for almost all groups, too, but with one exception. The group of long-term unemployed men benefits from participation and shows an employment rate which is 5.03 percentage points higher compared to the rate of matched non-participants in December 2002. In contrast, women in that region do on average benefit from participation (main group). With regard to the results in Table 6.4, it becomes clear that this finding does not hold for all of the target groups. While three groups clearly gain from participation, i.e. older unemployed with an employment effect of 12.67, long-term unemployed with 11.25, and hard-to-place women indicated by the number of placement propositions with 7.79 percentage points, the others do not experience any enhancement of their employability. Anyhow, the three significant effects are higher than the effects for the whole sample of women in West Germany.

Turning to the estimates for the East German groups reveals a quite similar picture. Again, most of the estimates are statistically insignificant and participants do neither suffer nor benefit from participation at all in December 2002. Whereas the results for men in this region have been significantly negative on average, this finding is confirmed by the result of one group only, namely by participants who have taken part in an ALMP programme before (-3.36 percentage points). All other estimates do not show significant differences to the non-participants' outcomes. Regarding the female participants in East Germany, I find long-term unemployed women to benefit from participation with an increase of the employment rate by 2.45 percentage points compared to non-participation. No significant differences in the employment rates can be established for the remaining groups.

Table 6.4: Effects for Selected Target Groups (December 2002)[1]

West Germany	**Men**			**Women**		
Target Group	Effect	Std. Err.	No. of Participants	Effect	Std. Err.	No. of Participants
Age $<$ 25 years	-0.0276	0.0326	440	-0.0679	0.0573	161
Age $>$ 50 years	0.0262	0.0241	344	**0.1267**	0.0562	159
Without prof. training	-0.0046	0.0169	1,323	0.0425	0.0297	451
Without work experience	-0.0040	0.0414	256	-0.0703	0.0595	128
Long-term unemployed	**0.0503**	0.0169	832	**0.1125**	0.0326	403
$>$ 5 placement props.	0.0300	0.0176	1,039	**0.0779**	0.0302	400
Vocational rehabilitation[2]	0.0300	0.0603	106	0.0571	0.0845	36
Placement restrictions[3]	0.0153	0.0287	335	0.1026	0.0562	130
Participation in ALMP before unemployment	-0.0323	0.0217	594	0.0541	0.0313	279

East Germany	**Men**			**Women**		
Target Group	Effect	Std. Err.	No. of Participants	Effect	Std. Err.	No. of Participants
Age $<$ 25 years	-0.0437	0.0503	240	0.0278	0.0589	148
Age $>$ 50 years	-0.0130	0.0079	1,109	-0.0020	0.0093	1,529
Without prof. training	0.0120	0.0161	833	-0.0215	0.0156	1,119
Without work experience	0.0069	0.0349	292	0.0225	0.0220	495
Long-term unemployed	-0.0018	0.0093	1,097	**0.0245**	0.0080	2,487
$>$ 5 placement props.	-0.0264	0.0145	1,201	-0.0054	0.0108	1,869
Vocational rehabilitation[2]	-0.0140	0.0369	217	-0.0068	0.0418	154
Placement restrictions[3]	0.0189	0.0254	394	-0.0166	0.0217	368
Participation in ALMP before unemployment	**-0.0336**	0.0114	1,378	-0.0028	0.0079	2,877

Bold letters indicate significance on a 5% level. Standard errors calculated by bootstrapping with 50 replications.

[1] Effects are estimated using 1-NN matching without replacement and calliper of 0.02.

[2] Persons in vocational rehabilitation are not able to work in their profession anymore and therefore have to be qualified for a new profession.

[3] Placement restrictions refer to the assessment of the caseworker that health restrictions of the job-seeker reduce the number the job opportunities.

Together with the results for the West German groups, especially long-term unemployed participants seem to benefit from programmes (except for men in East Germany). This finding is somewhat satisfactory since job creation schemes are especially arranged for this group.[10] Although the employment effects refer to one single

[10] The results of the employment effects with respect to the timing of treatments for West Germany establish a similar finding. However, positive employment effects are found for

month only, the results are plausible. Since occupations in job creation schemes have to be additional in nature, i.e. they are not allowed to compete with regular jobs to avoid substitution effects, the qualifying elements for market-competitive jobs have to be assumed to be negligible. Thus, the stabilising elements in the design of job creation schemes (to keep in touch with the labour market) may be more important for this group. Furthermore, as I have discussed in section 2.5, participation in job creation schemes may imply a stigmatisation of the participant if potential employers suspect a reduced productivity. However, long-term unemployment is a stigma itself, and hence the additional stigma effect of job creation schemes may be of minor relevance. On the contrary, for these groups participation must be seen as an indicator for individual motivation to change the personal situation. Thus, the stigma effect of job creation schemes may be more important for short-term unemployed and younger persons.

Summarising the findings for the selected target groups leads to three recommendations. First, due to the unsatisfactory results for most of the groups where no differences in the employment rates between participants and non-participants could be established, job creation schemes have to be critically reviewed in terms of their goals. Nevertheless, they are no complete failure for some participants as the results especially for long-term unemployed indicate. Second, a tighter targeting of programmes to persons for whom the possible negative aspects (like stigmatisation, lack of human capital transfer etc.) are of a merely minor importance for the individual labour market prospects should help to increase the efficiency of the programmes. Third, about 31.1 to 41.5 percent of the unemployed in West and East Germany are long-term unemployed (see figures in Table 2.1 in section 2.2). Since they are not the majority, the number of promotions should be significantly reduced. Job creation schemes are definitely sensible for the most disadvantaged workers, but no means for reducing unemployment permanently for all unemployed persons.

6.5.2 Effects for Target Groups Using Target Scores

The results for the target groups show that job creation schemes do not work for most of the analysed groups. Nevertheless, as the estimates are significantly positive especially for the most disadvantaged, i.e. the long-term unemployed persons, the question arises whether a higher number of explicit labour market disadvantages correlates with gains from participation. To answer this question, I build a simple index called target score. It is defined as the sum of the individual number of disadvantages from section 6.5.1. Without any particular weighting, each disadvantage adds one point to the target score. Persons who do not belong to any of the categories in section 6.5.1 have a target score of 0. The maximum level is 8 since the categories for the age groups are mutually exclusive. For example, if an individual is below 25 years old and owns no professional degree, he or she is assigned a target score of 2. If an individual belongs to three of the target groups, the target score is 3, and so

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persons only who have started the programmes in months 12 and 13 of the unemployment spell (see above).

on. Due to a small number of individuals with a target score of more than 5, I summarise these persons in one group, i.e. target score 5 (and more); the other categories refer to the actual number of disadvantages. I estimate the programme effect on the employment rates in December 2002 within each category of the target score.

Table 6.5: Estimated Effects for the Target Scores[1] (December 2002)[2]

West Germany	**Men**			**Women**		
Target Score	Effect	Std. Err.	No. of Participants	Effect	Std. Err.	No. of Participants
0	0.0182	0.0850	55	-0.0133	0.0789	76
1	-0.0138	0.0363	295	0.0518	0.0401	208
2	-0.0180	0.0212	740	0.0316	0.0474	305
3	0.0256	0.0261	652	0.0276	0.0339	257
4	0.0199	0.0331	274	**0.1176**	0.0527	100
5 and more	**0.1449**	0.0591	84	0.0455	0.1033	32

East Germany	**Men**			**Women**		
Target Score	Effect	Std. Err.	No. of Participants	Effect	Std. Err.	No. of Participants
0	**-0.1014**	0.0484	141	**-0.0812**	0.0333	271
1	-0.0293	0.0198	581	-0.0064	0.0118	1,090
2	-0.0225	0.0155	937	-0.0093	0.0110	1,754
3	0.0013	0.0191	821	0.0112	0.0103	1,289
4	-0.0161	0.0213	322	0.0062	0.0159	508
5 and more	-0.0532	0.0448	94	0.0000	0.0393	106

Bold letters indicate significance on a 5% level. Standard errors calculated by bootstrapping with 50 replications.

[1] Target Scores are calculated as the sum of the number of individual disadvantages from the selection of the target groups.

[2] Effects are estimated using 1-NN matching without replacement and calliper of 0.02.

If programmes are tailored to the needs of the most disadvantaged and if a higher target score indicates higher need of assistance, better outcomes for higher scores are expected. The estimates of the effects in December 2002 are given in Table 6.5. Ignoring the significance of the estimates at first, the results show non-negative effects for all groups in West Germany with a target score greater equal 3. For the lower target score groups, the picture is not that homogeneous. While men in West Germany with a target score of 1 or 2 are harmed, women with the same score seem to benefit. In East Germany, groups with a target score of less than 3 have reduced employment rates in December 2002. For women with more disadvantages, there seems to be no effect while for men the estimates tend to be negative except for a target score of 3.

The results for West Germany tend to support the hypothesis that a higher target score coincides with a higher need of assistance and a better fit of programmes for those groups, but a clear statement is hampered due to the insignificance of estimates for most groups. It is self-evident that the construction of the target score is very simple and not guided by some strong theory. First, the different targeting criteria are included with same weights and clearly may not have the same importance for the individual employability. Second, the selection of groups is incomplete. There are further characteristics that increase or decrease the individual employability. Third, the construction of the target score leaves room for further effect heterogeneity. The target score just notes the number of single targets, but does not identify clear sets of disadvantages where participation improves the employability.

Unfortunately, considering the significance of the results shows that this assumption cannot be approved empirically. For each of the West German groups only one estimate for the higher target scores is significant. For men with a target score of 5, i.e. five or more disadvantage criteria on the labour market, the employment rates increase by 14.49, for women with a target score of 4 by 11.76 percentage points after participation. For the other groups the estimates are insignificant, i.e. no clear increase or decrease in the employment rates by participation can be established. The estimates for East Germany show a slightly different picture. The results illustrate that allocating individuals without any of the selected targeting criteria and therefore a target score of zero to programmes, reduces the employment rates in December 2002 by 10.14 for men and 8.12 percentage points for women. Analogously to the finding for West Germany, there are no further significant results. Since the construction of the target score is very simple, a reasonable topic for further research may be a revision that considers whether the incorporation of further selection criteria and/or different weighting of the single targets may improve the significance of the estimates. Although the estimates are unsatisfying yet, they are in line with the other effects for job creation schemes. In addition, the usage of the target score provides some practical utility to identify possible sources for effect heterogeneity.

6.5.3 Targeting by Stratification Matching

As I have discussed in section 3.5.1, the estimated propensity score reflects the individual participation probability conditional on the relevant observable covariates. If allocation to the programme is target oriented, a higher participation probability should also correlate with a higher impact of treatment. Clearly, this argument holds only if programmes are tailored according to the needs of the participants. If this is not the case, i.e. if the programmes have the same effects for all participants, individuals with low participation probabilities may benefit more since a high participation probability can, to some extent, be interpreted as an indicator for bad labour market prospects. Furthermore, an interesting opportunity arises if the empirical evidence supports a positive relationship between a higher participation probability and a higher impact of treatment. If this is the case, the estimated participation probability could be used as an allocation instrument, i.e. persons with higher propensity score values should be primarily allocated to programmes.

An intuitively appealing method to check this hypothesis is stratification matching, also known as blocking or subclassification (see section 3.3.4 for details). The idea is to divide the sample of participants and non-participants conditional on the propensity score into several strata. Within these strata, participants and non-participants should have the approximately same probability of treatment. The average treatment effect is estimated within each stratum as if random assignment holds. Estimation of the treatment effect for the treated is carried out by weighting the within-strata average treatment effects by the number of treated units. Stratification matching can be interpreted as a crude form of non-parametric regression where the unknown function is approximated by a step function with fixed jump points (Imbens, 2004). An important issue in employing this estimator is to make sure that the covariates are balanced within each stratum. The distribution among treatment and comparison group should be balanced if the true propensity score is constant. Comparison of the distribution of covariates of both groups within strata yields a possibility to assess the adequacy of the statistical model.

Table 6.6: Results for Stratification Matching in East Germany

Strata		**Men**				**Women**			
		No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ	No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ
1	Part.	146	0.0001	0.1781	-0.0585	251	0.0002	0.1355	0.0134
	Non-part.	16,171		0.2366		18,980		0.1221	
2	Part.	146	0.9303	0.1781	-0.0666	252	0.0168	0.1032	-0.0235
	Non-part.	9,532		0.2446		11,309		0.1267	
3	Part.	146	0.0218	0.1233	**-0.0897**	252	0.1633	0.1190	-0.0267
	Non-part.	7,657		0.2130		7,396		0.1458	
4	Part.	146	0.3283	0.1575	-0.0347	252	0.1581	0.0913	-0.0568
	Non-part.	5,529		0.1923		5,641		0.1480	
5	Part.	147	0.0537	0.0816	*-0.0772*	251	0.2593	0.1633	0.0137
	Non-part.	4,432		0.1588		5,098		0.1497	
6	Part.	146	0.2077	0.1233	-0.0245	252	0.1555	0.1111	-0.0245
	Non-part.	3,093		0.1478		4,298		0.1356	
7	Part.	146	0.9609	0.0822	-0.0476	252	0.5875	0.1627	0.0178
	Non-part.	2,727		0.1298		3,852		0.1449	
8	Part.	146	0.4523	0.0685	-0.0497	252	0.3221	0.1071	*-0.0494*
	Non-part.	2,640		0.1182		2,804		0.1566	
9	Part.	146	0.5098	0.1027	-0.0201	251	0.2600	0.1036	*-0.0609*
	Non-part.	2,116		0.1229		2,785		0.1645	
10	Part.	147	0.7602	0.1020	-0.0173	252	0.1690	0.0952	-0.0423
	Non-part.	2,037		0.1193		2,276		0.1375	
11	Part.	146	0.4703	0.0616	-0.0440	252	0.3124	0.1190	-0.0192
	Non-part.	1,448		0.1057		2,228		0.1382	
12	Part.	146	0.4960	0.0959	-0.0165	252	0.9466	0.1508	0.0133
	Non-part.	1,592		0.1124		1,665		0.1375	
13	Part.	146	0.3424	0.0411	**-0.0729**	251	0.9627	0.1036	-0.0151
	Non-part.	1,132		0.1140		1,651		0.1187	

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Table 6.6: (continued)

Strata		**Men**				**Women**			
		No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ	No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ
14	Part.	146	0.8348	0.0616	-0.0373	252	0.0541	0.1310	0.0371
	Non-part.	980		0.0990		1,471		0.0938	
15	Part.	147	0.7724	0.1224	0.0296	252	0.2967	0.0992	0.0126
	Non-part.	948		0.0928		1,143		0.0866	
16	Part.	146	0.8285	0.0890	0.0152	252	0.9422	0.1071	0.0164
	Non-part.	772		0.0738		1,124		0.0907	
17	Part.	146	0.9521	0.0753	0.0253	251	0.3790	0.0797	-0.0071
	Non-part.	600		0.0500		910		0.0868	
18	Part.	146	0.4996	0.0822	*0.0403*	252	0.6872	0.0913	-0.0129
	Non-part.	645		0.0419		749		0.1041	
19	Part.	146	0.0053	0.0548	0.0193	252	0.7600	0.1349	0.0192
	Non-part.	479		0.0355		648		0.1157	
20	Part.	147	0.6655	0.0748	0.0244	252	0.6248	0.1548	0.0267
	Non-part.	258		0.0504		442		0.1281	
ATT:					**-0.0251**				-0.0084

Bold letters indicate significance at the 1% level. *Italic* letters refer to the 5% level.
Sub-groups are constructed using the estimated propensity score of the participants from the logit model reported in Table 6.1.
[1] Testing $H_0 : P(Z, D = 1) - P(Z, D = 0) = 0$. Corresponding H_A: $P(Z, D = 1) - P(Z, D = 0) \neq 0$ in stratum.

To check the hypothesis whether a higher participation probability correlates with a higher programme impact, I divide the samples into 20 subclasses each. This division is based on the estimated propensity scores of the participants.[11] Therefore, I have the same number of participants in each stratum, but different numbers of non-participants with approximately the same scores as the participants. Individuals with the lowest participation probabilities are placed in stratum 1, persons with the highest participation probabilities are placed in stratum 20 accordingly. It can be seen that this stratification leaves meaningful numbers of observations in each stratum for the main groups except for women in West Germany.

The estimated treatment effects for each stratum are presented in Table 6.6 for East Germany and in Table 6.7 for West Germany. The effectiveness of the programmes can be estimated by comparing the employment rates of participants and

[11] Due to the large number of observations in the samples, using the whole range of the propensity scores of participants and non-participants leads to a skewed stratification. Hence, I only refer to the propensity scores of the participants to reduce this skewness. The choice of 20 strata for each of the four groups emerged from balancing tests of the propensity score among treated and comparison persons using a smaller number of blocks (see the discussion in section 3.3.4).

non-participants in December 2002 given by $E(Y^1)$ and $E(Y^0)$ in the tables. The average treatment effect within each stratum, i.e. the difference of the mean outcomes of the participants and non-participants, is given by Δ. The last line of the tables provide the ATT. Obviously, these effects are similar to those estimated with the NN-matching estimators in section 6.3. In addition to the mean outcomes and the effects, the tables also present the results of the hypothesis testing of equal propensity scores in the treatment and comparison group. I tested the null hypothesis (H_0) that the difference of the mean propensity scores in both groups is zero. Therefore, the alternative hypothesis (H_A) imposes inequality of the propensity score. The p-values of the H_A are given in the tables; if I reject the hypothesis due to a larger value than 0.05, equality of the propensity scores and therefore balancing of the covariates among both groups could be assumed. I checked the balancing property of stratification by comparing the means of the incorporated variables in the logit models for participants and non-participants within each stratum as suggested by Rosenbaum and Rubin (1983b) as well. The results for selected variables are presented in Fig. B.1 to B.6 in the appendix to this chapter.

Table 6.7: Results for Stratification Matching in West Germany

Strata		**Men**				**Women**			
		No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ	No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ
1	Part.	107	0.0000	0.1869	*0.0764*	52	0.0005	0.3846	**0.2649**
	Non-part.	14,220		0.1105		12,954		0.1197	
2	Part.	107	0.1905	0.1963	-0.0046	53	0.1774	0.3585	*0.1194*
	Non-part.	4,913		0.2009		4,119		0.2391	
3	Part.	107	0.2521	0.2336	0.0034	52	0.5364	0.3077	0.0201
	Non-part.	4,065		0.2303		2,754		0.2876	
4	Part.	107	0.8130	0.2150	-0.0355	53	0.7943	0.3962	0.1169
	Non-part.	3,522		0.2504		2,782		0.2793	
5	Part.	107	0.0430	0.2617	0.0278	53	0.6186	0.3019	-0.0110
	Non-part.	2,403		0.2339		1,742		0.3129	
6	Part.	107	0.5197	0.1682	*-0.0998*	52	0.7633	0.2692	-0.0341
	Non-part.	2,384		0.2680		1,556		0.3033	
7	Part.	107	0.0045	0.2056	-0.0484	53	0.9023	0.3585	0.0370
	Non-part.	2,331		0.2540		1,347		0.3215	
8	Part.	107	0.4353	0.2056	-0.0593	52	0.6411	0.2885	-0.0307
	Non-part.	1,748		0.2649		1,366		0.3192	
9	Part.	107	0.2616	0.2336	-0.0364	53	0.9991	0.2830	-0.0481
	Non-part.	1,533		0.2701		1,214		0.3311	
10	Part.	107	0.3627	0.2804	0.0005	53	0.6523	0.3396	-0.0242
	Non-part.	1,229		0.2799		841		0.3639	
11	Part.	107	0.1798	0.1963	-0.0831	52	0.8903	0.3269	-0.0184
	Non-part.	1,049		0.2793		611		0.3453	
12	Part.	107	0.5893	0.2991	0.0343	53	0.3965	0.2830	-0.0608
	Non-part.	929		0.2648		733		0.3438	
13	Part.	107		0.2617		52		0.3846	

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Table 6.7: (continued)

Strata		**Men**				**Women**			
		No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ	No. of Obs.	p-value for H_A	$E(Y^1)$, $E(Y^0)$	Δ
	Non-part.	751	0.6554	0.2690	-0.0073	623	0.2097	0.3949	-0.0102
14	Part.	107	0.3683	0.2617	0.0088	53	0.3294	0.3208	-0.0260
	Non-part.	684		0.2529		571		0.3468	
15	Part.	107	0.5013	0.2056	-0.0667	53	0.2556	0.4340	0.1185
	Non-part.	661		0.2723		447		0.3154	
16	Part.	107	0.4412	0.2430	0.0452	52	0.0935	0.3077	0.0171
	Non-part.	551		0.1978		265		0.2906	
17	Part.	107	0.8646	0.1402	-0.0332	53	0.0282	0.3208	0.0615
	Non-part.	473		0.1734		108		0.2593	
18	Part.	107	0.0955	0.1308	0.0122	52	0.7560	0.3654	**0.1987**
	Non-part.	295		0.1186		78		0.1667	
19	Part.	107	0.4283	0.2617	**0.1413**	53	0.0389	0.3396	*0.1682*
	Non-part.	191		0.1204		70		0.1714	
20	Part.	107	0.0038	0.2710	**0.1606**	53	0.1637	0.3585	**0.2715**
	Non-part.	163		0.1104		38		0.0870	
ATT:					0.0018				**0.0565**

Bold letters indicate significance at the 1% level. *Italic* letters refer to the 5% level.
Sub-groups are constructed using the estimated propensity score of the participants from the logit model reported in Table 6.1.
[1] Testing $H_0 : P(Z, D = 1) - P(Z, D = 0) = 0$. Corresponding H_A: $P(Z, D = 1) - P(Z, D = 0) \neq 0$ in stratum.

The results of the hypothesis tests show that the division into 20 strata provides approximately equal propensity scores for most groups. The equality is hampered only for the groups at the borders of the propensity score range with some exceptions. For men in West Germany, strata 1, 5, 7, and 20 are imbalanced, for women in the same region so are strata 1, 17, and 19. In East Germany the strata with lower participation probabilities are imbalanced. For women, the propensity scores are not balanced in strata 1 and 2, for men in strata 1 and 3, but also in stratum 19. Although I find significant treatment effects for several strata, these findings do not support the hypothesis. Taking a look at the results for East Germany (Table 6.6), I find that for the first four strata (except for women in stratum 1) allocation of persons with a low participation probability has a tendential negative influence on the employment chances in December 2002. For men in this region, this tendency is stable for participants up to stratum 14; from stratum 15 onward the direction of the effects changes to positive. For women I could not establish a clear distinction since most of the effects are insignificant. For participants in West Germany (Table 6.7), the hypothesis cannot be empirically approved either. One can somehow see that higher participation probabilities correlate with higher impacts, but these findings may be inconsistent

as the balancing tests above show. It seems that the participation probability is no adequate measure for effect heterogeneity here; and successful integration into regular employment depends on different compositions of the individual characteristics rather than selection into programmes.

6.6 Summary

The findings of the analysis in chapter 5 as well as of previous empirical studies (see section 2.4) have shown that the average effects of job creation schemes on the re-integration into regular employment are negative for the participating individuals. Whereas this disappointing picture may be due to the poor quality of programmes, a possible reason may also be an inefficient allocation of potential participants to programmes. Allocation of individuals into programmes in Germany is accomplished by caseworker discretion. On the one hand, a positive aspect of this mechanism is that decisions are based on personal contact. On the other hand, since active ALMP consists of a variety of different programmes, caseworkers may lack knowledge regarding programme impacts. Since this problem is not specific to Germany, the topic of a potential improvement of allocation mechanisms has become important in recent literature. Broadly, two categories can be distinguished: non-statistical allocation mechanisms like caseworker discretion and statistical allocation mechanisms called profiling or targeting. Since statistical allocation systems are not introduced in the German labour market yet, there is no empirical evidence for their effectiveness.

In this chapter, I have tried to identify a possible effect heterogeneity that allows a more sophisticated assessment of the efficiency of job creation schemes. To do so, I have estimated the average treatment effects for men and women in East and West Germany participating in job creation schemes in February 2000 in a first step. In a second step, I made use of three different strategies to analyse the effect heterogeneity. In contrast to the analysis of chapter 5, I have used data on all participants who have started a job creation scheme in February 2000 and on non-participants who were eligible for participation, but did not enter the programmes in February. The employment effects of job creation schemes are evaluated in December 2002. The results show positive effects for women in West Germany and negative effects for men and women in East Germany. Men in West Germany do neither suffer nor benefit from participation.

For the three approaches used to analyse effect heterogeneity, I select target groups with disadvantages on the labour market oriented by the definition of the legal basis in a first step. The findings show that job creation schemes do neither harm nor improve the labour market chances for most of the groups. Exceptions are long-term unemployed men in West Germany, long-term unemployed women in both regions, older women and women who are hard-to-place in West Germany that benefit from participation. Given these results and remembering that (re-)integration into regular employment is the main purpose, it has to be recommended that job creation schemes should be targeted to those benefiting groups and should not be used on large scale. In a second step, I use these definitions to build up a simple indicator

(target score) as the sum of the individual number of disadvantages. If programmes are tailored to the needs of the more disadvantaged persons on the labour market, I expect positive impacts for groups with a higher score. Unfortunately, most of the estimates are insignificant and although the expected tendency is observable, one has to be careful in interpreting the results. Finally, I implement stratification matching to analyse whether a higher participation probability correlates with higher impacts. No clear picture can be revealed. The estimated participation probability is not an adequate measure for effect heterogeneity here, and successful integration into regular employment is determined by different compositions of the individual attributes than selection into programmes. The results show that heterogeneity in treatment effects is an important topic which has to be considered more accurately in further research. Moreover, taking account for effect heterogeneity may be a way to improve efficiency of ALMP and hence to allocate scarce resources more effectively.

7

Conclusion

Job creation schemes have been a major element of ALMP in Germany. They are a form of subsidised employment and aim at stabilisation and qualification of unemployed persons with barriers to employment. Recent empirical studies evaluating the impacts of job creation schemes in Germany indicate that programmes do not improve the employment chances of the participating individuals on average. In addition, international evidence on the effectiveness of ALMP suggests that programmes should be well-targeted to the needs of the individual job-seekers and the labour market and that treatment should start as early as possible in the unemployment spell (OECD, 1998). Whereas the empirical content of the first OECD recommendation has been analysed in a number of previous studies for job creation schemes in Germany by explicit consideration of possible effect heterogeneity (see, e.g., Caliendo, Hujer, and Thomsen, 2004; 2006a; 2006c), evidence for the second one has been missing.

Recommending an early intervention in the unemployment spell by participation in a job creation scheme requires empirical evidence on the effectiveness of the programmes with respect to the timing of treatment. The first goal of this study was to provide this evidence. I have estimated the employment effects of job creation schemes in Germany with explicit consideration of the time the individuals spent in unemployment until the start of the programmes. The second goal of this study considered the problem that negative mean impacts of job creation schemes may not apply to all participating individuals and that there may be groups who benefit from participation. Identifying the successful individuals and targeting the programmes to those persons bears the potential for a more effective and efficient labour market policy in the future. Due to the clearly different situation of the labour market in West and East Germany, I have estimated the effects separately. Programme effects have been evaluated according to the main purpose of the programmes, the (re-)integration of the participating individuals into regular (unsubsidised) employment. Other purposes of job creation schemes, e.g., the relief of the stock of unemployed in regions with great imbalances of the labour market, are secondary only and have not been evaluated here.

To answer both questions I applied matching methods, but in different set-ups. The basic idea of the matching estimator is to approximate the non-treatment outcome of the participants by the outcome of non-participants that are identical in all relevant observable characteristics that determine the participation decision and the labour market outcomes. However, definition of participation and non-participation is not straightforward in a comprehensive ALMP system. Programmes are ongoing and participants can join programmes at different points of the unemployment spell. Therefore, using the estimator for the static setting may lead to biased estimates and an extended version, where participation and non-participation in programmes are defined dynamically, had to be applied to answer the first question. To answer the second question, I have analysed the possibilities of an emphasised targeting in three steps. In the first step, I have evaluated the effects for certain target groups defined according to the legal definitions. Since programmes are specifically designed for those groups, I have expected larger impacts compared to the average. In the second step, I have constructed a simple indicator as the sum of the single target criteria each individual owns (target score) to indicate the individual's need of more intensive assistance. In the third step, I have used the estimated participation probability to answer the question whether a higher participation probability correlates with a higher programme impact. To do so, I have stratified the sample along the propensity score and estimated the employment effects.

The results of the analysis of the employment effects with respect to the timing of treatment show that, independently of the preceding unemployment duration, participants in job creation schemes experience strong locking-in effects whilst being in the programmes. These locking-in effects are observable for West as well as for East Germany. Due to this, the employment rates of the participants are clearly below that of the matched non-participants in the first months after programmes have started. After that time, the effects vary in both regions. Persons who have started the programmes early in the unemployment spell in West Germany, i.e. in the first, second, or fourth month, experience negative employment effects due to participation in a programme even 30 months after programmes have started. In contrast, long-term unemployed persons who are unemployed for about 12 or 13 months before they join the programmes benefit from participation at the end of the observation period in terms of improved employment chances in that region. For all other starting months in the unemployment spell considered in the evaluation, no significant employment effects could be established until 30 months after the start of the programmes in West Germany. Unfortunately, since the results for the long-term unemployed persons could not be confirmed by estimates for persons with longer unemployment durations, a clear recommendation on when to start a programme in the unemployment spell is difficult. For West Germany, the strong locking-in effects during the programmes and the slowly increasing employment rates afterwards in association with the insignificant estimates at the end of the observation period for most of the groups indicate a disappointing picture. Except for persons starting in month 12 or 13 of the unemployment spell, job creation schemes are on average not helpful in improving the employment chances of the participating individuals independently of the preceding unemployment duration.

For East Germany, the results of the employment effects are worse compared to the West. Similar to West Germany, participants are affected by the locking-in effects whilst being in the programmes, but job creation schemes are not able to improve the employment chances of the participating individuals within the first 30 months after the start of the programmes. Moreover, since most of the estimates are significantly negative at the end of the observation period, the employment chances of the majority of the participants are reduced, and job creation schemes harm the employability. Possible reasons may be the bad labour market situation and inefficiencies in the allocation of the individuals to the programmes, i.e. the representation of the specific target groups in the programmes is too low. In addition, job creation schemes may be not able to provide human capital that is in line with the demands of the market. A further reason may be the average duration of the programmes that is too long. Therefore, participants are habituated to regular work, but with not chance of prolongation after the end of the programmes. Hence, they return to unemployment and need some time to recover. Since the unemployment spell of the comparable non-participants is not 'interrupted' by the programme, their search intensity is not reduced by participation, and, on average, they find work earlier.

The results for the three aspects of the second question can be summarised as follows. The analysis of the target groups imply unsatisfactory results for most of the groups. Therefore, job creation schemes should be reviewed critically. However, they are not a complete failure for some participants, e.g., long-term unemployed persons. According to the findings, programmes should be targeted tighter to persons for whom the possible negative effects of job creation schemes are of merely minor relevance. Since long-term unemployed persons do not represent the majority of unemployment in Germany, the number of promotions has to be reduced significantly. Job creation schemes are a sensible instrument of ALMP for the most disadvantaged workers, but no means for reducing unemployment permanently for all unemployed persons. The results for the analysis of the target scores indicate that persons allocated to programmes with a higher score tend to benefit more (in West Germany), whereas persons with only a low score are more likely to be harmed. Unfortunately, since most of the estimates are statistically insignificant, this finding represents only a tendency and could not be empirically approved. The third aspect (targeting using the propensity scores) shows that the participation probability is no adequate measure for effect heterogeneity. Successful (re-) integration into regular employment depends on a different composition of the individual characteristics than selection into programmes.

Together with the previous empirical findings (see Hujer, Caliendo, and Thomsen, 2004; Caliendo, Hujer, and Thomsen, 2004; 2006a; 2006c), it is now possible to judge the performance of job creation schemes in Germany. In total, it can be said that job creation schemes are in general unable to improve the re-integration probability into regular employment for participating unemployed persons. The results are also concordant with recent evaluation studies of job creation schemes for other countries, finding large locking-in effects and overall negative effects, see, e.g., Sianesi (2004) for Sweden, Firth, Payne, and Payne (1999) for the UK, Gerfin and Lechner (2002) for Switzerland and Martin and Grubb (2001) for an overview of

OECD countries. The results of the study show that starting programmes early in the unemployment spell as suggested by OECD (1998) is no general recommendation. Moreover, programmes like job creation schemes that explicitly focus on long-term unemployed persons may be even harmful for the employment prospects of the participating individuals when offered too early in the unemployment spell. Since most of the effects are insignificant or negative, the overall picture of the employment effects of job creation schemes in Germany is rather disappointing. Participation in programmes does not help individuals to re-integrate into regular (unsubsidised) employment. Furthermore, the results show that participation in job creation schemes is associated with strong locking-in effects during the time of the programmes. Although this finding is not surprising as job creation schemes are some kind of work, it may be a major reason for the unsatisfying picture of the programme effects in almost all groups at the end of the observation period. However, the results of the analysis in chapter 5 have only shown positive employment effects 30 months after the start of the programmes for long-term unemployed individuals in West Germany with 12 and 13 months of unemployment preceding the treatment. Similar findings have been found in the analysis of this target group in chapter 6 (35 months after the start of the programmes).

Hence, one policy recommendation is to focus programmes more on long-term unemployed persons in the labour market and thereby reduce the number of participants. Clearly, this was not the case in Germany for a long period (in particular in East Germany, where job creation schemes have been used on a large scale during the 1990s and early 2000s) and is one possible explanation for the disappointing effects. For all these reasons, tailoring the programmes more specifically to fit the needs of the participants may also help to increase their efficiency. Finally, a further possible explanation for the negative effects, which has to be mentioned, is the connection between participation and the unemployment benefit system. During the observation period, participation in job creation schemes renewed the eligibility for unemployment benefits for participants in the same way as regular employment. Hence, participants who finished their programme were faced with possibly bad incentives to search and apply for regular employment. Meanwhile, this problematic design was changed from 2004 onward. Together with a reduction of the number of participants and a better orientation of the programmes to the needs of the participants, job creation schemes will play a minor role for specific problem groups in the labour market in the future.

