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Active labour market policy in East Germany: waiting for the economy to take off

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Active Labour Market Policy in East Germany

Waiting for the Economy to take off

Michael Lechner, Conny Wunsch

Bundesagentur für Arbeit

Active Labour Market Policy in East Germany
Waiting for the Economy to take off

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Auch mit seiner neuen Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

Also with its new series "IAB Discussion Paper" the research institute of the German Federal Employment Agency wants to intensify dialogue with external science. By the rapid spreading of research results via Internet still before printing criticism shall be stimulated and quality shall be ensured.

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Abstract

We investigate the effects of the most important East German active labour market programmes on the labour market outcomes of their participants. The analysis is based on a large and informative individual database coming from administrative data sources. Using matching methods, we find that over a horizon of 2.5 years after programme start the programmes fail to increase the employment chances of their participants in the regular labour market. However, the programmes may have other effects for their participants that may be considered important in the especially difficult situation experienced in the East German labour market.

Keywords: Matching estimation, causal effects, programme evaluation, panel data

JEL classification: J 68

1 Introduction*

Over the last decade, Germany spent more than 7 billion EUR per year on active labour market policies (ALMP) to combat the large and persistent unemployment problem in East Germany. In this paper, we investigate the effects of the most important parts of these policies on the labour market outcomes of their participants. The analysis is based on a large and informative individual database coming from administrative data sources and employs econometric matching methods. Concentrating on programmes that start between 2000 and 2002, we find that over a horizon of 2.5 years the programmes fail to increase the employment chances of their participants in the regular labour market. However, the programmes may have other effects, like keeping their participants occupied, that may, or may not, be worthwhile having in the special situation of the East German economy.

Although German Unification happened not too long ago, there is already a considerable literature about the effects of training and subsidised non-market jobs in East Germany, with mixed evidence though. Over time, the data quality of the studies increased considerably. The earlier studies for East Germany use survey data. These data are rather limited with respect to the length of the observation period, sample size and the availability of sufficiently detailed information to account for selectivity and programme heterogeneity.[1] Most of these studies find negative or insignificant short- to medium-term employment effects (e.g. Pannenberg, 1995; Hübler, 1998; Hujer and Wellner, 2000; Kraus, Puhani and Steiner, 2000), but there are also studies that obtain positive effects for some programmes

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[1] Pannenberg (1995), Steiner and Kraus (1995), Pannenberg and Helberger (1997), Fitzenberger and Prey (1998, 2000), Hübler (1997, 1998), Staat (1997), Kraus, Puhani and Steiner (1999, 2000), Lechner (1999, 2000), Prey (1999), Hujer and Wellner (2000), Eichler and Lechner (2002), Bergemann, Fitzenberger and Speckesser (2004), Bergemann (2005). These studies use the German Socioeconomic Panel or the Labour Market Monitor East or for Saxony-Anhalt (see the survey by Wunsch, 2005).

(e.g. Pannenberg and Helberger, 1997; Prey, 1999, Eichler and Lechner, 2002). The lack of robustness is due to the sensitivity of the results to different parametric assumptions, small sample sizes, and the inability to measure medium or even long-run effects, as well as problems in appropriately defining programme and outcome variables.

The next group of papers use new administrative data explicitly developed for the evaluation of training programmes. As for the previous studies, these programmes started in the early years after unification. Speckesser (2004) and Fitzenberger and Speckesser (2005) analyse one special type of government-sponsored training programme. Based on propensity score matching, they find negative lock-in effects up to 12-18 months after programme start. Fitzenberger and Speckesser (2005) obtain positive employment effects of 5 to 10 percentage points about 20 months after programme start. Lechner, Miquel, and Wunsch (2005b) use matching methods to assess the effectiveness of three types of training programmes conducted 1993-1994 and follow outcomes over eight years after programme start. Besides the typical lock-in effects, they find strong positive medium- to long-run employment effects at a magnitude of 10-15 percentage points for short training courses and for women for the longer training programmes.

However, the problem with that specific data set is not only the limited sample size, the lack of detailed information on the specific type of training programme, and the lack of information on other programmes, but also the measures for the short- to medium-run labour market outcomes. The data do not allow distinguishing between unsubsidised employment in the regular labour market and subsidised employment, such as non-market jobs in employment programmes, which is in fact part of the active labour market policy.

The third generation of data used to evaluate active labour market policies in East Germany comes from the so-called 'integrated employment histories (IEH)' data base of the Institute for Employment Research (IAB). These data are used in this paper as well. Compared to the previous administrative data available for East Germany, the IEH covers a larger sample, contains much more detailed programme and outcome information, and improves substantially the information about the selection proc-

ess. Due to the latter, all papers using this data so far are based on a selection on observables strategy to identify the causal effects of the programmes. Almost all employ some sort of semiparametric matching estimator. The general disadvantage of this database, which covers programmes and outcomes from 2000 to mid 2005 in its most recent version, is that only short to medium-term outcomes are available. This limited time horizon is the price to pay when interest is in recent programmes. For example, in our study, which is based on matching estimation as well, we consider programme participation between 2000 and 2002. Thus, we observe outcomes for all participants only up to 2.5 years.[2] However, as shown by Lechner, Miquel, and Wunsch (2005a, 2005b), after 30 months we can already get a reasonably accurate idea about the magnitude of possible long-term effects, at least for the shorter programmes. All papers analysing recent programmes have (and have to have) a similarly short or even shorter time horizon.

There are two groups of papers based on this database so far, depending whether they evaluate training programmes or employment programmes. Concerning the training programmes, Hujer, Thomsen, and Zeiss (2006) analyse the effects of programmes conducted in the period 2000-2002 on the transition rate into regular employment. Methodologically, this paper is an exception, because it is not based on matching estimation. Instead, the authors estimate a multivariate mixed proportional hazard rate model. Because of the short time horizon available to observe outcomes in their study, the negative lock-in effects drive their results. Based on similar data, Biewen, Fitzenberger, Osikominu, and Waller (2006) analyse the effects of three broad groups of training programmes for participants in 2000 until 2001 using matching methods. The authors conclude that about 20 months after the start of the programmes, there are no or only very small effects that are hard to pin down precisely.[3]

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[2] Going beyond that time horizon would imply dropping late starts. Thus, the differences after 2.5 years compared to the earlier results would reflect differences in the composition of participants, content of the programmes as well as effects that need longer time to materialise. Since such a composite effect is difficult to interpret as a policy parameter, we refrain from presenting these numbers.

[3] There is also the report for the government about the recent labour market reform in 2004 as suggested by the so-called Hartz-Kommission (see Schneider, Brenke, Kaiser,

Using a different version of the IEH, several papers by Caliendo, Hujer, and Thomson (2004, 2005a, 2005b) analyse the effects of employment programmes by comparing participants in February 2000 with eligible nonparticipants in the same month. Based on matching methods, they conclude that after 3 years the programmes did not improve the employment chances of their participants.

Our paper contributes in several dimensions to a better understanding of the individual effects of East German labour market programmes. First, this is the first study looking jointly at the effects of a large variety of training programmes as well as two employment programmes, allowing interesting comparisons across programme types. We do not only compare the programmes to some nonparticipation state, but also compare them with each other. The latter comparison gives interesting hints about the effectiveness of the caseworkers' allocation of different participants into different programmes. Second, we find interesting differences of the effects of the programmes with respect to individual heterogeneity of their participants: For example, their effects are much worse for individuals who have good pre-programme labour market prospects. Third, compared to the studies looking at post-unification training programmes, we have much larger samples, better information on the type of programmes, on individual labour market outcomes, as well as on the selection process into the programmes. Thus, we obtain considerably more comprehensive and more robust results as before. Finally, the paper contains new interesting findings that appear to be of policy relevance. Those results can be summarised as follows:

Programme participation leads to increased unemployment, more programme participation, and increased benefit receipt.

With respect to the chances in the regular labour market, some programmes actually harm participants, while other programmes did at least not increase the chances of their participants. There is so far no sign that there will be any positive long-term effects.

Steinwede, Jesske, Uhlendorff, 2006) which contains some hints about possible effects of training in East Germany. Those hints clearly provide no robust evidence for positive effects.

The selection process into the different programme types was not optimal, because a different allocation of participants among the programmes would have improved employment. Furthermore, too many people with intact labour market chances end up in programmes. Those people fared worst among all participants.

From the analysis, it appears clear that in the very depressed labour market of East Germany, the recent active labour market policy did not help in reintegrating the unemployed back into the unsubsidised part of the labour market. If one pursues the view that active labour market policies are supposed to cure some malfunctioning of the labour market, than one is led to conclude that with such amount of malfunctioning as seen in East Germany, this cure is not strong enough. It appears instead that other more substantial changes may be required that attack the roots of the problems, and not only its symptoms. However, these findings do not necessarily imply that the programmes had no positive effects on their participants, it just implies that those effects are probably in a different sphere (receiving earnings from work instead of benefits, having a daily routine, etc.) than earnings and employment in the regular labour market.

The remainder of the paper is organised as follows: The next section briefly reviews the economic and institutional environment of the East German labour market. Section 3 describes the data. Section 4 outlines our approach to identification and estimation of the programme effects. Section 5 contains the results from the econometric matching estimations. Section 6 discusses some of the sensitivity and heterogeneity checks conducted. The last section draws policy conclusions. Appendix A gives more information on the data used. Appendix B contains some more details on the econometrics applied, while Appendix C contains additional results not presented in the main body of the text. Finally, an appendix that is available in the internet contains more detailed background material concerning estimation, data, and results.

2 Economic conditions and labour market policy in East Germany

2.1 Economic development since German Unification

After the near-collapse of the East German economy following German Unification (by December 1990, production of goods had dropped to 46% of its 1989 level; Akerlof et al., 1991), the East German work force had declined by almost 3 million people in 1991 (BA, 2001). A substantial part of these people was directly absorbed by active labour market programmes to the effect that the official unemployment rate - which does not include participants in ALMP – is not skyrocketing. Furthermore, many older people left the labour force encouraged by generous early retirement schemes. In spite of this, registered unemployment rose rapidly to a rate of more than 10 per cent in 1991 (BA, 1992). Since then, the East German economy has been recovering only slowly. Unemployment has risen steadily and has become very persistent with a fraction of long-term unemployed of 40% in 2005. The fraction of young people and persons with low education or health problems has also increased steadily while female unemployment is declining, because more women leave the labour force.

Table1: GDP growth and unemployment since 1993

	1993	1995	1997	1999	2001	2003	2005
GDP growth	12.6	6.3	1.9	2.8	1.1	1.0	-0.1
Unemployment rate	15	15	19	19	19	20	19
Thereof:							
Women	65	64	58	54	51	49	47
Non German	3	4	4	4	5	5	6
No professional degree	23	21	21	24	24	23	26
Age < 25	12	11	12	12	13	12	14
Age ≥ 55	8	16	20	21	15	10	12
Unemployed for more than 1 year	31	29	30	32	35	43	40
Health problems	10	14	16	19	21	21	24

Note: Entries are in percent.

Sources: Statistische Ämter der Länder (2006), BA (1992-2006).

