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The Effectiveness of Targeted Wage Subsidies for Hard-to-Place Workers

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JEL-Codes: J68, J64, J65

Abstract: Targeted wage subsidies paid to employers are an important element of active labour market policies in Germany. This paper uses propensity score matching to investigate their effect on the employment and unemployment rates of subsidised hard-to-place workers. In a first scenario, we estimate the average treatment effect of a subsidy on previously unemployed individuals. A second scenario analyses the effects of a subsidy on employment probabilities conditional on taking-up employment. The third scenario investigates the additional effect of a subsidy on individuals, who have participated in a short-term training measure beforehand. Summing up and in line with the literature, the results show that subsidies have a favourable effect on the employment prospects of participants.

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1 Introduction

Long-term unemployment is still a huge problem in Germany (e.g. OECD 2006). Policies aimed at increasing the chances of reintegration of long-term unemployed persons thus deserve special attention. In a recent meta-study, Kluve (2006) concludes that it is mostly the programme type that matters for effectiveness; in particular, wage subsidies, services and sanctions seem to work. However, there are considerable differences in the design of wage subsidies. General wage subsidies are paid permanently for all low-wage earners in an economy, regardless of their employment history. Their obverse are negative payroll taxes for employees, which have been a major vehicle used by many governments to stimulate employment. Marginal wage subsidies concern only a firm's additional employment exceeding some reference level (Knabe et al. 2006). Targeted wage subsidies – or hiring subsidies (Orszag/Snowder 2003) – are tailored to particular groups of unemployed and usually granted for a limited time period. In the following, wage subsidies are considered to be payments to employers (Fay 1996).

This paper analyses the effectiveness of a targeted wage subsidy programme, covering a share of labour costs and paid to employers for a fixed period of time. We ask if taking up a subsidized job during the second quarter of 2002 has improved the employment prospects of participating hard-to-place workers in Germany. The programme will be described in detail in Section 2, which also discusses recent results from the literature. Section 3 presents the evaluation strategy and describes data and variables. The empirical results are depicted in Section 4. In Section 5 we draw a summary of the results and discuss their implications. A main feature of our analysis is that we extend the usual approach of estimating the effect of taking up subsidized employment compared to remaining unemployed: We also estimate the effect of receiving a subsidy on employment and unemployment rates conditional on taking up a job and conditional on having participated in a short-term training measure beforehand.

Wage subsidies are a policy that tries to affect employment via the wage rate (Hamermesh 1993, Chapter 5): They obviously reduce labour costs of a given employee for a firm. The sub-

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sidy can compensate the firm for a gap between a worker's productivity and his minimum wage. A temporary subsidy might have long-run positive effects on individual labour market prospects if employees are able to close the gap over time by learning on-the-job. Also, a period of subsidization might be necessary to reduce an employer's uncertainty about the employability of job applicants and might thus serve as a screening advice.

But wage subsidies for the unemployed are also often criticized (Layard et al. 1991, Chapter 10): First, several of those subsidised would have been recruited anyway, thus a deadweight loss occurs (see also Welters/Muysken 2006). Second, some of those recruited will merely replace others, thus the subsidy does only achieve preferential treatment for some and a substitution effect occurs. Third, if subsidies produce an increase in employment in some firms, this might be at the expense of jobs in other firms, thus the only effect is displacement. These effects cannot be identified by our research approach. A fourth argument against employer-based subsidies – in particular vouchers handed out to the unemployed – are potential stigma effects (Burtless 1985, Bell et al. 1999). A counterargument in favour of wage subsidies is made by Fay (1996). He makes the point that substitution effects may not be seen as detrimental from a policy perspective, since targeted subsidy schemes are intended to “shuffle the queue” of job-seekers. Thus, targeting should lead to preferential hiring of otherwise disadvantaged workers.

2 Programme features and empirical evidence

2.1 Characteristics and importance of the programme

Targeted wage subsidies to employers represent a standard instrument in the bundle of active labour market policies in Germany. For a long time, training programmes (see for instance Fitzenberger/Speckesser 2007) and job creation in the public sector (see for instance Caliendo et al. 2008) used to be the largest programmes in Germany. Wage subsidies gained importance first in East Germany during the economic restructuring following the reunification and thereafter again in the late nineties.

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4 The programme we are dealing with is one of three variants of a wage subsidy to employers –
5 called “Eingliederungszuschuss“ (EGZ) – that were in place during the period 1998 to 2003.
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7 We concentrate on the variant for hard-to-place workers (“EGZ bei erschwerter Vermittlung“),
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9 whose target group were unemployed with severe problems of reintegration, like e.g. long-term
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11 unemployed or disabled persons. Of the other two variants, one was granted for special training
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13 requirements, while the other was targeted at workers of age 50 and older (in Germany unem-
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15 ployment rates are high and reemployment-chances are low for the over 50s). A legal reform of
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17 the EGZ was enacted in 2004, when the three former variants were collapsed into a single wage
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19 subsidy for hard-to-place workers.
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25 The EGZ has been characterized by fairly high numbers of participants from 2001 to 2003,
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27 while other wage subsidy programmes phased out in this period. A peak was reached in 2002
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29 with roughly 190,000 entries into one of the three variants of the EGZ and 80,000 entries into
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31 the EGZ for hard-to-place workers. As a consequence of the high stock of participants, total
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33 expenditure for the EGZ reached a maximum of 1.3 billion Euro in 2003. Entries dropped to
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35 134,000 in 2005, but reached a new peak with nearly 250,000 entries in 2007.
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39 The decision to support an unemployed with an EGZ has to be reasoned in each individual
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41 case. Case managers in local employment agencies have latitude in the allowance decision as
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43 well as in the fixing of the amount and duration of the subsidy. The EGZ for hard-to-place per-
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45 sons could regularly account for as much as 50 percent of the monthly wage or salary and con-
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47 tinue for at most 12 months. These limits could be exceeded in exceptional cases. As a special
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49 feature of the EGZ, a follow-up period of further employment is obligatory after the expiration
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51 of the subsidy. If a person hired with an EGZ is dismissed within this period for reasons attrib-
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53 utable to the employer, the employer can be asked to reimburse part of the subsidy.
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57 **2.2 Selected review of the literature**