A

Additional Material to Chapter 5

A.1 Tables

Table A.1: Descriptive Statistics for Selected Characteristics – West Germany (Unemployment Duration up to 6 and 6 to 12 Months)

	Unemployment duration[1] <= 6 months				Unemployment duration[1] > 6 and <= 12 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
No. of observations	1,020		48,102		664		29,286	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Programme duration	290.30	126.90	*n.a.*	*n.a.*	283.30	122.20		
Age								
25 to 29 years	0.10	0.30	0.15	0.36	0.08	0.27	0.11	0.31
30 to 34 years	0.16	0.37	0.20	0.40	0.15	0.35	0.18	0.38
35 to 39 years	0.21	0.41	0.20	0.40	0.23	0.42	0.20	0.40
40 to 44 years	0.19	0.39	0.17	0.38	0.21	0.41	0.18	0.38
45 to 49 years	0.17	0.38	0.14	0.35	0.17	0.37	0.16	0.36
50 to 55 years	0.17	0.38	0.14	0.34	0.16	0.37	0.17	0.38
Foreigner	0.10	0.30	0.16	0.37	0.10	0.30	0.17	0.37
Asylum seeker	0.04	0.19	0.06	0.24	0.05	0.21	0.06	0.23
Woman	0.33	0.47	0.45	0.50	0.36	0.48	0.48	0.50
No. of placement propositions	10.23	11.31	4.07	7.80	9.54	9.77	3.90	7.29
No. of children	0.64	1.05	0.68	1.02	0.69	1.10	0.73	1.05
Placement restrictions	0.23	0.42	0.14	0.35	0.17	0.37	0.18	0.38
Vocational rehabilitation[2]	0.07	0.25	0.04	0.20	0.06	0.24	0.05	0.21
Health restrictions	0.33	0.47	0.23	0.42	0.27	0.44	0.27	0.44
Marriage/Cohabitation	0.47	0.50	0.57	0.50	0.51	0.50	0.59	0.49
Work experience	0.92	0.27	0.93	0.26	0.92	0.27	0.93	0.26
Programme before unemployment[3]	0.34	0.47	0.03	0.18	0.33	0.47	0.05	0.23
Reception of UI	0.82	0.38	0.82	0.38	0.77	0.42	0.84	0.36
Duration of Last Employment								
Up to 180 days	0.34	0.47	0.27	0.44	0.36	0.48	0.25	0.43
181 to 365 days	0.10	0.30	0.14	0.35	0.11	0.31	0.09	0.29
1 to 2 years	0.16	0.37	0.15	0.36	0.16	0.37	0.14	0.35
More than 2 years	0.40	0.49	0.44	0.50	0.38	0.48	0.52	0.50
Pension								
No pension	1.00	0.05	1.00	0.05	1.00	0.04	1.00	0.07
Vocational disability	0.00	0.03	0.00	0.03	0.00	0.00	0.00	0.05
Permanently unable to work	0.00	0.03	0.00	0.04	0.00	0.04	0.00	0.04
Social plan	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Schooling[4]								
No school	0.18	0.39	0.12	0.33	0.17	0.38	0.14	0.35
CSE	0.52	0.50	0.54	0.50	0.50	0.50	0.54	0.50
O-levels	0.12	0.33	0.19	0.39	0.12	0.32	0.18	0.38
Adv. technical college entrance[5]	0.05	0.22	0.05	0.21	0.07	0.25	0.04	0.20
A-levels	0.12	0.32	0.10	0.30	0.14	0.35	0.10	0.30
Assessment of Individual's Qualification								
Other	0.54	0.50	0.49	0.50	0.53	0.50	0.52	0.50
Unskilled employee	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.01
Skilled employee	0.32	0.47	0.42	0.49	0.31	0.46	0.39	0.49
Ass. to technical school[6]	0.02	0.15	0.02	0.15	0.03	0.16	0.02	0.14
Ass. to adv. technical college	0.05	0.23	0.02	0.15	0.07	0.26	0.02	0.15

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Table A.1: continued

	Unemployment duration[1] <= 6 months				Unemployment duration[1] > 6 and <= 12 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Ass. to university	0.06	0.24	0.04	0.19	0.07	0.25	0.04	0.20
Ass. to top-management	0.00	0.00	0.00	0.06	0.00	0.00	0.00	0.06
Professional Training								
Without completed prof. training	0.44	0.50	0.40	0.49	0.44	0.50	0.43	0.49
Apprenticeship (on-the-job)	0.39	0.49	0.48	0.50	0.36	0.48	0.45	0.50
Apprenticeship (off-the-job)	0.02	0.14	0.01	0.12	0.02	0.12	0.01	0.11
Full-time vocational school	0.02	0.13	0.02	0.14	0.02	0.14	0.02	0.14
Technical school	0.04	0.19	0.04	0.19	0.05	0.22	0.04	0.19
Advanced technical college	0.04	0.19	0.02	0.13	0.05	0.21	0.02	0.13
University	0.06	0.23	0.04	0.19	0.07	0.25	0.04	0.20
Month of Treatment Start								
July 2000	0.15	0.36	0.17	0.37	0.17	0.38	0.21	0.41
September 2000	0.18	0.38	0.19	0.39	0.20	0.40	0.23	0.42
November 2000	0.16	0.36	0.13	0.34	0.17	0.37	0.14	0.35
January 2001	0.15	0.36	0.12	0.33	0.12	0.32	0.10	0.30
March 2001	0.17	0.38	0.18	0.38	0.16	0.36	0.13	0.34
May 2001	0.19	0.39	0.20	0.40	0.19	0.39	0.19	0.39
Regional Context Variable								
Cluster II	0.32	0.47	0.23	0.42	0.35	0.48	0.28	0.45
Cluster III	0.42	0.49	0.41	0.49	0.42	0.49	0.42	0.49
Cluster IV	0.07	0.26	0.11	0.32	0.07	0.26	0.11	0.31
Cluster V	0.19	0.39	0.25	0.43	0.16	0.37	0.19	0.40
Desired Work Time								
Full-time work	0.93	0.25	0.85	0.36	0.90	0.30	0.82	0.39
Part-time work	0.07	0.25	0.15	0.36	0.10	0.30	0.18	0.39
Other (e.g., telework)	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.01
Work Time (Last Occupation)								
Full-time work	0.47	0.50	0.47	0.50	0.52	0.50	0.40	0.49
Part-time work	0.03	0.17	0.05	0.22	0.05	0.21	0.05	0.22
Not applicable	0.50	0.50	0.48	0.50	0.43	0.50	0.54	0.50
Desired Occupation								
Farming[7]	0.08	0.28	0.03	0.17	0.09	0.28	0.03	0.17
Mining, mineral extraction	0.00	0.03	0.00	0.04	0.00	0.03	0.00	0.05
Manufacturing	0.39	0.49	0.35	0.48	0.34	0.47	0.34	0.47
Technical professions	0.03	0.17	0.04	0.19	0.03	0.17	0.04	0.19
Service professions	0.48	0.50	0.56	0.50	0.54	0.50	0.57	0.50
Other occupations	0.01	0.09	0.02	0.15	0.00	0.06	0.03	0.16

[1] Unemployment duration until treatment start.
[2] Attendant for vocational rehabilitation.
[3] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[4] Schooling: CSE = Certificate of Secondary Education.
[5] Advanced technical college entrance qualification (*Fachhochschulreife*).
[6] Ass. = assimilable.
[7] Farming comprises plant cultivation, breeding and fishery.

Table A.2: Descriptive Statistics for Selected Characteristics – West Germany (Unemployment Duration 13 to 18 and 19 to 24 Months)

	Unemployment duration[1] > 12 months and <= 18 months				Unemployment duration[1] > 18 and <= 24 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
No. of observations	1,020		48,102		664		29,286	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Programme duration	305.50	114.80	—	—	297.20	121.90	—	—

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Table A.2: continued

	Unemployment duration[1] > 12 months and <= 18 months				Unemployment duration[1] > 18 and <= 24 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Age								
25 to 29 years	0.09	0.28	0.09	0.28	0.07	0.25	0.07	0.25
30 to 34 years	0.14	0.35	0.16	0.36	0.12	0.32	0.14	0.34
35 to 39 years	0.20	0.40	0.19	0.39	0.21	0.40	0.18	0.38
40 to 44 years	0.22	0.41	0.17	0.38	0.22	0.42	0.18	0.38
45 to 49 years	0.19	0.39	0.17	0.38	0.18	0.39	0.19	0.39
50 to 55 years	0.17	0.38	0.22	0.41	0.21	0.41	0.26	0.44
Foreigner	0.11	0.31	0.17	0.38	0.11	0.32	0.18	0.38
Asylum seeker	0.05	0.21	0.06	0.24	0.06	0.24	0.07	0.25
Woman	0.37	0.48	0.49	0.50	0.36	0.48	0.48	0.50
No. of placement propositions	9.60	9.80	3.61	6.87	9.04	8.40	3.54	6.64
No. of children	0.68	1.14	0.73	1.06	0.73	1.14	0.73	1.08
Placement restrictions	0.19	0.39	0.19	0.39	0.16	0.37	0.20	0.40
Vocational rehabilitation[2]	0.06	0.24	0.05	0.22	0.07	0.25	0.05	0.22
Health restrictions	0.27	0.45	0.29	0.45	0.25	0.43	0.29	0.46
Marriage/Cohabitation	0.47	0.50	0.60	0.49	0.51	0.50	0.60	0.49
Work experience	0.92	0.27	0.93	0.26	0.92	0.27	0.93	0.26
Programme before unemployment[3]	0.38	0.49	0.06	0.23	0.32	0.47	0.06	0.24
Reception of UI	0.74	0.44	0.85	0.36	0.82	0.39	0.85	0.36
Duration of Last Employment								
Up to 180 days	0.36	0.48	0.23	0.42	0.33	0.47	0.23	0.42
181 to 365 days	0.13	0.33	0.09	0.28	0.17	0.37	0.08	0.27
1 to 2 years	0.14	0.35	0.12	0.33	0.13	0.34	0.11	0.32
More than 2 years	0.37	0.48	0.56	0.50	0.37	0.48	0.58	0.49
Pension								
No pension	1.00	0.05	0.99	0.08	1.00	0.04	0.99	0.08
Vocational disability	0.00	0.04	0.00	0.06	0.00	0.00	0.00	0.06
Permanently unable to work	0.00	0.03	0.00	0.05	0.00	0.04	0.00	0.06
Social plan	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.01
Schooling[4]								
No school	0.17	0.38	0.15	0.36	0.21	0.41	0.16	0.37
CSE	0.49	0.50	0.55	0.50	0.53	0.50	0.56	0.50
O-levels	0.13	0.33	0.16	0.37	0.08	0.28	0.15	0.36
Adv. technical college entrance[5]	0.07	0.26	0.04	0.20	0.05	0.21	0.04	0.19
A-levels	0.14	0.35	0.09	0.29	0.13	0.34	0.09	0.28
Assessment of Individual's Qualification								
Other	0.51	0.50	0.55	0.50	0.61	0.49	0.57	0.49
Unskilled employee	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.02
Skilled employee	0.32	0.47	0.37	0.48	0.27	0.44	0.35	0.48
Ass. to technical school[6]	0.02	0.14	0.02	0.13	0.02	0.12	0.02	0.13
Ass. to adv. technical college	0.08	0.27	0.02	0.15	0.05	0.22	0.02	0.15
Ass. to university	0.07	0.25	0.04	0.19	0.06	0.24	0.04	0.19
Ass. to top-management	0.00	0.03	0.00	0.06	0.00	0.00	0.00	0.05
Professional Training								
Without completed prof. training	0.42	0.49	0.45	0.50	0.50	0.50	0.46	0.50
Apprenticeship (on-the-job)	0.36	0.48	0.43	0.50	0.33	0.47	0.42	0.49
Apprenticeship (off-the-job)	0.02	0.12	0.01	0.10	0.02	0.13	0.01	0.08
Full-time vocational school	0.03	0.16	0.02	0.14	0.02	0.14	0.02	0.14
Technical school	0.05	0.22	0.03	0.18	0.04	0.20	0.03	0.18
Advanced technical college	0.05	0.23	0.02	0.13	0.03	0.18	0.02	0.13
University	0.07	0.26	0.04	0.19	0.06	0.24	0.04	0.19
Month of Treatment Start								
July 2000	0.18	0.38	0.21	0.41	0.20	0.40	0.22	0.42
September 2000	0.21	0.41	0.24	0.43	0.23	0.42	0.23	0.42
November 2000	0.14	0.35	0.15	0.36	0.14	0.35	0.15	0.35
January 2001	0.14	0.34	0.10	0.30	0.11	0.31	0.10	0.30
March 2001	0.17	0.37	0.13	0.34	0.16	0.36	0.13	0.34
May 2001	0.16	0.37	0.16	0.37	0.16	0.37	0.16	0.37

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Table A.2: continued

	Unemployment duration[1] > 12 months and <= 18 months				Unemployment duration[1] > 18 and <= 24 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Regional Context Variable								
Cluster II	0.38	0.49	0.31	0.46	0.45	0.50	0.32	0.47
Cluster III	0.40	0.49	0.41	0.49	0.39	0.49	0.41	0.49
Cluster IV	0.08	0.28	0.10	0.31	0.06	0.24	0.10	0.30
Cluster V	0.13	0.34	0.17	0.38	0.09	0.29	0.16	0.37
Desired Work Time								
Full-time work	0.91	0.28	0.82	0.39	0.93	0.25	0.83	0.38
Part-time work	0.09	0.28	0.18	0.39	0.07	0.25	0.17	0.37
Other (e.g., telework)	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.02
Work Time (Last Occupation)								
Full-time work	0.52	0.50	0.35	0.48	0.56	0.50	0.34	0.47
Part-time work	0.05	0.21	0.05	0.22	0.04	0.19	0.05	0.21
Not applicable	0.43	0.50	0.60	0.49	0.40	0.49	0.62	0.49
Desired Occupation								
Farming[7]	0.08	0.28	0.03	0.17	0.10	0.30	0.03	0.17
Mining, mineral extraction	0.00	0.03	0.00	0.06	0.00	0.00	0.00	0.07
Manufacturing	0.34	0.47	0.34	0.47	0.34	0.47	0.35	0.48
Technical professions	0.02	0.15	0.04	0.18	0.02	0.13	0.03	0.18
Service professions	0.55	0.50	0.56	0.50	0.53	0.50	0.55	0.50
Other occupations	0.00	0.07	0.03	0.16	0.01	0.10	0.02	0.15

[1] Unemployment duration until treatment start.
[2] Attendant for vocational rehabilitation.
[3] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[4] Schooling: CSE = Certificate of Secondary Education.
[5] Advanced technical college entrance qualification (*Fachhochschulreife*).
[6] Ass. = assimilable.
[7] Farming comprises plant cultivation, breeding and fishery.

Table A.3: Descriptive Statistics for Selected Characteristics – East Germany (Unemployment Duration up to 6 and 7 to 12 Months)

	Unemployment duration[1] <= 6 months				Unemployment duration[1] > 6 and <= 12 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
No. of observations	8,089		241,092		6,984		84,104	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Programme duration	281.60	115.70	—	—	283.80	114.20	—	—
Age								
25 to 29 years	0.06	0.24	0.12	0.33	0.06	0.23	0.10	0.30
30 to 34 years	0.12	0.33	0.17	0.37	0.11	0.31	0.15	0.36
35 to 39 years	0.17	0.38	0.20	0.40	0.17	0.37	0.19	0.39
40 to 44 years	0.17	0.38	0.19	0.39	0.19	0.39	0.19	0.39
45 to 49 years	0.20	0.40	0.18	0.38	0.20	0.40	0.19	0.39
50 to 55 years	0.27	0.44	0.15	0.36	0.28	0.45	0.18	0.38
Foreigner	0.01	0.08	0.02	0.13	0.00	0.06	0.02	0.12
Asylum seeker	0.01	0.08	0.02	0.15	0.01	0.08	0.02	0.13
Woman	0.51	0.50	0.48	0.50	0.50	0.50	0.55	0.50
No. of placement propositions	7.50	6.48	4.09	5.66	7.12	6.00	4.37	5.28
No. of children	0.77	1.01	0.77	0.99	0.74	0.99	0.81	1.03
Placement restrictions	0.12	0.33	0.10	0.29	0.10	0.30	0.10	0.31
Vocational rehabilitation[2]	0.06	0.24	0.04	0.19	0.05	0.23	0.04	0.21
Health restrictions	0.22	0.41	0.17	0.37	0.19	0.39	0.18	0.38
Marriage/Cohabitation	0.67	0.47	0.64	0.48	0.67	0.47	0.64	0.48
Work experience	0.91	0.29	0.92	0.28	0.92	0.27	0.91	0.29

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Table A.3: continued

	Unemployment duration[1] <= 6 months				Unemployment duration[1] > 6 and <= 12 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Programme before unemployment[3]	0.57	0.49	0.24	0.43	0.57	0.49	0.34	0.48
Reception of UI	0.62	0.49	0.90	0.31	0.60	0.49	0.86	0.35
Duration of Last Employment								
Up to 180 days	0.35	0.48	0.17	0.38	0.38	0.48	0.18	0.39
181 to 365 days	0.12	0.32	0.14	0.35	0.15	0.35	0.13	0.34
1 to 2 years	0.19	0.39	0.21	0.41	0.19	0.39	0.21	0.41
More than 2 years	0.34	0.48	0.48	0.50	0.29	0.45	0.47	0.50
Pension								
No pension	0.99	0.07	1.00	0.07	1.00	0.06	1.00	0.07
Vocational disability	0.00	0.05	0.00	0.03	0.00	0.04	0.00	0.04
Permanently unable to work	0.00	0.05	0.00	0.06	0.00	0.05	0.00	0.06
Social plan	0.00	0.00	0.00	0.00	—	—	—	—
Schooling[4]								
No school	0.07	0.26	0.06	0.24	0.08	0.27	0.07	0.26
CSE	0.33	0.47	0.26	0.44	0.33	0.47	0.29	0.45
O-levels	0.53	0.50	0.60	0.49	0.53	0.50	0.57	0.50
Adv. technical college entrance[5]	0.02	0.12	0.02	0.12	0.01	0.12	0.01	0.12
A-levels	0.05	0.23	0.07	0.25	0.05	0.22	0.06	0.23
Assessment of Individual's Qualification								
Other	0.36	0.48	0.33	0.47	0.35	0.48	0.37	0.48
Unskilled employee	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Skilled employee	0.56	0.50	0.59	0.49	0.57	0.50	0.56	0.50
Ass. to technical school[6]	0.03	0.17	0.02	0.16	0.04	0.18	0.02	0.16
Ass. to adv. technical college	0.02	0.15	0.02	0.13	0.02	0.13	0.02	0.13
Ass. to university	0.02	0.15	0.03	0.17	0.02	0.15	0.02	0.15
Ass. to top-management	0.00	0.00	0.00	0.02	0.00	0.01	0.00	0.02
Professional Training								
Without completed prof. training	0.15	0.36	0.14	0.34	0.15	0.35	0.17	0.37
Apprenticeship (on-the-job)	0.73	0.44	0.76	0.42	0.74	0.44	0.73	0.44
Apprenticeship (off-the-job)	0.01	0.10	0.01	0.10	0.01	0.08	0.01	0.09
Full-time vocational school	0.01	0.10	0.01	0.09	0.01	0.11	0.01	0.10
Technical school	0.06	0.24	0.04	0.20	0.06	0.23	0.05	0.21
Advanced technical college	0.01	0.10	0.01	0.10	0.01	0.11	0.01	0.09
University	0.03	0.16	0.03	0.17	0.03	0.16	0.03	0.16
Month of Treatment Start								
July 2000	0.19	0.39	0.17	0.38	0.22	0.41	0.19	0.39
September 2000	0.21	0.41	0.19	0.39	0.23	0.42	0.23	0.42
November 2000	0.14	0.34	0.13	0.34	0.15	0.35	0.16	0.36
January 2001	0.06	0.24	0.12	0.33	0.05	0.22	0.11	0.31
March 2001	0.19	0.39	0.18	0.38	0.16	0.37	0.14	0.35
May 2001	0.22	0.41	0.21	0.40	0.19	0.39	0.18	0.38
Regional Context Variable								
Cluster Ia	0.17	0.38	0.15	0.36	0.16	0.37	0.17	0.38
Cluster Ib	0.70	0.46	0.67	0.47	0.70	0.46	0.67	0.47
Cluster Ic	0.12	0.32	0.15	0.36	0.12	0.32	0.13	0.34
Cluster II	0.01	0.11	0.03	0.17	0.02	0.13	0.03	0.17
Desired Work Time								
Full-time work	0.99	0.10	0.98	0.15	0.99	0.10	0.97	0.16
Part-time work	0.01	0.10	0.02	0.15	0.01	0.10	0.03	0.16
Other (e.g., telework)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Work Time (Last Occupation)								
Full-time work	0.41	0.49	0.50	0.50	0.48	0.50	0.46	0.50
Part-time work	0.12	0.32	0.06	0.23	0.13	0.34	0.08	0.28
Not applicable	0.48	0.50	0.44	0.50	0.39	0.49	0.46	0.50
Desired Occupation								
Farming[7]	0.09	0.29	0.06	0.23	0.09	0.28	0.07	0.25
Mining, mineral extraction	0.00	0.03	0.00	0.03	0.00	0.04	0.00	0.03
Manufacturing	0.40	0.49	0.41	0.49	0.40	0.49	0.37	0.48

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Table A.3: continued

	Unemployment duration[1] <= 6 months				Unemployment duration[1] > 6 and <= 12 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Technical professions	0.04	0.19	0.04	0.20	0.04	0.20	0.04	0.20
Service professions	0.47	0.50	0.47	0.50	0.46	0.50	0.50	0.50
Other occupations	0.01	0.08	0.02	0.14	0.00	0.07	0.02	0.14

[1] Unemployment duration until treatment start.
[2] Attendant for vocational rehabilitation.
[3] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[4] Schooling: CSE = Certificate of Secondary Education.
[5] Advanced technical college entrance qualification (*Fachhochschulreife*).
[6] Ass. = assimilable.
[7] Farming comprises plant cultivation, breeding and fishery.

Table A.4: Descriptive Statistics for Selected Characteristics – East Germany (Unemployment Duration 13 to 18 and 19 to 24 Months)

	Unemployment duration[1] > 12 months and <= 18 months				Unemployment duration[1] > 18 and <= 24 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
No. of observations	4,619		36,050		2,123		20,714	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Programme duration	298.10	100.10	—	—	295.50	103.40	—	—
Age								
25 to 29 years	0.05	0.21	0.08	0.27	0.05	0.22	0.07	0.25
30 to 34 years	0.10	0.30	0.15	0.35	0.12	0.32	0.13	0.34
35 to 39 years	0.17	0.37	0.19	0.39	0.15	0.36	0.19	0.39
40 to 44 years	0.18	0.38	0.19	0.39	0.18	0.39	0.20	0.40
45 to 49 years	0.23	0.42	0.21	0.40	0.23	0.42	0.21	0.41
50 to 55 years	0.29	0.45	0.19	0.39	0.27	0.44	0.20	0.40
Foreigner	0.01	0.08	0.01	0.12	0.01	0.07	0.02	0.13
Asylum seeker	0.01	0.07	0.02	0.12	0.00	0.07	0.02	0.13
Woman	0.59	0.49	0.60	0.49	0.62	0.48	0.64	0.48
No. of placement propositions	6.91	5.69	3.90	4.67	6.98	5.97	3.40	4.09
No. of children	0.79	1.01	0.84	1.04	0.81	1.05	0.87	1.07
Placement restrictions	0.10	0.30	0.11	0.31	0.10	0.29	0.11	0.32
Vocational rehabilitation[2]	0.06	0.23	0.04	0.20	0.05	0.22	0.04	0.21
Health restrictions	0.18	0.39	0.18	0.38	0.17	0.38	0.18	0.39
Marriage/Cohabitation	0.67	0.47	0.64	0.48	0.67	0.47	0.65	0.48
Work experience	0.92	0.27	0.90	0.29	0.91	0.28	0.90	0.30
Programme before unemployment[3]	0.62	0.49	0.36	0.48	0.59	0.49	0.35	0.48
Reception of UI	0.58	0.49	0.91	0.29	0.61	0.49	0.90	0.29
Duration of Last Employment								
Up to 180 days	0.36	0.48	0.15	0.36	0.35	0.48	0.15	0.35
181 to 365 days	0.23	0.42	0.21	0.41	0.26	0.44	0.21	0.41
1 to 2 years	0.14	0.35	0.16	0.37	0.10	0.30	0.13	0.34
More than 2 years	0.26	0.44	0.47	0.50	0.29	0.45	0.51	0.50
Pension								
No pension	1.00	0.07	1.00	0.07	1.00	0.07	0.99	0.08
Vocational disability	0.00	0.04	0.00	0.03	0.00	0.02	0.00	0.04
Permanently unable to work	0.00	0.05	0.00	0.06	0.00	0.06	0.00	0.06
Social plan	0.00	0.02	0.00	0.00	0.00	0.02	0.00	0.00
Schooling[4]								
No school	0.09	0.29	0.09	0.28	0.11	0.32	0.10	0.29
CSE	0.33	0.47	0.31	0.46	0.35	0.48	0.33	0.47
O-levels	0.52	0.50	0.55	0.50	0.50	0.50	0.52	0.50

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Table A.4: continued

	Unemployment duration[1] > 12 months and <= 18 months				Unemployment duration[1] > 18 and <= 24 months			
	Part.		**Non-Part.**		**Part.**		**Non-Part.**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Adv. technical college entrance[5]	0.01	0.12	0.01	0.11	0.01	0.08	0.01	0.11
A-levels	0.05	0.21	0.05	0.21	0.04	0.19	0.05	0.21
Assessment of Individual's Qualification								
Other	0.39	0.49	0.40	0.49	0.44	0.50	0.43	0.49
Unskilled employee	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.01
Skilled employee	0.54	0.50	0.54	0.50	0.52	0.50	0.52	0.50
Ass. to technical school[6]	0.03	0.17	0.02	0.14	0.02	0.14	0.02	0.14
Ass. to adv. technical college	0.02	0.14	0.01	0.12	0.01	0.11	0.01	0.11
Ass. to university	0.02	0.14	0.02	0.14	0.01	0.11	0.02	0.14
Ass. to top-management	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.01
Professional Training								
Without completed prof. training	0.16	0.37	0.19	0.39	0.20	0.40	0.20	0.40
Apprenticeship (on-the-job)	0.73	0.44	0.73	0.45	0.72	0.45	0.72	0.45
Apprenticeship (off-the-job)	0.01	0.08	0.01	0.08	0.01	0.08	0.01	0.07
Full-time vocational school	0.01	0.10	0.01	0.10	0.01	0.09	0.01	0.10
Technical school	0.06	0.24	0.04	0.20	0.04	0.20	0.04	0.20
Advanced technical college	0.01	0.09	0.01	0.09	0.01	0.09	0.01	0.08
University	0.02	0.14	0.02	0.15	0.02	0.13	0.02	0.15
Month of Treatment Start								
July 2000	0.19	0.39	0.19	0.39	0.16	0.37	0.17	0.38
September 2000	0.22	0.42	0.23	0.42	0.21	0.41	0.22	0.41
November 2000	0.14	0.35	0.14	0.35	0.15	0.36	0.15	0.35
January 2001	0.06	0.24	0.10	0.31	0.07	0.26	0.12	0.32
March 2001	0.16	0.37	0.14	0.35	0.19	0.39	0.15	0.36
May 2001	0.22	0.41	0.19	0.39	0.21	0.41	0.19	0.39
Regional Context Variable								
Cluster Ia	0.21	0.41	0.17	0.37	0.22	0.42	0.16	0.37
Cluster Ib	0.66	0.47	0.67	0.47	0.64	0.48	0.67	0.47
Cluster Ic	0.11	0.32	0.14	0.34	0.12	0.32	0.14	0.34
Cluster II	0.02	0.13	0.03	0.17	0.02	0.13	0.03	0.17
Desired Work Time								
Full-time work	0.99	0.10	0.97	0.17	0.99	0.12	0.97	0.18
Part-time work	0.01	0.10	0.03	0.17	0.01	0.12	0.03	0.18
Other (e.g., telework)	—	—	—	—	—	—	—	—
Work Time (Last Occupation)								
Full-time work	0.49	0.50	0.43	0.49	0.46	0.50	0.39	0.49
Part-time work	0.17	0.38	0.09	0.29	0.15	0.36	0.09	0.28
Not applicable	0.34	0.47	0.48	0.50	0.39	0.49	0.52	0.50
Desired Occupation								
Farming[7]	0.10	0.30	0.07	0.26	0.10	0.30	0.07	0.26
Mining, mineral extraction	0.00	0.03	0.00	0.04	0.00	0.04	0.00	0.04
Manufacturing	0.36	0.48	0.35	0.48	0.36	0.48	0.34	0.47
Technical professions	0.04	0.20	0.04	0.19	0.03	0.17	0.03	0.18
Service professions	0.50	0.50	0.52	0.50	0.51	0.50	0.53	0.50
Other occupations	0.00	0.07	0.02	0.14	0.00	0.05	0.02	0.14

[1] Unemployment duration until treatment start.
[2] Attendant for vocational rehabilitation.
[3] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[4] Schooling: CSE = Certificate of Secondary Education.
[5] Advanced technical college entrance qualification (*Fachhochschulreife*).
[6] Ass. = assimilable.
[7] Farming comprises plant cultivation, breeding and fishery.