2.2 Unemployment insurance in Germany 1998-2004

In Germany, unemployment insurance (UI) is compulsory for all employees with more than a minor employment including apprentices in voca-

tional training.[4] German UI does not cover self-employed. Persons who have contributed to the UI for at least 12 months within the three years preceding an unemployment spell are eligible for unemployment benefits (UB). The minimum UB entitlement is six months. In the period we consider, the maximum claim increases stepwise with the total duration of the contributions in the seven years before becoming unemployed, and age, up to a maximum of 32 months at age 54 or above with previous contributions of at least 64 months.[5] Actual payment of UB for eligible unemployed is conditional on active job search, regular show-up at the public employment service (PES), and participation in ALMP measures. Since 1994, the replacement rate is 67% of previous average net earnings from insured employment with dependent children, and 60% without.

Until 2005, unemployed became eligible for unemployment assistance (UA) after exhaustion of UB. In contrast to UB, UA was means tested and potentially indefinite. However, like UB, UA was proportional to previous earnings but with lower replacement rates than UB (57% / 53% with / without dependent children, respectively). Unemployed who were ineligible for UB and UA could receive social assistance, which was a fixed monthly payment unrelated to previous earnings, means tested and administered by local authorities.

2.3 East German ALMP 1998-2005

Directly after unification, short-time work, which is a reduction in work hours combined with a subsidy from the unemployment insurance system to compensate the resulting earnings loss, subsidised non-market jobs (so-called job creation schemes, JCS), and further vocational training (FVT) was used on a rather massive scale. In recent years, however, the focus shifted towards the internationally more common minor adjustments of skills in short so-called training measures (which are much cheaper than FVT). Furthermore, direct temporary wage subsidies as well as the

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[4] However, civil servants (Beamte), judges, professional soldiers, clergymen and some other groups of persons are exempted from contributions. For further details on the German UI and ALMP, see the comprehensive survey by Wunsch (2005).

[5] For example, a 40-year-old unemployed with at least two years of insured employment has a maximum claim of 12 months.

support of self-employment increased at the expense of subsidised non-market jobs (see Table 2).

Table 2: The most important instruments of ALMP in East Germany (1998-2005)

	1998	1999	2000	2001	2002	2003	2004	2005
	Expenditure in million EUR							
Total expenditure on ALMP	7920	8964	8620	8360	8265	7326	5042	2454
				Share in %				
Training measures (TM)	1	1	2	2	2	3	4	2
Further vocational training (FVT)	37	33	34	36	35	27	25	16
Short-time work	1	1	1	1	1	1	2	3
Job creation schemes (JCS)	35	32	31	25	22	18	19	7
Structural adjustment measures (SAM)	18	17	14	9	8	7	7	6
Temporary wage subsidies	2	3	5	7	10	12	12	8
Support of self-employment	2	2	3	3	3	6	14	36
Other	4	10	11	17	19	26	17	21
	Participation in 1000							
Training measures (TM)[a]	NA	167	191	227	332	376	400	287
Further vocational training (FVT)[a]	236	183	214	188	183	92	61	40
Short-time work[b]	34	27	24	27	45	35	29	25
Job creation schemes (JCS)[b]	151	168	153	123	92	70	65	36
Structural adjustment measures (SAM)[b]	162	180	98	67	58	40	28	12
Temporary wage subsidies[b]	NA	65	91	99	116	107	90	29
Support of self-employment[b]	NA	32	30	31	34	72	68	105

Note: NA: Not available. [a] Entries in 1000 persons. [b] Yearly average of stock in 1000 persons.
Source: BA (1992-2006).

One important feature of German ALMP is the large heterogeneity of training courses. Course contents, the amount of human capital added and planned durations vary considerably, particularly among FVT courses. With our data (see Section 3), we are able to account for heterogeneity in training measures and FVT in a detailed way.

Table 3: Descriptions of the programmes we evaluate

Programme type (acronym)	Description	Mean planned duration (days)
Short combined measures (SCM)	Acquisition of specific knowledge and skills	56
Jobseeker assessment (JSA)	Assessment of jobseekers ability and willingness to search for job and to work, basic job search assistance	45
Short training (ST)	Minor adjustment of skills	48
Job related training (JRT)	Combined off-the-job and on-the-job training in a specific field of profession	172
General further training ≤ 9 months GT-9M)	General update, adjustment and extension of knowledge and skills; mainly off the job, planned duration ≤ 9 months	173
General further training > 9 months (GT-9M+)	General update, adjustment and extension of knowledge and skills; mainly off the job, planned duration > 9 months	347
Degree course (DC)	Vocational training that awards a formal professional degree and that corresponds to regular vocational training in the German apprenticeship system	658
Job creation scheme (JCS)	Subsidised non-market jobs which are in the interest of the public	274
Structural adjustment measure (SAM)	Subsidised non-market jobs in economically weak regions	315

Note: Calculations of the mean planned durations are based on our evaluation sample (see Section 3.3).

Table 3 summarises the programme types we evaluate in our empirical analyses. Besides seven types of training courses, we evaluate the most important forms of subsidised non-market jobs. We do not include temporary wage subsidies and support of self-employment though, because our identification strategy (see Section 4) might not be valid for these programmes. Short-time work is not observable in our data.

Short combined measures (SCM) are a series of very short training courses aiming at removing specific minor skill deficits. *Jobseeker assessment* (JSA) courses have the main objective of assessing a jobseeker's availability, willingness, and ability for active job search or specific kinds of jobs or programmes, but they also provide basic job search assistance. *Short training* (ST) courses provide minor adjustments of skills. All three types of programmes belong to the category of so-called training measures (TM) and have durations of no more than three months with mean planned durations of below two months.

Job related training (JRT) combines off-the-job training with a substantial amount of on-the-job training in a specific field of profession, where the latter often takes place in a simulated work environment rather than a regular firm. The mean planned duration is about six months. *General training* (GT) subsumes the classical, mainly off the job, further vocational training courses which provide a general update, adjustment, and extension of knowledge and skills. Planned durations range from only a few months to up to two years. *Degree courses* (DC) provide a usually two-year training which is equivalent to an apprenticeship in the German apprenticeship system. It awards an officially recognised professional degree if completed successfully. JRT, GT, and DC belong to the category of further vocational training (FVT).

Job creation schemes (JCS) and *structural adjustment measures* (SAM) are subsidised jobs, which are outside of and should not compete with the regular labour market. JCS are targeted at unemployed with particularly bad employment prospects like the elderly or the long-term unemployed. SAM aim at smoothing the effects of large job losses in a region by absorbing the unemployed in subsidised employment. In both programmes, participants hold these jobs usually for about one year.

2.4 Interactions between programme participation and UI payments

One important feature of German labour market policy has always been that (most) programme participations extend the period in which unemployment benefits (UB) can potentially be drawn. The extension occurs either directly by explicitly counting programme participation in the same way as insured employment towards the acquisition of UB claims. Or it occurs indirectly by receiving a different form of benefit (so-called maintenance allowance, MA, during participation in FVT) of the same amount as UB (or UA) during participation without or only less than proportionately reducing the UB claim at programme start. Table 4 summarises the respective rules.

Table 4: Programme participation and accumulation of benefits

Year	Programme	Rules
Until 1997	FVT	Receipt of MA if eligible; UB claim stays constant; counts in the same way as insured employment
	JCS	Regular salary, no benefits; counts as insured employment
1998-2002	TM	Receipt of UB or UA if eligible; UB claim reduced by the programme duration
	FVT	Receipt of MA if eligible; UB claim stays constant; entitlement qualification period extended by up to 2 years
	JCS, SAM	Regular salary, no benefits; counts as insured employment
2003-2004	TM	Receipt of UB or UA if eligible; UB claim reduced by the programme duration
	FVT	Receipt of MA if eligible; UB claim reduced by half of the programme duration; entitlement qualification period extended by up to 2 years
	JCS, SAM	Regular salary, no benefits; no longer counts as insured employment
Since 2005	TM	Receipt of UB or UA if eligible; UB claim reduced by the programme duration
	FVT	Receipt of UB or UA if eligible; UB claim reduced by half of the programme duration
	JCS, SAM	Regular salary, no benefits; does not count as insured employment

Note: TM and SAM have been introduced in 1998. The regular entitlement qualification period are the three years before the beginning of an unemployment spell in which the duration of insured employment is counted for the acquisition of an UB claim. At least 12 months of insured employment within this period are needed to acquire a new UB claim and the total claim increases with the duration of insured employment in the seven years before the beginning of an unemployment spell.

Since 1998, all major reforms of German labour market policy have reduced the possibilities to renew or extent UB claims by programme participation as legislators have increasingly become aware of the adverse effects these rules have on search intensity and the budget of the public employment agency (PES).

3 Data and definition of the evaluation sample

3.1 The data

We use a new administrative database that has been built up by the Institute for Employment Research (IAB). The database is a 2% random sample from all individuals who have been subject to German social insurance at least once since 1990. It combines information from four different administrative sources: social insurance records, programme participation data as well as the benefit payment register and the jobseeker register of the PES. Table A.1 in Appendix A summarises the main features of these data sources.

Besides being very recent, the database is very rich in terms of covariate information and observed pre-programme employment histories (at least 10 years) to control for selectivity in programme participation (see Section 4.1). Moreover, it covers participation in all major German active labour market programmes for the unemployed from 2000 to mid 2005, and the information about programmes is very detailed so that it is possible to account for programme heterogeneity in a uniquely detailed way.

Nevertheless, the database also has several drawbacks that may be important for the interpretation of our results. Firstly, information on direct programme costs is not available in the data. It is therefore not possible to consider the actual net effects of programmes. Secondly, prior to 2000 there is no explicit information on participation in ALMP except for benefit payment during participation in training. In particular, it is not possible to distinguish subsidised from non-subsidised employment. Thirdly, the common observation period after programme start is relatively short (only 2.5 years) since we are interested in relatively recent programmes conducted 2000-2002. Because of the rather long durations of some programmes (see Table 1), Lechner, Miquel, and Wunsch (2005a, b) show that the ability to measure long-run effects is crucial for the evaluation of German ALMP. However, their results also imply that after 30 months we can already get a reasonable idea about the magnitude of possible long-term effects, at least for the shorter programmes.[6]

3.2 Definition of our evaluation sample and programme participation

Our population of interest is defined by those unemployed who receive unemployment benefits (UB) or unemployment assistance (UA) and who are eligible for programme participation. According to German legislation, this is also the main target group of German ALMP. Our sample consists of the inflow into unemployment from insured employment or out of labour force between January 2000 and the first half of December 2002. If there are multiple entries into unemployment of a person in this period, we consider the first one as the sample inflow date.

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[6] The studies of Gerfin and Lechner (2002); Frölich, Lechner, and Steiger (2003); Lechner and Smith (2005) and Sianesi (2004) faced similar problems.

When choosing the appropriate subpopulation from our inflow sample into unemployment, we aim at having a homogenous group of people that covers the prime age part of the East German[7] population who is eligible for participation in the programmes under consideration. Therefore, we require that all individuals were employed[8] at least once before programme participation and that they received unemployment benefits (UB) or assistance (UA) in the month before the programme start (as well as in the month of potential programme start for nonparticipants).[9] To avoid most influences coming from retirement, early retirement, and primary education, we also impose an age restriction (25-49 years). Concentrating on the main body of the active labour force, we exclude unemployed who were trainees, home workers, apprentices, or without previous employment. Furthermore, we exclude unemployed with an intensity of the last employment before programme participation below half of the usual full-time working hours.