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59 A number of studies have estimated the impact of targeted wage subsidies on participants. This
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requires knowledge of the potential labour market prospects participants would have had with-

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out the help of a subsidy. Several authors constructed comparison groups of similar, but non-treated individuals using statistical matching techniques and non-experimental data (see Section 3.1). For Germany, Jaenichen (2002, 2005) and Bernhard et al. (2006) used this approach and showed that different kinds of targeted wage subsidies had a positive effect on the employment prospects of previously unemployed persons.

Dorsett (2006) evaluated the British “New Deal” reform for young workers. A key option available to young workers of age 18 to 24, who had been claiming job seeker’s allowance for six months or more, was a voucher for a subsidy to prospective employers. The voucher covered 60 pounds per week; this amount was paid for six months. Other options were full-time education, placement in a voluntary sector organization or environmental task force or staying in an “extended gateway”. His result was that in the long run – after an initial lock-in effect – the subsidy dominated all other options in preventing unemployment.

Positive results on the effectiveness of targeted wage subsidies have also been obtained in a number of studies for Sweden. Sianesi (2002) used matching techniques to compare the effects of participation in different labour market programmes in Sweden. She looked at individuals who became unemployed during 1994. Employers could claim subsidies for adults with unemployment duration of at least 6 months; the grant covered 50 percent of labour costs up to a fixed amount. According to her results, recruitment subsidies were the only scheme that improved the individual probability to get and keep a job. Carling/Richardson (2004) similarly compared the effectiveness of different programmes in reducing the unemployment duration of participants in Sweden, but estimated a hazard rate model instead. Their results were again in favour of subsidised work and training provided by firms, compared to classroom vocational training. Fredriksson/Johansson (2004) and Forslund et al. (2004) analysed the effectiveness of a targeted, time-limited wage subsidy scheme in Sweden. The scheme under consideration was implemented in 1998 and granted up to 50 percent of wages for a maximum of six months to firms who recruited long-term unemployed. Fredriksson/Johansson (2004) applied nonparamet-

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ric survival function matching estimators to demonstrate the importance of the timing of programme entry. While the studies cited so far rely on the large number of observable characteristics available in their data sets to estimate treatment effects, Forslund et al. (2004) additionally used instrumental variable difference-in-difference techniques. Independently of the method used, the results suggested that wage subsidies had a positive effect on employment probabilities of the participants.

For the State of Wisconsin, Hamersma (2005) estimated the effects of eligibility compared to near eligibility on the labour market prospects of the eligible population. The programmes analysed are the “Work Opportunity Tax Credit” and the “Welfare-to-Work Tax Credit”. For both programmes employers have to apply and – if granted – claim the subsidy on their federal tax return. The former programme is targeted at new hires from certain disadvantaged groups and covers up to 40 percent of the wage rate for the first year of employment. The latter programme aims at long-term welfare recipients and reimburses 35 percent of wages in the first year and 50 percent in the second year. Using matching techniques, Hamersma found limited effects on the labour market results of the eligible population. However, her information was restricted to a period of 18 months after the subsidy had started and the numbers of individuals participating in the programme were rather small.

Few studies are based on social experiments, where treatment is allocated randomly among the unemployed (Burtless 1985), or on natural experiments, which utilize changes in legislation and apply difference-in-differences estimators to the treated group and a similar group that is unaffected by the changes (Boockmann et al. 2007). Burtless (1985) presented the results from an early social experiment of the effectiveness of targeted wage subsidies that was conducted in Ohio during the years 1980 to 1981. Some of the job seekers were given vouchers identifying them to employers as eligible for a tax credit or for a direct cash rebate. The subsidy amounted to 50 percent of wages during the first year and 25 percent of wages during the second year of employment, up to a threshold. Burtless showed that unemployed persons with a voucher were

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4 less likely to find employment than job-seekers without vouchers. He speculated that vouchers
5
6 had a stigmatizing effect and were used by employers as a screening device.
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10 For Germany, Boockmann et al. (2007) investigated changes in the legislation regarding the
11
12 EGZ for workers of 50 or older. The regular subsidy used to amount to 50 percent of the wage
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14 rate, paid in monthly rates for up to two years. In exceptional cases, the subsidy could be
15
16 granted at up to 70 percent for up to five years. Two changes took place in 2002 and 2004: In
17
18 2002, eligibility for the programme – which earlier had covered only the long-term unemployed
19
20 – was extended to all workers of 50 or older. In 2004, under the new EGZ, eligibility was again
21
22 confined to hard-to-place persons and the duration of the subsidy was cut down to a new maxi-
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24 mum duration of three years. The authors used a difference-in-differences estimator to compare
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26 changes in transition probabilities between the treatment group (defined as all workers of 50)
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28 and the comparison group (comprised of slightly younger workers). Significant effects of the
29
30 changes in conditions were found only for the subgroup of female workers in East Germany.
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32 The authors concluded that increases in subsidised employment for all other groups investi-
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34 gated are absorbed by deadweight losses. However, only a comparatively small percentage of
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36 individuals in the age groups investigated actually received the subsidy, and changes in legisla-
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38 tion affected mainly the duration of the subsidy.
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43 Finally, a comparison of subsidised and non-subsidised individuals taking-up a job has been
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45 conducted by Cockx et al. (1998). They analysed temporary wage subsidies that have been paid
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47 to employers in Belgium during 1991 and 1992. The subsidy was granted for 12 to 24 months
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49 and covered 10 to 50 percent of the wage rate; it was often targeted at particular groups. The
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51 authors utilized data from firms on their last five recruitments and estimate a duration model.
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53 They found positive, but insignificant effects of the subsidy on job tenure. In the already men-
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55 tioned study for the State of Wisconsin, Hamersma (2005) also estimated the effect of a subsidy
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57 on wages and tenure of subsidized workers, using propensity score matching. She found sig-
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59 nificantly positive effects on wages in the subsidized job (around 40 percent of the tax credit
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4 were passed through to workers in the form of a wage premium), but insignificant effects on
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6 tenure.
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9 Summing up, a number of studies based on non-experimental data obtain the result that targeted
10 wage subsidies improve the labour market chances of the unemployed. The few findings from
11 social and natural experiments are more ambiguous. Across countries, there are large differ-
12 ences in programme sizes as well as in the amount and duration of wage subsidies. Further-
13 more, the implementation of programmes differs across countries. Finally, the institutional
14 framework – for instance replacement rates and the importance of activation strategies – will
15 also have an impact on the success of a labour market programme.
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26 **3 Evaluation approach, data and variables**