Table A.5: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 1$ to $u = 4$ (West Germany)

	$u=1$		$u=2$		$u=3$		$u=4$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-2.261**	0.142	**-2.407**	0.164	**-2.721**	0.178	**-2.498**	0.197
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.102	0.069	*-0.174*	0.073	0.107	0.084	0.020	0.089
35 to 39 years	*0.206*	0.095	-0.203	0.108	0.032	0.119	0.028	0.127
40 to 44 years	0.168	0.136	*-0.383*	0.160	-0.012	0.170	0.014	0.183
45 to 49 years	0.139	0.187	*-0.527*	0.222	-0.069	0.233	0.118	0.251
50 to 55 years	0.212	0.246	*-0.591*	0.293	-0.071	0.307	0.027	0.332
Age (squared)	0.000	0.000	*0.000*	0.000	0.000	0.000	0.000	0.000
Foreigner	**-0.223**	0.051	*-0.112*	0.057	**-0.182**	0.065	**-0.266**	0.071
Asylum seeker	0.047	0.066	**-0.401**	0.114	-0.064	0.090	*-0.205*	0.101
Woman	**-0.095**	0.035	-0.044	0.041	-0.053	0.045	0.011	0.047
No. of placement propositions	**0.015**	0.001	**0.015**	0.001	**0.016**	0.002	**0.016**	0.002
No. of children	**0.053**	0.016	0.016	0.020	0.039	0.021	*0.053*	0.021
Placement restrictions	0.109	0.056	*0.181*	0.071	**0.182**	0.071	-0.052	0.076
Vocational rehabilitation[1]	-0.056	0.070	0.038	0.080	0.108	0.078	0.160	0.089
Health restrictions	*0.108*	0.050	-0.017	0.064	0.089	0.064	*0.142*	0.065
Marriage/Cohabitation	**-0.150**	0.035	*-0.087*	0.041	**-0.142**	0.044	-0.035	0.046
Work experience	0.008	0.059	-0.059	0.064	0.044	0.073	0.079	0.081
Programme bef. unemp.[2]	**0.871**	0.040	**1.050**	0.045	**0.951**	0.046	**0.790**	0.054
Reception of UI	-0.010	0.043	-0.063	0.046	-0.045	0.049	-0.006	0.056
Duration of Last Employment								
Up to 180 days	0.004	0.037	*0.095*	0.042	**0.147**	0.046	**0.145**	0.048
Between 180 and 365 days	**-0.174**	0.051	**-0.225**	0.064	0.001	0.060	**-0.339**	0.084
1 to 2 years	-0.001	0.042	-0.075	0.052	-0.056	0.057	0.016	0.056
More than 2 years	–	–	–	–	–	–	–	–
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	0.154	0.375	-0.009	0.465	–	–
Permanently unable to work	0.113	0.279	–	–	-0.032	0.378	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	**-0.154**	0.043	-0.084	0.053	-0.051	0.059	0.076	0.064
O-levels	-0.104	0.060	*-0.143*	0.072	-0.017	0.078	-0.003	0.087
Adv. technical college entrance[4]	-0.147	0.094	-0.053	0.103	-0.046	0.114	0.031	0.129
A-levels	*-0.185*	0.086	-0.078	0.097	0.051	0.101	0.128	0.111
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.055	0.042	-0.071	0.049	-0.102	0.053	-0.054	0.056
Apprenticeship (off-the-job)	*0.263*	0.106	-0.113	0.163	0.072	0.140	0.019	0.181
Full-time vocational school	*-0.337*	0.162	-0.123	0.144	-0.013	0.132	-0.034	0.150
Technical school	0.014	0.093	0.025	0.107	0.022	0.109	-0.191	0.137
Advanced technical college	0.046	0.140	-0.005	0.170	0.099	0.148	-0.014	0.174
University	0.054	0.116	-0.127	0.151	-0.171	0.145	-0.081	0.155
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	**-0.122**	0.042	0.029	0.049	0.019	0.052	-0.091	0.056
Ass. to technical school[5]	-0.038	0.117	0.103	0.129	0.164	0.123	0.214	0.138
Ass. to adv. technical college	*0.293*	0.122	0.114	0.152	**0.536**	0.132	**0.520**	0.154
Ass. to university	**0.382**	0.109	0.272	0.140	*0.295*	0.141	*0.384*	0.152
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.072	0.049	0.003	0.058	0.016	0.066	0.110	0.076

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Table A.5: (continued)

	$u=1$		$u=2$		$u=3$		$u=4$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
November 2000	-0.012	0.051	0.093	0.062	**0.182**	0.065	**0.315**	0.075
January 2001	0.083	0.051	*0.151*	0.061	0.071	0.070	**0.336**	0.076
March 2001	-0.076	0.051	-0.014	0.061	0.099	0.063	**0.326**	0.072
May 2001	-0.037	0.047	-0.087	0.066	-0.049	0.064	*0.164*	0.072
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	**-0.189**	0.035	*-0.103*	0.042	0.017	0.045	*-0.115*	0.047
Cluster IV	**-0.232**	0.055	**-0.218**	0.068	**-0.250**	0.080	**-0.260**	0.080
Cluster V	**-0.210**	0.042	*-0.104*	0.049	-0.095	0.055	**-0.198**	0.058
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.086	0.094	0.067	0.100	-0.023	0.111	0.078	0.110
Not applicable	0.040	0.032	*0.082*	0.038	-0.020	0.040	-0.020	0.043
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.015	0.060	-0.119	0.070	-0.138	0.078	**-0.262**	0.083
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	-0.463	0.402	–	–	-0.096	0.379	-0.203	0.401
Manufacturing	**-0.284**	0.060	**-0.317**	0.073	**-0.227**	0.075	**-0.337**	0.081
Technical professions	**-0.537**	0.109	**-0.436**	0.124	**-0.399**	0.119	**-0.852**	0.158
Service professions	**-0.356**	0.060	**-0.338**	0.073	**-0.323**	0.075	**-0.448**	0.081
Other occupations	**-0.560**	0.140	**-0.785**	0.198	**-0.615**	0.191	**-0.711**	0.199
N	125,430		93,477		69,869		55,358	
Log-Likelihood	-3,739.57		-2,686.84		-2,390.41		-2,073.35	
R^2	0.134		0.146		0.158		0.134	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.6: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 5$ to $u = 8$ (West Germany)

	$u=5$		$u=6$		$u=7$		$u=8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-2.357**	0.219	**-2.502**	0.213	**-2.087**	0.210	**-1.820**	0.223
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	-0.084	0.100	0.001	0.100	0.066	0.102	*0.215*	0.106
35 to 39 years	-0.005	0.140	-0.085	0.139	0.030	0.141	*0.313*	0.147
40 to 44 years	-0.061	0.202	-0.037	0.196	-0.036	0.200	0.402	0.210

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Table A.6: (continued)

	$u = 5$		$u = 6$		$u = 7$		$u = 8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
45 to 49 years	-0.003	0.278	0.030	0.266	-0.105	0.274	0.520	0.288
50 to 55 years	0.016	0.367	-0.065	0.354	-0.132	0.360	0.551	0.381
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	**-0.206**	0.074	-0.103	0.071	**-0.231**	0.074	**-0.203**	0.075
Asylum seeker	**-0.426**	0.133	**-0.409**	0.122	*-0.214*	0.109	0.083	0.101
Woman	-0.089	0.053	0.008	0.051	-0.077	0.052	*-0.117*	0.055
No. of placement propositions	**0.019**	0.002	**0.019**	0.002	**0.017**	0.002	**0.019**	0.002
No. of children	**0.063**	0.024	0.043	0.023	**0.064**	0.022	-0.002	0.026
Placement restrictions	**0.271**	0.092	-0.047	0.082	0.005	0.087	-0.026	0.090
Vocational rehabilitation[1]	-0.182	0.111	0.030	0.101	0.093	0.106	*0.232*	0.103
Health restrictions	-0.020	0.084	0.112	0.071	0.054	0.075	0.065	0.077
Marriage/Cohabitation	*-0.107*	0.052	0.023	0.051	-0.042	0.051	-0.030	0.053
Work experience	0.120	0.092	-0.081	0.079	0.106	0.091	0.110	0.093
Programme bef. unemp.[2]	**0.847**	0.057	**0.939**	0.055	**0.777**	0.056	**0.735**	0.060
Reception of UI	**-0.223**	0.057	-0.095	0.058	**-0.220**	0.058	*-0.154*	0.063
Duration of Last Employment								
Up to 180 days	**0.182**	0.053	**0.185**	0.051	*0.134*	0.054	*0.123*	0.055
Between 180 and 365 days	-0.128	0.083	-0.081	0.078	0.073	0.072	-0.042	0.080
1 to 2 years	-0.047	0.068	*-0.156*	0.070	*0.138*	0.061	0.041	0.067
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	0.391	0.422	–	–	–	–	–	–
Permanently unable to work	–	–	–	–	–	–	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	**-0.186**	0.065	-0.127	0.067	**-0.202**	0.066	-0.083	0.070
O-levels	-0.054	0.087	*-0.226*	0.093	**-0.254**	0.091	-0.093	0.096
Adv. technical college entrance[4]	-0.138	0.142	-0.137	0.133	-0.001	0.122	0.158	0.126
A-levels	-0.031	0.122	-0.024	0.115	0.022	0.109	0.139	0.115
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.034	0.064	0.095	0.061	-0.044	0.063	0.023	0.064
Apprenticeship (off-the-job)	0.218	0.169	*0.407*	0.158	0.052	0.199	**0.480**	0.153
Full-time vocational school	0.078	0.164	*0.342*	0.146	0.085	0.158	-0.105	0.194
Technical school	-0.196	0.162	**0.344**	0.122	*0.253*	0.116	0.017	0.135
Advanced technical college	-0.020	0.212	*0.406*	0.181	-0.014	0.175	0.043	0.190
University	-0.075	0.185	0.148	0.169	0.063	0.158	-0.272	0.175
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	-0.045	0.063	-0.035	0.059	0.014	0.062	*-0.144*	0.064
Ass. to technical school[5]	*0.361*	0.147	-0.010	0.163	0.004	0.167	0.195	0.150
Ass. to adv. technical college	**0.471**	0.182	0.200	0.161	**0.401**	0.146	0.168	0.173
Ass. to university	0.297	0.182	0.118	0.162	-0.006	0.159	0.223	0.161
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	0.124	0.071	0.046	0.072	-0.057	0.074	0.081	0.072
November 2000	0.079	0.081	0.143	0.081	**0.317**	0.071	0.041	0.080
January 2001	**0.256**	0.077	**0.266**	0.080	0.146	0.080	0.111	0.092
March 2001	0.121	0.076	0.044	0.078	0.091	0.076	0.152	0.078
May 2001	0.074	0.075	**0.181**	0.068	0.063	0.069	0.091	0.072
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–

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Table A.6: (continued)

	$u = 5$		$u = 6$		$u = 7$		$u = 8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	-0.053	0.053	*-0.120*	0.051	-0.039	0.050	*-0.105*	0.053
Cluster IV	-0.164	0.086	**-0.299**	0.090	**-0.400**	0.102	-0.113	0.084
Cluster V	*-0.140*	0.066	-0.107	0.062	-0.089	0.064	*-0.177*	0.070
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	*0.265*	0.116	-0.096	0.134	0.119	0.104	0.101	0.114
Not applicable	-0.022	0.048	-0.023	0.047	**-0.156**	0.047	**-0.161**	0.049
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	**-0.283**	0.096	-0.137	0.083	-0.158	0.082	-0.082	0.088
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	–	–	–	–	-0.239	0.406	0.033	0.432
Manufacturing	-0.028	0.104	-0.012	0.110	**-0.266**	0.092	*-0.236*	0.101
Technical professions	**-0.636**	0.192	-0.303	0.164	**-0.584**	0.153	**-0.512**	0.161
Service professions	-0.181	0.105	-0.135	0.111	**-0.354**	0.093	**-0.311**	0.102
Other occupations	-0.366	0.207	*-0.474*	0.242	**-1.090**	0.374	**-1.021**	0.364
N	38,091		35,860		27,347		23,602	
Log-Likelihood	-1,699.69		-1,807.31		-1,850.46		-1,665.33	
R^2	0.154		0.158		0.146		0.122	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.7: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 9$ to $u = 12$ (West Germany)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.956**	0.234	**-1.734**	0.249	**-1.945**	0.276	**-1.483**	0.221
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.041	0.118	-0.134	0.122	0.010	0.133	-0.065	0.113
35 to 39 years	0.159	0.158	-0.046	0.167	0.149	0.178	0.040	0.149
40 to 44 years	0.053	0.225	-0.144	0.240	0.131	0.256	0.063	0.210
45 to 49 years	-0.075	0.307	-0.169	0.328	0.074	0.348	-0.042	0.288
50 to 55 years	-0.079	0.406	-0.236	0.433	0.067	0.463	-0.103	0.376
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	*-0.180*	0.078	**-0.261**	0.091	*-0.205*	0.092	-0.099	0.074
Asylum seeker	**-0.410**	0.135	-0.029	0.119	-0.262	0.139	-0.048	0.098
Woman	*-0.126*	0.059	**-0.177**	0.065	-0.045	0.065	*-0.131*	0.055
No. of placement propositions	**0.020**	0.002	**0.020**	0.003	**0.020**	0.003	**0.025**	0.002
No. of children	0.003	0.027	-0.031	0.032	0.032	0.029	**0.081**	0.025

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Table A.7: (continued)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Placement restrictions	-0.137	0.097	-0.084	0.105	-0.039	0.115	0.042	0.097
Vocational rehabilitation[1]	0.190	0.117	0.233	0.122	*0.304*	0.131	0.143	0.120
Health restrictions	0.109	0.082	0.006	0.089	-0.036	0.100	-0.054	0.085
Marriage/Cohabitation	0.061	0.058	0.007	0.062	0.022	0.065	*-0.112*	0.055
Work experience	-0.101	0.089	0.051	0.101	*0.272*	0.124	0.021	0.083
Programme bef. unemp.[2]	**0.734**	0.065	**0.749**	0.067	**0.737**	0.070	**0.729**	0.061
Reception of UI	**-0.202**	0.066	*-0.160*	0.072	**-0.314**	0.070	**-0.450**	0.056
Duration of Last Employment								
Up to 180 days	**0.159**	0.058	**0.236**	0.063	**0.235**	0.067	*0.131*	0.055
Between 180 and 365 days	-0.103	0.092	-0.099	0.097	0.128	0.094	*-0.201*	0.088
1 to 2 years	-0.109	0.078	0.023	0.081	-0.008	0.088	0.003	0.069
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	–	–	–	–	–	–
Permanently unable to work	0.653	0.360	–	–	–	–	-0.052	0.500
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.114	0.077	*-0.154*	0.079	-0.012	0.085	0.063	0.074
O-levels	0.155	0.102	*-0.227*	0.109	-0.219	0.122	0.010	0.098
Adv. technical college entrance[4]	0.156	0.155	**-0.485**	0.181	-0.071	0.169	0.099	0.139
A-levels	-0.028	0.146	-0.298	0.153	0.065	0.145	0.122	0.122
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	**-0.245**	0.071	*0.162*	0.073	-0.081	0.081	-0.030	0.066
Apprenticeship (off-the-job)	0.156	0.198	0.086	0.284	-0.513	0.391	0.211	0.201
Full-time vocational school	-0.144	0.188	0.209	0.198	-0.154	0.237	0.107	0.159
Technical school	*0.268*	0.129	0.198	0.166	0.023	0.170	0.087	0.128
Advanced technical college	0.167	0.213	*0.506*	0.228	0.286	0.238	-0.230	0.197
University	*0.441*	0.190	0.051	0.216	-0.352	0.229	-0.049	0.166
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	-0.056	0.071	*-0.156*	0.073	0.063	0.081	0.054	0.065
Ass. to technical school[5]	*0.318*	0.156	-0.128	0.221	0.103	0.235	*0.345*	0.157
Ass. to adv. technical college	0.282	0.174	0.371	0.204	0.260	0.224	**0.582**	0.159
Ass. to university	-0.176	0.188	0.224	0.209	0.375	0.217	0.294	0.164
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	**0.264**	0.079	0.072	0.083	-0.168	0.088	-0.073	0.069
November 2000	*0.209*	0.092	**0.254**	0.086	-0.029	0.092	0.040	0.078
January 2001	**0.392**	0.093	*0.215*	0.093	0.134	0.097	0.059	0.088
March 2001	**0.263**	0.090	0.177	0.093	0.152	0.091	0.111	0.076
May 2001	*0.183*	0.086	0.103	0.087	-0.018	0.089	0.109	0.076
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	-0.109	0.057	*-0.151*	0.061	0.038	0.065	**-0.191**	0.053
Cluster IV	-0.162	0.092	-0.042	0.094	-0.078	0.111	**-0.269**	0.089
Cluster V	-0.135	0.074	-0.109	0.079	0.048	0.084	-0.122	0.068
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.112	0.122	-0.206	0.171	0.046	0.146	0.086	0.116
Not applicable	*-0.111*	0.053	*-0.131*	0.056	-0.033	0.060	**-0.212**	0.049

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Table A.7: (continued)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Desired Work Time								
Full-time work	–	–	–	–	–	–	–	–
Part-time work	-0.083	0.089	0.019	0.100	-0.184	0.100	-0.122	0.081
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	**-0.386**	0.099	**-0.406**	0.105	**-0.369**	0.118	*-0.233*	0.105
Technical professions	**-0.971**	0.199	**-0.652**	0.190	**-0.712**	0.210	**-0.513**	0.165
Service professions	**-0.393**	0.100	**-0.401**	0.105	**-0.376**	0.118	-0.188	0.104
Other occupations	**-0.727**	0.241	**-0.816**	0.283	–	–	**-0.931**	0.349
N	19,322		16,650		14,178		13,472	
Log-Likelihood	-1,441.50		-1,265.18		-1,135.90		-1,737.97	
R^2	0.139		0.146		0.143		0.156	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.8: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 13$ to $u = 16$ (West Germany)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-0.909**	0.217	**-1.322**	0.299	**-0.878**	0.319	**-2.277**	0.386
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	-0.074	0.110	0.109	0.142	-0.058	0.161	-0.211	0.197
35 to 39 years	-0.045	0.149	0.196	0.197	0.328	0.209	-0.247	0.258
40 to 44 years	-0.016	0.211	0.370	0.280	0.436	0.297	-0.016	0.356
45 to 49 years	-0.062	0.288	0.490	0.385	0.753	0.406	-0.069	0.489
50 to 55 years	-0.102	0.379	0.903	0.504	1.024	0.535	-0.363	0.642
Age (squared)	0.000	0.000	0.000	0.000	*-0.001*	0.000	0.000	0.000
Foreigner	**-0.265**	0.080	-0.070	0.094	*-0.247*	0.113	-0.091	0.119
Asylum seeker	**-0.376**	0.117	0.178	0.123	0.034	0.143	-0.044	0.168
Woman	-0.097	0.055	-0.072	0.074	-0.088	0.079	-0.066	0.096
No. of placement propositions	**0.020**	0.002	**0.023**	0.003	**0.027**	0.003	**0.022**	0.004
No. of children	**0.074**	0.025	*0.077*	0.032	0.043	0.036	0.022	0.044
Placement restrictions	0.184	0.098	0.102	0.123	-0.047	0.129	0.004	0.169
Vocational rehabilitation[1]	*0.268*	0.109	0.145	0.147	0.071	0.163	0.260	0.202
Health restrictions	-0.106	0.089	0.057	0.110	0.124	0.112	-0.004	0.146
Marriage/Cohabitation	-0.105	0.056	-0.131	0.073	*-0.155*	0.079	0.024	0.095
Work experience	-0.017	0.082	0.147	0.127	-0.091	0.123	0.308	0.180
Programme bef. unemp.[2]	**1.145**	0.057	**0.652**	0.083	**0.464**	0.092	**0.677**	0.103
Reception of UI	**-0.659**	0.055	-0.100	0.092	**-0.298**	0.093	-0.154	0.114

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Table A.8: (continued)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Duration of Last Employment								
Up to 180 days	*0.119*	0.056	*0.161*	0.077	0.094	0.085	**0.332**	0.098
Between 180 and 365 days	**-0.231**	0.090	0.060	0.104	**0.252**	0.098	0.164	0.129
1 to 2 years	-0.050	0.072	0.112	0.092	0.001	0.108	0.114	0.127
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	-0.158	0.457	–	–	–	–	0.892	0.636
Permanently unable to work	–	–	0.235	0.602	–	–	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	-0.063	0.076	-0.018	0.092	0.162	0.106	-0.111	0.114
O-levels	0.058	0.098	-0.003	0.128	-0.002	0.150	-0.207	0.173
Adv. technical college entrance[4]	0.101	0.136	0.019	0.190	-0.072	0.241	0.030	0.235
A-levels	0.026	0.127	0.008	0.178	0.060	0.204	-0.177	0.227
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	0.056	0.068	-0.087	0.089	0.136	0.091	-0.044	0.116
Apprenticeship (off-the-job)	*0.400*	0.188	0.372	0.244	0.370	0.286	–	–
Full-time vocational school	*0.354*	0.144	0.174	0.207	0.012	0.274	-0.383	0.416
Technical school	*0.316*	0.124	-0.019	0.207	-0.112	0.257	0.210	0.231
Advanced technical college	0.341	0.183	-0.057	0.316	0.221	0.330	-0.407	0.352
University	0.003	0.172	0.341	0.250	-0.019	0.296	0.079	0.328
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.038	0.066	0.038	0.088	*-0.220*	0.093	-0.119	0.121
Ass. to technical school[5]	0.067	0.179	-0.021	0.273	-0.063	0.266	0.097	0.346
Ass. to adv. technical college	**0.485**	0.160	0.352	0.254	0.126	0.296	*0.726*	0.288
Ass. to university	0.159	0.164	-0.077	0.252	0.218	0.285	0.147	0.327
Ass. to top-management	0.090	0.427	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.012	0.071	-0.020	0.093	-0.005	0.098	0.235	0.132
November 2000	0.065	0.080	-0.138	0.108	0.022	0.108	0.181	0.147
January 2001	**0.293**	0.078	-0.033	0.117	-0.149	0.138	*0.354*	0.142
March 2001	**0.259**	0.078	0.030	0.106	0.008	0.108	*0.302*	0.138
May 2001	-0.015	0.079	-0.093	0.102	-0.126	0.114	0.182	0.141
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	*-0.124*	0.054	*-0.141*	0.070	-0.114	0.075	-0.165	0.092
Cluster IV	-0.148	0.085	-0.140	0.112	-0.076	0.122	-0.214	0.155
Cluster V	-0.013	0.071	-0.150	0.098	*-0.265*	0.111	-0.149	0.126
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.005	0.118	-0.182	0.172	0.181	0.150	0.355	0.188
Not applicable	**-0.248**	0.050	**-0.253**	0.067	**-0.364**	0.073	-0.112	0.088
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	**-0.382**	0.088	0.022	0.110	-0.161	0.130	-0.277	0.160
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	–	–	-0.009	0.483	–	–	–	–

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Table A.8: (continued)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Manufacturing	**-0.424**	0.101	*-0.322*	0.127	-0.194	0.153	-0.281	0.165
Technical professions	**-0.870**	0.177	**-0.726**	0.253	*-0.737*	0.287	-0.442	0.298
Service professions	**-0.383**	0.101	**-0.359**	0.129	*-0.320*	0.154	-0.175	0.165
Other occupations	**-1.098**	0.296	*-0.871*	0.361	-0.429	0.309	–	–
N	11,465		9,563		8,829		7,421	
Log-Likelihood	-1,766.38		-939.64		-805.94		-554.13	
R^2	0.258		0.128		0.150		0.159	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.9: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 17$ to $u = 20$ (West Germany)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-2.063**	0.385	**-1.781**	0.407	**-2.251**	0.396	**-1.725**	0.460
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.109	0.193	-0.083	0.207	0.141	0.220	-0.152	0.241
35 to 39 years	0.088	0.257	-0.035	0.271	0.272	0.264	-0.113	0.318
40 to 44 years	-0.061	0.364	0.151	0.373	0.203	0.354	-0.262	0.446
45 to 49 years	-0.182	0.487	0.239	0.506	-0.108	0.480	-0.450	0.616
50 to 55 years	-0.434	0.654	0.426	0.662	-0.039	0.621	-0.247	0.806
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	-0.058	0.120	*-0.271*	0.138	-0.052	0.119	-0.062	0.152
Asylum seeker	*-0.571*	0.239	-0.178	0.200	-0.275	0.190	0.082	0.212
Woman	-0.139	0.098	-0.175	0.101	-0.003	0.091	-0.129	0.120
No. of placement propositions	**0.029**	0.003	**0.025**	0.004	**0.027**	0.004	**0.026**	0.005
No. of children	0.043	0.045	**0.115**	0.044	-0.029	0.045	0.055	0.054
Placement restrictions	0.246	0.167	0.135	0.194	0.044	0.162	0.041	0.212
Vocational rehabilitation[1]	*0.392*	0.178	**0.514**	0.194	0.292	0.192	*0.527*	0.231
Health restrictions	-0.106	0.153	-0.327	0.176	-0.086	0.142	-0.138	0.185
Marriage/ cohabitation	*-0.242*	0.095	-0.179	0.101	-0.004	0.092	*-0.298*	0.121
Work experience	0.244	0.164	-0.071	0.154	0.139	0.157	-0.265	0.162
Programme bef. unemp.[2]	**0.704**	0.102	**0.845**	0.106	**0.624**	0.101	**0.866**	0.114
Reception of UI	**-0.291**	0.110	-0.212	0.115	-0.105	0.118	-0.063	0.158
Duration of Last Employment								
Up to 180 days	*0.207*	0.097	0.135	0.102	**0.327**	0.093	0.183	0.126
Between 180 and 365 days	0.110	0.128	-0.097	0.151	0.203	0.127	0.154	0.151
1 to 2 years	-0.012	0.125	0.063	0.126	0.030	0.126	0.132	0.151

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Table A.9: (continued)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	–	–	–	–	–	–
Permanently unable to work	–	–	–	–	–	–	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	-0.063	0.112	-0.125	0.120	0.140	0.114	-0.191	0.135
O-levels	-0.007	0.163	-0.154	0.177	-0.116	0.178	-0.074	0.198
Adv. technical college entrance[4]	-0.294	0.300	0.343	0.231	-0.060	0.276	0.145	0.283
A-levels	-0.014	0.220	0.141	0.209	0.055	0.228	0.117	0.232
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.213	0.120	0.004	0.119	-0.063	0.107	-0.037	0.143
Apprenticeship (off-the-job)	-0.193	0.505	0.042	0.401	**1.046**	0.301	–	–
Full-time vocational school	0.395	0.235	-0.224	0.368	0.317	0.287	0.192	0.329
Technical school	0.279	0.227	-0.055	0.259	0.307	0.235	-0.104	0.315
Advanced technical college	0.063	0.413	-0.421	0.425	0.028	0.465	0.373	0.429
University	-0.013	0.341	-0.681	0.364	0.154	0.365	-0.117	0.395
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.023	0.121	-0.128	0.119	-0.043	0.108	-0.259	0.154
Ass. to technical school[5]	0.384	0.272	0.247	0.316	–	–	0.121	0.364
Ass. to adv. technical college	0.343	0.347	-0.003	0.362	0.039	0.406	-0.586	0.443
Ass. to university	0.411	0.329	0.249	0.328	-0.075	0.353	-0.094	0.374
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.052	0.131	0.159	0.137	-0.102	0.120	0.178	0.159
November 2000	0.156	0.132	0.220	0.152	0.126	0.124	0.226	0.169
January 2001	*0.322*	0.139	*0.321*	0.158	0.082	0.138	0.348	0.184
March 2001	0.032	0.139	0.136	0.153	-0.056	0.135	0.244	0.174
May 2001	-0.085	0.136	0.222	0.142	0.075	0.122	0.094	0.162
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	-0.136	0.089	-0.047	0.093	-0.109	0.088	-0.157	0.106
Cluster IV	-0.181	0.150	-0.110	0.161	-0.058	0.142	*-0.507*	0.227
Cluster V	-0.199	0.129	-0.157	0.141	-0.244	0.131	**-0.589**	0.201
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.594	0.401	0.266	0.198	0.040	0.204	0.204	0.256
Not applicable	*-0.207*	0.084	*-0.223*	0.091	*-0.193*	0.084	**-0.322**	0.110
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.065	0.156	-0.192	0.165	-0.224	0.151	-0.129	0.219
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	-0.031	0.158	0.148	0.208	-0.244	0.171	-0.095	0.200
Technical professions	*-0.599*	0.301	-0.652	0.447	*-0.810*	0.401	-0.247	0.382
Service professions	*-0.382*	0.164	0.143	0.209	-0.320	0.173	-0.060	0.203
Other occupations	–	–	0.180	0.374	-0.694	0.450	0.045	0.395

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Table A.9: (continued)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
N	7,012		6,486		6,040		5,512	
Log-Likelihood	-577.25		-517.66		-596.82		-374.87	
R^2	0.200		0.177		0.155		0.219	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.10: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 21$ to $u = 24$ (West Germany)

	$u = 21$		$u = 22$		$u = 23$		$u = 24$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.572**	0.438	-0.251	0.452	**-1.510**	0.472	**-1.762**	0.525
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	-0.210	0.249	-0.406	0.228	0.098	0.243	-0.010	0.331
35 to 39 years	-0.079	0.304	-0.405	0.315	-0.075	0.313	0.287	0.378
40 to 44 years	-0.207	0.415	-0.129	0.441	0.025	0.431	0.153	0.496
45 to 49 years	-0.101	0.550	-0.021	0.610	-0.050	0.573	0.116	0.647
50 to 55 years	-0.211	0.718	-0.092	0.811	0.137	0.755	-0.066	0.836
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	-0.045	0.141	-0.304	0.163	**-0.444**	0.161	**-0.565**	0.201
Asylum seeker	0.299	0.165	-0.180	0.211	*-0.633*	0.292	-0.137	0.214
Woman	-0.188	0.111	-0.128	0.119	-0.096	0.109	0.132	0.117
No. of placement propositions	**0.027**	0.004	**0.025**	0.005	**0.027**	0.005	**0.022**	0.006
No. of children	0.043	0.049	**0.171**	0.048	0.059	0.047	**0.137**	0.050
Placement restrictions	0.191	0.190	-0.148	0.235	-0.047	0.201	**-0.727**	0.235
Vocational rehabilitation[1]	**0.586**	0.184	**0.720**	0.246	0.174	0.250	*0.700*	0.277
Health restrictions	-0.075	0.174	-0.235	0.205	-0.003	0.172	0.215	0.167
Marriage/Cohabitation	-0.129	0.111	*-0.310*	0.123	0.165	0.111	0.039	0.123
Work experience	0.080	0.187	-0.250	0.161	0.291	0.217	0.103	0.194
Programme bef. unemp.[2]	**0.679**	0.116	**0.470**	0.130	*0.325*	0.130	**0.669**	0.136
Reception of UI	*-0.306*	0.127	*-0.278*	0.133	**-0.375**	0.132	-0.267	0.140
Duration of Last Employment								
Up to 180 days	0.213	0.112	0.153	0.124	0.147	0.118	-0.075	0.132
Between 180 and 365 days	0.161	0.153	*0.333*	0.150	0.185	0.156	-0.069	0.171
1 to 2 years	0.079	0.151	-0.045	0.162	*0.298*	0.136	0.015	0.164
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	–	–	–	–	–	–
Permanently unable to work	0.612	0.533	–	–	–	–	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.047	0.139	-0.128	0.135	-0.235	0.134	-0.135	0.164
O-levels	-0.298	0.218	-0.247	0.205	-0.324	0.212	-0.226	0.234
Adv. technical college entrance[4]	-0.191	0.312	-0.411	0.372	0.072	0.297	0.099	0.288
A-levels	-0.099	0.255	0.016	0.257	-0.084	0.257	0.340	0.246

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Table A.10: (continued)

	$u = 21$		$u = 22$		$u = 23$		$u = 24$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.055	0.130	-0.261	0.147	0.060	0.131	0.168	0.145
Apprenticeship (off-the-job)	0.083	0.532	0.394	0.430	0.581	0.533	0.575	0.479
Full-time vocational school	-0.533	0.490	–	–	0.088	0.354	0.516	0.302
Technical school	-0.196	0.313	0.249	0.267	-0.145	0.340	0.392	0.280
Advanced technical college	-0.022	0.395	-0.498	0.491	-0.734	0.501	-0.075	0.489
University	-0.599	0.368	-0.116	0.403	-0.189	0.412	0.144	0.380
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.044	0.131	0.136	0.150	-0.174	0.133	*-0.319*	0.142
Ass. to technical school[5]	0.547	0.305	-0.137	0.513	-0.296	0.472	-0.409	0.517
Ass. to adv. technical college	0.582	0.357	0.581	0.385	0.531	0.383	-0.159	0.411
Ass. to university	**0.944**	0.339	0.033	0.418	0.039	0.392	-0.478	0.373
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.058	0.136	0.006	0.140	0.053	0.135	0.186	0.154
November 2000	-0.034	0.163	-0.168	0.182	-0.246	0.179	0.147	0.168
January 2001	0.066	0.173	-0.037	0.180	0.092	0.164	-0.113	0.221
March 2001	0.026	0.153	-0.039	0.173	*0.291*	0.144	0.066	0.179
May 2001	-0.038	0.150	-0.020	0.151	-0.243	0.171	-0.123	0.184
Regional Context Variables								
Cluster Ia	–	–	–	–	–	–	–	–
Cluster Ib	–	–	–	–	–	–	–	–
Cluster Ic	–	–	–	–	–	–	–	–
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster III	-0.102	0.104	-0.081	0.110	-0.176	0.102	**-0.405**	0.114
Cluster IV	-0.156	0.175	-0.247	0.202	-0.236	0.181	**-1.303**	0.425
Cluster V	-0.238	0.159	-0.242	0.169	**-0.532**	0.188	-0.219	0.155
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.156	0.254	0.210	0.257	-0.190	0.280	-0.088	0.293
Not applicable	**-0.422**	0.101	-0.167	0.109	**-0.322**	0.100	**-0.443**	0.110
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.037	0.182	-0.047	0.184	*-0.445*	0.206	*-0.494*	0.199
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	**-0.557**	0.182	**-0.724**	0.170	-0.006	0.202	-0.261	0.250
Technical professions	*-0.742*	0.323	*-0.938*	0.378	-0.146	0.373	–	–
Service professions	-0.343	0.179	**-0.783**	0.174	-0.092	0.206	-0.098	0.250
Other occupations	-0.092	0.326	–	–	–	–	-0.032	0.494
N	5,294		4,547		4,363		3,954	
Log-Likelihood	-428.88		-376.51		-419.00		-358.49	
R^2	0.192		0.184		0.154		0.206	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.11: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 1$ to $u = 4$ (East Germany)

	$u = 1$		$u = 2$		$u = 3$		$u = 4$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.810**	0.117	**-2.034**	0.138	**-2.274**	0.137	**-2.303**	0.149
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	*0.135*	0.059	0.026	0.071	0.003	0.071	0.064	0.073
35 to 39 years	0.154	0.078	0.033	0.091	-0.063	0.092	-0.031	0.097
40 to 44 years	0.200	0.108	-0.161	0.126	-0.130	0.126	-0.120	0.135
45 to 49 years	0.245	0.147	-0.171	0.170	-0.151	0.169	-0.182	0.183
50 to 55 years	*0.423*	0.191	-0.212	0.221	-0.133	0.220	-0.144	0.237
Age (squared)	0.000	0.000	**0.000**	0.000	**0.000**	0.000	*0.000*	0.000
Foreigner	*-0.355*	0.138	*-0.389*	0.156	**-0.455**	0.173	**-0.480**	0.183
Asylum seeker	**-0.331**	0.115	**-0.411**	0.130	**-0.945**	0.214	**-0.714**	0.184
Woman	**0.077**	0.028	0.035	0.033	0.051	0.032	-0.028	0.035
No. of placement propositions	**0.021**	0.002	**0.027**	0.002	**0.025**	0.002	**0.025**	0.002
No. of children	0.016	0.014	*0.035*	0.016	**0.043**	0.016	0.008	0.017
Placement restrictions	0.011	0.055	-0.007	0.063	-0.025	0.065	*-0.166*	0.068
Vocational rehabilitation[1]	*0.126*	0.063	*0.171*	0.072	**0.201**	0.074	*0.195*	0.077
Health restrictions	0.068	0.043	*0.114*	0.049	0.065	0.050	**0.181**	0.051
Marriage/Cohabitation	-0.010	0.028	-0.038	0.033	-0.012	0.032	0.014	0.035
Work experience	0.019	0.043	0.058	0.052	-0.082	0.048	-0.027	0.052
Programme bef. unemp.[2]	**0.540**	0.026	**0.484**	0.031	**0.404**	0.031	**0.373**	0.033
Reception of UI	**-0.681**	0.029	**-0.707**	0.032	**-0.594**	0.033	**-0.593**	0.035
Duration of Last Employment								
Up to 180 days	**0.270**	0.032	**0.335**	0.036	**0.381**	0.037	**0.340**	0.039
Between 180 and 365 days	0.058	0.039	-0.030	0.048	*0.094*	0.047	0.028	0.051
1 to 2 years	-0.017	0.034	**-0.133**	0.041	0.039	0.039	0.029	0.042
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	-0.051	0.311	0.459	0.267	0.074	0.337	0.473	0.295
Permanently unable to work	-0.517	0.293	-0.431	0.272	-0.125	0.236	-0.069	0.269
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	-0.040	0.051	0.005	0.060	0.069	0.060	0.050	0.068
O-levels	*-0.131*	0.052	-0.041	0.062	0.004	0.062	0.037	0.069
Adv. technical college entrance[4]	0.028	0.113	*-0.347*	0.174	-0.012	0.148	0.092	0.159
A-levels	*-0.214*	0.090	-0.089	0.105	-0.036	0.105	0.016	0.113
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	0.030	0.040	-0.011	0.045	0.008	0.046	0.039	0.051
Apprenticeship (off-the-job)	0.133	0.129	0.042	0.163	0.169	0.147	0.226	0.153
Full-time vocational school	0.064	0.124	0.004	0.150	0.075	0.146	-0.106	0.185
Technical school	**0.326**	0.071	0.024	0.090	*0.183*	0.084	**0.265**	0.089
Advanced technical college	0.152	0.148	-0.157	0.207	0.045	0.182	-0.063	0.204
University	*0.282*	0.122	-0.057	0.148	0.061	0.143	0.146	0.154
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.007	0.029	-0.009	0.033	-0.006	0.033	*0.071*	0.036
Ass. to technical school[5]	0.001	0.082	-0.026	0.104	-0.007	0.096	0.140	0.100
Ass. to adv. technical college	0.058	0.110	0.208	0.133	0.071	0.133	0.151	0.133
Ass. to university	-0.095	0.107	0.053	0.133	0.155	0.125	0.032	0.138
Ass. to top-management	–	–	–	–	–	–	–	–