Note that drawing this subpopulation requires the use of variables measured relatively to the start date of the programme, which is only available for participants. Moreover, several variables potentially influencing both selection into programmes and outcomes should be measured relatively to the start of the programme. In this paper, we follow one of the approaches suggested by Lechner (1999, 2002b) to simulate start dates for nonparticipants. We regress the log start date of participants on a set of time invariant personal and regional characteristics and use the estimated coefficients plus a draw in the residual distribution to predict start dates for nonparticipants.

We define *participants* as those unemployed who participate at least once in a programme in the three years from the inflow into our sample. Accordingly, *nonparticipants* are all persons who do not enter a programme

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7 We exclude Berlin.

8 'Employed' means that we observe the person at least once in insured employment in the data.

9 In fact, receipt of UB or UA directly before entering a programme is not sufficient to ensure eligibility. Individuals must also have a formal professional degree or at least three years of work experience. Thus by also requiring individuals to be employed at least once before the programme, the remaining group of participants and nonparticipants is most likely to be eligible.

in this period. However, since we observe outcomes only up to mid 2005, we only evaluate the first participation of a person in a programme that occurs after the date of the inflow into the sample and before 2003.

3.3 Selected descriptive statistics

Table 5 presents descriptive statistics for selected variables. The numbers indicate that entry into the programmes is highly selective (for a full list of variables and statistics, see the internet appendix).

Table 5: Means and shares (in %) of selected variables

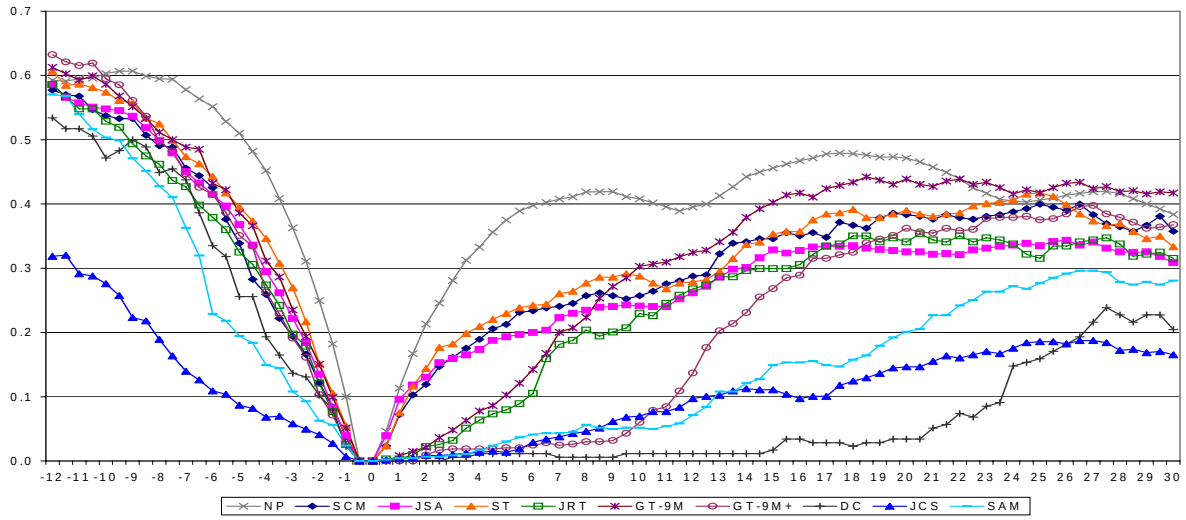
Treatment	NP	SCM	JSA	ST	JRT	GT-9M	GT-9M+	DC	JCS	SAM
Observations	4024	429	1066	549	313	605	533	176	587	463
					Personal characteristics					
Age (years)	38	38	37	37	37	38	38	34	40	38
Woman	38	45	40	44	36	29	43	40	34	28
No professional degree	11	10	10	7	8	7	6	14	10	9
Completed apprenticeship	85	81	88	86	89	88	81	82	87	89
University / polytechnic college degree	4	8	2	7	3	4	13	5	2	2
Health problems	14	11	11	9	10	9	9	11	19	7
					Characteristics of desired job					
Unskilled	30	28	27	22	26	21	18	38	31	24
Skilled	66	64	71	71	72	74	67	59	66	73
High-skilled	4	8	3	7	2	5	15	4	2	2
No work experience required	8	6	8	7	6	5	5	11	7	4
					Earnings of last job					
Monthly earnings (EUR)	1386	1400	1364	1447	1698	1445	1594	1382	1323	1343
	Remaining unemployment benefit claim at the beginning of the current unemployment spell									
No claim	50	57	45	50	38	27	26	40	65	34
Claim (days)	101	70	106	88	140	157	162	105	59	116
	Employment history over the 10 years before programme start									
Duration of current unemployment spell (months)	5	7	7	7	7	6	7	8	10	7
Fraction employed	66	66	65	69	66	70	70	64	58	68
Fraction unemployed	18	18	17	15	18	15	13	15	25	16
Fraction out of labour force	11	10	11	10	10	9	10	15	9	9
					Regional information					
Local unemployment rate ≤ 15%	8	9	8	9	6	10	8	10	4	6
Local unemployment rate > 25%	11	7	10	8	13	12	8	11	14	13

Note: If not stated otherwise, entries are in percent. All variables except the duration of the current unemployment spell are measured at or relative to the unemployment spell in which (simulated) programme start takes place. The duration of the current unemployment spell is measured at (simulated) programme start.

Women seem to be concentrated in SCM, ST, and GT-9M+ while GT-9M, JCS, and SAM exhibit a male bias. DC seems to be a device to provide younger and untrained unemployed with a first professional degree. JCS attracts a larger share of slightly older unemployed, unemployed with

health problems, low earnings, and long unemployment durations. It often takes place in the regions with the highest unemployment rates. The latter is also true for SAM and JRT. Participants in the latter seem to have, however, above average previous earnings. Nonparticipants differ from participants because of their lower current unemployment duration and their rather high fractions of untrained unemployed and unemployed with health problems.

Figure 1: Rates of unsubsidised employment before and after programme start (unmatched sample)



Note: Unsubsidised employment. Month zero is the (simulated) programme start. Negative values on the abscissa refer to months before programme start, positive values to the months after programme start.

In Figure 1, we show how nonparticipants and programme participants differ in terms of (unsubsidised) employment rates before and after programme start, and *before* correcting for any selectivity. By construction of our sample, the employment rates are zero at and in the period directly before programme start. Nonparticipants have substantially higher employment rates in the 10 months before their simulated programme start than all the different groups of participants. Participants in JCS exhibit particularly low employment rates before programme start while all other participants face rather similar rates though the rates of participants in DC and SAM seem to fall somewhat more rapidly six months before programme start. After the (simulated) programme start, none of the groups reaches its pre-programme levels. However, the employment rate of nonparticipants recovers quickly. For participants in the shortest programmes (SCM, JSA, and ST) there is also a steep ascent in the beginning but it be-

comes rather flat very early after programme start. For participants in both types of GT the ascent of the employment rate is somewhat delayed due to their longer durations but the development looks rather positive after completion of the programmes. The rates of participants in DC, JCS, and SAM recover only very slowly.

To get a better understanding of how selection into different programmes works with respect to employment prospects, we predict the employment chances the different groups of participants would have had without a programme conditional on a rich set of covariates. This prediction is based on a probit estimation of the employment chances of nonparticipants at the end of the observation window. For this purpose, we consider only employment that generates at least 90% of the earnings of the previous job. As explanatory variables, we use all variables that are important in the selection models for the different programme participations versus nonparticipation. This includes personal characteristics, variables that summarise individual pre-programme employment histories and regional characteristics.

In Table 6, we present various statistics for the predicted employment probabilities from this estimation. It shows that by various measures JCS received by far the most difficult cases in terms of reemployment chances (as already suggested by Figure 1), as opposed to the similar programme SAM whose participants appear to be very similar to the average, or even a bit better. The differences for the remaining groups are not that striking and there is a considerable heterogeneity within all programmes. Finally, the last column shows that the predicted nonparticipation employment chances are, as expected, negatively correlated with the predicted participation probabilities. However, given the official policies, these correlations are surprisingly small.

Table 6: Descriptive statistics for the predicted probability to be employed in a job with at least 90% of previous earnings in half-month 60 after programme start

Participation / Subsample	Acronym	Mean	Median	33%-Quantile	67%-Quantile	Correlation with participation probability**
Nonparticipation	NP	.27	.19	.10	.34	
Short combined measures	SCM	.27	.16	.07	.33	-.10*
Jobseeker assessment	JSA	.26	.20	.10	.31	-.04*
Short training	ST	.27	.19	.10	.31	-.02*
Job related training	JRT	.29	.21	.12	.35	-.02
General training ≤ 9 months	GT-9M	.32	.27	.15	.41	-.09*
General training > 9 months	GT-9M+	.30	.26	.14	.38	-.09*
Degree course	DC	.25	.18	.10	.27	-.02*
Job creation scheme	JCS	.17	.09	.03	.15	-.22*
Structural adjustment measure	SAM	.29	.24	.16	.34	-.05*
Total		.27	.19	.10	.33	

Note: Predicted probabilities from a probit estimation among nonparticipants. Dependent variable: Employed in unsubsidised employment with at least 90% of the earnings of the last job before programme start, measured in half-month 60 after programme start. * Correlation is significant on the 5% level. ** Predicted probability to participate in the respective programme or not to participate at all. Correlation computed in the population.

3.4 Measurement of the labour market outcomes

According to German legislation, the main objective of German ALMP is to reduce unemployment by improving the chances of the unemployed to find regular (unsubsidised) employment. However, since in East Germany there are particularly bad labour market conditions, other objectives like preventing or reducing human capital depreciation, keeping the unemployed attached to the labour market or providing social contacts and organised daily routines by "keeping them busy" in subsidised employment or training programmes without the direct prospect of finding a regular job have become non-negligible weight. Since in a situation with more than 20% of people not employed, providing a decent income for those people and avoiding social unrests may be other implicit goals of that policy.

We try to capture the different aspects of the potential effectiveness of the different programmes by considering a variety of outcome variables. The outcome *unsubsidised employment* measures the programmes' success in helping their participants to find regular employment. We also assess the quality of employment in terms of stability of the earnings compared to previous jobs as well as potential gains in productivity measured by actual earnings differences. In contrast, *registered unemployment*, which here includes programme participation, measures whether individual unem-

ployment is indeed reduced. The outcome *programme participation* assesses whether the programme participation we evaluate changes the probability of future programme participation in the same or a different programme.

We measure whether participants are better off in terms of *total earnings*, i.e. the sum of earnings from subsidised and unsubsidised employment and any benefits from the PES. In contrast, to assess some of the programme costs, *received benefits* measures the benefits and subsidies paid by the PES to the unemployed. This outcome variable includes all benefits (UB, UA, MA) received during participation in training courses and 60% of the wages from subsidised employment. The latter is a conservative proxy for subsidies paid by the PES, since that share is not directly observable in the data. In many cases, the subsidised fraction of the wage is certainly much higher.

We also assess whether the programmes succeed in keeping their participants busy through any form of employment or participation in any kind of programme. Finally, to enable the comparison with previous findings from earlier studies, we consider the outcome total employment that includes both subsidised and unsubsidised employment. In Section 5, we present the main findings from the different outcome variables and the different comparisons of the programmes. Table C.1 in Appendix C contains effects accumulated over the 2.5 years in which we observe the various outcome variables. Further results are available in the internet appendix.

All effects are measured half-monthly based on time relative to the start of the programme (with simulated start dates for nonparticipants): *Half-month 1* is the half-month after the programme started. Focusing on the beginning instead of the end takes into account the potential endogeneity of actual programme duration.