27 **3.1 The estimands of interest**

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29 We are interested in the mean effect of taking up subsidized employment during the second
30 quarter of 2002 on the consecutive unemployment and employment rates of participants. The
31 fundamental evaluation problem is caused by the fact that participants in labour market pro-
32 grammes will sort themselves or will be sorted into programmes on the basis of their current as
33 well as of their expected labour market prospects. In the absence of an experimental design, the
34 challenge is to find a very similar group of non-participating individuals, whose outcomes can
35 be interpreted as counterfactual outcomes of the group of participants. We estimate these coun-
36 terfactual outcomes using statistical matching techniques. Appendix A gives a formal definition
37 of the estimated parameter and explains the applied method.
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52 An important topic is the choice of the classification window in time, which defines participa-
53 tion and non-participation in a labour market programme. Following Sianesi (2004, 2008), our
54 comparison state is either no participation at all or participation within a period of three years
55 after the start of the subsidy. Consequently, our estimands display the effect of taking up a sub-
56 sidized job during the second quarter of 2002, compared to no or latter participation in a labour
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4 market programme. As Sianesi and also Fredriksson/Johansson (2004) have pointed out, labour
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6 market programmes in Europe are ongoing and any unemployed is a potential participant. Indi-
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8 viduals may decide against entering a programme because they expect to find a job soon. Thus
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10 selecting a comparison group consisting solely of individuals who never participated in any
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12 programme during the observational period implies to condition on their expected future out-
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14 comes (see Frederiksson/Johansson 2004, Proposition 4).
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18 A novel feature of our analysis is that we estimate three different counterfactuals for persons
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20 entering wage subsidy programmes:
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- 22 1) The effect of taking up a subsidised job versus remaining unemployed.
- 23 2) The effect of taking up a subsidised job versus taking up non-subsidised employment.
- 24 25 26 27 28 29 30 31 32 3) The effect of taking up a subsidised job after a short period of on-the-job training versus
33 participation in on-the-job training only.

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35 The first scenario assumes that – conditional on the observed characteristics of unemployed
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37 persons – access to the wage subsidy is random. However, to place an unemployed person on a
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39 subsidized job, an employer must agree to hire her or him. Accordingly, the fact that a person
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41 has been able to get a subsidised job may as such be a hint on unobserved individual heteroge-
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43 neity. However, unobserved heterogeneity will be strongly correlated with the observed ex-
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45 plaining variables; this should hold particularly for information on an individual's labour mar-
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47 ket history (Heckman et. al 1999). As we have detailed information on previous employment
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49 histories, this should capture most of the effects of unobserved individual factors. Employer
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51 characteristics may be another source of unobserved heterogeneity. Previous studies for Ger-
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53 many indeed find that factors like firm size, industry, sales and previous experiences with wage
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55 subsidies have an influence on the probability that firms make use of wage subsidies (Hartmann
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57 2004, Jaenichen 1999). More recently, there is some evidence that average wages and the size
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59 of firms may influence the success of wage subsidies (Krug et al. 2008). Unfortunately, our
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data are lacking information on employers.

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4 The second scenario has been presented less often in the literature: We compare participants
5 supported with EGZ to persons who have left unemployment for regular employment at the
6 same time. Again, individual heterogeneity could exist, since treated individuals have only been
7 able to find a subsidised job. Here the arguments given above apply again: Information on the
8 previous employment history should absorb most of individual heterogeneity. Still, employer
9 heterogeneity may have a distortive effect on the results.
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11
12 The third scenario is suggested by the observation that subsidized employment is often pre-
13 ceded by a short-term firm-related training measure (“betriebliche Trainingsmaßnahme”). The
14 question we ask is, whether there is an additional advantage of an EGZ in this situation. On the
15 one hand, if short-term training serves as a filter for employment, only workers showing a suf-
16 ficiently high performance during their training period will be offered a subsidised job. On the
17 other hand, employers who are informed about the “true“ productivity after the training period
18 may have better arguments to bargain for a further period of subsidised employment. In this
19 case, workers with combined participation of training and EGZ would be those who really need
20 an integration subsidy.
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40 **3.2 Data and variables**