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Table A.11: (continued)

	$u = 1$		$u = 2$		$u = 3$		$u = 4$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	0.041	0.039	-0.013	0.043	0.041	0.046	0.083	0.047
November 2000	-0.004	0.041	-0.086	0.048	0.066	0.052	*0.134*	0.053
January 2001	**-0.173**	0.048	**-0.441**	0.059	**-0.246**	0.062	*-0.142*	0.065
March 2001	0.052	0.038	-0.073	0.046	**0.165**	0.044	**0.253**	0.049
May 2001	**-0.110**	0.037	*-0.100*	0.048	0.022	0.044	**0.227**	0.046
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	**-0.113**	0.032	**-0.097**	0.037	0.016	0.039	0.011	0.041
Cluster Ic	**-0.329**	0.045	**-0.286**	0.052	*-0.105*	0.052	-0.095	0.055
Cluster II	**-0.254**	0.095	**-0.332**	0.115	-0.197	0.117	-0.196	0.118
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	**0.241**	0.043	**0.247**	0.052	0.044	0.051	*0.126*	0.053
Not applicable	**0.114**	0.027	**0.125**	0.032	**0.193**	0.031	**0.170**	0.033
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	*-0.241*	0.104	*-0.265*	0.118	**-0.718**	0.183	**-0.421**	0.150
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	-0.233	0.424	-0.113	0.464	–	–	-0.158	0.512
Manufacturing	-0.086	0.048	**-0.155**	0.054	*-0.114*	0.053	-0.106	0.059
Technical professions	*-0.199*	0.081	**-0.263**	0.095	**-0.307**	0.095	-0.140	0.094
Service professions	**-0.137**	0.047	**-0.227**	0.053	**-0.159**	0.052	*-0.140*	0.058
Other occupations	**-0.684**	0.159	**-0.564**	0.144	**-0.933**	0.209	**-0.517**	0.165
N	64,541		46,723		40,163		33,499	
Log-Likelihood	-6,250.13		-4,598.57		-4,644.01		-4,146.22	
R^2	0.162		0.177		0.153		0.134	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.12: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 5$ to $u = 8$ (East Germany)

	$u = 5$		$u = 6$		$u = 7$		$u = 8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.780**	0.159	**-1.732**	0.148	**-1.765**	0.146	**-1.833**	0.158
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.065	0.082	0.074	0.077	-0.049	0.073	-0.001	0.085
35 to 39 years	0.016	0.107	0.078	0.099	-0.159	0.096	0.089	0.107

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Table A.12: (continued)

	$u=5$		$u=6$		$u=7$		$u=8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
40 to 44 years	-0.069	0.148	0.062	0.136	-0.207	0.131	0.034	0.144
45 to 49 years	-0.117	0.200	0.080	0.183	-0.253	0.177	-0.011	0.194
50 to 55 years	-0.015	0.258	0.320	0.236	-0.176	0.230	0.002	0.251
Age (squared)	0.000	0.000	0.000	0.000	*0.000*	0.000	0.000	0.000
Foreigner	-0.216	0.164	*-0.342*	0.168	*-0.462*	0.188	**-1.162**	0.375
Asylum seeker	**-0.505**	0.156	**-0.480**	0.144	-0.203	0.140	**-0.565**	0.206
Woman	0.016	0.038	**-0.109**	0.035	**-0.114**	0.034	-0.057	0.037
No. of placement propositions	**0.028**	0.002	**0.025**	0.002	**0.026**	0.002	**0.026**	0.002
No. of children	*0.038*	0.018	0.029	0.017	0.012	0.017	-0.010	0.019
Placement restrictions	0.018	0.073	0.091	0.073	**-0.191**	0.073	0.021	0.077
Vocational rehabilitation[1]	0.097	0.084	0.068	0.082	**0.265**	0.083	*0.212*	0.085
Health restrictions	0.099	0.056	-0.028	0.058	*0.119*	0.053	0.002	0.060
Marriage/Cohabitation	0.000	0.038	-0.002	0.035	**0.108**	0.035	0.018	0.037
Work experience	*-0.135*	0.053	*-0.116*	0.051	0.047	0.054	0.030	0.059
Programme bef. unemp.[2]	**0.260**	0.036	**0.277**	0.033	**0.249**	0.033	**0.244**	0.035
Reception of UI	**-0.585**	0.038	**-0.540**	0.036	**-0.658**	0.035	**-0.531**	0.038
Duration of Last Employment								
Up to 180 days	**0.265**	0.043	**0.328**	0.040	**0.304**	0.040	**0.320**	0.043
Between 180 and 365 days	0.006	0.058	0.062	0.052	*0.125*	0.049	*0.118*	0.054
1 to 2 years	-0.080	0.046	0.021	0.043	*0.099*	0.041	0.053	0.046
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	0.080	0.378	0.309	0.290	0.166	0.409	0.369	0.338
Permanently unable to work	-0.088	0.245	-0.614	0.381	-0.792	0.437	-0.071	0.316
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.012	0.070	0.008	0.063	0.090	0.066	-0.079	0.065
O-levels	-0.012	0.072	-0.058	0.065	0.039	0.068	-0.086	0.067
Adv. technical college entrance[4]	0.018	0.175	-0.302	0.173	-0.045	0.171	-0.187	0.182
A-levels	-0.172	0.130	-0.130	0.117	0.119	0.114	-0.031	0.118
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.015	0.051	0.089	0.046	**0.137**	0.050	*0.101*	0.050
Apprenticeship (off-the-job)	0.105	0.170	0.285	0.149	0.039	0.180	0.116	0.197
Full-time vocational school	-0.259	0.205	0.182	0.157	0.234	0.141	**0.434**	0.148
Technical school	0.131	0.100	**0.341**	0.093	**0.269**	0.093	**0.343**	0.094
Advanced technical college	-0.028	0.224	*0.493*	0.200	0.155	0.209	0.418	0.219
University	-0.011	0.181	0.278	0.162	0.230	0.151	0.141	0.165
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.036	0.038	-0.052	0.035	0.043	0.034	-0.007	0.037
Ass. to technical school[5]	0.026	0.117	-0.079	0.110	0.043	0.100	0.114	0.107
Ass. to adv. technical college	0.191	0.147	-0.035	0.141	0.100	0.144	-0.043	0.153
Ass. to university	0.068	0.162	-0.170	0.151	-0.026	0.138	-0.143	0.159
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.025	0.048	**-0.189**	0.048	**-0.145**	0.043	0.016	0.048
November 2000	0.096	0.054	**-0.165**	0.056	**-0.261**	0.051	**-0.238**	0.052
January 2001	**-0.240**	0.073	**-0.195**	0.067	**-0.302**	0.063	**-0.569**	0.075
March 2001	0.080	0.056	0.018	0.052	0.089	0.048	0.024	0.054
May 2001	**0.150**	0.051	**0.133**	0.046	-0.053	0.044	-0.031	0.049
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	**-0.171**	0.042	0.077	0.042	0.075	0.041	0.016	0.043

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Table A.12: (continued)

	$u = 5$		$u = 6$		$u = 7$		$u = 8$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster Ic	**-0.231**	0.058	-0.014	0.056	0.043	0.054	-0.076	0.059
Cluster II	*-0.327*	0.128	*-0.300*	0.132	-0.077	0.111	**-0.380**	0.129
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.064	0.063	0.041	0.054	0.071	0.050	-0.089	0.056
Not applicable	**0.122**	0.036	**0.174**	0.034	-0.055	0.033	0.005	0.036
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	*-0.342*	0.156	-0.146	0.120	*-0.248*	0.124	-0.080	0.128
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	0.545	0.446	0.023	0.417	-0.122	0.383	0.060	0.611
Manufacturing	-0.106	0.062	-0.102	0.058	-0.074	0.058	0.026	0.063
Technical professions	-0.210	0.112	**-0.305**	0.105	**-0.322**	0.101	*-0.288*	0.115
Service professions	**-0.170**	0.061	**-0.161**	0.057	*-0.142*	0.057	-0.080	0.063
Other occupations	**-0.395**	0.150	**-0.615**	0.170	**-0.570**	0.183	**-0.640**	0.199
N	24,594		25,593		21,596		18,545	
Log-Likelihood	-3,498.06		-4,115.42		-4,491.39		-3,771.74	
R^2	0.122		0.119		0.125		0.119	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.13: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 9$ to $u = 12$ (East Germany)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.442**	0.165	**-1.357**	0.173	**-1.665**	0.177	**-1.777**	0.170
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.094	0.087	-0.055	0.092	0.086	0.097	0.115	0.095
35 to 39 years	0.083	0.111	0.052	0.117	0.036	0.122	0.059	0.118
40 to 44 years	0.152	0.152	0.048	0.160	0.041	0.163	0.064	0.156
45 to 49 years	0.185	0.204	0.181	0.215	-0.072	0.219	-0.190	0.210
50 to 55 years	0.342	0.264	0.339	0.280	0.103	0.283	-0.086	0.272
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	*0.000*	0.000
Foreigner	*-0.612*	0.239	*-0.603*	0.271	-0.261	0.205	-0.317	0.194
Asylum seeker	**-0.525**	0.194	-0.376	0.201	-0.331	0.202	**-0.536**	0.184
Woman	**-0.127**	0.039	-0.077	0.042	-0.074	0.042	-0.044	0.040
No. of placement propositions	**0.028**	0.003	**0.027**	0.003	**0.031**	0.003	**0.035**	0.003

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Table A.13: (continued)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
No. of children	0.002	0.019	0.001	0.021	0.035	0.020	-0.023	0.020
Placement restrictions	-0.041	0.082	-0.151	0.086	**-0.408**	0.090	0.031	0.086
Vocational rehabilitation[1]	0.164	0.097	*0.211*	0.099	**0.442**	0.102	*0.236*	0.096
Health restrictions	0.057	0.062	0.121	0.064	**0.199**	0.063	0.004	0.066
Marriage/Cohabitation	0.064	0.040	-0.028	0.042	-0.032	0.042	-0.015	0.040
Work experience	0.072	0.063	0.038	0.064	-0.068	0.062	0.098	0.062
Programme bef. unemp.[2]	**0.164**	0.038	**0.151**	0.041	**0.106**	0.041	**0.170**	0.039
Reception of UI	**-0.677**	0.040	**-0.566**	0.042	**-0.519**	0.043	**-0.583**	0.040
Duration of Last Employment								
Up to 180 days	**0.323**	0.046	**0.201**	0.049	**0.315**	0.050	**0.589**	0.048
Between 180 and 365 days	**0.197**	0.056	**0.203**	0.058	**0.164**	0.059	**0.233**	0.059
1 to 2 years	0.059	0.049	0.015	0.053	0.024	0.054	**0.188**	0.052
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	–	–	0.198	0.445	-0.290	0.494
Permanently unable to work	0.078	0.295	0.121	0.330	–	–	-0.003	0.287
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.001	0.068	0.027	0.074	-0.050	0.073	-0.033	0.073
O-levels	-0.002	0.071	-0.004	0.077	-0.068	0.076	-0.034	0.075
Adv. technical college entrance[4]	-0.201	0.197	0.025	0.193	-0.414	0.226	-0.251	0.183
A-levels	-0.186	0.138	*-0.371*	0.163	-0.114	0.139	-0.043	0.128
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	-0.008	0.054	0.064	0.057	*0.126*	0.059	0.083	0.056
Apprenticeship (off-the-job)	0.167	0.207	0.339	0.198	0.219	0.239	-0.265	0.276
Full-time vocational school	-0.092	0.168	-0.013	0.182	0.237	0.179	0.169	0.159
Technical school	0.107	0.103	0.041	0.116	0.050	0.116	**0.428**	0.100
Advanced technical college	0.270	0.239	0.109	0.252	**0.638**	0.247	**0.774**	0.212
University	0.164	0.187	0.407	0.219	-0.204	0.202	0.157	0.188
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	0.008	0.040	-0.015	0.042	-0.005	0.042	0.030	0.041
Ass. to technical school[5]	-0.026	0.118	0.137	0.129	*0.323*	0.126	-0.114	0.112
Ass. to adv. technical college	-0.115	0.170	0.043	0.176	0.028	0.174	**-0.464**	0.170
Ass. to university	-0.020	0.166	0.095	0.188	*0.414*	0.175	0.030	0.167
Ass. to top-management	–	–	–	–	0.699	0.776	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	*0.099*	0.050	-0.046	0.053	-0.026	0.056	*-0.123*	0.053
November 2000	-0.062	0.059	-0.060	0.062	-0.029	0.060	-0.005	0.057
January 2001	**-0.361**	0.074	**-0.459**	0.074	**-0.475**	0.082	**-0.461**	0.078
March 2001	*0.124*	0.058	-0.033	0.059	0.050	0.060	**-0.175**	0.057
May 2001	*0.121*	0.056	0.095	0.056	0.086	0.059	**-0.154**	0.057
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	0.035	0.046	-0.050	0.046	-0.015	0.046	0.063	0.046
Cluster Ic	-0.043	0.063	*-0.135*	0.066	*-0.141*	0.067	-0.062	0.066
Cluster II	-0.138	0.128	-0.252	0.142	**-0.424**	0.160	-0.036	0.120
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.049	0.059	**0.251**	0.058	0.094	0.060	0.027	0.057

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Table A.13: (continued)

	$u = 9$		$u = 10$		$u = 11$		$u = 12$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Not applicable	0.003	0.038	0.049	0.041	0.000	0.041	-0.043	0.039
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.150	0.137	-0.145	0.135	**-0.495**	0.179	-0.283	0.149
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	-0.194	0.475	0.212	0.440	–	–	0.201	0.440
Manufacturing	*-0.148*	0.063	-0.084	0.067	-0.011	0.069	*-0.150*	0.067
Technical professions	*-0.243*	0.115	-0.212	0.119	-0.194	0.121	-0.124	0.111
Service professions	**-0.172**	0.062	**-0.181**	0.066	*-0.147*	0.068	*-0.165*	0.066
Other occupations	**-0.633**	0.202	**-0.713**	0.236	**-1.227**	0.418	**-0.766**	0.213
N	16,466		14,056		13,171		12,372	
Log-Likelihood	-3,356.18		-2,989.06		-2,958.79		-3,344.25	
R^2	0.119		0.110		0.116		0.162	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.14: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 13$ to $u = 16$ (East Germany)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.583**	0.163	**-0.529**	0.204	**-0.577**	0.207	**-0.976**	0.237
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	0.039	0.093	0.182	0.112	-0.072	0.111	-0.037	0.128
35 to 39 years	0.070	0.113	0.274	0.142	-0.046	0.141	0.049	0.157
40 to 44 years	0.065	0.150	*0.402*	0.190	-0.256	0.192	-0.065	0.212
45 to 49 years	0.004	0.199	*0.572*	0.255	-0.175	0.258	-0.017	0.282
50 to 55 years	0.150	0.256	*0.790*	0.329	-0.077	0.334	0.048	0.362
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	-0.228	0.176	-0.461	0.262	-0.331	0.223	-0.009	0.229
Asylum seeker	**-0.655**	0.188	*-0.656*	0.259	-0.380	0.212	**-1.038**	0.366
Woman	-0.027	0.039	0.042	0.049	0.017	0.050	0.013	0.055
No. of placement propositions	**0.034**	0.003	**0.052**	0.004	**0.050**	0.004	**0.060**	0.005
No. of children	0.022	0.018	0.012	0.024	0.000	0.024	0.051	0.027
Placement restrictions	-0.125	0.083	-0.011	0.103	-0.198	0.111	-0.051	0.119
Vocational rehabilitation[1]	**0.466**	0.091	*0.234*	0.119	**0.558**	0.128	**0.585**	0.132
Health restrictions	0.072	0.062	0.035	0.080	0.124	0.079	-0.008	0.089
Marriage/Cohabitation	0.069	0.039	0.012	0.049	-0.046	0.049	-0.026	0.054
Work experience	*0.130*	0.061	0.061	0.076	0.061	0.075	*0.181*	0.089
Programme bef. unemp.[2]	**0.261**	0.038	**0.251**	0.047	**0.180**	0.048	*0.137*	0.053
Reception of UI	**-0.775**	0.038	**-1.101**	0.052	**-1.082**	0.054	**-0.950**	0.060

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Table A.14: (continued)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Duration of Last Employment								
Up to 180 days	**0.595**	0.046	**0.356**	0.061	**0.325**	0.063	**0.495**	0.067
Between 180 and 365 days	**0.235**	0.054	0.007	0.065	**0.211**	0.066	-0.004	0.072
1 to 2 years	**0.148**	0.051	-0.004	0.065	0.124	0.067	0.092	0.075
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	-0.206	0.502	0.176	0.624	0.795	0.517	-0.126	0.829
Permanently unable to work	-0.408	0.332	-0.724	0.594	–	–	0.352	0.336
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.112	0.070	-0.074	0.081	-0.074	0.081	-0.059	0.090
O-levels	0.113	0.073	-0.111	0.084	-0.083	0.085	-0.116	0.094
Adv. technical college entrance[4]	-0.058	0.173	-0.034	0.236	-0.261	0.249	0.051	0.266
A-levels	0.068	0.126	0.011	0.161	-0.146	0.166	*-0.473*	0.225
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	**0.143**	0.055	**0.208**	0.065	0.033	0.065	*0.158*	0.071
Apprenticeship (off-the-job)	0.324	0.195	0.378	0.253	0.310	0.247	-0.718	0.466
Full-time vocational school	0.070	0.166	0.382	0.211	-0.235	0.244	-0.386	0.288
Technical school	**0.422**	0.098	0.110	0.143	0.198	0.132	-0.023	0.162
Advanced technical college	0.232	0.216	-0.077	0.329	-0.192	0.344	-0.119	0.375
University	0.200	0.177	0.027	0.259	-0.025	0.258	-0.269	0.317
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	-0.014	0.039	**-0.143**	0.048	-0.049	0.049	0.003	0.054
Ass. to technical school[5]	-0.128	0.117	0.086	0.163	0.044	0.149	0.214	0.189
Ass. to adv. technical college	-0.029	0.145	-0.031	0.224	0.110	0.232	0.373	0.226
Ass. to university	0.019	0.163	-0.378	0.246	0.150	0.226	*0.657*	0.256
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	**-0.153**	0.051	-0.022	0.064	0.031	0.064	0.032	0.069
November 2000	-0.098	0.056	-0.090	0.072	-0.004	0.073	-0.013	0.083
January 2001	**-0.268**	0.066	**-0.374**	0.087	**-0.345**	0.093	**-0.272**	0.100
March 2001	0.086	0.054	0.074	0.071	-0.055	0.070	0.004	0.079
May 2001	-0.009	0.052	0.026	0.065	0.009	0.068	0.095	0.075
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	-0.067	0.042	**-0.239**	0.052	**-0.177**	0.054	*-0.134*	0.060
Cluster Ic	**-0.245**	0.061	**-0.298**	0.072	**-0.238**	0.074	**-0.253**	0.085
Cluster II	-0.062	0.113	**-0.591**	0.185	**-0.511**	0.161	-0.146	0.149
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.083	0.051	*0.166*	0.064	0.101	0.064	**0.214**	0.076
Not applicable	**-0.168**	0.038	**-0.140**	0.049	**-0.182**	0.051	**-0.157**	0.055
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	*-0.316*	0.132	**-0.723**	0.199	*-0.420*	0.177	**-0.604**	0.202
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	-0.762	0.572	–	–	-0.350	0.691	–	–

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Table A.14: (continued)

	$u = 13$		$u = 14$		$u = 15$		$u = 16$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Manufacturing	-0.080	0.064	-0.108	0.080	**-0.223**	0.076	**-0.315**	0.084
Technical professions	0.037	0.103	-0.248	0.153	**-0.402**	0.156	*-0.395*	0.163
Service professions	-0.055	0.062	-0.120	0.078	*-0.181*	0.075	**-0.381**	0.084
Other occupations	*-0.415*	0.196	*-0.579*	0.226	**-1.008**	0.328	**-0.906**	0.291
N	11,219		7,992		7,772		6,501	
Log-Likelihood	-3,797.26		-2,301.36		-2,258.16		-1,839.82	
R^2	0.196		0.216		0.195		0.202	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.15: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 17$ to $u = 20$ (East Germany)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-0.789**	0.233	*-0.572*	0.253	**-1.061**	0.270	**-0.995**	0.308
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	-0.229	0.135	0.075	0.146	0.083	0.151	-0.011	0.165
35 to 39 years	-0.156	0.165	0.152	0.178	-0.110	0.190	-0.095	0.203
40 to 44 years	-0.198	0.218	0.068	0.237	-0.149	0.253	-0.281	0.277
45 to 49 years	-0.277	0.291	0.284	0.313	-0.045	0.338	-0.154	0.369
50 to 55 years	-0.249	0.375	0.378	0.403	0.072	0.437	-0.161	0.476
Age (squared)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	-0.308	0.274	-0.100	0.280	-0.796	0.435	-0.131	0.319
Asylum seeker	*-0.602*	0.258	-0.471	0.251	–	–	-0.544	0.382
Woman	0.047	0.057	0.063	0.062	0.050	0.067	0.089	0.071
No. of placement propositions	**0.059**	0.005	**0.057**	0.005	**0.073**	0.005	**0.063**	0.006
No. of children	0.032	0.027	0.013	0.029	0.008	0.032	0.023	0.034
Placement restrictions	0.106	0.122	-0.102	0.133	-0.021	0.161	0.008	0.149
Vocational rehabilitation[1]	0.241	0.149	*0.350*	0.151	**0.647**	0.170	**0.550**	0.168
Health restrictions	-0.059	0.094	0.047	0.106	-0.236	0.123	0.078	0.119
Marriage/Cohabitation	0.077	0.057	0.023	0.062	0.025	0.065	0.008	0.071
Work experience	-0.106	0.081	-0.025	0.089	0.053	0.098	0.135	0.114
Programme bef. unemp.[2]	*0.125*	0.055	**0.188**	0.059	**0.237**	0.063	*0.140*	0.069
Reception of UI	**-1.033**	0.059	**-1.032**	0.066	**-0.975**	0.067	**-0.895**	0.077

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Table A.15: (continued)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Duration of Last Employment								
Up to 180 days	**0.210**	0.071	**0.316**	0.075	**0.492**	0.079	**0.269**	0.088
Between 180 and 365 days	0.038	0.074	-0.009	0.081	0.136	0.086	0.064	0.094
1 to 2 years	0.027	0.077	-0.026	0.087	-0.017	0.097	0.002	0.098
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	0.285	0.836	-0.090	0.793	-0.062	0.765	–	–
Permanently unable to work	0.002	0.418	0.085	0.445	0.095	0.629	-0.291	0.539
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	0.046	0.092	-0.060	0.096	-0.138	0.105	-0.171	0.110
O-levels	0.043	0.097	-0.015	0.103	0.081	0.110	-0.200	0.117
Adv. technical college entrance[4]	*0.561*	0.258	-0.093	0.330	-0.466	0.423	-0.851	0.467
A-levels	-0.123	0.210	*0.402*	0.186	0.079	0.239	-0.212	0.244
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	0.066	0.075	-0.083	0.079	-0.069	0.085	0.048	0.091
Apprenticeship (off-the-job)	–	–	*0.606*	0.303	0.142	0.376	0.159	0.385
Full-time vocational school	0.251	0.251	-0.089	0.273	-0.762	0.392	-0.722	0.455
Technical school	0.056	0.162	-0.141	0.186	-0.006	0.182	0.033	0.194
Advanced technical college	-0.460	0.386	-0.314	0.442	0.375	0.487	*0.985*	0.464
University	0.191	0.312	*-0.670*	0.331	-0.718	0.380	**0.945**	0.362
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	-0.053	0.055	-0.061	0.062	-0.029	0.064	0.039	0.070
Ass. to technical school[5]	0.101	0.184	-0.265	0.222	-0.166	0.228	0.078	0.225
Ass. to adv. technical college	0.422	0.239	-0.304	0.313	-0.081	0.340	-0.138	0.339
Ass. to university	*-0.727*	0.297	-0.009	0.311	-0.069	0.344	**-1.188**	0.379
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	0.066	0.077	-0.085	0.084	0.115	0.086	-0.072	0.099
November 2000	0.049	0.084	-0.087	0.090	0.090	0.094	0.118	0.102
January 2001	**-0.312**	0.111	**-0.375**	0.117	-0.106	0.108	**-0.323**	0.124
March 2001	*0.173*	0.085	-0.004	0.093	0.131	0.093	*0.236*	0.101
May 2001	0.032	0.077	-0.009	0.085	0.073	0.090	0.096	0.096
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	**-0.205**	0.061	**-0.278**	0.067	**-0.192**	0.070	**-0.204**	0.077
Cluster Ic	**-0.336**	0.088	**-0.355**	0.093	**-0.394**	0.105	**-0.313**	0.108
Cluster II	*-0.452*	0.181	**-0.618**	0.198	-0.112	0.193	*-0.685*	0.269
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.131	0.074	*0.176*	0.085	*0.216*	0.086	-0.058	0.099
Not applicable	**-0.151**	0.058	*-0.158*	0.063	-0.054	0.068	*-0.165*	0.071
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	*-0.501*	0.212	*-0.551*	0.223	-0.266	0.179	-0.317	0.207
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	0.852	0.599	–	–	-0.228	0.598	–	–

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Table A.15: (continued)

	$u = 17$		$u = 18$		$u = 19$		$u = 20$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Manufacturing	*-0.191*	0.085	0.007	0.104	-0.170	0.103	-0.118	0.115
Technical professions	-0.324	0.166	-0.277	0.193	0.072	0.198	-0.090	0.197
Service professions	**-0.333**	0.085	-0.143	0.103	-0.155	0.099	-0.165	0.113
Other occupations	**-1.214**	0.428	*-1.162*	0.454	*-1.207*	0.590	*-1.053*	0.434
N	6,092		5,305		4,939		4,309	
Log-Likelihood	-1,732.20		-1,468.08		-1,294.15		-1,099.37	
R^2	0.195		0.203		0.234		0.177	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.16: Estimation Results of the Probit-Models for the Propensity Scores for Treatment Starting in Month $u = 21$ to $u = 24$ (East Germany)

	$u = 21$		$u = 22$		$u = 23$		$u = 24$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-1.013**	0.299	**-1.349**	0.337	*-0.860*	0.337	*-0.864*	0.373
Age								
25 to 29 years	*Reference*		*Reference*		*Reference*		*Reference*	
30 to 34 years	-0.168	0.175	-0.001	0.189	0.325	0.194	-0.085	0.202
35 to 39 years	*-0.433*	0.215	-0.207	0.232	0.063	0.236	-0.067	0.254
40 to 44 years	*-0.632*	0.284	0.072	0.305	0.212	0.310	0.103	0.338
45 to 49 years	-0.664	0.376	-0.061	0.404	0.328	0.412	0.289	0.449
50 to 55 years	*-0.952*	0.483	0.124	0.519	0.339	0.526	0.766	0.586
Age (squared)	*0.001*	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Foreigner	-0.522	0.459	-0.755	0.512	0.062	0.289	-0.448	0.412
Asylum seeker	**-1.221**	0.446	-0.176	0.317	-0.422	0.328	-0.520	0.464
Woman	0.035	0.072	0.014	0.078	-0.042	0.079	0.081	0.090
No. of placement propositions	**0.070**	0.006	**0.059**	0.006	**0.072**	0.007	**0.077**	0.007
No. of children	0.032	0.034	0.014	0.035	0.050	0.036	0.059	0.041
Placement restrictions	-0.124	0.160	-0.266	0.157	0.082	0.178	0.186	0.213
Vocational rehabilitation[1]	0.123	0.203	**0.581**	0.186	**0.669**	0.205	*0.542*	0.213
Health restrictions	0.101	0.113	**0.311**	0.113	-0.122	0.140	-0.303	0.175
Marriage/Cohabitation	0.053	0.072	*0.195*	0.081	*-0.160*	0.079	0.017	0.088
Work experience	-0.157	0.098	0.238	0.127	0.130	0.121	0.167	0.138
Programme bef. unemp.[2]	**0.218**	0.068	**0.202**	0.076	0.017	0.077	0.117	0.086
Reception of UI	**-0.946**	0.076	**-0.845**	0.081	**-0.927**	0.082	**-0.824**	0.094

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Table A.16: (continued)

	$u = 21$		$u = 22$		$u = 23$		$u = 24$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Duration of Last Employment								
Up to 180 days	**0.521**	0.086	**0.438**	0.096	**0.313**	0.096	**0.518**	0.109
Between 180 and 365 days	0.028	0.094	0.076	0.105	0.157	0.102	0.178	0.117
1 to 2 years	*0.206*	0.097	0.037	0.114	-0.029	0.116	-0.013	0.134
More than 2 years	*Reference*		*Reference*		*Reference*		*Reference*	
Pension								
No pension	*Reference*		*Reference*		*Reference*		*Reference*	
Vocational disability	–	–	–	–	–	–	–	–
Permanently unable to work	**2.138**	0.775	0.171	0.444	–	–	–	–
Social plan	–	–	–	–	–	–	–	–
Schooling[3]								
No school	*Reference*		*Reference*		*Reference*		*Reference*	
CSE	-0.049	0.112	0.150	0.127	-0.123	0.122	-0.019	0.144
O-levels	-0.198	0.118	-0.050	0.137	-0.165	0.132	-0.097	0.154
Adv. technical college entrance[4]	-0.733	0.469	-0.396	0.544	*-1.057*	0.490	-0.788	0.522
A-levels	-0.544	0.305	-0.061	0.290	-0.123	0.251	-0.221	0.343
Professional Training								
Without compl. prof. training	*Reference*		*Reference*		*Reference*		*Reference*	
Apprenticeship (on-the-job)	0.122	0.093	0.055	0.101	-0.043	0.101	0.196	0.112
Apprenticeship (off-the-job)	0.489	0.379	0.032	0.514	0.270	0.437	–	–
Full-time vocational school	-0.210	0.312	-0.020	0.315	-0.581	0.488	0.051	0.379
Technical school	-0.081	0.217	0.083	0.219	-0.096	0.221	0.331	0.253
Advanced technical college	0.503	0.541	0.581	0.615	*1.155*	0.527	0.670	0.578
University	0.255	0.420	0.338	0.395	-0.217	0.435	-0.107	0.614
Assessment of Individual's Qualification								
Other	*Reference*		*Reference*		*Reference*		*Reference*	
Unskilled employee	–	–	–	–	–	–	–	–
Skilled employee	-0.053	0.069	-0.042	0.078	0.061	0.078	-0.118	0.085
Ass. to technical school[5]	0.061	0.233	-0.008	0.277	-0.211	0.309	-0.537	0.376
Ass. to adv. technical college	-0.401	0.467	-0.180	0.364	-0.220	0.358	-0.131	0.408
Ass. to university	0.319	0.346	-0.283	0.413	-0.225	0.406	-0.182	0.508
Ass. to top-management	–	–	–	–	–	–	–	–
Month of Treatment Start								
July 2000	*Reference*		*Reference*		*Reference*		*Reference*	
September 2000	-0.022	0.097	0.078	0.109	0.019	0.120	-0.029	0.140
November 2000	-0.059	0.112	-0.001	0.128	0.030	0.122	0.173	0.141
January 2001	*-0.319*	0.132	-0.095	0.138	*-0.403*	0.165	*-0.357*	0.178
March 2001	-0.022	0.106	0.058	0.115	0.143	0.122	*0.277*	0.132
May 2001	0.027	0.104	-0.082	0.118	0.068	0.117	0.159	0.130
Regional Context Variables								
Cluster Ia	*Reference*		*Reference*		*Reference*		*Reference*	
Cluster Ib	**-0.378**	0.076	-0.166	0.085	-0.166	0.088	**-0.400**	0.091
Cluster Ic	**-0.365**	0.106	**-0.382**	0.123	**-0.335**	0.124	**-0.550**	0.137
Cluster II	-0.375	0.214	*-0.578*	0.275	-0.448	0.244	*-0.632*	0.303
Cluster III	–	–	–	–	–	–	–	–
Cluster IV	–	–	–	–	–	–	–	–
Cluster V	–	–	–	–	–	–	–	–
Work Time (Last Occupation)								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	0.131	0.097	0.203	0.105	-0.028	0.115	-0.095	0.126
Not applicable	-0.005	0.073	-0.064	0.080	*-0.203*	0.079	-0.170	0.090
Desired Work Time								
Full-time work	*Reference*		*Reference*		*Reference*		*Reference*	
Part-time work	-0.398	0.225	-0.343	0.235	*-0.847*	0.401	-0.504	0.288
Other (e.g., telework)	–	–	–	–	–	–	–	–
Desired Occupation								
Farming[6]	*Reference*		*Reference*		*Reference*		*Reference*	
Mining, mineral extraction	0.301	0.666	–	–	-0.307	0.632	–	–

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Table A.16: (continued)

	$u = 21$		$u = 22$		$u = 23$		$u = 24$	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Manufacturing	-0.147	0.112	-0.128	0.126	-0.186	0.124	-0.087	0.138
Technical professions	-0.278	0.229	-0.292	0.258	-0.258	0.281	-0.584	0.351
Service professions	-0.179	0.111	-0.103	0.121	-0.204	0.122	-0.101	0.136
Other occupations	*-1.843*	0.782	–	–	-0.864	0.489	-0.219	0.389
N	4,332		3,548		3,521		2,981	
Log-Likelihood	-1,079.16		-882.81		-875.41		-693.19	
R^2	0.220		0.206		0.197		0.215	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Attendant for vocational rehabilitation.
[2] Similar programme before unemployment, e.g., job creation or structural adjustment scheme.
[3] Schooling: CSE = Certificate of Secondary Education.
[4] Advanced technical college entrance qualification (*Fachhochschulreife*).
[5] Ass. = assimilable.
[6] Farming comprises plant cultivation, breeding and fishery.