4 Identification and estimation

4.1 Conditional independence

We are interested in the average effects of the programme on the programme participants compared to participation in another specific programme or no participation at all. To identify these parameters we rely on

the conditional independence assumption to solve the selection problem that arises from the fact that persons in the different treatments differ systematically in a way that might be related to the outcome variables of interest (see Section 3.3). The assumption states that if we can observe all factors that jointly influence outcomes in the comparison state and the participation decision, then - conditional on these factors - participation and the outcomes, which the participants would have obtained in the comparison state, are independent, and the effects of interest are identified (Rubin, 1974; Imbens, 2000; Lechner, 2001, 2002a, b).

Selection into programmes is determined by three main factors: eligibility, selection by caseworkers and self-selection by potential participants. Eligibility is ensured by the choice of our evaluation sample (see Section 3.2). Given eligibility, based on an assessment of the employment prospects and the specific deficits or needs of the unemployed the caseworker decides - usually in consultation with the potential participant – about programme participation. According to German legislation, caseworkers have to take into account the chances of the unemployed for completing a specific programme successfully, and the situation in the local labour market. The latter is particularly important in East Germany. Therefore, we merged rich regional information to our data that allows us to control for local labour market conditions in a detailed way. This data contains information on the industrial, employment, population, and wealth composition of the region as well as migration streams, tax revenues and local unemployment rates. Individual variables in our data capturing information about employment prospects and chances for successful completion of a programme include age, educational attainment, family and health status, characteristics of the desired job as well as employment histories for at least 10 years before the programme. The latter include information on employment status, employers, earnings, position in previous job, specific occupation, and industry.

From the point of view of the unemployed, his decision whether or not to participate in a programme is guided by considerations very similar to those of the caseworker, but there are also additional reasons for joining or not joining a programme. If, for example, the unemployed sees no chance to find a job with or without a programme, he may prefer not to join a programme that reduces his leisure time. This again requires con-

trolling for all factors that determine individual employment prospects and labour market conditions. Moreover, legislation provides rather strong incentives to participate. On the one hand, unemployed who refuse to join a programme, risk suspension of their unemployment benefits. On the other hand, most programmes count towards acquisition of new unemployment benefit claims (see Table 2). Therefore, we include a variable that indicates the UB claim at the beginning of an unemployment spell.

The internet appendix, Table IA.1, contains a complete list of all variables that are available in the data. In contrast to administrative data previously available for Germany, we observe whether a jobseeker has health problems or a disability affecting employability. We also observe a set of characteristics of the job the unemployed is looking for, the number of placement propositions by the PES, as well as information on benefit sanctions and compliance to benefit conditions (e.g. attendance at interview with PES or cooperation with PES staff). Thus, though we are still not able to observe soft characteristics directly like motivation and ability of the unemployed, we have a set of previously unavailable important proxy variables and we are able to capture their indirect effects on pre-programme employment history that is starting effectively observed shortly after unification in 1990.

4.2 Estimation

All possible parametric, semi- and nonparametric estimators of treatments effects with observational data are built on the principle that for every comparison of two programmes, for participants in the programme of interest, we need comparison observations from the other programme with the same distribution of relevant characteristics. Characteristics are relevant if they jointly influence selection and outcomes (see Section 4.1 for these variables). Here, we use adjusted propensity score matching estimators for multiple treatments as our baseline estimator to produce such comparisons. A clear advantage of these estimators is that they are essentially nonparametric and that they allow arbitrary individual effect heterogeneity (see Heckman, LaLonde, and Smith, 1999, for matching with a binary treatment, and Imbens, 2000, and Lechner, 2001, for multiple treatments).

To obtain estimates of the conditional choice probabilities (the so-called propensity scores), which we use in our selection correction mechanism to form our comparison groups, we estimate probit models for all comparisons (all programme types against each other as well as nonparticipation). The analysis revealed that gender, age, qualification, and family status are important individual characteristics that determine participation. Furthermore, observed employment and unemployment histories are significantly correlated with participation choice. Moreover, the characteristics of the desired job an unemployed is looking for differ systematically among programmes. Regional information, such as the industrial, employment, and wealth composition of the region as well as tax revenues, which entered the probits in a highly disaggregated way to capture the specifics of supply and demand in the local labour market, play important roles in the selection process. Finally, remaining unemployment benefit claims indeed seem to provide rather strong incentives to enter a programme.

We use a matching procedure that incorporates the improvements suggested by Lechner, Miquel, and Wunsch (2005a). These improvements aim at two issues: (i) To allow for higher precision when many 'good' comparison observations are available, they incorporate the idea of calliper or radius matching (e.g. Dehejia and Wahba, 2002) into the standard algorithm used for example by Gerfin and Lechner (2002). (ii) Furthermore, matching quality is increased by exploiting the fact that appropriate weighted regressions that use the sampling weights from matching have the so-called double robustness property. This property implies that the estimator remains consistent if either the matching step is based on a correctly specified selection model, or the regression model is correctly specified (e.g. Rubin, 1979; Joffe, Ten Have, Feldman, and Kimmel, 2004). Moreover, this procedure should reduce small sample bias as well as asymptotic bias of matching estimators (see Abadie and Imbens, 2006) and thus increase robustness of the estimator. The actual matching protocol is shown in Table B.1. See Lechner, Miquel, and Wunsch (2005a) for more information on this estimator.

5 The effects of programme participation

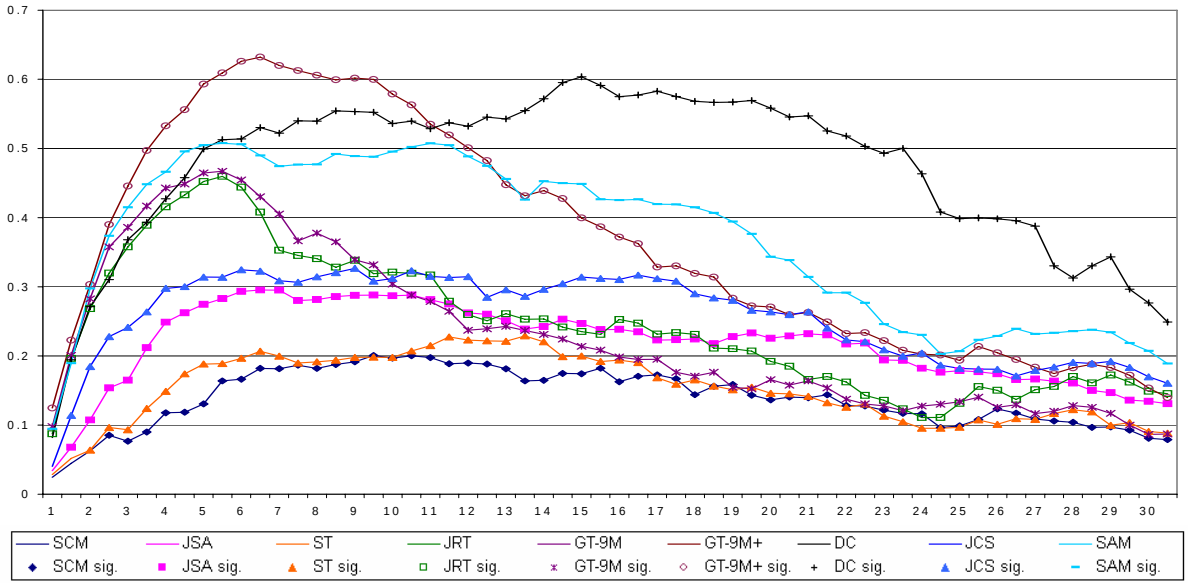
Below we present various figures displaying the average programme effects of the programme participants of the different programmes com-

pared to nonparticipation for various outcome variables. Each line in the respective figure represents a different programme and relates to the effects for the specific population of participants in that programme. Dots appear on a particular line if the effect is pointwise significant on the 5%-level. Outcomes are either measured in percentage points when they relate to changes in labour market status, or in differences of EUR when they relate to some earnings or income variable. The results are given for every half-month after the programme start, but the labeling on the corresponding axes refers to the respective month after the start of the programme. In the figures presented below, we only focus on the comparisons with nonparticipation. Extensive inter-programme comparisons, however, are available in the internet appendix of this paper, as well as in one of the following tables..

5.1 Programmes increase unemployment of their participants

Figure 2 shows the first of our key findings, namely that programme participation generally increases individual unemployment compared to nonparticipation. From the figure, we see that this effect differs substantially between the programmes, but there is not a single programme leading to a reduction in unemployment.

Figure 2: Effects of programme participation compared to nonparticipation: registered unemployment



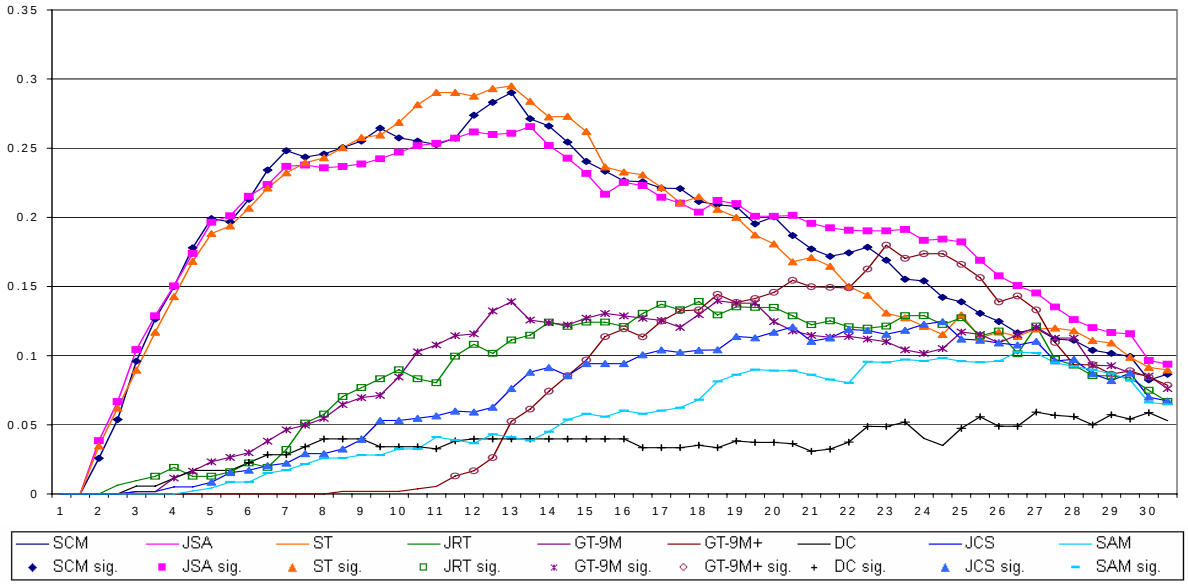
Note: Abscissa: Months after programme start. Ordinate: Effect in %-points. Each line represents the respective population of participants, which may differ for each programme. Dots indicate that the effect is significant on the 5% level (sig.).

Generally, the negative effects are worst in the beginning and decline somewhat over time. They are also worse for the longer programmes: Over the 30 months considered, participants in DC accumulate 14 (!) months of additional unemployment, with SAM 11 months, with JCS 8 months and with GT-9+ they accumulate 7 months (see Table C.1 in Appendix C for detailed results). The increase in the unemployment duration may well be due to the fact that all programmes increase the period in which benefits can be received by the unemployed.