41 We utilize administrative data collected and provided by the German Federal Employment
42 Agency. The newly constructed research data set Integrated Employment Biographies (IEB)
43 contains detailed information on socio-demographic characteristics, programme participation,
44 employment and unemployment histories. Hummel et al. (2005) describe a sample of the data-
45 base that is open for public use through the Research Data Center of the Federal Employment
46 Agency. The combination of information on programme episodes and on employment episodes
47 allows us to distinguish between times in subsidised employment (through a wage subsidy or in
48 a public job creation measure) and regular employment. Since the data flows integrated in the
49 IEB stem from different sources, we had to correct several inconsistencies.
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4 For our first and second scenario, the treatment sample consists of all individuals taking up
5 subsidised employment during the second quarter of 2002, who have been unemployed before-
6 hand (these were about 85 percent of all individuals receiving an EGZ during this period).
7
8 Thus, our analysis avoids problems of dynamic programme entry by sampling only persons,
9 who started their subsidised employment during a short interval of time. The analysis of the
10 third scenario is based on a sample of all individuals that have participated in firm-related
11 short-term training (with a maximum duration of three months) during the first half-year of
12 2002 and have taken up a subsidised job within three months after the training programme has
13 ended. From those receiving an EGZ in the second quarter of 2002, about 20 percent had previ-
14 ously participated in short-term training; 64 percent of these measures had taken place within
15 establishments.
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20 Special data sets were built up to get a pool of potential control persons for each scenario. For
21 the first scenario, comparison groups were taken from monthly 2.5-percent-samples of entries
22 into unemployment since 2000. For the second scenario, we utilize a 75-percent-sample of en-
23 tries into employment from the first half-year of 2002. Finally, a sample of 310,000 exits from
24 short-term training measures between October 2001 and June 2002 forms the basis for the con-
25 struction of comparison groups for the third scenario.
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29 Our analysis distinguishes between several subgroups of participants. First, all analyses were
30 conducted separately by gender and region (West vs. East Germany). Furthermore, we distin-
31 guish short-term subsidies with duration of four to six months from long-term subsidies lasting
32 from seven to twelve months. Because of small case-loads for short-term subsidies, the impact
33 of the EGZ in the third scenario is only estimated for long-term subsidies. Since own estimates
34 (ZEW et al. 2006) showed no systematic differences across groups with different durations of
35 unemployment, we do not present results for samples stratified by unemployment duration.
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39 We consider two binary outcome variables to describe the labour market status. Our first out-
40 come variable indicates whether an individual is in unsubsidised employment at the beginning
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4 of a month. The second outcome variable shows whether a person has successfully avoided
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6 unemployment at the beginning of a month (he/she is not registered as unemployed or as par-
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8 ticipating in a labour market programme). Beyond the period of subsidization itself, we inter-
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10 pret the follow-up period, during which the employer is obliged to sustain the employment rela-
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12 tionship, as a further period of programme participation. If we observe differences in estimated
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14 treatment effects between the two outcome variables, than the share of individuals in the treated
15
16 group and in the comparison group switching to an “unobserved” state differs, reflecting in
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18 most cases a withdrawal from the labour market for a period of time (examples are home-time
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20 or early retirement). To compute the outcomes of matched comparison persons, we assign to
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22 them the starting date of the treated counterparts in the first scenario. In the second scenario,
23
24 outcomes of comparison persons are measured beginning with the month of entry into employ-
25
26 ment. In the third scenario, we assign a starting date to matched comparison persons by adding
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28 the time span between the participant's training programme exit and his entry into the EGZ to
29
30 the control's end date of the training programme.
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36 The non-testable conditional independence assumption requires the observation of all explain-
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38 ing variables that determine selection into the programme as well as the outcome in the case of
39
40 non-participation. The selection into the programme is the result of individual choice, of case-
41
42 workers' assessments and negotiation strategies as well as of firms' choices and behaviour. We
43
44 model selection using the following observable information on individuals:
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- 47 a) Socio-demographic characteristics (nationality, age, education, health and – for female
48 workers – information on the family status),
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- 51 b) Variables on the five-year-history prior to the respective unemployment spell (participa-
52 tion in measures and years in unsubsidised regular employment),
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- 55 c) The timing of entry into unemployment,
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- 58 d) Information on the regional labour market situation (performance cluster suggested by
59 Blien et al. 2004).
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4 All variables are categorized as dummy variables and measured at the beginning of the subsi-
5 dised employment spell. For members of the comparison groups, time-varying variables are
6 measured in the midth of the first quarter 2002.
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11 Table B.1 in the Appendix shows variable means of selected explanatory variables for subsi-
12 dised workers as well as for our samples of potential control persons (detailed information on
13 the variable means before and after matching can be found in ZEW et al. 2006). Subsidised
14 persons might be regarded to be a positive selection compared to all unemployed, but as a nega-
15 tive selection compared to those who have been hired into a regular unsubsidised job. The sam-
16 ple of individuals, who do not take up a subsidised job after having completed a training mea-
17 sure, might have some labour market advantages compared to those who receive a subsidy later.
18 Participants in East Germany have on average a better qualification than those in West Ger-
19 many. This reflects mainly the fact that the average unemployed person in East Germany is
20 much better qualified; and this is also the case for the population average (Autorengruppe
21 Bildungsberichterstattung 2008, 236). Furthermore, participants in East Germany have less
22 health problems and have more often participated in programmes during their employment his-
23 tory.
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41 As we have already discussed in detail in Section 3.1, we are confident that unobserved indi-
42 vidual heterogeneity should not be a major problem for our analysis. Common support (see
43 Appendix A) is achieved for all treated individuals in our sample. The validity of the stable unit
44 treatment value assumption (see also Appendix A) depends mainly on the size of the pro-
45 gramme. A large-scale programme can be expected to change the supply and demand condi-
46 tions in some segment of the labour market or the labour market as a whole. While (especially
47 in East Germany) the use of wage subsidies has been very intensive in some periods, the impor-
48 tance of subsidized employment compared to regular employment in the period of our study is
49 fairly limited. As Table B.2 shows, the most frequent transition out of unemployment is still
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4 into regular employment. Transitions into (temporarily) subsidised employment like the EGZ
5 accounted for only 2.6 percent of all unemployment exits in 2004.
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10 **4 Empirical results**