Table A.17: Cumulated Exit Rates for West Germany by Month of Treatment Start for $u = 1, \ldots, 12$ (cumulated frequencies)

Month of Exit	$u=1$	$u=2$	$u=3$	$u=4$	$u=5$	$u=6$	$u=7$	$u=8$	$u=9$	$u=10$	$u=11$	$u=12$
1	2.87	2.37	1.69	2.22	1.13	1.54	1.91	0.54	2.72	3.39	5.62	1.68
2	5.30	5.93	3.16	4.20	3.95	3.86	4.30	1.36	5.74	11.86	9.74	3.78
3	8.74	9.09	6.75	6.17	8.19	6.94	7.64	6.25	11.48	16.61	11.61	5.67
4	12.75	13.44	10.55	9.38	12.43	9.77	14.80	15.76	17.52	20.68	14.23	7.35
5	20.20	19.76	17.51	18.77	20.34	20.82	27.45	22.83	22.96	28.14	19.85	11.55
6	25.64	25.10	23.42	28.40	26.27	29.31	36.04	27.45	28.40	32.88	25.09	13.87
7	31.52	28.66	25.53	33.33	32.49	33.68	38.42	30.16	31.42	35.93	27.72	17.86
8	35.67	32.21	32.28	40.25	36.72	38.56	41.29	33.42	34.74	39.32	30.71	20.17
9	39.54	37.55	37.76	45.68	40.40	41.65	45.58	37.77	39.58	43.39	34.08	24.79
10	43.84	46.05	42.41	49.14	43.50	43.70	48.69	41.85	43.50	45.08	36.70	26.68
11	79.51	73.72	75.95	78.77	75.99	76.86	75.89	79.08	77.04	74.58	76.40	80.25
12	**93.41**	**93.68**	**94.09**	**94.07**	**92.94**	**92.54**	**95.70**	**96.47**	**95.47**	**93.90**	**92.51**	**93.28**
13	93.70	93.87	94.30	94.57	93.22	93.06	95.94	96.47	95.47	93.90	93.63	93.49
14	94.13	94.47	94.30	94.57	93.79	94.34	95.94	96.47	96.07	94.92	94.01	93.49
15	94.41	94.47	94.51	94.57	93.79	94.34	96.18	97.01	96.07	94.92	94.38	93.49
16	94.41	94.47	94.73	94.57	93.79	94.60	96.18	97.01	96.37	94.92	94.38	93.49
17	94.56	94.47	94.73	94.57	94.92	94.60	96.18	97.01	96.37	94.92	94.38	93.49
18	94.84	94.66	94.73	94.57	95.20	94.60	96.66	97.01	96.37	95.25	94.38	93.49
19	95.13	94.66	94.73	94.57	95.20	94.60	96.66	97.01	96.68	95.25	94.38	93.49
20	95.42	94.66	94.73	94.81	95.20	94.60	96.66	97.01	96.98	95.25	94.38	93.49
21	95.42	94.86	94.73	94.81	95.20	94.60	96.66	97.55	96.98	95.25	94.38	93.49
22	95.70	95.26	94.73	95.06	95.20	94.60	96.66	97.55	96.98	95.25	94.38	93.49
23	96.56	96.05	95.57	95.80	96.05	97.43	97.37	98.37	98.19	95.93	95.13	94.33
24	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
25	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
26	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
27	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
28	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
29	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33
30	97.56	98.62	97.26	98.52	96.61	98.46	98.33	98.37	99.40	96.27	95.51	94.33

u denotes the month(s) spent in open unemployment until treatment start.

Table A.18: Cumulated Exit Rates for West Germany by Month of Treatment Start for $u = 13, \ldots, 24$ (cumulated frequencies)

Month of Exit	$u = 13$	$u = 14$	$u = 15$	$u = 16$	$u = 17$	$u = 18$	$u = 19$	$u = 20$	$u = 21$	$u = 22$	$u = 23$	$u = 24$
1	1.31	1.75	2.53	1.53	2.68	0.78	1.32	0.00	0.00	1.05	6.67	3.13
2	2.96	4.39	5.05	4.58	3.36	3.13	3.31	5.26	1.83	6.32	8.57	4.17
3	6.40	7.02	9.09	6.11	6.04	4.69	5.30	7.37	8.26	9.47	10.48	5.21
4	7.72	11.84	14.14	9.16	8.72	9.38	10.60	11.58	14.68	11.58	13.33	5.21
5	11.17	21.05	17.17	18.32	14.77	17.97	15.89	20.00	20.18	22.11	20.95	12.50
6	13.96	25.88	24.24	21.37	21.48	23.44	27.81	27.37	25.69	26.32	24.76	14.58
7	15.93	28.51	28.28	24.43	27.52	28.91	30.46	31.58	27.52	30.53	28.57	18.75
8	18.56	31.58	30.81	31.30	38.93	32.81	35.10	33.68	35.78	33.68	29.52	23.96
9	20.36	35.09	36.36	35.88	40.94	34.38	38.41	40.00	41.28	40.00	33.33	31.25
10	21.67	44.30	41.41	37.40	42.95	38.28	43.05	43.16	45.87	43.16	39.05	31.25
11	65.68	78.51	74.75	75.57	79.19	77.34	77.48	72.63	81.65	84.21	79.05	87.50
12	**96.72**	**94.30**	**90.91**	**94.66**	**96.64**	**91.41**	**93.38**	**89.47**	**98.17**	**95.79**	**95.24**	**96.88**
13	96.72	94.74	91.41	95.42	96.64	92.19	93.38	90.53	98.17	95.79	95.24	96.88
14	97.04	96.05	92.42	95.42	96.64	92.19	93.38	92.63	98.17	95.79	95.24	96.88
15	97.04	96.05	92.42	95.42	96.64	92.19	93.38	92.63	98.17	95.79	95.24	96.88
16	97.04	96.05	92.42	95.42	96.64	92.97	94.04	92.63	98.17	95.79	95.24	96.88
17	97.37	96.49	92.93	95.42	97.99	93.75	94.04	93.68	98.17	95.79	97.14	96.88
18	97.37	96.49	92.93	95.42	97.99	93.75	94.70	93.68	98.17	96.84	97.14	96.88
19	97.37	96.49	92.93	95.42	97.99	94.53	94.70	93.68	98.17	96.84	97.14	96.88
20	97.37	96.93	92.93	95.42	97.99	94.53	94.70	93.68	98.17	96.84	97.14	96.88
21	97.37	96.93	92.93	95.42	98.66	94.53	94.70	93.68	98.17	96.84	97.14	96.88
22	97.37	96.93	92.93	95.42	98.66	94.53	94.70	93.68	98.17	96.84	97.14	96.88
23	98.03	98.68	94.95	96.95	99.33	94.53	96.69	97.89	100.00	97.89	98.10	96.88
24	99.01	99.56	95.45	96.95	100.00	94.53	98.68	97.89	100.00	98.95	98.10	98.96
25	99.01	99.56	95.45	96.95	100.00	94.53	98.68	97.89	100.00	98.95	98.10	98.96
26	99.01	99.56	95.45	96.95	100.00	94.53	98.68	97.89	100.00	98.95	98.10	98.96
27	99.01	99.56	95.45	96.95	100.00	94.53	98.68	97.89	100.00	98.95	98.10	98.96
28	99.01	99.56	95.45	96.95	100.00	94.53	98.68	97.89	100.00	98.95	98.10	98.96
29	99.01	99.56	95.45	96.95	100.00	95.31	98.68	97.89	100.00	98.95	98.10	98.96
30	99.01	99.56	95.45	96.95	100.00	95.31	98.68	97.89	100.00	98.95	98.10	98.96

u denotes the month(s) spent in open unemployment until treatment start.

Table A.19: Cumulated Exit Rates for East Germany by Month of Treatment Start for $u = 1, \ldots, 12$ (cumulated frequencies)

Month of Exit	$u=1$	$u=2$	$u=3$	$u=4$	$u=5$	$u=6$	$u=7$	$u=8$	$u=9$	$u=10$	$u=11$	$u=12$
1	2.01	1.00	1.39	1.20	1.49	1.14	1.23	1.32	1.38	0.77	2.27	1.15
2	6.42	5.65	4.82	4.42	6.50	5.34	4.99	5.61	6.02	6.73	6.28	6.38
3	11.26	12.62	8.82	9.22	10.77	11.02	9.04	11.14	11.83	11.91	9.85	9.17
4	15.67	16.53	12.41	13.27	14.93	14.61	13.97	15.88	17.46	17.75	13.42	14.73
5	25.11	27.57	20.24	21.57	23.67	25.20	23.52	24.12	25.64	25.47	21.75	20.79
6	33.17	33.97	28.98	27.47	29.85	34.21	31.91	29.47	32.35	32.19	29.55	25.04
7	36.06	36.71	33.14	29.95	34.12	37.27	35.17	32.89	35.31	36.16	32.25	27.82
8	39.96	40.03	40.90	34.93	38.70	41.56	39.36	36.75	39.15	41.35	36.47	31.42
9	42.48	42.69	43.10	37.79	41.68	44.18	42.33	40.00	41.32	43.99	38.74	33.88
10	44.18	44.77	44.49	39.45	43.18	45.41	44.07	40.96	42.60	46.20	39.94	35.02
11	85.15	77.82	82.20	80.55	80.49	83.38	82.85	82.02	79.19	82.69	80.09	85.52
12	**95.22**	**94.02**	**94.78**	**95.21**	**95.74**	**94.84**	**95.08**	**94.91**	**95.27**	**95.04**	**93.94**	**95.42**
13	95.34	94.52	94.86	95.30	95.84	94.93	95.30	95.70	95.86	95.04	94.05	95.58
14	96.85	95.68	96.41	96.50	97.23	96.68	96.60	96.93	97.24	96.47	95.89	96.56
15	97.36	96.18	96.82	97.05	97.65	97.29	97.11	97.19	97.83	97.24	96.21	97.38
16	97.67	96.93	97.39	97.51	98.08	97.81	97.90	98.33	98.32	97.91	96.97	97.87
17	97.80	97.18	97.47	97.60	98.08	97.90	97.90	98.42	98.32	98.13	96.97	97.95
18	97.80	97.26	97.55	97.88	98.29	98.16	98.05	98.60	98.32	98.35	97.29	98.12
19	97.86	97.51	97.71	98.06	98.40	98.16	98.05	98.68	98.42	98.57	97.40	98.28
20	97.86	97.59	97.71	98.06	98.40	98.34	98.05	98.77	98.52	98.57	97.40	98.45
21	97.86	97.67	97.88	98.06	98.51	98.43	98.12	98.77	98.52	98.57	97.73	98.61
22	97.86	97.67	97.96	98.06	98.51	98.43	98.19	98.77	98.62	98.57	97.73	98.61
23	98.11	97.92	98.29	98.06	98.61	98.60	98.41	98.77	98.72	99.01	98.16	98.61
24	98.55	98.26	98.69	98.62	98.93	99.04	98.63	98.77	98.82	99.12	98.27	98.61
25	98.55	98.26	98.69	98.62	98.93	99.13	98.63	98.77	98.82	99.12	98.27	98.61
26	98.55	98.26	98.69	98.62	99.04	99.13	98.63	98.77	98.82	99.23	98.27	98.61
27	98.55	98.26	98.69	98.62	99.04	99.13	98.63	98.77	98.82	99.23	98.27	98.61
28	98.55	98.26	98.69	98.62	99.04	99.21	98.63	98.77	99.11	99.23	98.27	98.69
29	98.55	98.26	98.69	98.62	99.04	99.21	98.63	98.77	99.11	99.23	98.27	98.69
30	98.55	98.26	98.69	98.62	99.04	99.21	98.63	98.77	99.11	99.23	98.27	98.69

u denotes the month(s) spent in open unemployment until treatment start.

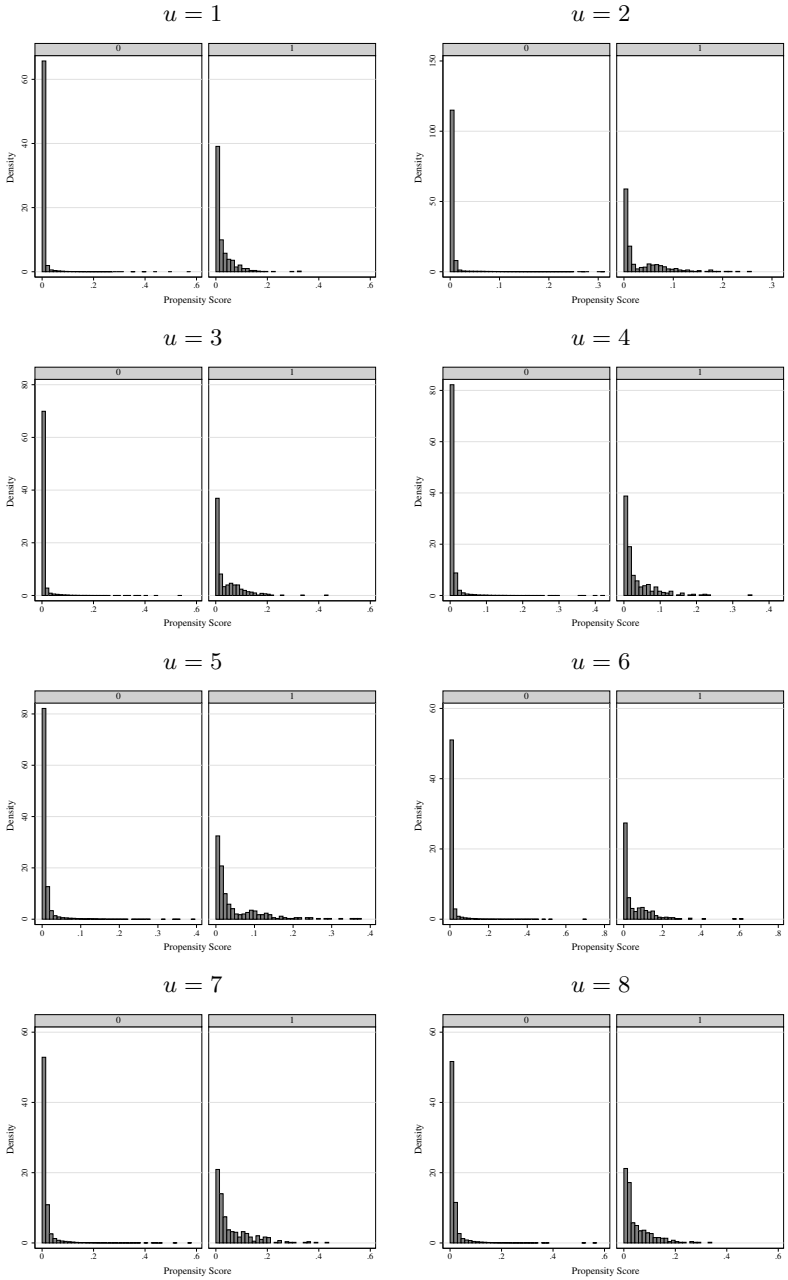
Table A.20: Cumulated Exit Rates for East Germany by Month of Treatment Start for $u = 13, \ldots, 24$ (cumulated frequencies)

Month of Exit	$u = 13$	$u = 14$	$u = 15$	$u = 16$	$u = 17$	$u = 18$	$u = 19$	$u = 20$	$u = 21$	$u = 22$	$u = 23$	$u = 24$
1	0.60	0.73	0.55	0.94	0.29	1.02	1.31	0.25	0.71	0.60	0.91	1.15
2	4.12	4.27	2.97	5.12	3.33	3.07	3.93	2.99	4.98	4.17	4.27	2.69
3	6.63	7.18	5.49	7.41	7.10	7.51	7.87	6.97	7.82	6.85	8.23	6.54
4	8.61	10.20	8.46	10.65	10.72	10.75	10.30	9.20	11.85	10.42	10.98	9.23
5	14.52	18.42	17.14	19.95	19.71	19.45	18.73	18.41	19.19	17.86	18.60	17.69
6	20.86	26.53	22.86	25.74	29.57	26.79	28.09	28.11	23.93	25.60	23.17	29.23
7	22.41	29.24	26.70	28.17	32.32	29.86	31.65	29.60	26.78	29.76	25.61	31.54
8	26.36	33.09	31.32	32.61	36.67	36.69	34.64	35.57	30.57	33.33	32.32	35.77
9	29.41	36.52	34.73	35.04	39.13	39.59	37.45	39.55	34.12	36.90	35.98	39.62
10	30.78	38.19	36.15	36.52	40.87	40.96	38.58	41.29	35.31	37.20	36.89	40.38
11	76.87	80.75	81.32	78.17	79.71	77.99	78.09	81.84	77.25	77.68	80.18	82.31
12	**96.89**	**96.67**	**96.92**	**95.28**	**96.67**	**94.54**	**97.00**	**97.26**	**95.73**	**94.94**	**96.04**	**93.46**
13	97.07	96.88	97.14	95.96	96.96	95.22	97.38	97.76	95.73	95.24	96.65	93.85
14	97.79	97.81	97.58	96.77	98.12	97.10	97.94	99.00	97.16	96.43	97.26	95.38
15	98.45	98.34	97.80	97.98	98.70	97.61	98.50	99.00	97.39	97.32	97.56	96.15
16	98.98	99.48	98.46	98.38	99.42	98.29	99.81	99.50	98.58	97.62	98.48	96.15
17	99.04	99.48	98.57	98.52	99.42	98.29	99.81	99.75	98.58	97.92	98.48	96.54
18	99.04	99.58	98.57	98.52	99.42	98.29	99.81	99.75	98.82	98.21	98.48	96.92
19	99.16	99.69	98.68	98.52	99.57	98.46	99.81	99.75	98.82	98.51	98.78	97.69
20	99.16	99.69	98.68	98.52	99.57	98.46	99.81	99.75	98.82	98.51	98.78	97.69
21	99.28	99.69	98.79	98.52	99.57	98.46	99.81	99.75	98.82	98.81	99.09	97.69
22	99.28	99.69	98.90	98.52	99.57	98.46	99.81	99.75	98.82	98.81	99.09	97.69
23	99.28	99.69	99.01	98.52	99.57	98.98	99.81	100.00	99.05	98.81	99.39	97.69
24	99.40	99.90	99.01	98.65	99.57	99.15	99.81	100.00	99.05	99.40	99.39	98.08
25	99.40	99.90	99.01	98.65	99.57	99.15	99.81	100.00	99.05	99.40	99.39	98.08
26	99.40	99.90	99.01	98.65	99.57	99.15	99.81	100.00	99.05	99.40	99.39	98.08
27	99.40	99.90	99.01	98.65	99.57	99.15	99.81	100.00	99.05	99.40	99.39	98.08
28	99.40	99.90	99.01	98.65	99.57	99.15	99.81	100.00	99.29	99.40	99.39	98.08
29	99.40	99.90	99.01	98.65	99.71	99.15	99.81	100.00	99.29	99.40	99.39	98.08
30	99.40	99.90	99.01	98.65	99.71	99.15	99.81	100.00	99.29	99.40	99.39	98.08

u denotes the month(s) spent in open unemployment until treatment start.

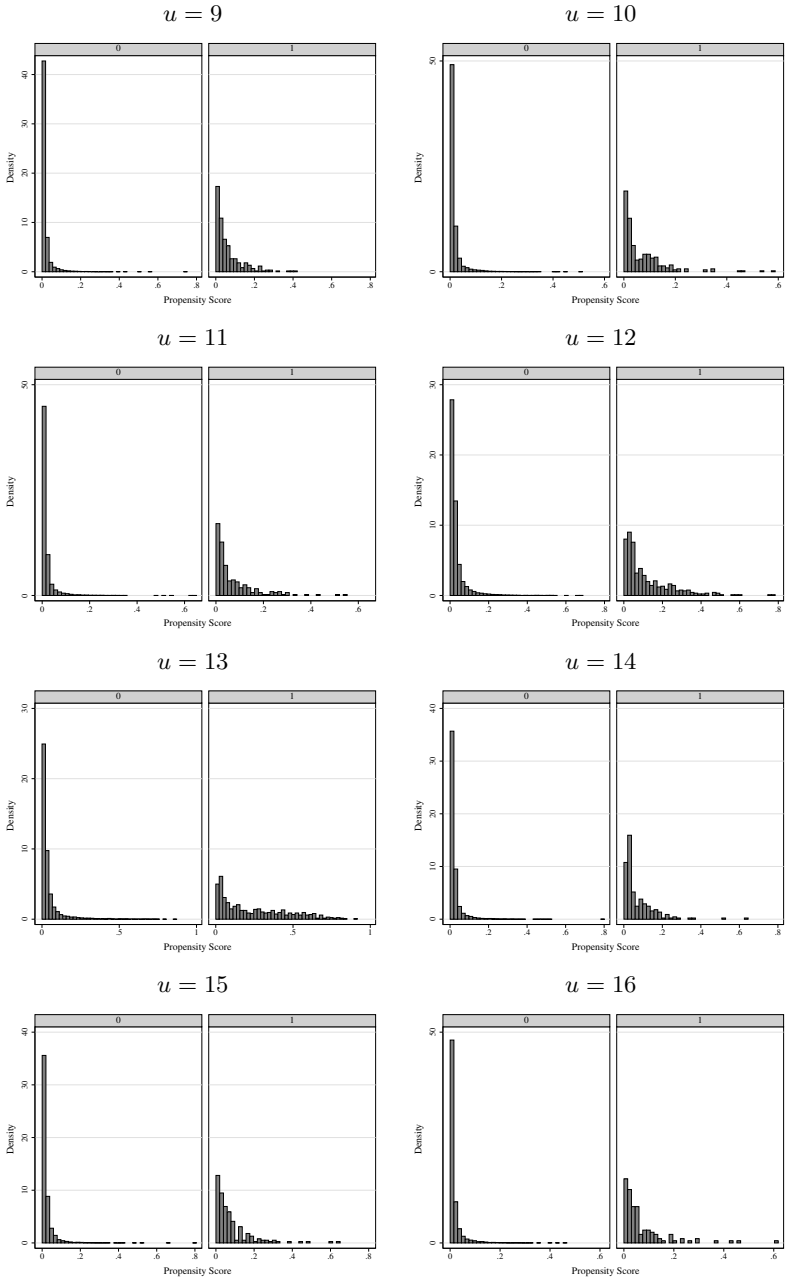
A.2 Figures

Fig. A.1: Common Support for West Germany (Treatment Start: Month $u = 1$ to $u = 8$)[a]



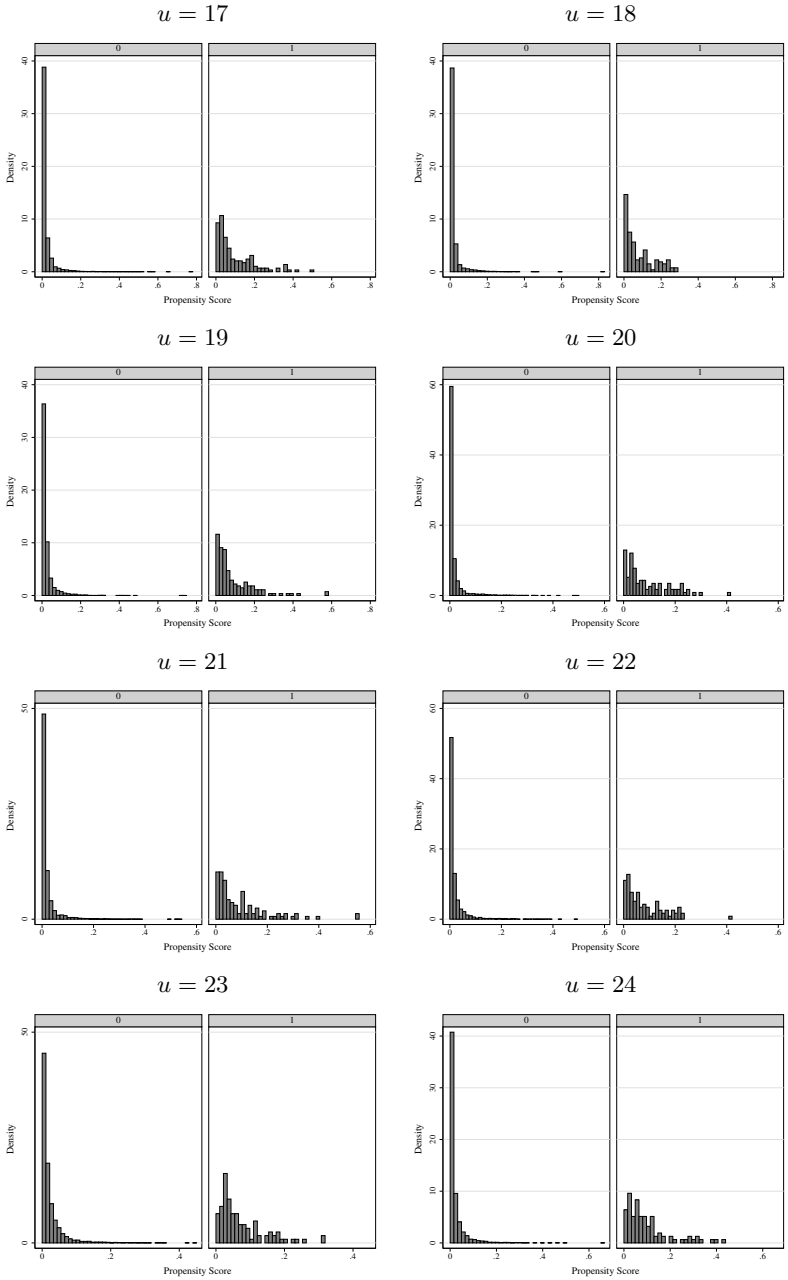
[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

Fig. A.2: Common Support for West Germany (Treatment Start: Month $u = 9$ to $u = 16$)[a]



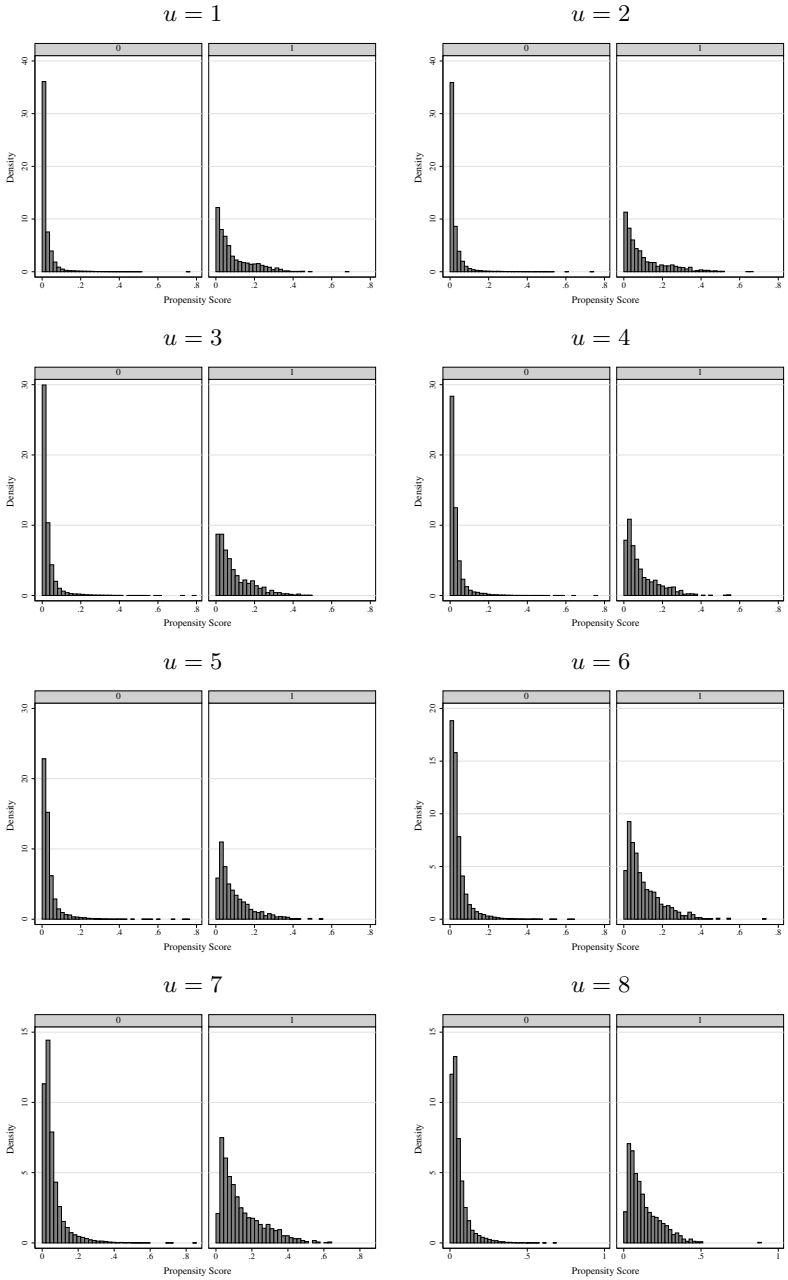
[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

Fig. A.3: Common Support for West Germany (Treatment Start: Month $u = 17$ to $u = 24$)[a]



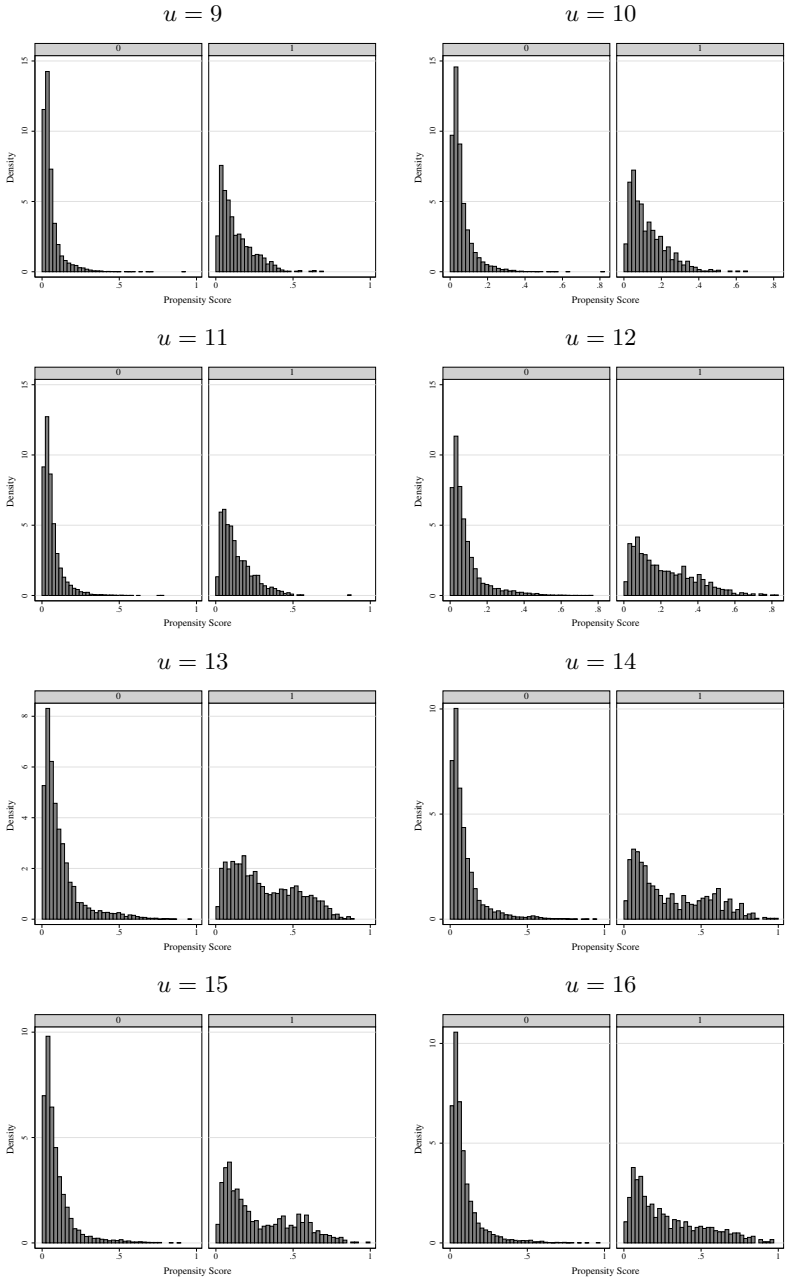
[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

Fig. A.4: Common Support for East Germany (Treatment Start: Month $u = 1$ to $u = 8$)[a]



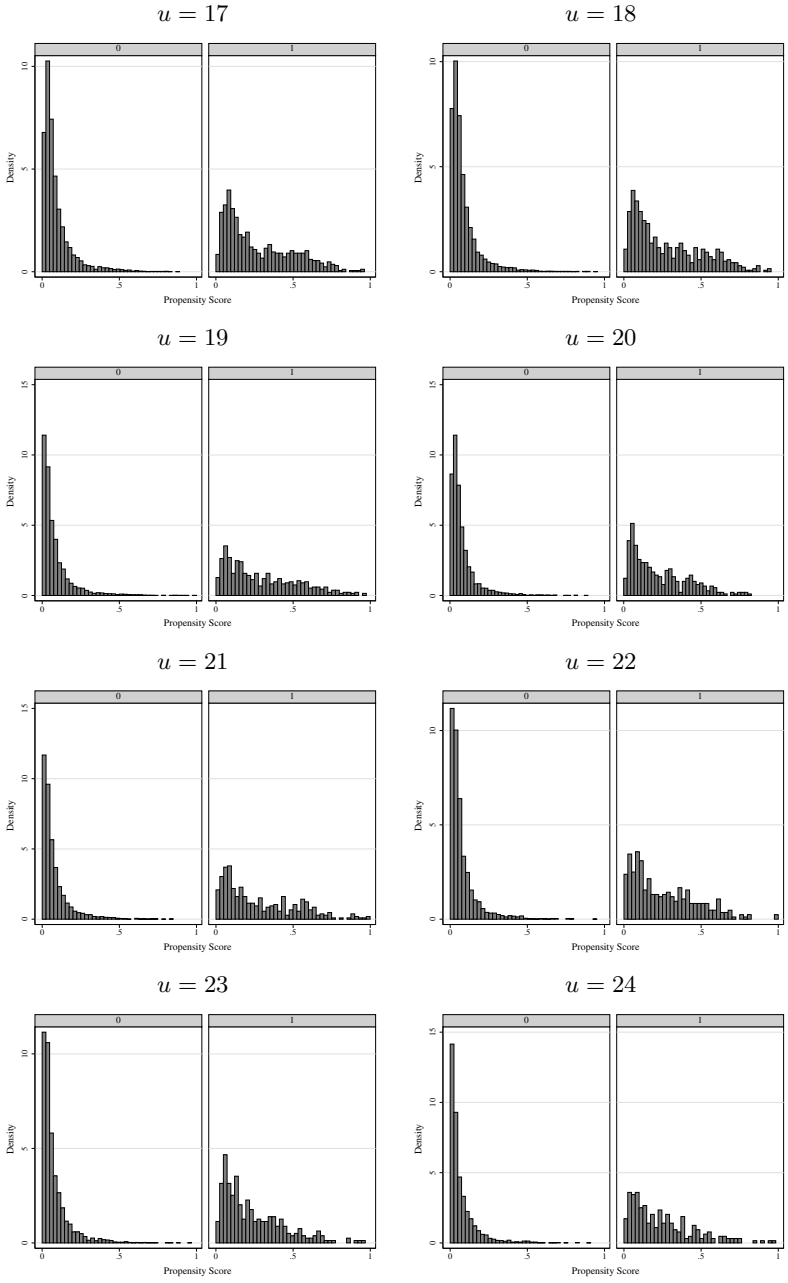
[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

Fig. A.5: Common Support for East Germany (Treatment Start: Month $u = 9$ to $u = 16$)[a]



[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

Fig. A.6: Common Support for East Germany (Treatment Start: Month $u = 17$ to $u = 24$)[a]



[a] The left side of the graphs refers to non-participants ($D_u = 0$), the right side to participants ($D_u = 1$) in each group.