5.2 Programmes keep participants busy and increase benefit receipt

Figure 3 shows that the programmes do not only increase the unemployment duration, but they also increase the likelihood of attending another programme in the future. This seems particularly true for the three types of the short training measures. In total over the 2.5 years after programme starts, these programmes accumulate each about 5-6 months of additional programme participation, whereas the other programmes add about 2-3 months of additional programme participation.

Figure 3: Effects of programme participation compared to nonparticipation: further programme participation



Note: See note below Figure 2. For programme participants we only consider further participations after the actual programme evaluated.

Table 7 shows in what kind of programmes unemployed participate. Non-participants also exhibit some programme participation after the 3-year window for which we require them not to participate, mainly in the cate-

gory other programmes, the largest fraction of which are temporary wage subsidies for regular jobs and support of self-employment. This category is also frequented by most of the participants who exhibit future participations.[10] Participation in the short training measures is often followed by GT and for JSA also by DC. Participants in GT, on the other hand, often participate in a JCS after the completion of GT.

Table 7: Further programme participation (%)

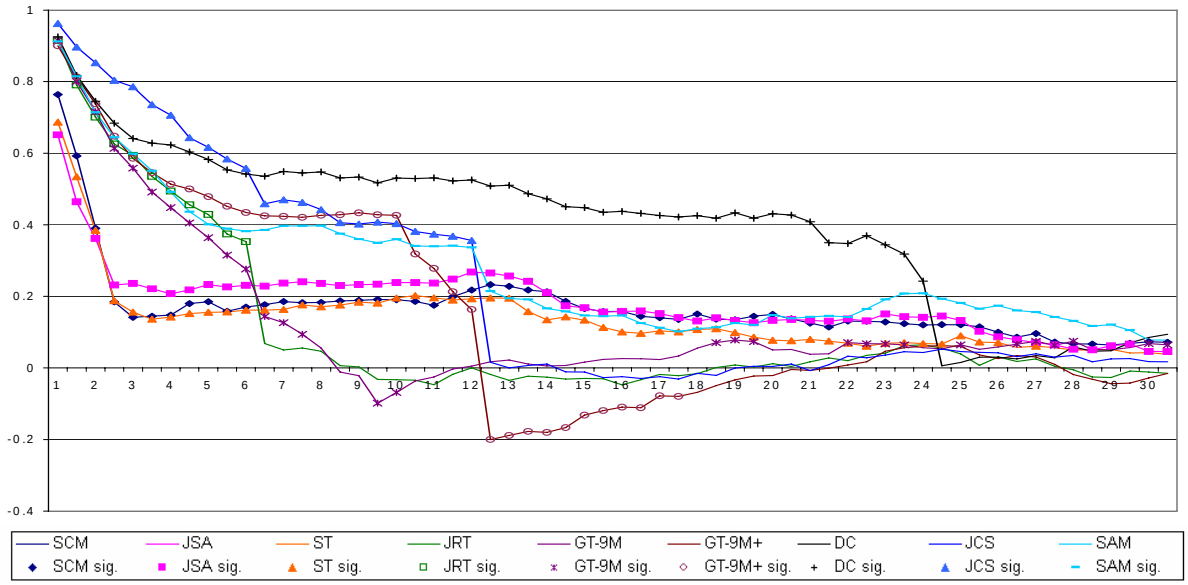
Treatment status	Acronym	SCM	JSA	ST	JRT	GT	DC	JCS	SAM	Other	At least one
Nonparticipation	NP	1	3	2	0.1	0.8	0.2	3	0.4	7	0.16
Short combined measures	SCM	10	8	5	2	18	6	7	2	16	0.62
Jobseeker assessment	JSA	2	14	4	2	10	11	7	3	17	0.59
Short training	ST	3	8	9	2	11	5	8	5	20	0.59
Job related training	JRT	5	12	6	4	5	2	9	4	13	0.51
General training	GT	3	9	7	1	6	1	8	3	20	0.51
Degree course	DC	4	10	6	1	6	3	3	1	13	0.41
Job creation scheme	JCS	4	7	3	1	4	2	13	2	20	0.49
Structural adjustment measure	SAM	4	6	4	1	3	2	7	4	13	0.38

Note: The largest fraction of *Other* are temporary wage subsidies followed by support of self-employment.

Our next finding in Figure 4 shows the effect of programme participation on any form of employment, including the time in any programme. It shows that one of the effects of programmes in East Germany is keeping the unemployed busy. For all programmes, Figure 4 shows that for this definition of employment large drops occur around the time when most participants complete their programme.

[10] In many cases, regular programmes were followed by periods of employment accompanied by a 6 or 12 month wage subsidy. To avoid having to pay back that subsidy, firms have to keep the initially subsidised employers for at least another period of unsubsidised employment of the same length as the subsidised employment period. Therefore, for some comparisons we see large drops in programme participation (for the definition of the outcome variables and the state of nonparticipation, all wage subsidy programmes are coded as programme participation, even if they are not explicitly evaluated in this paper) about 6 months after the end of a 6-month wage subsidy. See the internet appendix for all details.

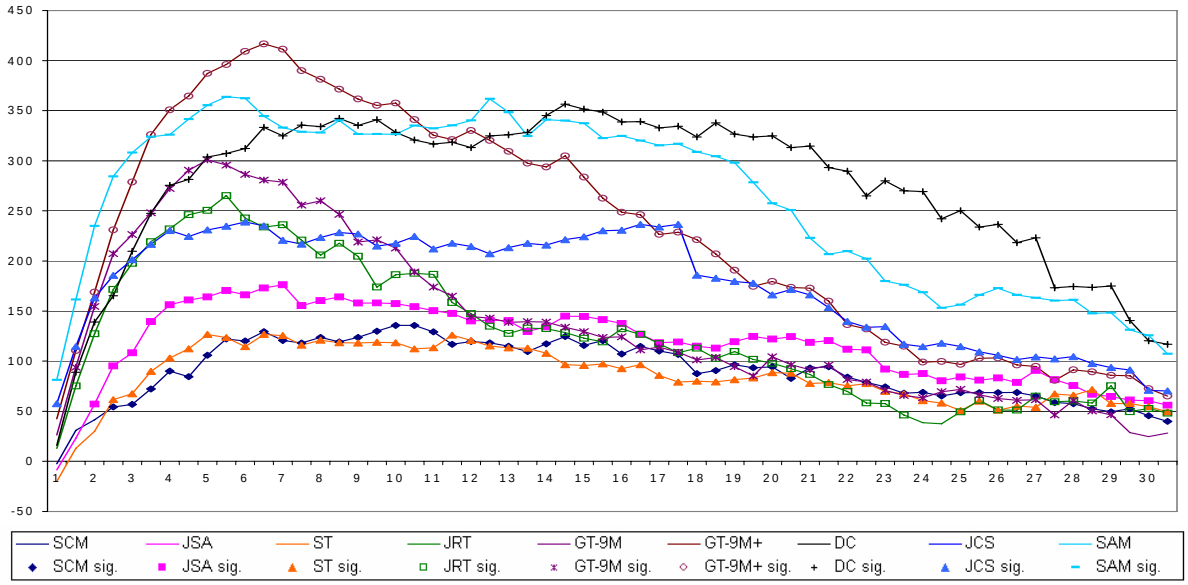
Figure 4: Effects of programme participation compared to nonparticipation: subsidised and unsubsidised employment and programme participation



Note: See note below Figure 2.

Figure 5 shows that the programmes do not only keep their participants busy, but the programmes (directly and indirectly) reward their participants by increasing the amount of unemployment benefits paid to them (incl. all benefits and 60% of wages received while participating in subsidised employment). Summing up these payments over the 2.5 year horizon, it appears that participants in DC and SAM get an extra amount of about 8000 EUR of benefits, participants in GT-9M+ about 7000 EUR, in JCS about 5000 EUR, in JRT and GT-9M about 4000 EUR, in JSA about 3500 EUR and in SCM and ST about 2500 EUR. These numbers are substantial and hint at the large cost of the programmes in terms of benefits and wage subsidies.

Figure 5: Effects of programme participation compared to nonparticipation: benefits (EUR)



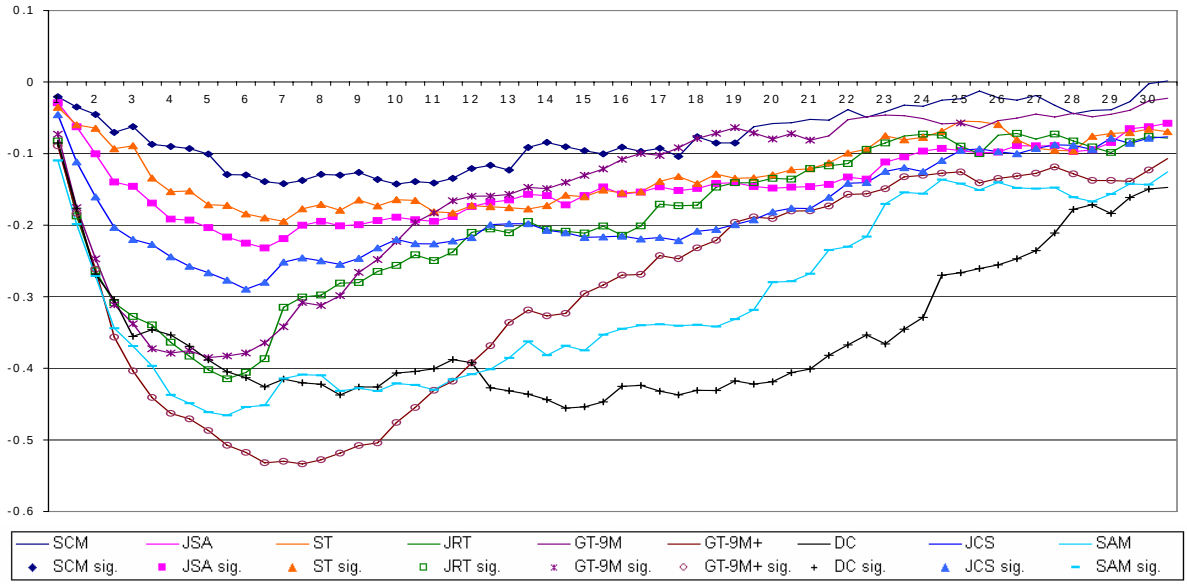
Note: See note below Figure 2.

5.3 Programmes do not increase the employment chances of their participants

It should clearly be one of the primary goals of East German training and employment programmes to improve the chances of the participants to find an unsubsidised job. Figure 6 shows that, however, after 2.5 years such effects are absent.

The only programmes that have no, or almost no, negative effect at the end of the observation period are the short combined measures (SCM) and short general training (GT-9M) programmes. All other programmes have significant negative effects. However, 2.5 years might be too short an observation period for a programme that has a typical duration of 2 years, like DC, and a corresponding large (huge!) lock-in effect. This programme may or may not show future positive effects. Even for this programme, it is worrying that the negative effect after 2.5 years is quite large with about -15%. Certainly, for the short training programmes and probably also the employment programmes, the negative effects after 2.5 years are an indication that negative long-run effects should be expected.

Figure 6: Effects of programme participation compared to nonparticipation: unsubsidised employment



Note: See note below Figure 2.

5.4 Several groups of participants would have been better off had they participated in a different programme

We already saw that some groups of programme participants would have had better labour market chances had they not participated in any programmes. In this section, we show that even ignoring the option of non-participation, some programme groups would have fared better had they participated in a different programme.