11 **4.1 Effect of a subsidy on the treated**

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16 In the first scenario, we estimate the average effect of the EGZ on the employment prospects of
17 previously unemployed persons. The counterfactuals are employment and unemployment rates
18 when participating never or only later in a labour market programme. Table 1 displays in Panel
19 IV mean standardized biases before and after matching. The bias reduction obtained through
20 the matching procedure is on average slightly less than 80 percent, while the absolute value of
21 the mean standardized bias after matching never exceeds the value of 5. Due to these results the
22 quality of the matching seems quite satisfactory.
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33 Panel I and II of Table 1 summarize the results three years after programme start, documenting
34 the share of treated and of matched control persons in regular unsubsidised employment, the
35 share that is not unemployed (or in a labour market measure) as well as average treatment ef-
36 fects on the treated. The evolution of the estimated average treatment effects over time is dis-
37 played in Figure 1. Plots above the abscissa have to be interpreted as a “success” of the par-
38 ticular programme.
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46 Figure 1 shows for the period of subsidization and for the compulsory period of further em-
47 ployment that treated individuals were less often in unsubsidised employment and have less
48 often avoided unemployment. These locking-in effects are a necessary side-effect of the con-
49 struction of the outcome variables. We find large and significant positive effects of the treat-
50 ment on the treated immediately after the end of the support by the programme, which, how-
51 ever, decline slightly over time.
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60 Three years after the start of subsidization, the share in “regular unsubsidised employment” is
still 0.25 to 0.42 higher in the treated group than in the matched comparison group. In other

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4 words, we estimate that without treatment 25 to 42 percentage points of the treated would not
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6 have been regular employed. Turning to our second outcome variable, the difference in the
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8 shares “not unemployed and not in measure” varies between 0.14 and 0.28; thus 14 to 28 addi-
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10 tional percentage points of the treated would have been unemployed without treatment.
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14 The striking difference between both outcome variables results from a different share of indi-
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16 viduals with an unknown state in the treated and the matched comparison group (Panel III of
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18 Table 1). This implies that a higher percentage of untreated individuals withdraw from the la-
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20 bour market as discouraged workers. Thus the subsidies help to activate hard-to-place individu-
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22 als, who might otherwise have withdrawn from the labour market.
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26 Comparisons of estimates of the treatment effect between the different groups investigated must
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28 be interpreted with caution, since characteristics of group members differ for each group (see
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30 Table 1). Nonetheless, some results should be mentioned:
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- 32 • Across comparison groups, the average outcome “unsubsidised employment” (Panel I of
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34 Table 1) does not differ remarkably by gender or region, but is slightly smaller for the long-
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36 term subsidy. However, across treated groups the grant of an EGZ seems to have had a par-
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38 ticularly large effect on employment opportunities of female workers, which was even
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40 stronger in East Germany. Accordingly, estimated treatment effects are much higher for fe-
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42 male than for male workers.
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- 45 • For the outcome “not unemployed and not in measure” (Panel II of Table 1), we observe a
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47 striking result for the matched control sample of females from West Germany – they are less
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49 often unemployed than females from East Germany or males. An obvious explanation is that
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51 housework has always been a rather accepted alternative for women in West Germany,
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53 which facilitates withdrawing from the labour market. In contrast, women in East Germany
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55 are much more attached to the labour market. This is partly a result of the generally higher
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57 labour-force participation rate of women in East Germany (Statistisches Bundesamt 2006,
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59 89), a heritage of working norms in the former communist society. Furthermore, wages are
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4 lower in East Germany, thus couples might be less able to afford one partner staying at
5 home (Statistisches Bundesamt 2006, 336; we are indebted to a referee for pointing this
6 out). Looking at the treatment groups, the outcome is again more advantageous for female
7 workers. The net result is that the estimated treatment effect is highest for the group of fe-
8 males in East Germany and does not differ much among the other groups.

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16 • Comparing the recipients of short-term and long-term EGZ, labour market outcomes are
17 always more favourable for treated individuals who received a short EGZ as well as for their
18 matched control persons. This is not surprising, since the duration of the subsidy should be a
19 function of placement difficulties. However, estimated average treatment effects are rather
20 similar for short- and long-term subsidies at each point of time after the employment promo-
21 tion had expired (Figure 1).
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30 A deadweight loss in a narrower sense only occurs when the same individual would have got
31 the same job without the help of the EGZ. Data and technique applied do not provide a measure
32 for this effect. Some studies for other labour market measures interpret the share of the matched
33 comparison group that has successfully found an unsubsidised job as a measure of the dead-
34 weight loss in a wider sense (Winterhager et al. 2006, 513). In this sense, the deadweight ac-
35 companying EGZ would be around 20 percentage points (Panel I of Table 1).
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43 44 **4.2 Effect of a subsidy conditional on taking up a job**

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46 The previous estimates referred to the combined effect of receiving a subsidy and taking a job,
47 while the second scenario investigates solely the effect of the subsidy, conditional on having
48 found a – subsidised or unsubsidised – job. The results of the estimates can be found in Table 2
49 and Figure 2 (note that only significant effects are displayed in the figures). The information on
50 the mean standardized bias in Panel IV of Table 2 indicates again a satisfactory matching qual-
51 ity, with a mean bias reduction of more than 80 percent through the matching procedure.
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Figure 2 shows clearly the initial lock-in effect of subsidization. However, after the subsidy and the obligation period have expired, the average treatment effect is rather small, follows no obvious trend and is very similar for short-term and long-term subsidies: Three years after the subsidy started, the effect is insignificant for all groups except for female workers in East Germany, which fare slightly better, if they have started in a subsidised employment relationship (Panel I and II of Table 2). Estimated treatment effects do not differ remarkably between both outcome variables; around 10 percentage points of the treatment group as well as of the matched control sample “vanish” into an unobserved labour market state.