B

Additional Material to Chapter 6

B.1 Tables

Table B.1: Estimation Results of the Logit-Models for the Propensity Score for Men in West Germany

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**19.2224**	5.3552	*-43.6753*	20.9759	0.4978	0.3218	0.9086	0.7966
Age	**-1.8352**	0.5199	*1.5942*	0.7585	**-0.1449**	0.0179	**-0.1705**	0.0472
Age (squared)	**0.0390**	0.0125	*-0.0156*	0.0068	**0.0014**	0.0002	*0.0014*	0.0006
Married	0.0387	0.1929	-0.0389	0.1291	*-0.1602*	0.0800	0.0442	0.2047
No. of children	0.0755	0.1666	-0.0547	0.0774	**0.0976**	0.0340	0.1014	0.1015
German	*0.3141*	0.1399	-0.0853	0.1777	**0.4157**	0.0754	0.0465	0.1798
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	1.0806	0.5670	0.3597	0.5474	**1.0216**	0.2378	0.5442	0.5945
50% to under 80%	0.4628	0.5570	*0.5372*	0.2575	**0.7344**	0.1806	*1.0699*	0.4172
30% to under 50%	–	–	*1.2877*	0.5559	0.7656	0.6504	**3.0225**	1.0560
30% to under 50%, no equalis.[2]	–	–	0.0615	0.2890	0.0593	0.2356	0.4056	0.5639
Other health restrictions	-0.1973	0.2862	-0.0312	0.1986	-0.1524	0.1221	-0.1985	0.3172
Professional Training								
No training, no CSE	*Reference*		*Reference*		–	–	*Reference*	
No training, with CSE	**-0.5419**	0.1213	0.2171	0.2055	–	–	-0.2807	0.1766
Industrial training	**-2.0355**	0.2144	0.1957	0.2135	–	–	**-1.1182**	0.2098
Full-time vocational school	–	–	-0.5766	0.7566	–	–	-0.9716	0.5937
Technical school	–	–	0.7335	0.3761	–	–	-1.4109	0.7459
Polytechnic	–	–	0.5298	0.4771	–	–	0.1760	0.5737
College, University	–	–	**1.2210**	0.3538	–	–	0.1697	0.3763
Occupational Group								
Farming[3]	-0.0987	0.2409	*0.5068*	0.2435	0.1700	0.1090	0.2699	0.2992
Mining, mineral extraction					-0.3894	0.5229	*1.6121*	0.8025
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	-0.4096	1.0465	-0.2693	0.2824	-0.4265	0.3578	-0.3647	0.3876
Service professions	**-0.4072**	0.1344	**-0.4416**	0.1363	**-0.3708**	0.0682	-0.1020	0.1635
Other professions	-0.3725	0.2166	-1.7136	1.0186	0.0605	0.1724	-0.0556	0.2798
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	**-0.9983**	0.3401	**-0.6198**	0.2054	-0.2760	0.1456	-0.5143	0.4088
WC, simple occupations	0.1994	0.3773	-0.2276	0.2518	-0.3988	0.2080	0.6075	0.3716
WC, advanced occupations	1.4763	1.0867	-0.5604	0.3153	0.0212	0.3928	**1.2666**	0.4649
Other	*0.3160*	0.1296	**-0.5193**	0.1477	-0.0628	0.0665	0.2073	0.1897
Work experience	*-0.3040*	0.1245	0.4182	0.4037	**-0.3672**	0.0945		
Duration of empl. (months)[5]	*-0.0136*	0.0062	**-0.0045**	0.0007	**-0.0048**	0.0007	-0.0010	0.0020
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.1889	0.1179	0.0003	0.1684	0.0588	0.0761	-0.2072	0.1622
More than 52 weeks	**0.7101**	0.2177	**-0.7455**	0.1634	**0.3454**	0.0853	**0.5780**	0.2070
No. of placement propositions	**0.0609**	0.0107	**0.0849**	0.0077	**0.0518**	0.0039	**0.0690**	0.0104
Last contact[6]	*-0.1044*	0.0424	**0.0725**	0.0248	**-0.0733**	0.0186	-0.0422	0.0436

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Table B.1: (continued)

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Vocational rehabilitation[7]	0.4729	0.3783	0.0499	0.2767	**-0.4860**	0.1826	-0.1064	0.3513
Placement restrictions	-0.0452	0.3690	*-0.4378*	0.2095	-0.1973	0.1389	-0.3973	0.3343
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	-0.4245	0.3299	0.1782	0.2440	0.0688	0.1108	-0.0664	0.2518
VT, vocational adjustment	1.2458	0.6416	**1.2807**	0.4213	0.5829	0.3074	–	–
Job-preparative measure	-0.5547	1.0609	–	–	-0.6492	1.0378	–	–
Job creation scheme	**1.6855**	0.2160	**2.0873**	0.1748	**1.9736**	0.0974	**2.0412**	0.2784
Rehabilitation measure	-0.7142	0.8680	–	–	0.1987	0.4158	0.6798	0.4625
Regional Context Variables								
Cluster II	*0.4709*	0.1986	-0.3371	0.1723	-0.0203	0.0972	0.2019	0.2716
Cluster III	**0.6413**	0.1906	-0.2342	0.1699	-0.0732	0.0981	0.2807	0.2715
Cluster IV	**0.9096**	0.2530	-0.1870	0.2316	0.0923	0.1329	0.5891	0.3539
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.2: Estimation Results of the Logit-Models for the Propensity Score for Men in West Germany

	Long-term unemployed		**More than 5 plac. prop.**		**Vocational rehabilitation**		**Placement restr.**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-3.4319**	0.6632	**-3.0821**	0.5156	-0.2817	1.5120	-1.7075	1.0243
Age	**0.1343**	0.0322	0.0497	0.0264	-0.0089	0.0785	0.0604	0.0458
Age (squared)	**-0.0022**	0.0004	-0.0006	0.0003	-0.0003	0.0010	*-0.0011*	0.0005
Married	-0.0299	0.0935	0.0156	0.0864	-0.0380	0.2723	-0.2144	0.1507
No. of children	0.0400	0.0416	0.0415	0.0380	-0.1511	0.1534	0.0570	0.0745
German	**0.3684**	0.1124	**0.3881**	0.1053	0.0846	0.4137	0.3481	0.2152
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**0.7115**	0.2666	**0.8719**	0.3054	–	–	-0.5085	0.4461
50% to under 80%	**0.6035**	0.1927	**1.0262**	0.1931	-0.3366	0.3719	-0.5368	0.4159
30% to under 50%	0.7528	0.6132	0.5902	0.6946	0.4220	0.6799	–	–
30% to under 50%, no equalis.[2]	*0.4390*	0.2075	**0.6419**	0.2142	*-0.8671*	0.4378	**-1.2335**	0.4444
Other health restrictions	-0.1283	0.1327	-0.0250	0.1246	**-1.3746**	0.3405	**-1.4093**	0.4097
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	-0.1748	0.1003	*-0.2001*	0.0954	-0.0945	0.3512	-0.1272	0.1670
Industrial training	*-0.2282*	0.1082	**-0.4294**	0.1004	0.0491	0.3290	-0.2994	0.1700
Full-time vocational school	-0.4562	0.4059	-0.2681	0.3126	0.9032	0.7261	-0.7384	0.6508
Technical school	-0.2446	0.3231	0.0112	0.2594	0.6162	0.7099	0.4007	0.4501

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Table B.2: (continued)

	Long-term unemployed		More than 5 plac. prop.		Vocational rehabilitation		Placement restr.	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Polytechnic	0.3810	0.3187	0.5062	0.2751	–	–	1.0165	1.0994
College, University	*0.5130*	0.2410	0.1574	0.2472	–	–	-0.0780	1.0816
Occupational Group								
Farming[3]	0.2409	0.1515	*0.2631*	0.1319	0.5692	0.4419	0.0848	0.2380
Mining, mineral extraction	-0.4042	0.6084	-0.6981	1.0886	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	-0.2679	0.2184	*-0.4379*	0.2124	-0.1689	0.5774	-0.9056	0.5052
Service professions	**-0.3318**	0.0840	**-0.3393**	0.0791	-0.0619	0.2315	**-0.3566**	0.1284
Other professions	-0.1265	0.3543	-0.3269	0.3786	-0.4286	0.5426	*-0.7632*	0.3577
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.3017	0.1592	**-0.3710**	0.1308	-0.4078	0.4221	-0.2729	0.2407
WC, simple occupations	-0.1964	0.1931	0.1210	0.1582	0.0609	0.6174	0.0401	0.3178
WC, advanced occupations	-0.4424	0.2336	0.2259	0.2207	-0.1126	1.0978	-0.2111	0.6009
Other	**-0.3765**	0.0884	-0.0569	0.0824	-0.2908	0.2805	-0.2594	0.1448
Work experience	**-0.3806**	0.1323	-0.2136	0.1291	-0.1053	0.2895	-0.1547	0.2074
Duration of empl. (months)[5]	**-0.0041**	0.0006	-0.0018	0.0009	-0.0031	0.0023	**-0.0045**	0.0011
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	–	–	0.1815	0.0947	-0.3252	0.2785	-0.2131	0.1642
More than 52 weeks	–	–	0.0554	0.0983	-0.5193	0.2829	**-0.4451**	0.1620
No. of placement propositions	**0.0376**	0.0044	–	–	**0.0466**	0.0129	**0.0645**	0.0069
Last contact[6]	0.0003	0.0157	**0.0575**	0.0166	0.0195	0.0554	0.0030	0.0296
Vocational rehabilitation[7]	*-0.4055*	0.1786	-0.3357	0.1743	–	–	-0.2406	0.1419
Placement restrictions	**-0.4780**	0.1442	-0.1814	0.1411	-0.4808	0.2471	–	–
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	0.1148	0.1231	0.0099	0.1057	*0.8633*	0.3785	0.3985	0.2114
VT, vocational adjustment	0.3810	0.3328	**0.6754**	0.2618	0.9562	0.8129	-0.1987	0.6269
Job-preparative measure	–	–	–	–	–	–	–	–
Job creation scheme	**1.2748**	0.1195	**1.7545**	0.1039	**2.3799**	0.3687	**2.4849**	0.1771
Rehabilitation measure	-0.3715	0.4346	-0.2587	0.4404	0.1933	0.3239	-0.4136	0.4035
Regional Context Variables								
Cluster II	**-0.5906**	0.1182	*-0.2395*	0.1064	*-0.7510*	0.3158	**-0.5914**	0.1775
Cluster III	**-0.5332**	0.1211	*-0.2596*	0.1061	-0.5817	0.2976	**-0.4404**	0.1707
Cluster IV	-0.1169	0.1613	-0.1289	0.1458	0.1469	0.3827	-0.0153	0.2310
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.3: Estimation Results of the Logit-Models for the Propensity Score for Men in West Germany

	ALMP part.		**Target Score=0**		**Target Score=1**		**Target Score=2**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	-1.1523	0.6245	*-12.7292*	5.2292	**-9.5376**	1.3108	**-3.1125**	0.6067
Age	0.0022	0.0318	0.4404	0.2683	**0.2551**	0.0596	0.0216	0.0308
Age (squared)	-0.0001	0.0004	-0.0044	0.0034	**-0.0030**	0.0007	-0.0007	0.0004
Married	-0.0473	0.1167	**-1.1781**	0.4002	**-0.4564**	0.1569	-0.0951	0.1049
No. of children	-0.0607	0.0569	-0.0085	0.2005	**0.1943**	0.0653	0.0563	0.0490
German	*0.3607*	0.1521	-0.7306	0.4188	**0.6747**	0.2091	**0.6053**	0.1152
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**1.2038**	0.3569	–	–	*1.1736*	0.5110	0.1174	0.5047
50% to under 80%	**0.8403**	0.2372	0.8950	1.1553	**1.1693**	0.3166	**0.8766**	0.2375
30% to under 50%	**1.5702**	0.5424	–	–	1.4680	1.0473	0.8693	0.6953
30% to under 50%, no equalis.[2]	**0.7426**	0.2627	–	–	0.0180	0.5254	0.2812	0.3021
Other health restrictions	-0.0598	0.1701	-0.2893	0.6209	-0.1991	0.2452	-0.0052	0.1478
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	*-0.2901*	0.1233	–	–	**-0.7805**	0.2322	**-0.4602**	0.1046
Industrial training	**-0.4386**	0.1317	-1.3327	0.7054	0.0507	0.2526	*-0.3343*	0.1485
Full-time vocational school	*-1.1618*	0.5301	–	–	0.2700	0.5206	-0.1708	0.4250
Technical school	0.0174	0.3089	-0.1781	0.8019	0.7426	0.3997	0.2159	0.3149
Polytechnic	0.6798	0.3511	–	–	*1.0705*	0.4461	**0.9360**	0.3205
College, University	0.4395	0.2963	0.0461	0.7112	0.7425	0.3807	**0.7509**	0.2749
Occupational Group								
Farming[3]	*0.3444*	0.1555	*1.1522*	0.4853	0.0668	0.2894	**0.5773**	0.1511
Mining, mineral extraction	0.7208	0.5750	–	–	–	–	-0.4393	0.7264
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	**-1.0490**	0.2980	-1.4744	0.8222	**-0.9471**	0.3448	-0.4577	0.2415
Service professions	**-0.3799**	0.1058	-0.6599	0.3811	-0.2307	0.1433	**-0.2812**	0.0926
Other professions	-0.4823	0.4468	1.2718	1.1861	-0.0689	0.5340	0.2322	0.2657
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	*-0.4017*	0.1967	*-0.9997*	0.4991	**-0.6029**	0.2295	**-0.4647**	0.1637
WC, simple occupations	0.0948	0.2312	-0.1965	0.5315	0.1456	0.2420	-0.2424	0.2119
WC, advanced occupations	0.0816	0.2888	-0.1395	0.6067	0.3616	0.3252	-0.0408	0.2406
Other	**-0.7330**	0.1050	-0.4018	0.3748	-0.0203	0.1547	-0.0289	0.0936
Work experience	-0.0088	0.1505	–	–	0.5903	0.4101	**0.6133**	0.1955
Duration of empl. (months)[5]	**-0.0059**	0.0014	-0.0051	0.0037	**-0.0054**	0.0013	**-0.0040**	0.0007
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	**-0.3224**	0.1194	**1.0955**	0.3335	**0.9276**	0.1498	-0.0073	0.0994
More than 52 weeks	**-0.5737**	0.1241			*0.5773*	0.2612	0.0192	0.1414
No. of placement propositions	**0.0268**	0.0056	**0.3188**	0.0820	**0.0354**	0.0095	**0.0229**	0.0062
Last contact[6]	0.0372	0.0225	-0.1381	0.1047	-0.0360	0.0381	0.0174	0.0207
Vocational rehabilitation[7]	*-0.4080*	0.1929	–	–	-0.8100	1.0488	-0.1955	0.3191
Placement restrictions	-0.0933	0.1821	–	–	*-0.9641*	0.4065	**-0.7972**	0.2152
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	–	–	–	–	-0.0058	0.3441	-0.0937	0.1750
VT, vocational adjustment	–	–	–	–	–	–	0.1002	0.6361
Job-preparative measure	–	–	–	–	–	–	–	–
Job creation scheme	–	–	–	–	**3.5543**	0.4890	**2.5153**	0.2058
Rehabilitation measure	–	–	–	–	–	–	-0.2777	0.7435

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Table B.3: (continued)

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Regional Context Variables								
Cluster II	-0.1859	0.1521	0.3537	0.4333	**-0.5475**	0.1710	-0.2403	0.1240
Cluster III	-0.0564	0.1482	0.0459	0.4370	**-0.5184**	0.1667	-0.1566	0.1220
Cluster IV	0.0737	0.2300	-0.0873	0.7085	*-0.5733*	0.2596	0.1244	0.1616
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.4: Estimation Results of the Logit-Models for the Propensity Score for Men in West Germany

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-2.4589**	0.5681	**-3.5758**	0.9300	-2.1657	1.8925
Age	**0.0912**	0.0339	**0.1577**	0.0573	0.1973	0.1109
Age (squared)	**-0.0016**	0.0004	**-0.0024**	0.0007	*-0.0031*	0.0014
Married	-0.0051	0.1141	0.0522	0.1765	-0.0896	0.3203
No. of children	0.0393	0.0510	0.0397	0.0805	-0.2176	0.1846
German	**0.3926**	0.1183	0.1095	0.2035	-0.1647	0.3963
Health Restrictions[1]						
No health restrictions	*Reference*		*Reference*		*Reference*	
80% and over	**1.0874**	0.2967	0.5722	0.4630	1.2702	0.8358
50% to under 80%	*0.5096*	0.2459	0.6399	0.3544	0.9301	0.7090
30% to under 50%	0.3038	0.8097	0.7255	1.1258	*2.5090*	1.0776
30% to under 50%, no equalis.[2]	0.2459	0.2773	-0.2344	0.4507	1.1759	0.7405
Other health restrictions	-0.2496	0.1733	-0.1448	0.2881	0.2110	0.6186
Professional Training						
No training, no CSE	*Reference*		*Reference*		*Reference*	
No training, with CSE	**-0.3482**	0.1033	0.0280	0.1680	-0.0480	0.2986
Industrial training	-0.1975	0.1597	*0.5679*	0.2581	0.4361	0.4696
Full-time vocational school	-1.4002	1.0332	0.2420	0.8111	2.4643	1.4494
Technical school	0.1022	0.4480	-0.0808	1.0874	2.1555	1.1949
Polytechnic	-0.3408	0.6082	*2.3560*	1.1117	–	–
College, University	0.5084	0.4149	1.2067	0.7381	–	–
Occupational Group						

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Table B.4: (continued)

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Farming[3]	-0.0388	0.1810	0.0657	0.2472	-0.0282	0.4276
Mining, mineral extraction	-0.4361	0.7592	-0.0155	1.1016	–	–
Manufacturing	*Reference*		*Reference*		*Reference*	
Technical professions	-0.5321	0.3738	0.3709	0.4497	-1.4005	1.2338
Service professions	**-0.3095**	0.0980	*-0.3555*	0.1573	*-0.6175*	0.2805
Other professions	-0.0743	0.2382	-0.3854	0.5079	-0.4427	0.7944
Professional Rank[4]						
BC, not skilled worker	*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.3613	0.1989	-0.0174	0.3152	0.4018	0.5675
WC, simple occupations	0.2734	0.2302	0.1069	0.3997	-0.6239	1.2415
WC, advanced occupations	-0.1799	0.3804	-1.2395	0.9251	0.2825	1.3117
Other	-0.1259	0.1017	-0.0713	0.1661	-0.1103	0.2988
Work experience	0.0637	0.1496	0.4275	0.2307	*0.7607*	0.3788
Duration of empl. (months)[5]	**-0.0059**	0.0010	**-0.0050**	0.0017	-0.0013	0.0027
Duration of Unemployment (Weeks)						
Up to 13 weeks	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	**-0.3681**	0.1248	-0.0213	0.2392	*-1.6015*	0.6220
More than 52 weeks	**-0.6417**	0.1526	**-1.4310**	0.2597	**-2.0825**	0.4928
No. of placement propositions	**0.0211**	0.0068	**0.0299**	0.0098	0.0004	0.0224
Last contact[6]	-0.0125	0.0218	0.0183	0.0324	-0.0655	0.0633
Vocational rehabilitation[7]	*-0.5750*	0.2396	**-1.2044**	0.2710	**-1.1681**	0.3849
Placement restrictions	**-0.7584**	0.2021	-0.6125	0.3177	-1.0170	0.5829
Programme Before Unemployment						
No programme	*Reference*		*Reference*		*Reference*	
VT[8], further training	**-0.5632**	0.1636	*-0.5002*	0.2471	**-1.3990**	0.5166
VT, vocational adjustment	-0.2795	0.4511	0.2555	0.4139	-0.5763	0.7766
Job-preparative measure	-0.6283	1.0551	–	–	–	–
Job creation scheme	**1.6530**	0.1593	**1.0649**	0.2346	**1.1016**	0.3930
Rehabilitation measure	-0.5505	0.5395	*-1.3360*	0.6290	-0.7736	0.5631
Regional Context Variables						
Cluster II	-0.2225	0.1396	-0.2073	0.2278	0.2153	0.5174
Cluster III	*-0.2891*	0.1416	-0.0992	0.2271	0.6268	0.5070
Cluster IV	-0.0858	0.1923	0.0780	0.3204	0.9152	0.6355
Cluster V	*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.5: Estimation Results of the Logit-Models for the Propensity Score for Women in West Germany

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	10.6581	7.8807	**-111.2167**	43.0028	-0.8437	0.5601	-0.7924	1.1611
Age	-0.9888	0.7691	**4.0834**	1.5741	**-0.0851**	0.0311	-0.1003	0.0656
Age (squared)	0.0184	0.0187	**-0.0391**	0.0144	0.0005	0.0004	0.0007	0.0009
Married	0.0682	0.2379	**-0.5664**	0.1796	**-0.3669**	0.1148	-0.2893	0.2401
No. of children	-0.4780	0.3609	0.0783	0.1876	*-0.1465*	0.0694	-0.2055	0.1641
German	-0.0050	0.2242	0.0165	0.3582	*0.2934*	0.1419	-0.1205	0.2679
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**2.7825**	0.6054	*1.5175*	0.6174	**1.2836**	0.3801	**1.7312**	0.6289
50% to under 80%	1.1635	0.8207	0.1760	0.4324	*0.6874*	0.2895	*1.2989*	0.5372
30% to under 50%	–	–	1.5630	0.8197	**1.9682**	0.5949	–	–
30% to under 50%, no equalis.[2]	–	–	0.1916	0.4326	-0.3593	0.4512	0.1477	1.0737
Other health restrictions	*0.8537*	0.3558	-0.1662	0.2842	-0.1738	0.2042	0.3157	0.4104
Professional Training								
No training, no CSE	*Reference*		*Reference*		–	–	*Reference*	
No training, with CSE	-0.3774	0.2346	0.8822	0.5569	–	–	0.3371	0.3300
Industrial training	**-1.4577**	0.3249	0.8080	0.5650	–	–	-0.7344	0.3825
Full-time vocational school	-0.9174	0.5927	0.1838	0.8561	–	–	-0.2524	0.5664
Technical school	0.2209	0.5662	0.6720	0.7545	–	–	0.2984	0.5765
Polytechnic	–	–	-0.3876	1.0902	–	–	**1.6320**	0.5228
College, University	–	–	**1.8171**	0.6672	–	–	*1.1050*	0.4366
Occupational Group								
Farming[3]	-0.0887	0.5347	–	–	0.2196	0.3147	0.3226	0.6791
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	–	–	**1.5068**	0.5151	0.7829	0.4845	-0.2706	0.8026
Service professions	-0.1598	0.2175	0.4405	0.2812	0.2270	0.1204	0.5290	0.3048
Other professions	-0.2963	0.3708	-0.2078	1.0569	0.2886	0.2952	0.5474	0.4525
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.2388	0.5168	0.1499	0.4686	-0.3179	0.3433	0.4922	0.5298
WC, simple occupations	-0.0900	0.3411	0.3606	0.3008	-0.0422	0.1846	-0.1878	0.4352
WC, advanced occupations	0.1511	0.8479	-0.0164	0.4366	**0.9614**	0.2916	-0.0541	0.5886
Other	0.2885	0.2316	0.0051	0.2773	0.0706	0.1281	0.1383	0.3228
Work experience	**-0.5454**	0.1863	0.4637	0.5544	**-0.5000**	0.1412	–	–
Duration of empl. (months)[5]	-0.0093	0.0084	**-0.0031**	0.0011	**-0.0044**	0.0012	-0.0071	0.0050
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.1991	0.1810	-0.4244	0.2586	-0.0510	0.1269	-0.1168	0.2131
More than 52 weeks	0.3524	0.3344	**-0.7664**	0.2476	-0.0150	0.1442	-0.1482	0.2824
No. of placement propositions	**0.0681**	0.0158	**0.0869**	0.0100	**0.0523**	0.0068	**0.0586**	0.0143
Last contact[6]	0.0315	0.0592	*0.0881*	0.0388	0.0311	0.0279	0.0087	0.0557
Vocational rehabilitation[7]	0.7018	0.5635	-0.0673	0.5776	0.1092	0.3094	0.1244	0.5039
Placement restrictions	*-1.1975*	0.4925	-0.3778	0.3115	-0.2301	0.2279	-0.3234	0.4479
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	0.4239	0.4510	**1.0261**	0.2656	**0.5548**	0.1622	-0.4605	0.3826
VT, vocational adjustment	–	–	0.7044	1.0477	–	–	–	–
Job-preparative measure	**2.5023**	0.6643	–	–	**2.4653**	0.5620	2.3640	1.4426
Job creation scheme	**2.9243**	0.3941	**3.0391**	0.2624	**2.9377**	0.1698	**3.3938**	0.3609
Rehabilitation measure	0.6365	0.9236	**2.5011**	0.8859	-0.6581	1.0570	1.0536	0.6708
Regional Context Variables								
Cluster II	-0.4451	0.2672	**-0.8248**	0.2309	**-0.4691**	0.1438	-0.4169	0.2842
Cluster III	0.0718	0.2440	**-0.6931**	0.2260	**-0.4339**	0.1407	-0.2347	0.2810

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Table B.5: (continued)

	Age < 25		Age > 50		Without professional training		Without work experience	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster IV	-0.1442	0.3898	**-1.6149**	0.4328	*-0.4885*	0.2154	-0.4429	0.4665
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.6: Estimation Results of the Logit-Models for the Propensity Score for Women in West Germany

	Long-term unemployed		More than 5 plac. prop.		Vocational rehabilitation		Placement restr.	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-5.4332**	1.0102	**-4.5914**	0.8809	-1.8893	2.7422	*-3.9176*	1.5800
Age	**0.1365**	0.0493	0.0842	0.0437	0.1097	0.1299	0.0602	0.0705
Age (squared)	**-0.0022**	0.0006	*-0.0011*	0.0005	-0.0020	0.0017	-0.0014	0.0009
Married	**-0.6957**	0.1200	**-0.4723**	0.1194	-0.0295	0.3923	-0.0755	0.2066
No. of children	0.0570	0.0661	0.0222	0.0669	-0.8314	0.4903	-0.0077	0.1311
German	0.0420	0.1998	0.1781	0.2087	0.8757	1.0601	*1.8635*	0.7347
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	0.4078	0.4406	**1.3298**	0.4492	*-2.2439*	1.0308	–	–
50% to under 80%	-0.0672	0.3305	*0.7595*	0.2962	*-1.7208*	0.8585	-0.5676	0.3247
30% to under 50%	**1.6529**	0.6050	**2.7754**	0.7771	–	–	0.8967	0.5438
30% to under 50%, no equalis.[2]	-0.2535	0.4141	0.2761	0.4160	*-2.3093*	0.9412	-0.7913	0.4051
Other health restrictions	-0.4034	0.2340	-0.1980	0.2209	**-2.4023**	0.8109	**-1.5582**	0.3006
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	*0.4642*	0.2172	*0.4938*	0.2328	0.4518	0.7319	*0.7987*	0.3772
Industrial training	*0.4573*	0.2229	0.3425	0.2370	0.3333	0.7169	0.5703	0.3860
Full-time vocational school	-0.0711	0.4482	0.4339	0.3623	-0.5076	1.2884	0.3579	0.7246
Technical school	**1.0278**	0.3146	**1.1603**	0.3169	0.2133	1.2935	*1.4259*	0.5797
Polytechnic	**1.5600**	0.3580	**1.8466**	0.3448	*3.4128*	1.3831	**2.4725**	0.7054
College, University	**1.0214**	0.3059	**1.4542**	0.3259	–	–	1.3604	0.7586
Occupational Group								
Farming[3]	0.0688	0.4333	0.0713	0.4402	–	–	-0.4686	1.0611
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	0.1601	0.3686	-0.0266	0.3934	–	–	-0.0637	0.8109
Service professions	0.2795	0.1511	0.1633	0.1631	0.1029	0.4475	-0.0840	0.2390
Other professions	0.6185	0.4727	*1.1656*	0.5348	-0.9081	0.9460	-0.4072	0.5664
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	0.3060	0.3135	-0.5078	0.3279	0.5353	0.8670	0.0818	0.5019
WC, simple occupations	*0.4173*	0.2043	0.1267	0.1871	0.8302	0.6371	0.4694	0.3131

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Table B.6: (continued)

	Long-term unemployed		More than 5 plac. prop.		Vocational rehabilitation		Placement restr.	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
WC, advanced occupations	0.3429	0.2603	0.2572	0.2482	–	–	-0.1237	0.5420
Other	0.0718	0.1750	-0.0588	0.1627	-0.4042	0.5535	-0.4823	0.2586
Work experience	-0.0452	0.2013	-0.1255	0.1878	-0.5030	0.4526	-0.4411	0.2898
Duration of empl. (months)[5]	**-0.0032**	0.0010	-0.0023	0.0013	-0.0017	0.0033	*-0.0045*	0.0019
Duration of Unemployment (Weeks)								
Up to 13 weeks	–	–	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	–	–	0.2079	0.1607	-0.0044	0.5324	-0.2695	0.2631
More than 52 weeks	–	–	0.2077	0.1612	-0.0536	0.5579	-0.4715	0.2659
No. of placement propositions	**0.0487**	0.0063			0.0460	0.0242	**0.0642**	0.0113
Last contact[6]	**0.0634**	0.0225	0.0223	0.0275	0.0028	0.0869	*0.0900*	0.0428
Vocational rehabilitation[7]	0.1098	0.2976	-0.2131	0.3471	–	–	0.0425	0.2476
Placement restrictions	0.0226	0.2502	-0.1526	0.2493	-0.1923	0.4260	–	–
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	**0.4974**	0.1658	**0.4425**	0.1462	-0.2455	1.0999	**0.9273**	0.3132
VT, vocational adjustment	0.9691	0.5325	0.3913	0.4974	–	–	–	–
Job-preparative measure	1.6544	1.1257	2.0072	1.1866	–	–	–	–
Job creation scheme	**2.4785**	0.1654	**2.5109**	0.1638	**1.8661**	0.6655	**2.9205**	0.3027
Rehabilitation measure	0.6461	0.5796	*1.0948*	0.5293	0.7531	0.4639	0.7394	0.4562
Regional Context Variables								
Cluster II	**-0.7261**	0.1551	**-0.5160**	0.1519	-0.8996	0.4734	*-0.6045*	0.2600
Cluster III	**-0.7440**	0.1540	**-0.4901**	0.1472	*-0.9586*	0.4629	**-0.7750**	0.2595
Cluster IV	**-0.6171**	0.2384	*-0.4636*	0.2170	-0.5157	0.6559	-0.4691	0.3638
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.7: Estimation Results of the Logit-Models for the Propensity Score for Women in West Germany