Table 8 presents this comparison for all programmes and their participants (given in lines) compared to all alternatives (given in the columns) based on the outcome variable measuring unsubsidised employment. Whereas the upper part of the table contains the point-in-time estimate for the end of the observation period, the lower panel presents the number of months accumulated over those 2.5 years. Whenever an effect is negative, it means that on average the programme group would have fared better in the alternative programme. The shaded fields on the main diagonal of this table show the level of the outcome variable for the actual participants in the respective programme.

Table 8: Effects of programme participation for participants in one programme had they participated in another programme: unsubsidised employment

Programmes	Comparison state									
	SCM	JSA	ST	JRT	GT-9M	GT-9M+	DC	JCS	SAM	NP
SCM	0.36	0.05	*0.08*	0.10	0.01	0.08	-0.02	*0.13*	**0.14**	-0.01
JSA	*-0.09*	0.31	-0.04	0.02	**-0.08**	-0.03	0.03	0.02	*0.07*	-0.06*
ST	-0.07	0.03	0.33	-0.11	-0.06	0.06	*0.14*	0.10	**0.13**	-0.06
JRT	-0.14	-0.02	-0.07	0.32	**-0.10**	-0.04	-0.01	0.10	0.08	-0.09
GT-9M	-0.04	*0.06*	0.00	*0.10*	0.42	0.07	**0.22***	0.12	**0.10**	-0.01
GT-9M+	-0.09	0.03	-0.01	0.07	*-0.08*	0.37	**0.19**	0.10	0.10	-0.10
DC	-0.11	-0.06	**-0.16***	-0.05	**-0.15***	-0.09	0.20	-0.02	-0.02	-0.12
JCS	-0.04	-0.01	-0.03	-0.04	**-0.12***	**-0.13**	0.01	0.17	-0.01	-0.06
SAM	-0.02	-0.03	-0.02	-0.04	-0.09	-0.03	0.03	0.03	0.28	-0.11
	Cumulated months over 2.5 years									
SCM	9.2	**2.1***	1.1	*2.6*	0.8	**3.8***	**5.9***	**4.8***	**6.3***	**-2.2***
JSA	**-2.4**	8.2	**-2.0**	1.1	-0.5	**2.3***	**5.2***	*2.2*	**4.3***	**-4.3***
ST	-0.4	*1.3*	9.4	-0.5	0.9	**4.0***	**7.8***	**4.1***	**6.0***	**-3.1***
JRT	**-5.2**	**-1.8**	**-4.0***	7.5	*-1.9*	1.8	**5.2***	1.7	**4.4***	**-5.8***
GT-9M	**-4.0**	0.7	-1.9	1.4	9.4	**3.2***	**7.2***	*2.7*	**4.8***	**-4.6***
GT-9M+	**-7.0***	**-1.8**	**-4.3***	-0.9	**-3.4***	6.5	**5.1***	1.6	**2.9***	**-8.5***
DC	**-9.3***	**-5.0***	**-9.3***	**-4.8***	**-6.3***	**-3.8***	1.8	**-2.3***	**-1.9***	**-10.4***
JCS	**-2.9**	**-2.5***	**-2.6**	-2.0	**-2.3***	**-2.0**	1.8	3.1	0.2	**-5.0***
SAM	**-7.3***	**-3.3***	**-5.6***	**-3.2***	**-3.1***	**-1.8***	**1.9***	-0.8	4.3	**-8.6***

Note: Numbers in *italics* indicate significance on the 10% level, **bold** numbers on the 5% level, and ***** on the 1% level. Dark shaded entries on the diagonal are the levels of the respective potential outcome in the respective group of participants. Off-diagonal elements are the effects of the programme given in the line for its participants compared the state that those participants would have participated in the programme given as headings of a column.

Ignoring the state of nonparticipation and taking only the case where both outcome measures agree, we see (at least) that participants in JSA, GT-9M+, DC, JCS, and SAM would have improved their employment chances had they participated in the shorter programmes in GT-9M (or SCM). This indicates that there is room for improving the process of allocating the unemployed to the various programmes.

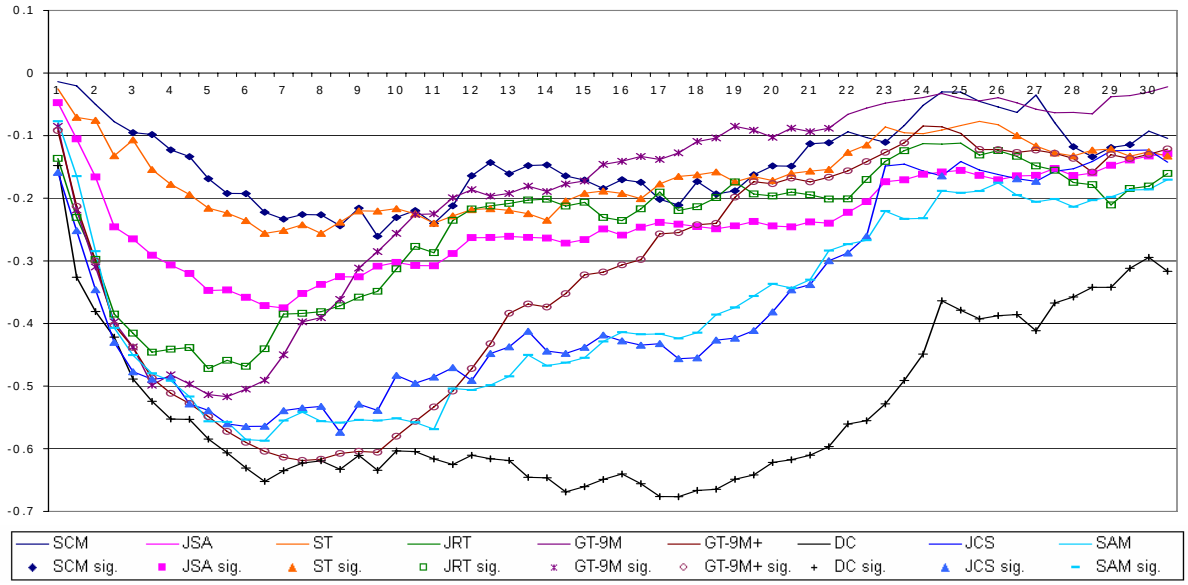
5.5 Unemployed with reasonable chances on the labour market got hurt most

Next, we analyse the effects of the programmes on unsubsidised employment for the groups with good and bad no-programme labour market chances separately. This separation is performed according to the no-programme employment index discussed in Section 3.3.

The results for the group with better chances are presented in Figure 7. They are striking in the sense that all programmes with the exception of

GT-9M (and perhaps SCM) hurt this group. SAM, JCS, and DC have large negative effects of about -30%, whereas the shorter programmes have negative effects between -10% and -20%.

Figure 7: Effects of programme participation compared to nonparticipation: unsubsidised employment; unemployed with non-programme employment chances above the median

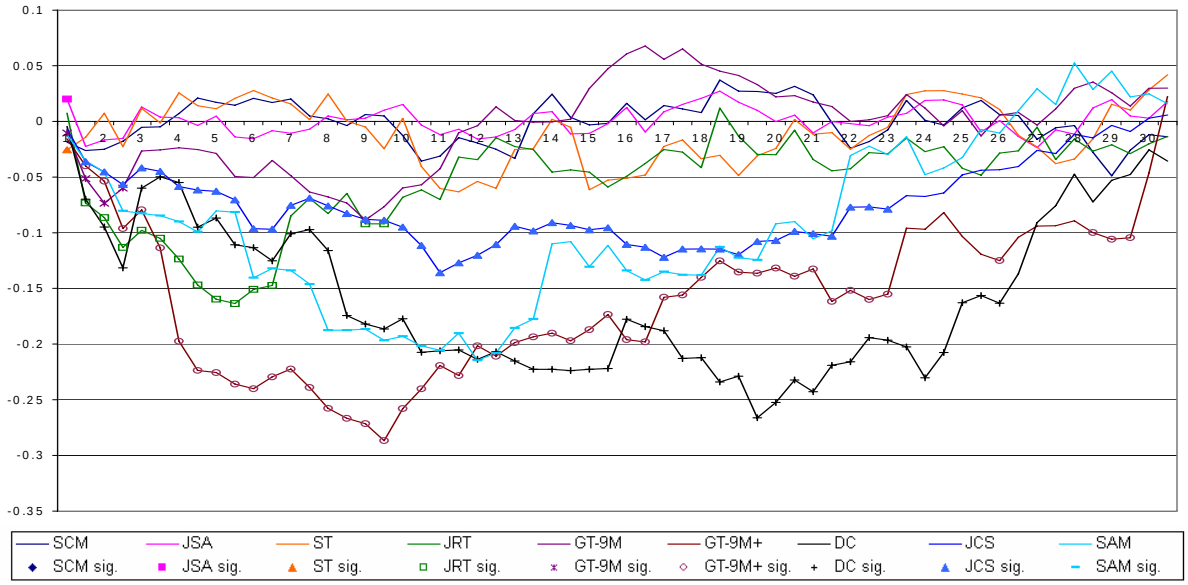


Note: See note below Figure 2. The employment index is equal to the predicted probabilities from a probit in the pool of nonparticipants. Dependent variable: employed in unsubsidised employment with at least 90% of the earnings of the last job before programme start, measured in half-month 60 after programme start.

For the group which has worse labour market chances even without the programmes (Table 8), at least none of the programmes seem to reduce employment after 30 months significantly, however, a significant positive effect cannot be detected either.

A straightforward reason for this finding may be due to differential lock-in effects. The better the pre-programme employment chances, the quicker an unemployed finds a job. Therefore, the reduction in employment rates due to a lack of job search and reduced job offers while participation in a programme is larger for 'better' unemployed, leading to a larger lock-in effect as compared to 'worse' unemployed who would need longer anyway to find a job. Apparently, the lock-in effects got so large that 'good' programme participants could recover.

Figure 8: Effects of programme participation compared to nonparticipation: unsubsidised employment; unemployed with non-programme employment chances below the 33% quantile



Note: See note below Figure 2. The employment index is equal to the predicted probabilities from a probit in the pool of nonparticipants. Dependent variable: employed in unsubsidised employment with at least 90% of the earnings of the last job before programme start, measured in half-month 60 after programme start.