In choosing the members of the matched comparison group, we focus on their labour market success – finding an unsubsidised job. We control for a number of observable characteristics of workers, which should also account at least partly for unobserved characteristics (see Section 3.2). However, the matched comparison group will differ from the treated group at least regarding some characteristics of the job. Jobs taken up by unsubsidised workers are probably – in terms of realised or perceived productivity – a positive selection compared to jobs taken up by subsidised workers. The results show, however, that employment prospects of individuals taking-up subsidised employment are – in the longer run – not worse than those of individuals, who entered an unsubsidised employment relationship.

4.3 Effect of a subsidy conditional on participation in a short training measure

In the last scenario, we estimate the average effect of the EGZ on supported individuals conditional on having participated in a firm-related short-term training measure beforehand. The analysis thus hinges on having gained at least some work experience within a firm during the last quarter. However, we cannot distinguish between situations, where employers have already filtered the more suited candidates for a subsidised job from training participants, and situations, where employers claim the subsidy because a worker's lower productivity has become visible during the training period.

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Table 3 and Figure 3 display the main results. The mean standardized bias (Panel IV of Table 3) is reduced considerably through matching for three of four groups, while the bias reduction for female unemployed in West Germany is below 50 percent. The main conclusions are:

- Locking-in effects are rather large for the group of individuals, who take up a subsidised job after a short-term training measure (Figure 3): Matched control persons find regular employment rather quickly and/or avoid unemployment directly after their training measure.
- Labour market outcomes of unemployed, who participated in a short-term training measure and received a wage subsidy (Table 3, Panel I and II), are rather similar to the results of all subsidised individuals (Table 1, Panel I and II for the long-term subsidy). However, labour market outcomes of the matched control sample are much more favourable for previous participants in a short-term training measure. Accordingly, Panel I in Table 3 shows positive treatment effects – in the range of 14 to 31 percentage points – of a subsequent wage subsidy on the probability to be in regular (unsubsidised) employment three years after the start of the subsidy. But these effects are smaller than those found in Panel I of Table 1, which were not conditional on the participation in a short-term training measure.
- Panel II in Table 3 implies that a subsequent wage subsidy increases the probability to avoid unemployment in East Germany. However, for West Germany the analysis finds no additional significant effect of a wage subsidy on the avoidance of unemployment, if a short-term training measure has already taken place.

One explanation might be that (rather cheap) short-term training measures within firms already have a favourable effect on the labour market possibilities of participants; the effect may sometimes be of such a size that a subsequent EGZ does not exert any additional impact. On the other hand, particularly individuals who were found less productive during their training period may have received a subsequent subsidy.

5 Summary

We apply matching methods to estimate the average effect of targeted wage subsidies for hard-to-place workers in Germany. Our results show that wage subsidies may increase the employment prospects of supported workers, compared to being never in any programme or participating only later, to a considerable amount. For previously unemployed individuals, three years after the start of the programme, the share in regular employment is 25 to 42 percent higher in the treatment group than in the matched comparison group. A comparison between groups of unemployed persons taking subsidised employment with matched comparison groups of individuals moving directly into unsubsidised employment indicates that differences in the employment prospects are rather small after three years. Finally, participation in short-term training measures goes hand in hand with better labour market prospects compared to the entire sample of unemployed. This may result, however, from the previous training as well as from the selection into these measures. As a consequence, the effect of subsidization on participants in a previous short-term training measure is more modest: The share in regular employment increases by 14 to 31 percentage points, if a short-term training measure is followed by a wage subsidy.

However, some points deserve further discussion. Comparing the estimated impacts in the first two scenarios, one may be tempted to doubt the effectiveness of subsidies: The comparison group in the second scenario is characterised by a comparable distribution of characteristics, but its members entered unsubsidized employment directly. So, was it really necessary to support those who actually got the subsidy? A simple answer would be that a large part of the subsidized jobs are just deadweight effects. Another explanation is that heterogeneity in the matches for workers with the same characteristics is responsible for these results. Thus, the same worker, who needs a subsidy to get one job, will be fully productive in another job. A line for future research of course is to gather more information on jobs (while we analysed the workers' side of the match only).

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4 The results of the third scenario – even if the estimated impact is positive – raise the question to
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6 what degree short training measures within firms might obtain results similar to an EGZ (at
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8 much lower costs). In addition to the previously raised argument of heterogeneity in the job
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10 matches, selectivity with respect to further EGZ support might occur, since employers learn
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12 about a participant's productivity during the training period. It is also plausible that case man-
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14 agers utilize short training measures in a kind of trial and error process. If a cheaper training
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16 measure suffices to integrate a previously unemployed person into a firm, than the more time-
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18 consuming decision for or against an EGZ may readily be postponed. There is some evidence
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20 from case studies in selected agencies that short training measures in firms and the EGZ were
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22 sometimes seen as substitutes, thus the choice between them was rather an incidental matter
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26 (ZEW et al. 2006, 61).
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29 To conclude, our findings are in line with results from the literature; most studies that estimate
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31 effects of targeted wage subsidy programmes on the treated find positive effects on individual
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33 employment probabilities. For any assessment of the benefits of wage subsidies, it should be
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35 noted that the methods applied in this paper do not identify deadweight loss or potential dis-
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37 placement and substitution effects. In this sense Fay (1996) emphasises that careful controls are
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39 an important part of designing wage subsidy programmes. Otherwise, there is a risk that firms
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41 use schemes as a permanent subsidy to their workforce.
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Tables