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	-0.3780	0.9678	*-11.2637*	4.6088	**-9.5382**	1.8273	**-5.6442**	0.9976
Age	-0.0818	0.0481	0.2730	0.2370	**0.2278**	0.0860	*0.1287*	0.0516
Age (squared)	0.0009	0.0006	-0.0030	0.0031	**-0.0029**	0.0011	**-0.0021**	0.0007
Married	-0.1814	0.1326	**-0.9377**	0.2660	**-0.4296**	0.1608	**-0.4958**	0.1401
No. of children	0.0274	0.0714	0.1895	0.1385	-0.0723	0.0854	-0.0976	0.0835
German	0.3292	0.2466	0.6444	0.7461	0.4824	0.3083	0.3463	0.2104
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	*1.1858*	0.4773	*2.6820*	1.2685	0.6539	1.0585	**1.6272**	0.4838
50% to under 80%	**0.9146**	0.3247	–	–	**1.2478**	0.4612	**1.1134**	0.3590
30% to under 50%	*2.8507*	1.2929	–	–	**4.0920**	0.9626	**2.1872**	0.7901
30% to under 50%, no equalis.[2]	*0.8966*	0.3932	–	–	-0.0081	0.7536	-0.1669	0.6085

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Table B.7: (continued)

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Other health restrictions	-0.0358	0.2497	-0.4295	0.7374	0.1051	0.3006	-0.2467	0.2575
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	-0.0392	0.2367	–	–	0.3488	0.4219	0.1097	0.2408
Industrial training	-0.0557	0.2409	**-2.3817**	0.4053	0.2694	0.4666	0.3212	0.2853
Full-time vocational school	-0.5468	0.4582	**-1.6778**	0.6302	-0.1841	0.6656	*0.9711*	0.4073
Technical school	0.2193	0.3608	*-1.0572*	0.4800	*1.1596*	0.5136	*0.8941*	0.3954
Polytechnic	**1.0270**	0.3876			**2.3153**	0.5337	*1.1810*	0.4639
College, University	**0.8918**	0.3182	-0.6161	0.4352	**1.5042**	0.5118	**1.3939**	0.4067
Occupational Group								
Farming[3]	-0.4377	0.4955	*2.4877*	1.1846	0.3924	0.5811	-0.0812	0.5039
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	*-0.9949*	0.4736	0.8804	1.2520	-0.5334	0.5576	0.0022	0.5012
Service professions	0.0388	0.1718	1.9242	1.0203	0.4853	0.2570	0.2795	0.1792
Other professions	-0.1020	0.6413	–	–	–	–	-0.3206	0.6467
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.3995	0.4043	-0.0885	0.7307	-0.2056	0.4389	0.1312	0.3138
WC, simple occupations	0.2750	0.2344	0.2360	0.5821	0.2358	0.2997	0.2074	0.2186
WC, advanced occupations	*0.6297*	0.2963	0.6928	0.6286	**1.0444**	0.3384	0.3507	0.3100
Other	**-0.5500**	0.1961	0.4875	0.5532	0.2077	0.2675	0.0516	0.1830
Work experience	-0.0092	0.1842	–	–	0.0558	0.3767	*0.5020*	0.2486
Duration of empl. (months)[5]	*-0.0029*	0.0013	-0.0007	0.0023	-0.0028	0.0015	**-0.0047**	0.0014
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.0211	0.1631	0.5010	0.2781	0.1418	0.1783	-0.0596	0.1598
More than 52 weeks	-0.0982	0.1687			0.2619	0.3049	-0.4016	0.2158
No. of placement propositions	**0.0350**	0.0076	**0.4197**	0.0704	**0.0526**	0.0140	**0.0341**	0.0092
Last contact[6]	**0.1067**	0.0270	0.0038	0.0947	0.0701	0.0411	0.0524	0.0324
Vocational rehabilitation[7]	-0.2651	0.3145	–	–	–	–	0.3125	0.5072
Placement restrictions	-0.1167	0.2672	–	–	-0.1387	0.4875	**-1.1418**	0.3454
Programme Before Unemployment								
No programme	–	–	*Reference*		*Reference*		*Reference*	
VT[8], further training	–	–	–	–	*0.8017*	0.3257	-0.4191	0.2585
VT, vocational adjustment	–	–	–	–	1.1010	1.1327	–	–
Job-preparative measure	–	–	–	–	–	–	–	–
Job creation scheme	–	–	–	–	**3.9529**	0.4613	**3.1186**	0.2843
Rehabilitation measure	–	–	–	–	–	–	0.8893	0.8683
Regional Context Variables								
Cluster II	**-0.7062**	0.1766	*-0.7375*	0.3363	-0.2661	0.2119	**-0.5907**	0.1786
Cluster III	*-0.3780*	0.1634	-0.4186	0.3001	-0.2772	0.2040	*-0.3827*	0.1684
Cluster IV	*-0.6868*	0.2947	-0.6720	0.5025	0.1992	0.2822	*-0.5609*	0.2661
Cluster V	*Reference*		*Reference*		*Reference*		*Reference*	

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– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.8: Estimation Results of the Logit-Models for the Propensity Score for Women in West Germany

	Target Score=3		**Target Score=4**		**Target Score=5**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	*-2.0677*	0.9618	**-6.4537**	1.6772	-0.8188	3.5259
Age	0.0172	0.0565	**0.2522**	0.0981	-0.0305	0.1805
Age (squared)	-0.0008	0.0007	**-0.0038**	0.0012	-0.0006	0.0022
Married	-0.2870	0.1578	-0.4952	0.2618	0.3636	0.4809
No. of children	0.0930	0.0924	-0.0962	0.1970	0.1728	0.3581
German	0.1469	0.2147	0.1404	0.3961	-0.3164	0.9595
Health Restrictions[1]						
No health restrictions	*Reference*		*Reference*		*Reference*	
80% and over	**1.7470**	0.4737	-0.3389	0.9326	1.4437	1.3247
50% to under 80%	0.1768	0.4050	-1.7496	0.9089	**3.0112**	1.0297
30% to under 50%	1.4298	1.1010	-0.7444	1.3497	**5.4024**	1.8548
30% to under 50%, no equalis.[2]	-0.0439	0.4834	-1.9871	1.0439	*2.7876*	1.2381
Other health restrictions	0.0056	0.2679	*-2.0575*	0.8473	1.2525	0.9469
Professional Training						
No training, no CSE	*Reference*		*Reference*		*Reference*	
No training, with CSE	0.0771	0.2088	*0.8758*	0.3992	0.6153	0.6374
Industrial training	0.3784	0.2713	**2.2079**	0.5137	-0.1222	0.9341
Full-time vocational school	-0.6942	0.6825	1.7334	1.2030	–	–
Technical school	0.5980	0.5533	**3.7362**	0.8643	–	–
Polytechnic	**1.6924**	0.5314	**4.3327**	1.1994	–	–
College, University	**1.4155**	0.4378	**2.8327**	0.8348	–	–
Occupational Group						
Farming[3]	0.7099	0.4127	–	–	–	–
Mining, mineral extraction	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*	
Technical professions	0.3265	0.5378	1.1161	0.7589	–	–
Service professions	0.1881	0.1786	0.0018	0.2895	0.3875	0.5441
Other professions	0.1099	0.4068	*1.5070*	0.5877	1.2327	1.1498
Professional Rank[4]						
BC, not skilled worker	*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.4886	0.4575	0.6791	0.7294	0.3146	1.9828
WC, simple occupations	0.1196	0.2591	0.0414	0.4036	0.6139	0.7434
WC, advanced occupations	0.3046	0.3683	-1.4322	0.7623	0.7520	1.1769
Other	0.1861	0.1967	-0.1531	0.3179	0.5623	0.6279
Work experience	-0.0003	0.2187	**1.9150**	0.4281	-0.4089	0.5603
Duration of empl. (months)[5]	*-0.0033*	0.0014	-0.0019	0.0021	0.0016	0.0047
Duration of Unemployment (Weeks)						
Up to 13 weeks	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	*-0.4308*	0.1935	0.1185	0.4791	0.3590	1.3316
More than 52 weeks	**-0.7119**	0.2464	**-1.5047**	0.5008	-0.5469	1.2949
No. of placement propositions	*0.0252*	0.0108	0.0003	0.0188	**0.0947**	0.0297
Last contact[6]	*0.0666*	0.0333	-0.0141	0.0533	0.1204	0.0762
Vocational rehabilitation[7]	-0.0367	0.3706	**-1.7651**	0.5478	-0.7359	0.6378
Placement restrictions	-0.5638	0.3129	0.5361	0.8848	**-2.1334**	0.8297
Programme Before Unemployment						
No programme	*Reference*		*Reference*		*Reference*	
VT[8], further training	0.1747	0.2327	*-0.9468*	0.4170	0.0224	0.7509
VT, vocational adjustment	1.0132	0.6516	-1.5667	1.1074	–	–
Job-preparative measure	*1.6697*	0.7627	1.7796	1.0223	–	–
Job creation scheme	**2.7217**	0.2540	**1.3740**	0.3775	**3.2965**	0.7295
Rehabilitation measure	0.6358	0.5649	–	–	1.3382	0.8574
Regional Context Variables						
Cluster II	**-0.8182**	0.1862	0.1536	0.3822	**-1.7147**	0.6205
Cluster III	**-0.7933**	0.1810	0.1304	0.3822	**-1.7696**	0.6427

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Table B.8: (continued)

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster IV	-0.5212	0.2746	-0.4673	0.5963	*-2.3422*	1.1663
Cluster V	*Reference*		*Reference*		*Reference*	

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– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.9: Estimation Results of the Logit-Models for the Propensity Score for Men in East Germany

	Age $<$ 25		Age $>$ 50		Without professional training		Without work experience	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	13.5732	7.9058	**-238.8922**	16.9061	**-2.1066**	0.5394	**-2.6827**	1.0429
Age	-1.3078	0.7583	**8.7264**	0.6133	*-0.0554*	0.0228	0.0149	0.0417
Age (squared)	0.0269	0.0182	**-0.0805**	0.0056	**0.0008**	0.0003	-0.0003	0.0006
Married	-0.7581	0.5709	**0.3589**	0.0840	**0.3101**	0.0988	**0.5086**	0.1951
No. of children	0.4678	0.2619	-0.0139	0.0658	-0.0173	0.0481	-0.0482	0.1030
German	0.7945	1.0549	-0.0440	0.3721	**0.9481**	0.3140	1.3789	0.7295
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**2.0207**	0.5844	-0.5481	0.7512	0.2325	0.5445	1.0344	0.5790
50% to under 80%	1.0831	0.6371	-0.1421	0.2377	**0.7160**	0.2380	0.4732	0.3689
30% to under 50%	–	–	0.3587	0.2846	0.1333	0.4933	–	–
30% to under 50%, no equalis.[2]	1.4045	0.8426	-0.1966	0.2607	0.1666	0.3529	0.1653	0.4587
Other health restrictions	-0.0501	0.3147	**-0.3627**	0.1162	-0.1197	0.1320	-0.0002	0.2303
Professional Training								
No training, no CSE	*Reference*		*Reference*		–	–	*Reference*	
No training, with CSE	*-0.4693*	0.1943	0.2663	0.1672	–	–	-0.4111	0.2109
Industrial training	**-1.1019**	0.2146	-0.1078	0.1544	–	–	**-0.6138**	0.1965
Full-time vocational school	–	–	-0.0837	0.3697	–	–	–	–
Technical school	–	–	0.1356	0.1975	–	–	-1.0304	0.6838
Polytechnic	–	–	-0.1202	0.2834	–	–	0.0510	0.6065
College, University	–	–	-0.0309	0.2153	–	–	–	–
Occupational Group								
Farming[3]	0.5612	0.2898	-0.1539	0.1487	0.1625	0.1275	0.0487	0.3003
Mining, mineral extraction	–	–	-1.9224	1.0228	-0.0618	0.7523	–	–

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Table B.9: (continued)

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	-0.2571	1.0377	**-0.3047**	0.1356	-0.0833	0.2543	-0.0494	0.4547
Service professions	*-0.4135*	0.2085	**-0.3372**	0.0809	*-0.2137*	0.0874	*-0.3113*	0.1555
Other professions	**-1.2800**	0.4093	**-1.1671**	0.3980	**-1.0285**	0.3021	*-0.9041*	0.4002
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	**-1.0221**	0.2752	0.0183	0.0963	-0.1313	0.1219	*-0.4685*	0.2313
WC, simple occupations	-0.3466	1.0371	0.2383	0.1484	0.1547	0.2836	*0.9869*	0.4335
WC, advanced occupations	–	–	0.0183	0.2063	-0.3857	0.4608	0.1593	1.1088
Other	-0.3301	0.1716	-0.1418	0.0967	-0.1544	0.0888	-0.1220	0.1648
Work experience	-0.2366	0.1444	0.3674	0.2089	-0.1547	0.1154		
Duration of empl. (months)[5]	0.0015	0.0051	**-0.0040**	0.0004	**-0.0037**	0.0008	0.0003	0.0012
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	*0.3105*	0.1576	**0.3536**	0.0990	0.1483	0.1008	0.0992	0.1585
More than 52 weeks	**1.6580**	0.2336	0.0397	0.1021	*0.2620*	0.1099	*0.4405*	0.1957
No. of placement propositions	**0.0721**	0.0165	**0.0862**	0.0066	**0.0719**	0.0061	**0.0511**	0.0122
Last contact[6]	**-0.1381**	0.0479	**-0.1497**	0.0193	**-0.1904**	0.0242	**-0.1620**	0.0388
Vocational rehabilitation[7]	*0.7648*	0.3325	0.0510	0.1885	0.2319	0.1790	**0.6629**	0.2306
Placement restrictions	-0.3430	0.3308	**-0.4122**	0.1493	*-0.3244*	0.1644	-0.2742	0.2317
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	0.3034	0.3269	**0.7066**	0.1195	0.1444	0.1281	0.3408	0.1986
VT, vocational adjustment	0.9493	0.5581	**0.6153**	0.1483	**0.5512**	0.1773	**1.0850**	0.3200
Job-preparative measure	0.3428	0.5354	–	–	0.3456	0.6045	0.7811	0.6154
Job creation scheme	**2.0412**	0.2293	**1.5890**	0.0885	**1.3481**	0.0997	**1.7503**	0.1937
Rehabilitation measure	-0.3730	0.7565	0.2735	1.0474	-1.2114	1.0192	0.1776	0.3833
Regional Context Variables								
Cluster Ia	**-1.4758**	0.2578	0.1526	0.2292	**-0.7788**	0.1977	**-1.6941**	0.2777
Cluster Ib	**-1.7657**	0.2366	-0.0174	0.2210	**-0.9016**	0.1856	**-1.6751**	0.2605
Cluster Ic	**-1.3678**	0.2889	-0.1565	0.2432	**-0.7590**	0.2097	**-1.4147**	0.3041
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.10: Estimation Results of the Logit-Models for the Propensity Score for Men in East Germany

	Long-term unemployed		**More than 5 plac. prop.**		**Vocational rehabilitation**		**Placement restr.**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-5.9168**	0.7221	**-4.9170**	0.6898	-1.4614	1.1090	**-4.3694**	1.3654
Age	**0.1591**	0.0293	**0.0800**	0.0271	-0.0120	0.0520	0.0617	0.0401
Age (squared)	**-0.0020**	0.0003	-0.0005	0.0003	0.0001	0.0007	-0.0008	0.0005
Married	**0.3925**	0.0807	**0.2883**	0.0778	0.2902	0.1863	**0.3837**	0.1341
No. of children	-0.0526	0.0417	-0.0224	0.0399	0.0532	0.0923	-0.0683	0.0727
German	0.4366	0.2926	-0.1056	0.2911	–	–	0.8685	1.0224
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	-0.1608	0.5295	0.3180	0.6840	–	–	–	–
50% to under 80%	-0.1127	0.2237	**0.7183**	0.2319	-0.0707	0.4813	-0.0690	0.3126
30% to under 50%	-0.4211	0.4022	**0.8056**	0.3024	-0.2961	0.5867	-0.0067	0.3811
30% to under 50%, no equalis.[2]	-0.5038	0.2755	0.0957	0.3228	-0.8062	0.5362	*-0.7772*	0.3661
Other health restrictions	**-0.4426**	0.1145	**-0.3411**	0.1163	-0.6332	0.4507	*-0.6473*	0.2944
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	0.1208	0.1268	-0.0052	0.1391	-0.2495	0.3086	0.0710	0.2214
Industrial training	-0.0439	0.1133	-0.1906	0.1243	-0.0820	0.2646	-0.0142	0.1984
Full-time vocational school	0.0365	0.3729	-0.2389	0.3338	0.7135	0.7082	0.1636	0.5937
Technical school	0.2924	0.2018	-0.0869	0.2028	–	–	-0.7370	0.5265
Polytechnic	0.1120	0.3498	-0.2883	0.3236	–	–	-1.0257	1.0923
College, University	0.1630	0.2297	-0.4449	0.2321	–	–	-0.3160	0.5791
Occupational Group								
Farming[3]	0.1972	0.1263	0.1527	0.1455	*0.6934*	0.3243	*0.4980*	0.2249
Mining, mineral extraction	-1.0655	0.7196	–	–	–	–	0.3790	1.0674
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	*-0.3399*	0.1708	0.1450	0.1568	0.6047	0.4503	0.3676	0.3206
Service professions	**-0.2181**	0.0747	-0.0764	0.0764	0.0848	0.1781	0.0167	0.1246
Other professions	**-1.1442**	0.3446	**-1.6202**	0.4620	**-1.4704**	0.4833	**-1.1446**	0.2997
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	0.1281	0.0966	-0.1348	0.0935	-0.4500	0.2732	-0.3090	0.1832
WC, simple occupations	-0.0490	0.1830	**0.4488**	0.1745	0.8930	0.5462	**0.9444**	0.2966
WC, advanced occupations	*-0.6612*	0.2746	0.1608	0.2631	0.6932	1.1194	-0.3722	0.7656
Other	*-0.1765*	0.0844	-0.1627	0.0837	-0.1348	0.1977	*-0.3282*	0.1403
Work experience	-0.1601	0.1188	-0.0097	0.1251	*-0.3903*	0.1775	-0.2143	0.1531
Duration of empl. (months)[5]	**-0.0036**	0.0004	**-0.0031**	0.0009	-0.0019	0.0014	**-0.0040**	0.0010
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	–	–	0.0494	0.0874	0.0180	0.2119	-0.0066	0.1530
More than 52 weeks	–	–	-0.0787	0.0927	-0.4262	0.2262	*-0.3274*	0.1593
No. of placement propositions	**0.0443**	0.0051	–	–	**0.0605**	0.0134	**0.0494**	0.0092
Last contact[6]	**-0.0922**	0.0158	**-0.1120**	0.0185	**-0.1544**	0.0450	**-0.1693**	0.0339
Vocational rehabilitation[7]	0.0091	0.1478	0.0624	0.1791	–	–	**0.3212**	0.1154
Placement restrictions	-0.2586	0.1398	-0.2112	0.1496	0.0884	0.1869	–	–
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	0.1706	0.1017	**0.4341**	0.0909	0.3133	0.3342	**0.7741**	0.1874
VT, vocational adjustment	0.0241	0.1429	**0.5085**	0.1252	-0.2150	0.5447	*0.5275*	0.2675
Job-preparative measure	–	–	1.2273	0.7541	*1.7065*	0.8205	1.6120	1.1051
Job creation scheme	**0.7989**	0.0919	**1.3695**	0.0847	**1.6557**	0.2041	**1.4924**	0.1509
Rehabilitation measure	-0.0621	0.4301	*0.9425*	0.3869	0.0411	0.2647	0.4731	0.2598
Regional Context Variables								
Cluster Ia	0.2545	0.2270	*0.6670*	0.2733	-0.3128	0.4634	0.0172	0.3905
Cluster Ib	0.0321	0.2211	0.3889	0.2662	-0.1557	0.4389	0.2081	0.3741

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Table B.10: (continued)

	Long-term unemployed		More than 5 plac. prop.		Vocational rehabilitation		Placement restr.	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster Ic	0.3036	0.2364	0.4589	0.2774	-0.2369	0.4866	0.3785	0.3946
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.11: Estimation Results of the Logit-Models for the Propensity Score for Men in East Germany

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-5.0989**	0.6654	*-7.7446*	3.2242	**-10.8623**	1.1611	**-9.9124**	0.7484
Age	**0.1227**	0.0244	0.0226	0.1536	**0.2931**	0.0459	**0.2748**	0.0298
Age (squared)	**-0.0011**	0.0003	0.0005	0.0020	**-0.0031**	0.0006	**-0.0032**	0.0004
Married	**0.2217**	0.0741	0.1126	0.2091	0.1898	0.1092	**0.3323**	0.0917
No. of children	-0.0429	0.0393	0.0771	0.0977	-0.0021	0.0547	-0.0405	0.0478
German	0.4753	0.3544	–	–	0.5132	0.3358	*0.9348*	0.3905
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	0.7351	0.4746	*3.0140*	1.2926	-0.1103	1.0487	-0.3938	0.7343
50% to under 80%	**0.8839**	0.1847	*1.2876*	0.6372	0.6629	0.3538	0.1097	0.2670
30% to under 50%	**0.9190**	0.2500	**3.3576**	0.9152	0.4727	0.6344	0.4030	0.3962
30% to under 50%, no equalis.[2]	0.2166	0.2662	**1.9122**	0.6655	-0.5096	0.6004	-0.0596	0.3188
Other health restrictions	-0.0369	0.1019	-0.1425	0.3597	*-0.3453*	0.1726	**-0.3539**	0.1307
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	-0.0412	0.1224	–	–	-0.1720	0.3116	0.1684	0.1650
Industrial training	-0.0906	0.1070	-0.9326	0.8010	-0.0392	0.2970	**0.5971**	0.1667
Full-time vocational school	-0.4836	0.3778	–	–	-0.2246	0.6629	0.6243	0.4585
Technical school	0.2067	0.1728	0.1624	0.8212	0.6499	0.3649	**0.8531**	0.2334
Polytechnic	0.2536	0.2717	–	–	-0.1038	0.5720	*0.7773*	0.3485
College, University	0.1727	0.1827	0.0778	0.8267	0.2896	0.3900	**0.8098**	0.2471
Occupational Group								
Farming[3]	0.1965	0.1170	-1.4060	0.7412	-0.2877	0.2038	*0.2781*	0.1410
Mining, mineral extraction	-0.6182	0.7379	–	–	0.2509	0.7387	-1.4235	1.0175
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	*-0.3458*	0.1397	-0.1144	0.4372	-0.3830	0.2333	-0.2837	0.1710
Service professions	-0.0887	0.0717	0.0330	0.2023	**-0.3312**	0.1094	-0.1326	0.0845
Other professions	**-1.4777**	0.4609	–	–	*-2.2040*	1.0094	**-1.4322**	0.5112
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.1216	0.0908	*-0.6658*	0.2648	-0.1169	0.1256	0.0345	0.1037
WC, simple occupations	**0.4489**	0.1568	-0.2116	0.4530	0.0834	0.2420	0.1819	0.1802
WC, advanced occupations	0.2560	0.2268	-0.8872	1.0821	-0.3469	0.4238	-0.2334	0.2611
Other	**-0.4202**	0.0719	0.3472	0.2302	0.0380	0.1169	0.0027	0.0945

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Table B.11: (continued)

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Work experience	0.0032	0.1064	–	–	0.1907	0.3619	**0.8908**	0.1861
Duration of empl. (months)[5]	**-0.0050**	0.0009	-0.0011	0.0016	**-0.0055**	0.0009	**-0.0036**	0.0005
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	**-0.2765**	0.0799	**1.6114**	0.2328	**0.6552**	0.1102	-0.0994	0.0975
More than 52 weeks	**-0.6797**	0.0871	–	–	**1.2364**	0.1712	**-0.4695**	0.1233
No. of placement propositions	**0.0463**	0.0046	**0.3232**	0.0522	**0.0496**	0.0091	**0.0209**	0.0071
Last contact[6]	**-0.1133**	0.0163	-0.0743	0.0567	**-0.1167**	0.0277	**-0.1108**	0.0196
Vocational rehabilitation[7]	0.1646	0.1437	–	–	0.8126	0.4715	*-0.5639*	0.2709
Placement restrictions	*-0.3297*	0.1294	–	–	-0.0946	0.3216	**-0.7430**	0.1937
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], further training	–	–	–	–	0.2679	0.2048	-0.1562	0.1332
VT, vocational adjustment	–	–	–	–	*0.5768*	0.2944	-0.1245	0.1832
Job-preparative measure	–	–	–	–	–	–	–	–
Job creation scheme	–	–	–	–	**2.2036**	0.1922	**1.2987**	0.1220
Rehabilitation measure	–	–	–	–	*2.3296*	1.1037	–	–
Regional Context Variables								
Cluster Ia	0.2448	0.2372	1.3894	1.0294	0.2169	0.3002	-0.0223	0.2308
Cluster Ib	-0.0903	0.2316	1.2162	1.0198	-0.0967	0.2936	-0.1868	0.2233
Cluster Ic	0.0775	0.2429	0.5121	1.0770	-0.2939	0.3271	-0.2013	0.2453
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.12: Estimation Results of the Logit-Models for the Propensity Score for Men in East Germany

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-5.7408**	0.6956	**-4.8684**	1.4002	0.9850	2.4444
Age	**0.2167**	0.0310	**0.2325**	0.0512	0.0453	0.0953
Age (squared)	**-0.0025**	0.0004	**-0.0030**	0.0006	-0.0009	0.0012
Married	*0.2317*	0.0992	**0.5990**	0.1589	*0.7314*	0.2955
No. of children	0.0518	0.0506	*-0.2144*	0.0956	-0.0934	0.1705
German	0.0073	0.3647	0.9497	1.0661	-2.9748	1.5415
Health Restrictions[1]						
No health restrictions	*Reference*		*Reference*		*Reference*	
80% and over	0.5874	0.4630	0.7115	0.6061	-0.0150	0.9522
50% to under 80%	**0.6210**	0.2295	0.3372	0.3259	-0.1112	0.6539
30% to under 50%	0.6467	0.3389	0.0896	0.4346	-0.5540	0.9318
30% to under 50%, no equalis.[2]	-0.2788	0.3406	0.0343	0.3994	-0.5374	0.7698

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Table B.12: (continued)

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Other health restrictions	*-0.2746*	0.1326	-0.1540	0.2246	-0.6321	0.5269
Professional Training						
No training, no CSE	*Reference*		*Reference*		*Reference*	
No training, with CSE	-0.0123	0.1350	-0.1723	0.1800	-0.3596	0.3057
Industrial training	**0.5892**	0.1473	0.2597	0.2175	0.2669	0.3701
Full-time vocational school	0.3653	0.4439	0.6573	0.8598	0.0300	1.2223
Technical school	**0.8203**	0.2395	-0.0111	0.4376	–	–
Polytechnic	0.6428	0.3884	0.3720	0.7452	–	–
College, University	**0.7710**	0.2656	-0.5230	0.6143	–	–
Occupational Group						
Farming[3]	0.0805	0.1584	0.3512	0.2337	-0.4037	0.6117
Mining, mineral extraction	–	–	0.5523	1.1274	–	–
Manufacturing	*Reference*		*Reference*		*Reference*	
Technical professions	-0.0706	0.1864	0.1261	0.3241	0.3841	0.8423
Service professions	-0.1788	0.0923	-0.1476	0.1437	0.2998	0.2796
Other professions	**-0.8137**	0.3046	*-1.1322*	0.4757	-1.8930	1.0448
Professional Rank[4]						
BC, not skilled worker	*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.0726	0.1199	-0.0011	0.2035	-0.0044	0.3957
WC, simple occupations	0.1425	0.2188	**0.8663**	0.3565	0.9885	0.7354
WC, advanced occupations	-0.1973	0.3152	-0.0360	0.5586	1.8340	1.2481
Other	*-0.2122*	0.1013	-0.3035	0.1556	0.0027	0.2933
Work experience	**0.5822**	0.1426	**0.8357**	0.2087	0.2454	0.3230
Duration of empl. (months)[5]	**-0.0042**	0.0008	-0.0019	0.0011	-0.0049	0.0035
Duration of Unemployment (Weeks)						
Up to 13 weeks	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	*-0.2989*	0.1166	-0.2562	0.2153	-0.2725	0.5222
More than 52 weeks	**-1.1749**	0.1346	**-1.4876**	0.2342	-0.8675	0.5236
No. of placement propositions	0.0155	0.0080	0.0166	0.0127	*0.0468*	0.0212
Last contact[6]	**-0.1105**	0.0207	**-0.1611**	0.0361	**-0.2678**	0.0762
Vocational rehabilitation[7]	-0.2666	0.1792	**-0.6217**	0.2237	-0.2308	0.3561
Placement restrictions	**-1.2543**	0.1713	**-0.8460**	0.2498	-0.3965	0.5029
Programme Before Unemployment						
No programme	*Reference*		*Reference*		*Reference*	
VT[8], cont. education	**-0.5229**	0.1428	*-0.4673*	0.2356	-0.0503	0.4116
VT, voc. adjustment	**-0.5068**	0.1750	-0.1613	0.2666	-0.2973	0.5278
Job-preparative measure	0.3547	0.5584	-1.2863	1.0505	-0.0529	1.1475
Job creation scheme	**0.4971**	0.1318	0.3609	0.2141	**0.9353**	0.3627
Rehabilitation measure	-0.7411	0.4791	-0.0987	0.3616	-0.6435	0.5650
Regional Context Variables						
Cluster Ia	-0.1688	0.2543	**-1.2811**	0.3329	1.0778	1.1091
Cluster Ib	-0.4185	0.2447	**-1.1692**	0.3033	0.7015	1.0855
Cluster Ic	-0.0703	0.2619	**-0.9552**	0.3333	0.2774	1.1287
Cluster II	*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.13: Estimation Results of the Logit-Models for the Propensity Score for Women in East Germany

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work Experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	17.7318	9.5714	**-193.6120**	15.2633	**-4.6508**	0.6740	**-5.4542**	0.9700
Age	*-1.8032*	0.9166	**7.0812**	0.5562	**0.0797**	0.0244	**0.1079**	0.0376
Age (squared)	0.0422	0.0218	**-0.0659**	0.0051	**-0.0009**	0.0003	*-0.0012*	0.0005
Married	-0.4856	0.3740	*0.1752*	0.0692	0.0945	0.0702	**0.3683**	0.1144
No. of children	-0.0545	0.2476	0.0457	0.0767	-0.0394	0.0368	-0.0219	0.0608
German	–	–	0.7526	0.6024	0.8813	0.4577	0.7561	0.6046
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**2.3752**	0.9162	0.5872	0.5431	0.7035	0.6256	1.0744	0.6614
50% to under 80%	0.9588	0.8022	0.4074	0.2084	-0.0186	0.3095	0.6111	0.3954
30% to under 50%	–	–	**0.8819**	0.3079	**1.2322**	0.3669	1.0275	0.6575
30% to under 50%, no equalis.[2]	–	–	-0.1752	0.2989	-0.4625	0.4311	-0.2475	0.6249
Other health restrictions	-0.4905	0.4813	-0.1709	0.1031	**-0.3287**	0.1251	-0.2220	0.1994
Professional Training								
No training, no CSE	*Reference*		*Reference*		–	–	*Reference*	
No training, with CSE	0.5163	0.4127	0.0627	0.1480	–	–	0.2847	0.2837
Industrial training	-0.4776	0.4265	0.1293	0.1414	–	–	-0.0254	0.2743
Full-time vocational school	0.4411	0.8352	**0.7855**	0.2416	–	–	-0.4208	0.5562
Technical school	**1.7244**	0.5621	**0.5129**	0.1708	–	–	*0.8702*	0.3413
Polytechnic	1.5520	1.1829	**0.8068**	0.2879	–	–	1.0921	0.8527
College, University	–	–	**0.9729**	0.2282	–	–	0.3168	0.4883
Occupational Group								
Farming[3]	0.0411	0.3944	0.2139	0.1444	0.2184	0.1225	0.2822	0.2079
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	-0.1679	0.6728	**0.4442**	0.1411	0.3228	0.2129	-0.1591	0.3262
Service professions	-0.2629	0.2527	-0.0386	0.0788	-0.0885	0.0757	0.0212	0.1359
Other professions	-0.9534	0.5810	-0.8607	0.4697	-0.8312	0.4251	-0.5124	0.4853
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	-0.3429	0.3390	0.1272	0.0940	0.2160	0.1172	0.0958	0.1917
WC, simple occupations	0.2204	0.4453	0.1466	0.1057	**0.3840**	0.1449	0.1484	0.2504
WC, advanced occupations	–	–	-0.0759	0.1901	0.2284	0.3235	-0.0432	0.5182
Other	-0.1266	0.2596	-0.0040	0.0831	0.0512	0.0829	0.2369	0.1486
Work experience	-0.0505	0.1844	-0.1479	0.1408	*-0.2728*	0.1058	–	–
Duration of empl. (months)[5]	-0.0091	0.0074	**-0.0019**	0.0003	**-0.0029**	0.0006	-0.0019	0.0010
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.0076	0.2087	0.0088	0.0983	0.0718	0.1077	-0.1255	0.1456
More than 52 weeks	**0.9603**	0.2867	**-0.4272**	0.0955	-0.0147	0.1065	0.0708	0.1482
No. of placement propositions	*0.0463*	0.0200	**0.1470**	0.0074	**0.0959**	0.0071	**0.1023**	0.0094
Last contact[6]	-0.0470	0.0582	**-0.1242**	0.0166	**-0.1739**	0.0215	-0.0201	0.0259
Vocational rehabilitation[7]	0.4309	0.5532	*0.3754*	0.1851	0.3001	0.2189	0.0278	0.2592
Placement restrictions	-0.5051	0.5328	*-0.3492*	0.1398	**-0.5766**	0.1792	-0.3118	0.2452
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], cont. education	0.6073	0.3367	**0.6793**	0.0852	**0.4857**	0.0898	**0.4726**	0.1269
VT, voc. adjustment	0.0705	0.7552	**0.9768**	0.1289	**0.4891**	0.1751	*0.6610*	0.2677
Job-preparative measure	0.6113	0.6196	–	–	0.0680	1.0234	0.4889	0.7493
Job creation scheme	**2.4487**	0.3042	**1.8564**	0.0754	**1.2749**	0.0858	**1.5513**	0.1477
Rehabilitation measure	*1.3329*	0.6570	–	–	0.4775	0.6269	*0.9370*	0.4448
Regional Context Variables								
Cluster Ia	**-1.8211**	0.3194	0.0610	0.2301	*-0.4112*	0.2034	**-1.1810**	0.2736
Cluster Ib	**-2.2639**	0.2936	-0.0167	0.2241	**-0.6813**	0.1955	**-1.2800**	0.2635