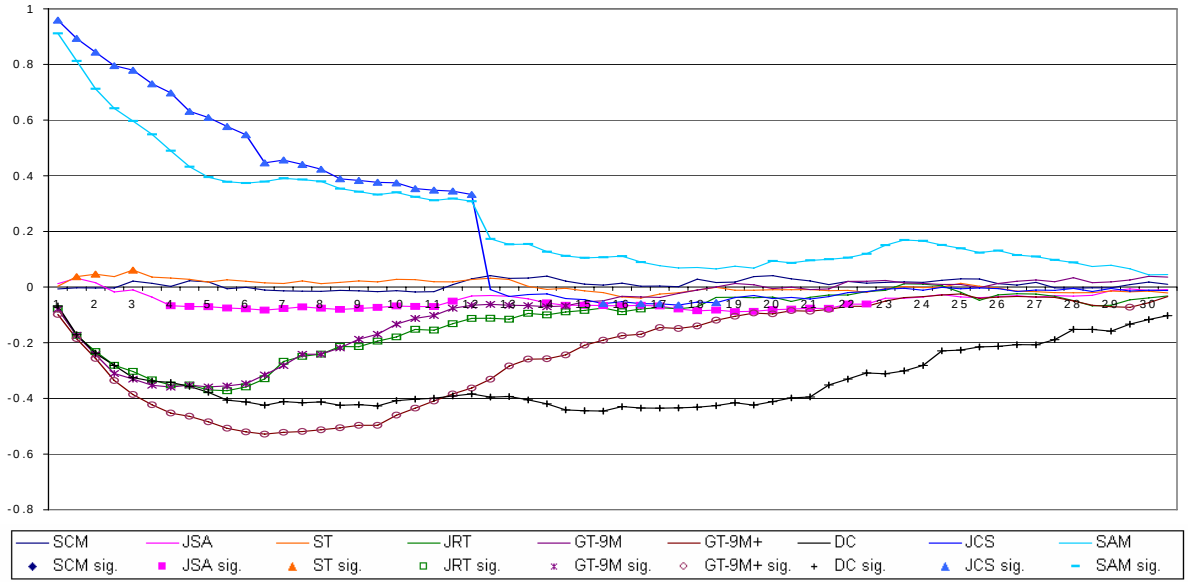
5.6 Why were the previous results more positive?

In a previous study by Lechner, Miquel, and Wunsch (2005b) that was based on a similar methodology, we found generally more positive effects of the training programmes that were in effect 10 years earlier. For the three training programmes that we looked at (there was no information on employment programmes and subsidies in the old data), we found positive effects for retraining (similar to DC) after about 35 months. This is beyond our horizon available in this paper, but for the shorter training programmes we obtained significant positive effects compared to nonparticipation after 25 months for training courses longer than 6 months, and after about 12 months for training courses 6 months and shorter. What has changed? One thing that is different between the two studies is that for the first six years the old data did not allow to distinguish subsidised and non-subsidised employment.

Figure 9 presents the results of our current analysis based on an employment variable similar to the one used previously. We see that the negative effects of programmes disappear, with the exception of DC, which still shows negative effects of about -15%. However, no positive effects occur

either, leading us to the conclusion that the definition of the outcome variable is not the reason for the discrepancies in the findings of the two papers.

Figure 9: Effects of programme participation compared to nonparticipation: total (subsidised and unsubsidised) employment



Note: See note below Figure 2.

Since there is no data available on programmes between 1997 and 2000, it is very hard to analyse the reasons for the changes. It could be that the programme quality, or the quality of the selection process into the programmes, or the quality of the suitable potential participants declined, or that the labour market changed in a direction that made it harder to reward programme participation. This issue remains open for future research.

6 Sensitivity checks

We conducted several sensitivity analyses, the details of which are given in the internet appendix.

We did not find any substantial heterogeneity of the programme effects for the socio-economic groups we looked at, other than the general feature mentioned above, namely that unemployed with intact pre-programme labour market chances fair worse than unemployed with bad pre-programme labour market chances.

With respect to the technical properties of the estimation, we varied the criteria to define the common support as well as the time window used to define participation as well as nonparticipation, but no significant differences appeared.

7 Conclusions

In this paper, we analyse the most important components of the East German active labour market policy between 2000 and 2002. Our empirical investigation is based on a well-suited, large, and informative individual database of participants and nonparticipants that originated from administrative records. These data are analysed with econometric matching methods. We considered various labour market outcomes over a period of 30 months after the respective programmes started. Our analysis leads us to the following policy conclusions:

If the success of the programmes is measured by the primary goal of the official active labour market policy (ALMP), namely, ALMP should bring its participants back into jobs in the first labour market, all programmes failed. They do *not improve* the employment chances or earnings. In particular, for the group of individuals with better employment chances in the labour market, several programmes *reduce* those chances by a considerable amount. This finding is however not surprising. By using training and employment programmes, active labour market policies can at best reduce unemployment due to mismatch in the labour market. Furthermore, it may prevent a deterioration of the general human capital of the workforce due to individual interruptions of the employment spells and the lack of learning on the job while in unemployment. ALMP can certainly not solve the deep structural problems in the labour market experienced in East Germany over the last decade. In other words, it tries to alleviate some of symptoms of the sickness of the East German labour market, but cannot cure the disease.

If ALMP has to fail to deliver better individual labour market outcomes given the specific circumstances of the East German economy, could it still be worthwhile running ALMP programmes? Indeed, one may argue that ALMP is still required in East Germany at least for two reasons: The first reason is that participation in those programmes keeps people busy and provides them with some income from work or work-related tasks. In

other words, it may be and is used to combat social unrest in an environment that saw (official) unemployment rates around 20% for a long time, and non-employment rates that are considerably higher. The second reason for having ALMP could be to keep people ready for work, i.e. use short training and employment programmes to keep their working skills and human capital from deteriorating, so that they actually will find jobs when (if) the structural problems of the East German economy will be overcome and the economy will ultimately pick up. Our analysis shows that the programmes are effective in the first dimension. The effectiveness in the second dimension however remains to be seen.

Taking those arguments seriously, an active labour market policy for East Germany should give up the goal to increase the individual probability of unemployed to find regular employment, which cannot be achieved anyway. Instead, it should concentrate on the two smaller goals explained above, which are worth to reach as well. However, such a policy would look differently than the one we analysed. In particular, it would drastically reduce the expensive long-term courses that make only sense if the unemployed were educated with skills that are in considerable short supply, which does not appear to be the case on a large scale in East Germany. Unemployed should participate in employment programmes and take up jobs related to hiring subsidies, even if those jobs will only be of a temporary nature. Furthermore, sending unemployed from time to time to shorter training programmes to practice and update their skills should also be considered as worthwhile. However, there remains the overarching issue about the costs to reach the limited goals of such a policy. Although costs would be probably somewhat smaller than for today's policy, it is not clear at all how much the taxpayer will be willing to pay for such a reorganised active labour market policy in East German.

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Appendix A: Data

A.1 Features of the data sources merged in the combined database

Table A.1: Data sources used

Source	Period	Available information	Important variables
Social insurance records	Jan. 1990-Dec. 2004	Times of insured employment; personal characteristics	Form of employment, industry, earnings, position in job; profession, education, age, gender, nationality, regional information
Benefit payment register	Jan. 1990-Jun. 2005	Times of receipt of unemployment benefits, unemployment assistance or maintenance allowance; personal characteristics	Type and amount of benefit, remaining benefit claim, benefit sanctions; marital status, number of children
Programme participation data	Jan. 2000-Jun. 2005	Participation in ALMP measures; programme information; personal characteristics	Type of programme, planned and actual duration, (un)successful completion, capacity; profession, education, health problems, regional information
Jobseeker register	Jan. 2000-Jun. 2005	Job search relevant information; personal characteristics	Desired form of employment, reason for determination of last employment, number of job offers, compliance to benefit conditions, date of last interview, health problems and judgement whether these affect employability

A.2 Further details on the data

Several groups of people are not included in the data either because they have not been subject to social insurance contributions like civil servants and the self-employed, or because they receive benefits that are not administered by the PES like recipients of social assistance. However, focusing on unemployed individuals who receive unemployment benefits or unemployment assistance - which is the main target group of German ALMP and for whom is full record of these people in the data - alleviates this problem to a large extend.

Appendix B: Technical details of the matching estimator used

Table B.1: A matching protocol for the estimation of a counterfactual outcome and the effects

Step 1	Specify a reference distribution defined by X.
Step 2	Pool the observations forming the reference distribution and the participants in the respective period. Code an indicator variable *W*, which is 1 if the observation belongs to the reference distribution. All indices, 0 or 1, used below relate to the actual or potential values of *W*.
Step 3	Specify and estimate a binary probit for $p(x) := P(W = 1 \mid X = x)$
Step 4	Restrict sample to common support: Delete all observations with probabilities larger than the smallest maximum and smaller than the largest minimum of all subsamples defined by *W*.
Step 4	*Estimate the respective (counterfactual) expectations of the outcome variables.* **Standard propensity score matching step (multiple treatments)** a-1) Choose one observation in the subsample defined by *W*=1 and delete it from that pool. b-1) Find an observation in the subsample defined by *W*=0 that is as close as possible to the one chosen in step a-1) in terms of $p(x), \tilde{x}$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again. c-1) Repeat a-1) and b-1) until no observation with *W*=0 is left. **Exploit thick support of X to increase efficiency (radius matching step)** d-1) Compute the maximum distance (*d*) obtained for any comparison between member of reference distribution and matched comparison observations. a-2) Repeat a-1). b-2) Repeat b-1). If possible, find other observations in the subsample of *W*=0 that are at least as close as $R * d$ to the one chosen in step a-2) (to gain efficiency). Do not remove these observations, so that they can be used again. Compute weights for all chosen comparisons observations that are proportional to their distance. Normalise the weights such that they add to one. c-2) Repeat a-2) and b-2) until no participant in *W*=1 is left. d-2) For any potential comparison observation, add the weights obtained in a-2) and b-2). **Exploit double robustness properties to adjust small mismatches by regression** e) Using the weights $w(x_i)$ obtained in d-2), run a weighted linear regression of the outcome variable on the variables used to define the distance (and an intercept). f-1) Predict the potential outcome $y^0(x_i)$ of every observation using the coefficients of this regression: $\hat{y}^0(x_i)$. f-2) Estimate the bias of the matching estimator for $E(Y^0 \mid W = 1)$ as: $\sum_{i=1}^{N} \frac{1(W=1)\hat{y}^0(x_1)}{N^1} - \frac{1(W=0)w_i\hat{y}^0(x_1)}{N^1}$. g) Using the weights obtained by weighted matching in d-2), compute a weighted mean of the outcome variables in *W=0*. Subtract the bias from this estimate to get $\widehat{E(Y^0 \mid W = 1)}$.
Step 5	Repeat Steps 2 to 4 with the nonparticipants playing the role of participants before. This gives the desired estimate of the counterfactual nonparticipation outcome.
Step 6	The difference of the potential outcomes gives is the desired estimate of the effect with respect to the reference distribution specified in Step 1.

Note: We use the fixed-weight heteroscedasticity robust standard errors suggested by Lechner, Miquel, and Wunsch (2005a). Since participants and nonparticipants are independent, variance of the effect is the sum of the variances of the potential outcomes. $\tilde{x}$ includes gender, elapsed unemployment duration until programme start, and programme start date. $\tilde{x}$ is included to ensure a high match quality with respect to these critical variables. R is fixed to 90% in this application (different values are checked in the sensitivity analysis).