Table 1: Individuals taking up a subsidised job (Treated) and matched unemployed persons (Controls): Labour market status, estimated average treatment effect on the treated (ATT), mean standardized bias before and after matching and bias reduction three years after programme start during the 2nd quarter of 2002

		Short-term subsidy				Long-term subsidy				
		Male West	Fe-male West	Male East	Fe-male East	Male West	Fe-male West	Male East	Fe-male East	
I.	Share in regular unsubsidised employment	Treated	0.50	0.56	0.50	0.65	0.44	0.55	0.48	0.60
		Controls	0.23	0.23	0.24	0.24	0.19	0.21	0.22	0.22
		ATT	0.27	0.34	0.26	0.42	0.25	0.35	0.26	0.38
II.	Share not unemployed and not in labour market measure	Treated	0.62	0.73	0.60	0.72	0.57	0.70	0.58	0.68
		Controls	0.47	0.58	0.44	0.44	0.44	0.54	0.40	0.43
		ATT	0.15	0.14	0.15	0.28	0.13	0.16	0.18	0.25
III.	Share with unknown destination (Diff. II-I)	Treated	0.12	0.16	0.10	0.06	0.13	0.14	0.10	0.09
		Controls	0.24	0.35	0.20	0.20	0.25	0.33	0.18	0.22
IV.	Mean standardized bias	Before	10.42	12.13	12.32	11.93	13.16	13.92	13.37	12.62
		After	2.03	4.10	3.12	4.90	2.49	3.09	1.78	2.44
		Reduction	0.80	0.66	0.75	0.59	0.81	0.78	0.87	0.81
V.	Observations		1269	597	339	242	1880	983	3293	3045

Note: All estimated treatment effects are significant at $\alpha = 0.05$. Short-term subsidies are paid for 4 to 6 months, while long-term subsidies are paid for 7 to 12 months. Small differences in the number of observations compared to Table B.1 result from the fact that each matched control person was assigned the date of the treatment start of his/her treated counterpart. If the matched control person was no longer unemployed on this date (which is the case for few observations, since we analyse only entries in a period of three months and also match on the duration of unemployment), the matched pair was dropped from further analysis.

Table 2: Individuals taking up a subsidised job (Treated) and matched persons taking up an unsubsidised job (Controls): Labour market status, estimated average treatment effect on the treated (ATT), mean standardized bias before and after matching and bias reduction three years after programme start during the 2nd quarter of 2002

		Short-term subsidy				Long-term subsidy			
		Male West	Female West	Male East	Female East	Male West	Female West	Male East	Female East
I. Share in regular unsubsidised employment	Treated	0.50	0.57	0.48	0.64	0.44	0.56	0.49	0.60
	Controls	0.49	0.60	0.47	0.50	0.46	0.56	0.45	0.52
	ATT	n.s.	n.s.	n.s.	0.14	n.s.	n.s.	n.s.	0.08
II. Share not unemployed and not in labour market measure	Treated	0.62	0.72	0.60	0.70	0.57	0.70	0.58	0.68
	Controls	0.61	0.74	0.59	0.62	0.59	0.71	0.55	0.64
	ATT	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	0.04
III. Share with unknown destination (Diff. II-I)	Treated	0.12	0.15	0.11	0.06	0.13	0.14	0.09	0.09
	Controls	0.12	0.15	0.12	0.12	0.14	0.15	0.10	0.12
IV. Mean standardized bias	Before	15.85	13.00	16.45	14.66	20.75	15.41	19.94	14.38
	After	1.77	3.10	2.53	4.29	1.91	2.53	1.45	2.06
	Reduction	0.89	0.76	0.85	0.71	0.91	0.84	0.93	0.86
V. Observations		1398	657	372	253	2044	1064	3580	3379

Note: n.s. = not significant at $\alpha = 0.05$. Short-term subsidies are paid for 4 to 6 months, while long-term subsidies are paid for 7 to 12 months.

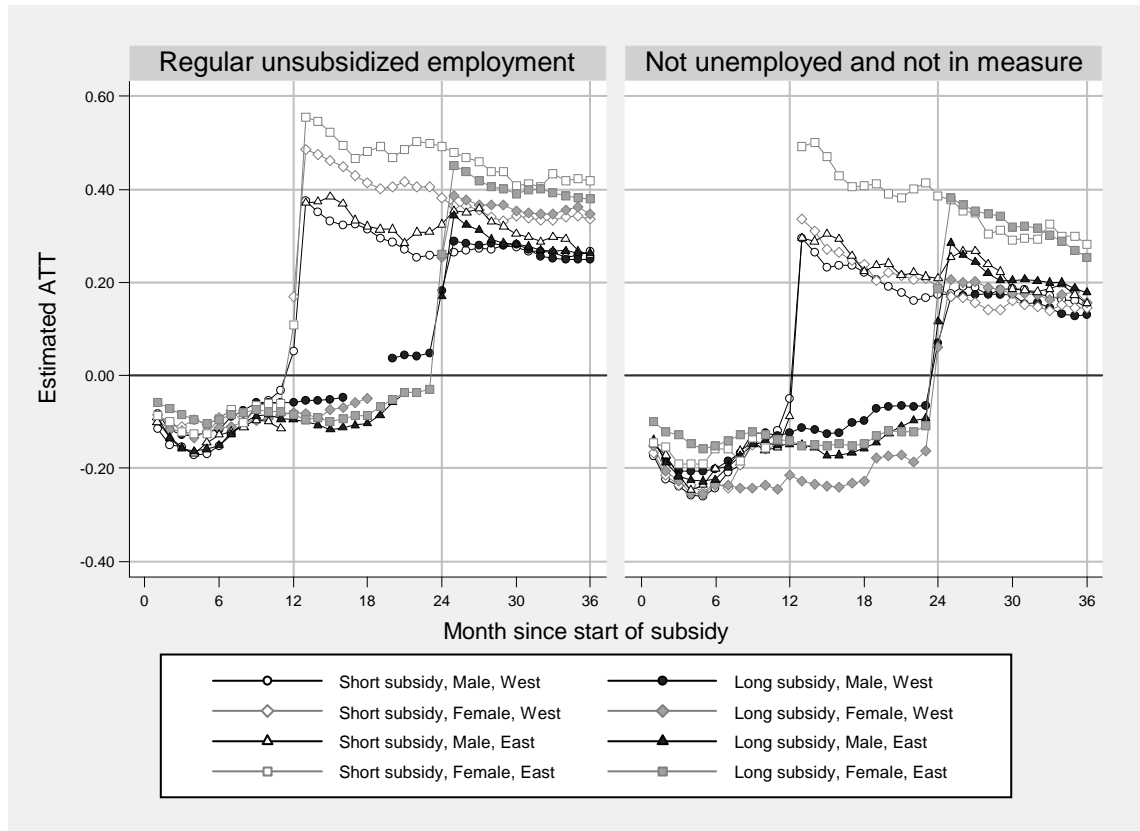
Table 3: Individuals taking up a subsidised job following a short-term training measure (Treated) and matched persons that have participated solely in a short-term training measure (Controls): Labour market status, estimated average treatment effect on the treated (ATT), mean standardized bias before and after matching and bias reduction three years after programme start during the 2nd quarter of 2002