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Table B.13: (continued)

	Age $<$ 25		**Age $>$ 50**		**Without professional training**		**Without work Experience**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster Ic	**-2.1955**	0.3935	-0.0874	0.2382	**-0.7156**	0.2149	**-1.2981**	0.3017
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.14: Estimation Results of the Logit-Models for the Propensity Score for Women in East Germany

	Long-term unemployed		**More than 5 plac. prop.**		**Vocational rehabilitation**		**Placement restr.**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-8.5335**	0.6292	**-5.9804**	0.7889	*-3.4940*	1.5118	**-5.9015**	1.6294
Age	**0.2325**	0.0225	**0.1161**	0.0254	0.0479	0.0672	**0.1325**	0.0505
Age (squared)	**-0.0028**	0.0003	**-0.0011**	0.0003	-0.0004	0.0008	**-0.0016**	0.0006
Married	0.0804	0.0478	0.0992	0.0571	0.3322	0.2017	**0.5260**	0.1325
No. of children	-0.0364	0.0254	0.0129	0.0305	-0.1086	0.1168	-0.1138	0.0754
German	0.5473	0.3466	0.4379	0.5385	–	–	0.2362	1.1186
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	0.5178	0.4759	**1.4518**	0.5573	–	–	–	–
50% to under 80%	*0.4365*	0.1969	*0.5807*	0.2347	**-1.4326**	0.5567	**-0.7914**	0.2946
30% to under 50%	0.4529	0.3158	*0.7554*	0.3569	-0.2376	0.6014	-0.5929	0.3551
30% to under 50%, no equalis.[2]	-0.3667	0.2868	0.2628	0.3228	*-1.5368*	0.5986	**-1.4669**	0.3575
Other health restrictions	-0.1379	0.0829	*-0.2702*	0.1102	**-1.4582**	0.4757	**-1.4201**	0.2704
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	*0.2601*	0.1111	*0.3509*	0.1640	0.5223	0.5107	0.6162	0.3679
Industrial training	**0.3229**	0.1042	**0.4682**	0.1550	0.7740	0.4846	**1.0380**	0.3500
Full-time vocational school	**0.8186**	0.1923	**1.0866**	0.2239	1.5601	0.8076	*1.1518*	0.5658
Technical school	**0.8768**	0.1338	**1.0290**	0.1791	0.9645	0.6850	**1.6763**	0.4136
Polytechnic	**1.1698**	0.2657	0.6220	0.3315	–	–	1.4532	0.8711
College, University	**0.7909**	0.1961	**0.9003**	0.2259	**2.0844**	1.0417	**1.7580**	0.5851
Occupational Group								
Farming[3]	**0.2647**	0.0959	0.1891	0.1247	0.4605	0.4937	-0.0783	0.3578
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	**0.3947**	0.1165	0.1993	0.1373	1.1130	0.5869	0.6039	0.3457
Service professions	-0.0181	0.0548	0.0205	0.0718	0.1787	0.2438	0.1047	0.1530
Other professions	**-1.2045**	0.4590	*-1.6496*	0.7227	*-1.1534*	0.5723	**-1.1029**	0.3940
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	0.1086	0.0749	0.0792	0.0900	0.4266	0.3127	0.1689	0.2051
WC, simple occupations	0.1047	0.0865	*0.2467*	0.1020	**0.8961**	0.4137	0.3767	0.2532

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Table B.14: (continued)

	Long-term unemployed		More than 5 plac. prop.		Vocational rehabilitation		Placement restr.	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
WC, advanced occupations	-0.2570	0.1618	-0.0078	0.2035	0.6756	1.1003	0.7158	0.4599
Other	-0.0286	0.0602	0.0828	0.0762	-0.1914	0.2424	-0.0977	0.1621
Work experience	-0.1475	0.0757	*-0.1722*	0.0869	0.0118	0.2266	-0.0730	0.1759
Duration of empl. (months)[5]	**-0.0023**	0.0003	**-0.0022**	0.0006	*-0.0036*	0.0015	**-0.0030**	0.0008
Duration of Unemployment (Weeks)								
Up to 13 weeks	–	–	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	–	–	0.1092	0.0858	-0.1973	0.2680	0.0806	0.1775
More than 52 weeks	–	–	-0.0971	0.0853	-0.4276	0.2680	-0.2595	0.1770
No. of placement propositions	**0.0751**	0.0044			**0.1030**	0.0194	**0.1177**	0.0119
Last contact[6]	**-0.0548**	0.0113	**-0.0448**	0.0145	**-0.2311**	0.0587	**-0.0845**	0.0308
Vocational rehabilitation[7]	0.0394	0.1473	0.0743	0.2060	–	–	**0.3679**	0.1259
Placement restrictions	**-0.4019**	0.1179	-0.0938	0.1520	0.0799	0.2267	–	–
Programme Before Unemployment								
No programme	*Reference*		*Reference*		*Reference*		*Reference*	
VT[8], cont. education	**0.3857**	0.0589	**0.3756**	0.0687	0.2770	0.4142	**0.5584**	0.1756
VT, voc. adjustment	**0.6921**	0.1017	**0.3540**	0.1173	0.2049	0.6470	0.5412	0.3001
Job-preparative measure	0.7818	0.6239	0.8678	0.6220	–	–	–	–
Job creation scheme	**1.1226**	0.0590	**1.3201**	0.0706	**1.3785**	0.2465	**1.5575**	0.1570
Rehabilitation measure	0.2434	0.4755	-0.0416	0.6348	0.5243	0.2984	-0.2402	0.4080
Regional Context Variables								
Cluster Ia	*0.4612*	0.1952	*0.5028*	0.2108	-0.1445	0.5643	-0.3686	0.3380
Cluster Ib	0.3262	0.1920	0.0424	0.2053	0.1129	0.5345	-0.4145	0.3203
Cluster Ic	0.1912	0.2033	-0.0727	0.2160	0.3546	0.5734	-0.0070	0.3423
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.15: Estimation Results of the Logit-Models for the Propensity Score for Women in East Germany

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-7.3543**	0.6275	-3.7688	2.2948	**-9.7937**	1.0491	**-10.3091**	0.7327
Age	**0.1833**	0.0203	-0.0896	0.1151	**0.2138**	0.0405	**0.3020**	0.0273
Age (squared)	**-0.0019**	0.0002	0.0016	0.0015	**-0.0026**	0.0005	**-0.0036**	0.0003
Married	**0.1552**	0.0465	0.0821	0.1429	0.1024	0.0725	0.0391	0.0584
No. of children	-0.0450	0.0256	**0.1926**	0.0684	-0.0315	0.0357	-0.0400	0.0314
German	*0.9316*	0.3952	–	–	0.1887	0.3497	0.6934	0.4241
Health Restrictions[1]								
No health restrictions	*Reference*		*Reference*		*Reference*		*Reference*	
80% and over	**1.3778**	0.4208	–	–	**1.5039**	0.5690	**1.1216**	0.4316
50% to under 80%	**0.6615**	0.1877	**2.1834**	0.6706	*0.8030*	0.3261	**0.8575**	0.2086
30% to under 50%	**0.8842**	0.2807	–	–	0.2386	0.6536	0.6925	0.3656
30% to under 50%, no equalis.[2]	-0.1998	0.2955	0.3870	1.0712	-0.1905	0.5179	0.2908	0.3040
Other health restrictions	-0.0561	0.0851	0.1333	0.2646	-0.2037	0.1386	-0.0568	0.0987

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Table B.15: (continued)

	ALMP part.		Target Score=0		Target Score=1		Target Score=2	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Professional Training								
No training, no CSE	*Reference*		*Reference*		*Reference*		*Reference*	
No training, with CSE	0.2187	0.1173	–	–	0.1501	0.3331	**0.4983**	0.1725
Industrial training	*0.2450*	0.1103	-0.9665	0.6323	0.3714	0.3276	**0.9045**	0.1751
Full-time vocational school	**0.7326**	0.1877	-0.7729	0.8146	*0.9874*	0.3941	**1.4513**	0.2508
Technical school	**0.9459**	0.1305	-0.0033	0.6451	**1.3011**	0.3420	**1.4696**	0.1968
Polytechnic	**0.8606**	0.2319	–	–	*1.0864*	0.4466	**1.9362**	0.3069
College, University	**0.8461**	0.1719	-0.5305	0.6951	**1.2193**	0.3662	**1.5150**	0.2475
Occupational Group								
Farming[3]	**0.4204**	0.0880	0.0045	0.2898	**0.4083**	0.1364	**0.3249**	0.1119
Mining, mineral extraction	–	–	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*		*Reference*	
Technical professions	*0.2151*	0.1065	0.0668	0.3529	0.2143	0.1693	0.0291	0.1425
Service professions	*0.1150*	0.0564	0.1345	0.1737	0.0710	0.0869	-0.0484	0.0684
Other professions	*-2.4967*	1.0079	–	–	*-2.0183*	1.0131	*-1.2813*	0.5147
Professional Rank[4]								
BC, not skilled worker	*Reference*		*Reference*		*Reference*		*Reference*	
BC, skilled worker	*0.1685*	0.0713	-0.0391	0.1964	-0.1836	0.1092	0.1638	0.0898
WC, simple occupations	**0.3703**	0.0820	0.0097	0.2411	0.2255	0.1242	*0.2516*	0.1027
WC, advanced occupations	-0.0948	0.1713	0.0304	0.5593	0.1391	0.2542	-0.2057	0.2107
Other	**-0.3845**	0.0532	0.0766	0.1690	0.0276	0.0894	0.0692	0.0756
Work experience	-0.1155	0.0697	–	–	0.4064	0.2432	**0.6318**	0.1246
Duration of empl. (months)[5]	**-0.0028**	0.0004	**-0.0032**	0.0015	**-0.0030**	0.0006	**-0.0027**	0.0004
Duration of Unemployment (Weeks)								
Up to 13 weeks	*Reference*		*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.1212	0.0694	**0.7815**	0.1627	**0.3235**	0.0962	-0.0874	0.0851
More than 52 weeks	**-0.3214**	0.0691	–	–	**0.5843**	0.1402	**-0.6910**	0.1028
No. of placement propositions	**0.0806**	0.0043	**0.4758**	0.0376	**0.0899**	0.0099	**0.0491**	0.0070
Last contact[6]	**-0.0500**	0.0108	0.0289	0.0381	-0.0306	0.0186	**-0.0624**	0.0142
Vocational rehabilitation[7]	0.1415	0.1571	–	–	-0.1721	0.7493	-0.2613	0.2432
Placement restrictions	*-0.3044*	0.1220	–	–	-0.2999	0.2976	**-0.8847**	0.1649
Programme Before Unemployment								
No programme	–	–	*Reference*		*Reference*		*Reference*	
VT[8], cont. education	–	–	–	–	*0.3056*	0.1396	0.0264	0.0963
VT, voc. adjustment	–	–	–	–	-0.0498	0.2367	-0.1040	0.1456
Job-preparative measure	–	–	–	–	–	–	–	–
Job creation scheme	–	–	–	–	**1.5409**	0.1390	**1.0577**	0.0936
Rehabilitation measure	–	–	–	–	–	–	-0.7858	1.0412
Regional Context Variables								
Cluster Ia	0.2586	0.2121	0.3204	0.5300	**1.0593**	0.3699	0.2173	0.2188
Cluster Ib	0.0624	0.2089	0.1852	0.5200	**0.9867**	0.3658	0.0349	0.2143
Cluster Ic	-0.1564	0.2171	-0.4020	0.5754	*0.8377*	0.3803	-0.1919	0.2290
Cluster II	*Reference*		*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.16: Estimation Results of the Logit-Models for the Propensity Score for Women in East Germany

	Target Score=3		**Target Score=4**		**Target Score=5**	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Constant	**-9.4019**	0.9522	**-8.2956**	0.9689	-3.4244	2.3470
Age	**0.3279**	0.0300	**0.3380**	0.0480	0.1438	0.1008
Age (squared)	**-0.0040**	0.0004	**-0.0040**	0.0006	-0.0018	0.0012
Married	**0.2165**	0.0704	*0.2306*	0.1132	0.2905	0.2556
No. of children	-0.0684	0.0409	-0.0369	0.0706	-0.2178	0.1927
German	1.1553	0.7320	–	–	-0.9381	1.2628
Health Restrictions[1]						
No health restrictions	*Reference*		*Reference*		*Reference*	
80% and over	0.5004	0.5222	*1.4642*	0.5994	0.5876	1.1427
50% to under 80%	0.1031	0.2542	0.4059	0.3297	-0.7586	0.8327
30% to under 50%	0.7089	0.3768	*1.0000*	0.4307	-0.2938	0.9879
30% to under 50%, no equalis.[2]	-0.1779	0.3472	-0.2638	0.4280	–	–
Other health restrictions	*-0.2542*	0.1258	-0.3546	0.1989	-0.9858	0.6646
Professional Training						
No training, no CSE	*Reference*		*Reference*		*Reference*	
No training, with CSE	0.2433	0.1462	0.1489	0.1721	0.1807	0.3631
Industrial training	**0.9678**	0.1551	**0.9351**	0.2041	0.4043	0.4438
Full-time vocational school	**1.3885**	0.2807	**1.5385**	0.4891	–	–
Technical school	**1.6689**	0.1928	**0.9147**	0.3320	0.6527	0.8928
Polytechnic	*0.9845*	0.4491	**2.2644**	0.7013	–	–
College, University	**1.5700**	0.2741	*1.2290*	0.5758	–	–
Occupational Group						
Farming[3]	0.0744	0.1443	0.3742	0.2188	0.4983	0.6275
Mining, mineral extraction	–	–	–	–	–	–
Manufacturing	*Reference*		*Reference*		*Reference*	
Technical professions	0.3159	0.1637	**0.8039**	0.2839	1.0893	0.6660
Service professions	0.0388	0.0827	-0.0562	0.1293	0.4940	0.3101
Other professions	*-1.0677*	0.5161	-1.2010	0.7368	0.1282	0.8041
Professional Rank[4]						
BC, not skilled worker	*Reference*		*Reference*		*Reference*	
BC, skilled worker	*0.2333*	0.1096	0.2948	0.1844	0.7054	0.3654
WC, simple occupations	*0.2536*	0.1223	*0.5018*	0.2143	-0.4972	0.6897
WC, advanced occupations	0.1020	0.2281	-0.5324	0.4396	0.8921	1.2048
Other	0.0613	0.0903	0.1888	0.1453	0.0524	0.3099
Work experience	**0.4693**	0.1144	**0.5986**	0.1706	-0.2520	0.2902
Duration of empl. (months)[5]	**-0.0026**	0.0005	-0.0011	0.0008	-0.0045	0.0023
Duration of Unemployment (Weeks)						
Up to 13 weeks	*Reference*		*Reference*		*Reference*	
Between 13 and 52 weeks	-0.2099	0.1285	-0.3230	0.2729	-0.7341	0.5949
More than 52 weeks	**-1.0925**	0.1362	**-1.5612**	0.2742	-0.8532	0.5495
No. of placement propositions	**0.0392**	0.0077	**0.0461**	0.0122	0.0392	0.0271
Last contact[6]	**-0.1073**	0.0176	-0.0142	0.0242	**-0.2505**	0.0760
Vocational rehabilitation[7]	**-0.6310**	0.2032	-0.2549	0.2324	0.0968	0.3644
Placement restrictions	**-0.8055**	0.1704	**-1.0862**	0.2418	0.0211	0.6638
Programme Before Unemployment						
No programme	*Reference*		*Reference*		*Reference*	
VT[8], cont. education	*-0.2280*	0.1136	-0.1241	0.2088	0.7459	0.4421
VT, voc. adjustment	-0.0454	0.1499	0.1614	0.2658	0.8615	0.5636
Job-preparative measure	-0.9887	1.0263	0.1747	0.8019	1.0704	1.2199
Job creation scheme	**0.7500**	0.1114	**0.8692**	0.2045	**1.4599**	0.4315
Rehabilitation measure	-0.0674	0.4974	-0.2202	0.4681	0.0743	0.6860
Regional Context Variables						
Cluster Ia	-0.3988	0.2204	-0.2230	0.3446	-0.2794	0.8194
Cluster Ib	**-0.6045**	0.2136	-0.5581	0.3323	-0.1361	0.7871

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Table B.16: (continued)

	Target Score=3		Target Score=4		Target Score=5	
	Coeff.	**Std. Err.**	**Coeff.**	**Std. Err.**	**Coeff.**	**Std. Err.**
Cluster Ic	**-0.7589**	0.2305	-0.4487	0.3536	0.0587	0.8278
Cluster II	*Reference*		*Reference*		*Reference*	

Bold letters indicate significance at 1% level, *italic* letters refer to the 5% level.
– Variables not included in estimation due to collinearity to other variables, perfect prediction of participation decision or missing.
[1] Percentages refer to accepted degree of restriction.
[2] People with accepted degree of restriction, but no equalisation to other persons with the same DoR.
[3] Farming comprises plant cultivation, breeding and fishery.
[4] BC = blue-collar worker, WC = white-collar worker.
[5] Duration of last employment in months.
[6] Time since last contact to job-center in weeks.
[7] Attendant for vocational rehabilitation.
[8] VT = Vocational training.

Table B.17: Results of the Calculations for the Means of the Standardised Differences in Percent Before and After Matching

West	**Men**		**Women**	
	before	after	before	after
Main Group	14.62	2.51	16.08	3.20
Target Group				
Age $<$ 25	10.48	3.08	12.50	6.82
Age $>$ 50	17.82	5.83	20.48	6.62
Without professional training	14.31	3.29	16.79	4.25
Without work experience	14.02	5.69	15.93	6.36
Long-term unemployed (more than 52 weeks)	17.77	3.06	19.13	4.18
Number of placement propositions	8.28	1.95	11.42	4.00
Vocational rehabilitation	18.13	8.45	23.96	16.31
Placement restrictions	19.29	4.61	26.99	4.99
Participation in ALMP before unemployment	18.59	6.46	16.93	8.80
Target Scores				
0	15.58	10.10	14.16	6.73
1	10.51	3.93	14.25	5.79
2	15.30	2.42	16.36	4.51
3	21.40	3.72	25.06	4.42
4	26.25	3.81	31.58	5.68
5 and more	24.90	11.65	27.99	29.14

East	**Men**		**Women**	
	before	after	before	after
Main Group	12.01	1.78	10.83	1.60
Target Group				
Age $<$ 25	14.74	4.94	13.73	8.90
Age $>$ 50	16.79	2.55	14.98	1.55
Without professional training	11.17	2.48	11.04	2.72
Without work experience	12.10	4.18	12.17	3.35
Long-term unemployed (more than 52 weeks)	13.55	2.00	11.61	1.69
Number of placement propositions	11.67	2.52	8.62	1.62
Vocational rehabilitation	12.88	4.38	15.87	5.87
Placement restrictions	15.35	3.91	18.37	3.11
Participation in ALMP before unemployment	13.20	4.82	10.62	3.08
Target Scores				
0	15.71	7.41	7.68	4.39
1	9.92	3.56	9.68	2.48
2	12.61	2.78	12.85	2.49
3	17.56	3.12	15.65	2.26
4	18.91	3.75	18.04	2.14
5 and more	16.80	4.69	22.51	8.84

B.2 Figures

Fig. B.1: Balancing of AGE within Strata

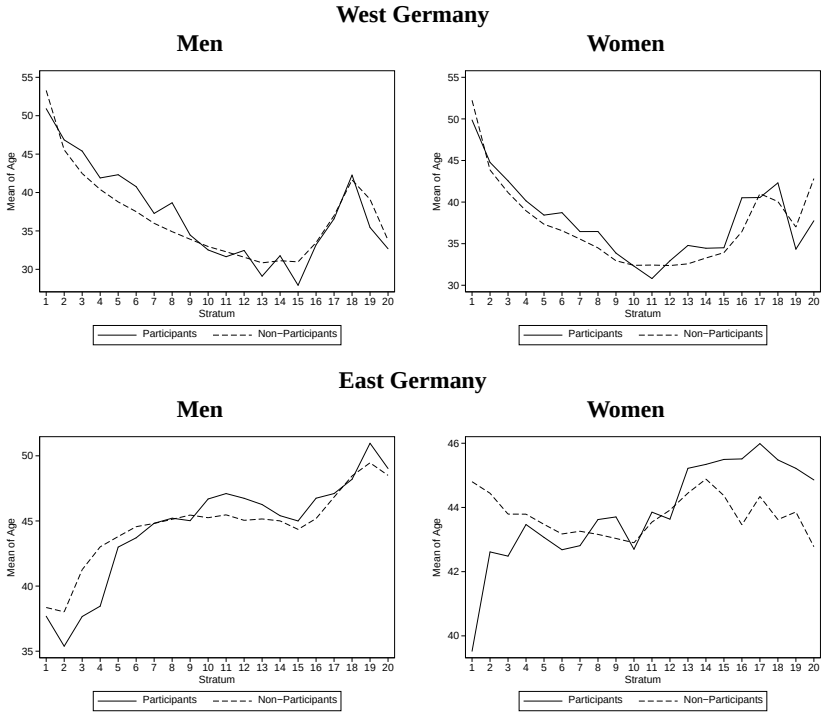


Fig. B.2: Balancing of MARITAL STATUS within Strata

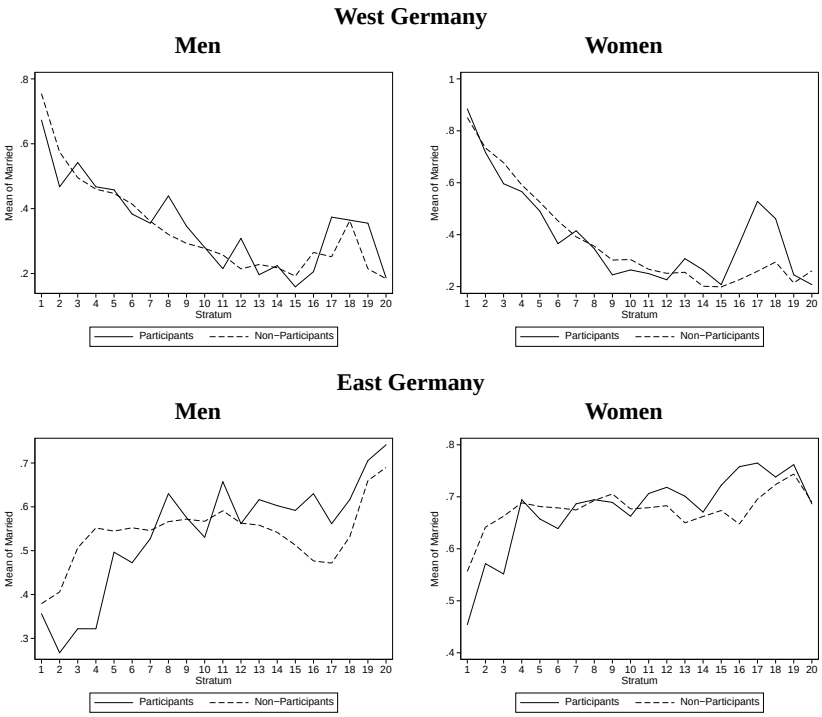


Fig. B.3: Balancing of PLACEMENT RESTRICTIONS within Strata

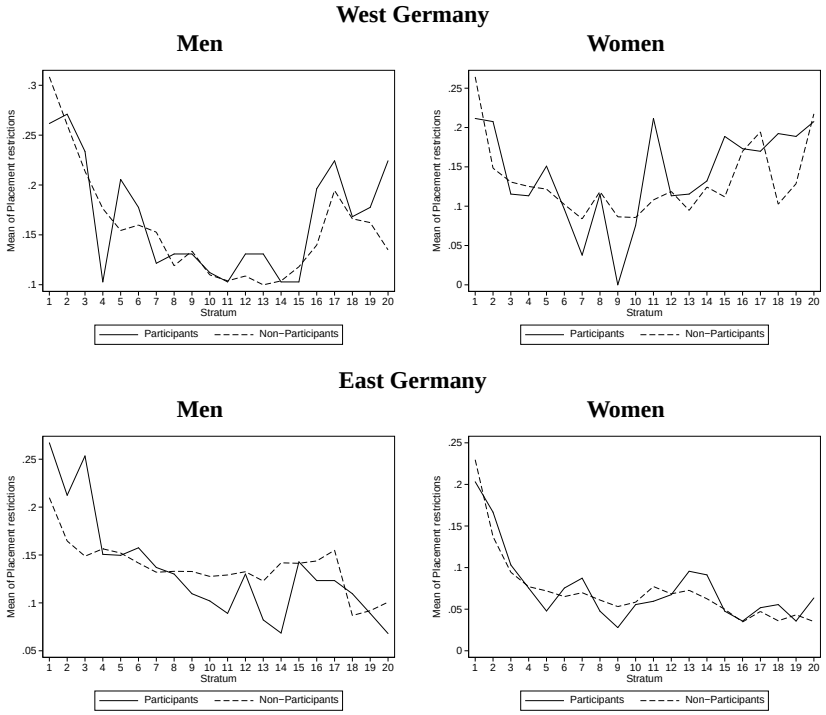


Fig. B.4: Balancing of NO. OF PLACEMENT PROPOSITIONS within Strata

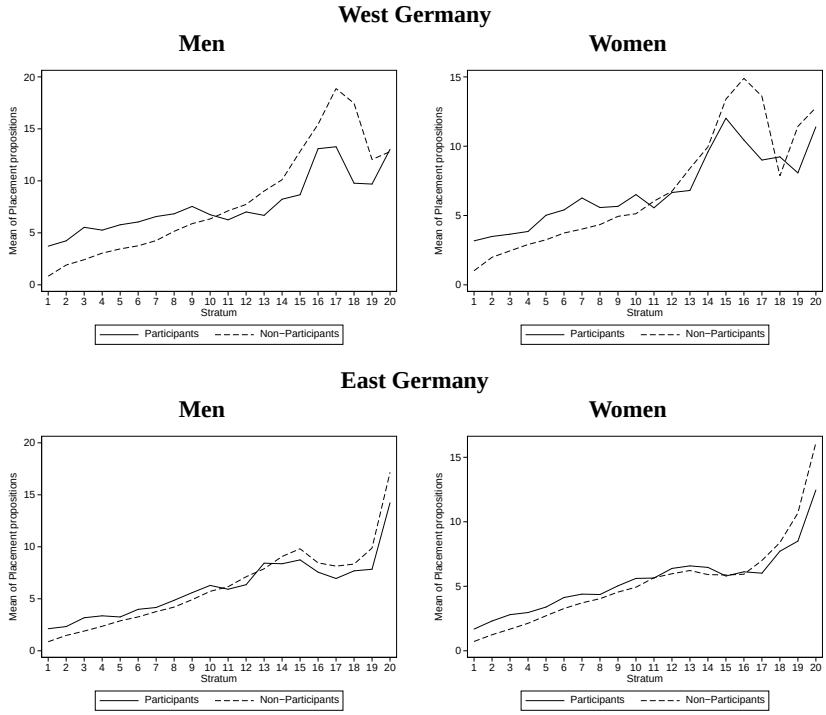


Fig. B.5: Balancing of DURATION OF LAST EMPLOYMENT within Strata

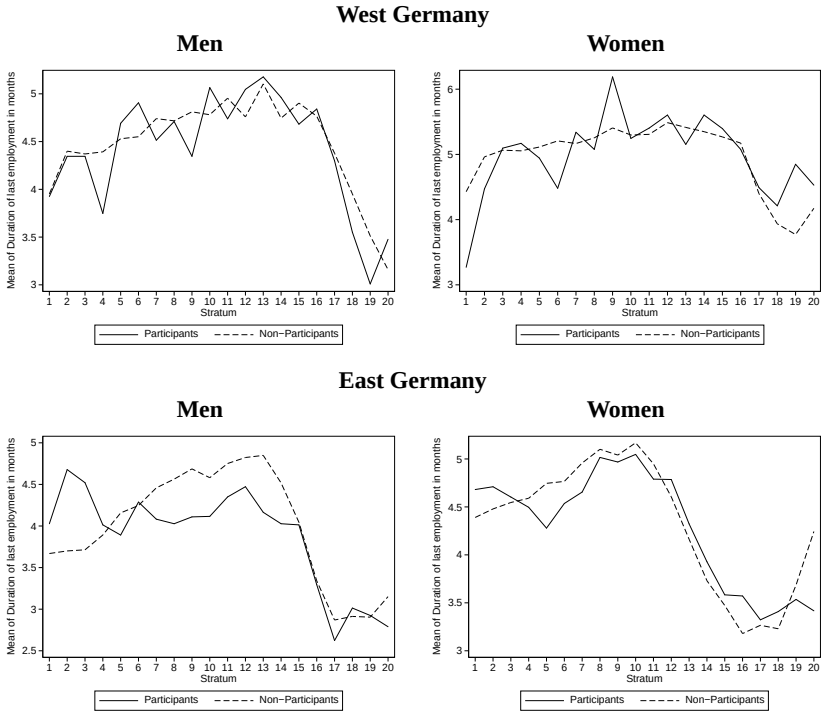
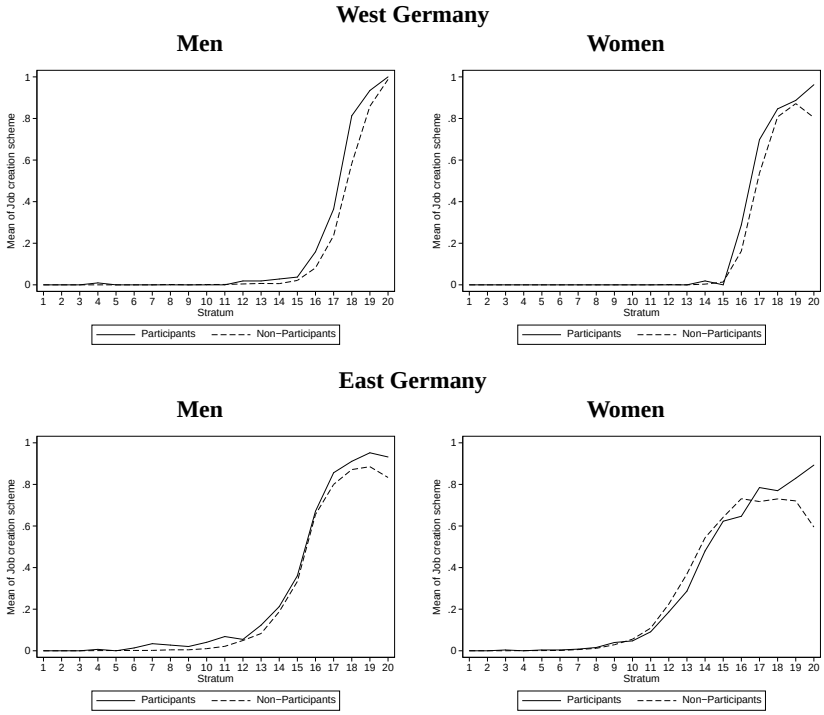


Fig. B.6: Balancing of JOB CREATION SCHEME within Strata



Abbreviations

AFG	Work Support Act (*Arbeitsförderungsgesetz*)
ALMP	Active Labour Market Policy
ATE	Average Effect of Treatment
ATT	Average Effect of Treatment on the Treated
ATU	Average Effect of Treatment on the Untreated
BAE	Before-After Estimator
BewA	Job-seekers Database (*Bewerberangebotsdatei*)
BSt	Employment Statistics Register (*Beschäftigtenstatistik*)
cDiD	Conditional Difference-in-Differences Estimator
CM	Calliper Matching
CMEA	Council for Mutual Economic Assistance
CrSE	Cross-Section Estimator
CSE	Certificate of Secondary Education (*Hauptschulabschluss*)
DiD	Difference-in-Differences Estimator
FDSS	Frontline Decision Support System
FEA	Federal Employment Agency
GDP	Gross Domestic Product
GDR	German Democratic Republic
GSOEP	German Socio-Economic Panel
IAB	Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung*)
JSCI	Job-seeker Classification Instrument
JTPA	Job Training Partnership Act
LEA	Local Employment Agency
LMM	Labour Market Monitor
LMM-SA	Labour Market Monitor Saxony-Anhalt
MMPH	Multivariate Mixed Proportional Hazards Model
MTG	Programme Participants' Master Data Set (*Maßnahme-Teilnehmer-Gesamtdatei*)
NN	Nearest-Neighbour Matching

OECD	Organisation for Economic Co-operation and Development
PEP	Public Employment Programme
SGB III	Social Code III (*Sozialgesetzbuch III*)
SGI	Service Group Indicator
SM	Stratification Matching
SOMS	Service and Outcome Measurement System
ST11	Programme Participants' Database of Subsidised Employment
UA	Unemployment Assistance
UB	Unemployment Benefits
UI	Unemployment Insurance
UK	United Kingdom
US	United States
WPRS	Worker Profiling and Reemployment System

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