Appendix C: Additional results

Table C.1: Cumulated outcomes 2.5 years after programme start

Treatment	Comparison	Employment (months)			Unemployment (months)	Programme participation (months)	Not unemployed (months)	Earnings (EUR)		
		Unsubsidised	Unsubsidised with stable earnings	Total (incl. subsidised)				From unsubsidised employment	Received benefits	Total earnings (incl. benefits)
SCM	NP	**-2.2***	**-2.0***	0.3	**4.2***	**5.4***	**5.1***	**-3388***	**2703***	584
(N = 417)	JSA	**2.1***	**1.7***	**2.1**	**-2.4**	0.1	-0.3	**3269***	-677	*2628*
	ST	1.1	1.0	0.6	-0.7	0.2	1.3	1593	14	1359
	JRT	*2.6*	1.4	*3.2*	-2.1	**2.6***	0.5	*3798*	-126	4011
	GT ≤ 9 Months	0.8	-0.3	0.1	-1.5	**1.7**	-1.4	1439	-1464	-192
	GT > 9 Months	**3.8***	**2.7***	**5.6***	*-3.7*	**4.1***	-0.9	**6237***	-1616	**5501***
	DC	**5.9***	**3.2***	***8.3****	-7.2	**4.7***	*-7.7*	**7848***	-4327	4719
	JCS	**4.8***	**3.2***	*-3.8*	*-4.9*	**3.8***	0.0	**7440***	*-2692*	959
	SAM	**6.3***	**3.6***	**-5.6**	**-6.8**	**4.4***	-1.6	**9302***	**-5067***	-1043
JSA	NP	**-4.3***	**-3.1***	**-1.5***	**6.5***	**5.6***	**5.5***	**-5654***	**3509***	-911
(N = 1081)	SCM	**-2.4**	**-2.0**	*-2.0*	**2.9**	0.9	0.6	**-3630**	**1420**	-2089
	ST	**-2.0**	-0.5	**-2.3**	**2.7**	**1.2**	1.5	**-2311**	**1679***	-839
	JRT	1.1	0.3	*2.0*	-0.6	**2.6***	2.0	528	-43	839
	GT ≤ 9 Months	-0.5	-0.3	0.2	0.2	**2.9***	0.8	-1410	-359	-1424
	GT > 9 Months	**2.3***	**1.6***	**3.5***	-2.1	**3.6***	-0.6	**3732***	*-1670*	*2577*
	DC	**5.2***	**2.5***	**7.3***	**-5.9**	**4.1***	**-5.6**	**6715***	**-4259**	3536
	JCS	*2.2*	0.7	**-5.8***	-2.0	**3.7***	0.2	*2940*	-1582	-1992
	SAM	**4.3***	**2.0***	**-9.1***	**-4.7**	**4.6***	-2.7	**6058***	**-4507***	-4405
ST	NP	**-3.1***	**-2.7***	0.1	**4.6***	**5.4***	**4.2***	**-4370***	**2555***	-262
(N = 551)	SCM	-0.4	-0.9	0.7	-0.1	-0.3	-0.3	-734	-296	-476
	JSA	*1.3*	1.2	*1.5*	*-2.1*	-0.6	*-1.9*	**2496**	**-1475**	1063
	JRT	-0.5	0.4	1.1	-0.1	**2.3**	-2.0	-360	762	1156
	GT ≤ 9 Months	0.9	0.3	2.1	-2.1	**2.8***	-0.5	599	-956	234
	GT > 9 Months	**4.0***	**2.7***	**6.3***	*-4.4*	**4.0***	-0.1	**5773***	*-2365*	**4489**
	DC	**7.8***	**4.3***	**10.7***	**-9.7**	**4.6***	*-6.4*	**10841***	**-7093***	*5197*
	JCS	**4.1***	1.8	**-5.3**	-4.0	**3.5***	-2.4	**6486***	-2487	25
	SAM	**6.0***	**3.5***	**-6.3**	**-7.8***	**4.5***	-2.7	**9083***	**-5025***	-1309

To be continued.

Table C.1: Cumulated outcomes 2.5 years after programme start - continued

Treatment	Comparison	Employment (months)			Unemploy-ment (months)	Programme participation (months)	Not unem-ployed (months)	Earnings (EUR)		
		Unsub-sidised	Unsubsidised with stable earnings	Total (incl. subsidised)				Unsubsidised employment	Received benefits	Total earn-ings (incl. benefits)
JRT	NP	**-5.8***	**-3.7***	**-3.7***	**7.3***	**2.6***	**3.2***	**-7909***	**3776***	**-3181**
(N = 323)	SCM	**-5.2**	**-3.4**	**-5.0**	*4.5*	-0.3	-1.0	-5600	1968	-3659
	JSA	**-1.8**	0.1	**-2.9***	**1.1**	**-2.5***	**-2.5**	-998	486	-928
	ST	**-4.0***	-1.3	**-5.6***	**4.2**	**-1.6**	-1.5	*-3684*	*2007*	-2619
	GT ≤ 9 Months	*-1.9*	-1.2	-1.5	1.3	*0.6*	-0.6	-2051	476	-1471
	GT > 9 Months	1.8	**2.1***	**2.7**	-2.8	**0.9**	-1.7	**3786**	-1810	2337
	DC	**5.2***	**3.3***	**7.0***	-6.5	0.8	*-6.3*	**7934***	**-5491**	3291
	JCS	1.7	**2.1**	**-6.8***	-2.3	-0.7	-1.7	2721	-1896	-2830
	SAM	**4.4***	**2.6***	**-10.0***	*-4.5*	**1.4***	*-4.0*	**7261***	**-4218***	-3028
GT ≤ 9 Months	NP	**-4.6***	**-3.5***	**-2.8***	**6.9***	**2.7***	**3.9***	**-6514***	**4134***	-1478
(N = 619)	SCM	**-4.0**	-1.9	*-3.8*	**4.2**	-0.9	-1.2	*-6295*	1912	-4504
	JSA	0.7	0.9	-0.4	*-0.5*	**-3.3***	**-1.9**	*1977*	-57	1462
	ST	-1.9	-0.1	**-3.5***	*2.6*	**-2.0***	-0.4	-1561	**1671**	-739
	JRT	1.4	0.5	1.1	-0.4	-0.9	0.8	1653	700	2347
	GT > 9 Months	**3.2***	**2.7***	**3.3***	**-3.4***	0.3	-1.5	**5697***	**-2081***	**3709**
	DC	**7.2***	**5.0***	**8.9***	**-8.2**	0.6	*-5.9*	**10968***	**-6444***	**5324**
	JCS	*2.7*	*1.7*	**-7.1***	-3.0	-0.2	-1.7	**4982**	-1546	-828
	SAM	**4.8***	**2.8***	**-10.1***	**-4.9**	**1.4***	**-4.3**	**7925***	**-4498***	-3684
GT > 9 Months	NP	**-8.5***	**-5.6***	**-6.9***	**11.0***	**2.3***	**4.8***	**-13772***	**6848***	**-6133***
(N = 538)	SCM	**-7.0***	*-2.6*	**-7.8***	**7.4***	*-1.5*	-0.4	**-11670***	**4215***	*-8167*
	JSA	**-1.8**	**-1.7**	**-3.2***	*2.3*	**-3.9***	-0.3	**-2932**	1264	-2424
	ST	**-4.3***	**-2.3***	**-5.5***	**5.5***	**-2.1***	1.9	**-6503***	**3233***	-4018
	JRT	-0.9	-1.3	-1.1	1.2	-0.6	*3.0*	*-4632*	1710	-2953
	GT ≤ 9 Months	**-3.4***	**-2.0***	**-3.5***	**3.3**	*-0.8*	1.0	**-6090***	*1642*	**-4531**
	DC	**5.1***	**2.7***	**6.5***	-5.8	1.2	-3.6	**7338***	**-5461**	2580
	JCS	1.6	0.6	**-10.8***	-1.8	-0.7	-0.1	**3009**	-699	-2969
	SAM	**2.9***	1.0	**-14.2***	-3.3	0.7	-3.5	**4356***	*-3727*	**-7369**

To be continued.

Table C.1: Cumulated outcomes 2.5 years after programme start - continued

Treatment	Comparison	Employment (months)			Unemployment (months)	Programme participation (months)	Not unemployed (months)	Earnings (EUR)		
		Unsubsidised	Unsubsidised with stable earnings	Total (incl. subsidised)				From unsubsidised employment	Received benefits	Total earnings (incl. benefits)
DC	NP	**-10.4***	**-6.3***	**-10.0***	**14.1***	**1.1***	**12.1***	**-14706***	**8213***	**-6291***
(N = 170)	SCM	**-9.3***	**-5.5***	**-10.9***	**10.7***	**-3.8***	**6.7***	**-13091***	**6304***	**-7690**
	JSA	**-5.0***	**-3.6***	**-7.2***	**5.5***	**-5.3***	**6.1***	**-6617***	**3984***	**-3662***
	ST	**-9.3***	**-4.5***	**-11.4***	**11.2***	**-2.9***	**7.0***	**-12251***	**7107***	**-6231**
	JRT	**-4.8***	**-3.6***	**-7.1***	**5.8**	**-2.9***	**8.0***	**-7470***	**3970***	-4553
	GT ≤ 9 Months	**-6.3***	**-4.2***	**-7.8***	**7.0***	**-1.8***	**7.8***	**-9059***	**4749***	**-5009***
	GT > 9 Months	**-3.8***	**-2.0***	**-4.3***	**4.2**	-0.6	**6.0***	**-5547***	**3087***	-2697
	JCS	**-2.3**	-0.9	**-13.3***	3.7	**-2.0**	**7.2***	**-2966**	2079	**-5969**
	SAM	**-1.9***	**-1.7**	**-18.5***	2.4	0.0	3.1	**-2628**	-380	**-10921***
JCS	NP	**-5.0***	**-3.2***	**5.9***	**7.7***	**2.2***	**6.8***	**-6634***	**5261***	**3284***
(N = 577)	SCM	**-2.9**	*-1.6*	**5.6***	2.6	**-2.3**	2.6	**-3701**	**3012**	2673
	JSA	**-2.5***	**-1.9***	**4.8***	2.5	**-3.7***	-0.3	**-3484***	**1900**	1444
	ST	**-2.6**	**-1.7**	**5.8***	3.4	**-1.7**	*3.3*	**-3522**	**3077**	2903
	JRT	-2.0	-1.4	**6.7***	2.2	-0.7	0.2	*-3271*	*2328*	2534
	GT ≤ 9 Months	**-2.3***	**-1.7***	**6.9***	2.0	0.3	1.3	**-3345***	*1546*	2068
	GT > 9 Months	**-2.0**	-0.9	**8.4***	2.6	**1.2***	-1.3	*-1880*	1762	**4166**
	DC	1.8	1.0	**12.3***	-2.5	1.5	-7.3	2365	-1102	5698
	SAM	0.2	-0.2	-2.0	0.0	**1.0**	-1.8	417	-450	-1167
SAM	NP	**-8.6***	**-5.2***	**7.2***	**11.0***	**1.6***	**8.1***	**-11501***	**7909***	**3585**
(N = 430)	SCM	**-7.3***	**-3.9**	**6.0**	**8.0***	**-2.5**	2.9	**-9711***	**5585***	1626
	JSA	**-3.3***	**-1.7***	**9.0***	**3.7***	**-4.0***	1.9	**-4846***	**2773***	**3553**
	ST	**-5.6***	**-3.6***	**5.2**	**6.4***	**-2.0**	*3.8*	**-7744***	**5169***	2385
	JRT	**-3.2***	**-2.5**	**10.7***	3.7	**-1.4**	**4.4**	**-5583***	**4058***	*4654*
	GT ≤ 9 Months	**-3.1***	**-1.9***	**10.6***	**3.1**	**-1.5***	**4.4***	**-4921***	**3555***	**4967***
	GT > 9 Months	**-1.8**	*-1.2*	**12.2***	1.8	-0.6	1.8	**-2820**	1554	**5112**
	DC	**1.9***	0.7	**17.4***	-2.6	0.2	-2.9	**2370***	536	**9954***
	JCS	-0.8	-0.5	**2.9**	0.9	**-1.5***	2.4	-1523	*1828*	2277

Note: Numbers in *italics* indicate significance on the 10% level, **bold** numbers on the 5% level, and * on the 1% level.

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