			Long-term subsidy			
			Male West	Female West	Male East	Female East
I.	Share in regular unsubsidised employment	Treated	0.48	0.59	0.52	0.65
		Controls	0.34	0.38	0.29	0.34
		ATT	0.14	0.21	0.23	0.31
II.	Share not unemployed and not in labour market measure	Treated	0.58	0.67	0.59	0.69
		Controls	0.53	0.65	0.39	0.44
		ATT	n.s.	n.s.	0.20	0.25
III.	Share with unknown destination (Diff. II-I)	Treated	0.10	0.08	0.07	0.05
		Controls	0.19	0.27	0.10	0.11
IV.	Mean standardized bias	Before	12.39	11.36	10.47	10.83
		After	3.14	6.06	1.29	2.06
		Reduction	0.75	0.47	0.88	0.81
V.	Observations		285	117	537	551

Note: n.s. = not significant at $\alpha = 0.05$. Long-term subsidies are paid for 7 to 12 months.

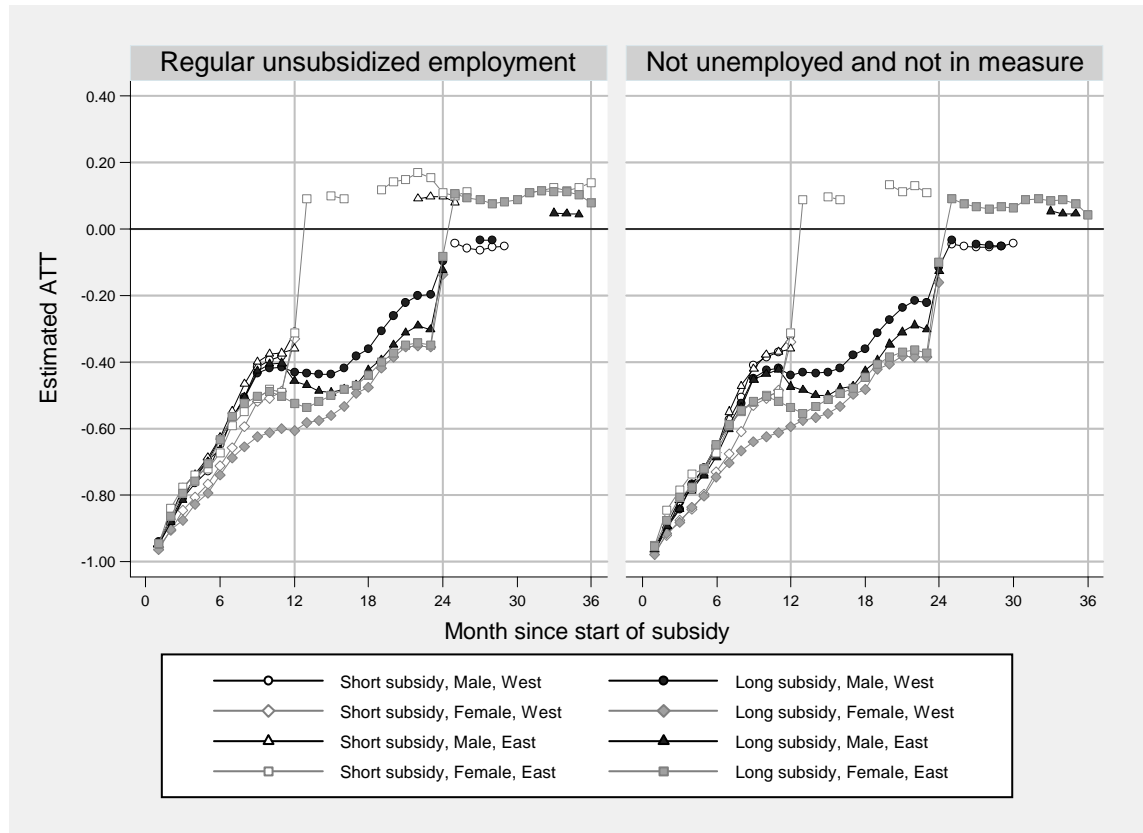
Figures

Figure 1: Estimated average treatment effects on individuals taking up a subsidised job (ATT) during the 2nd quarter of 2002



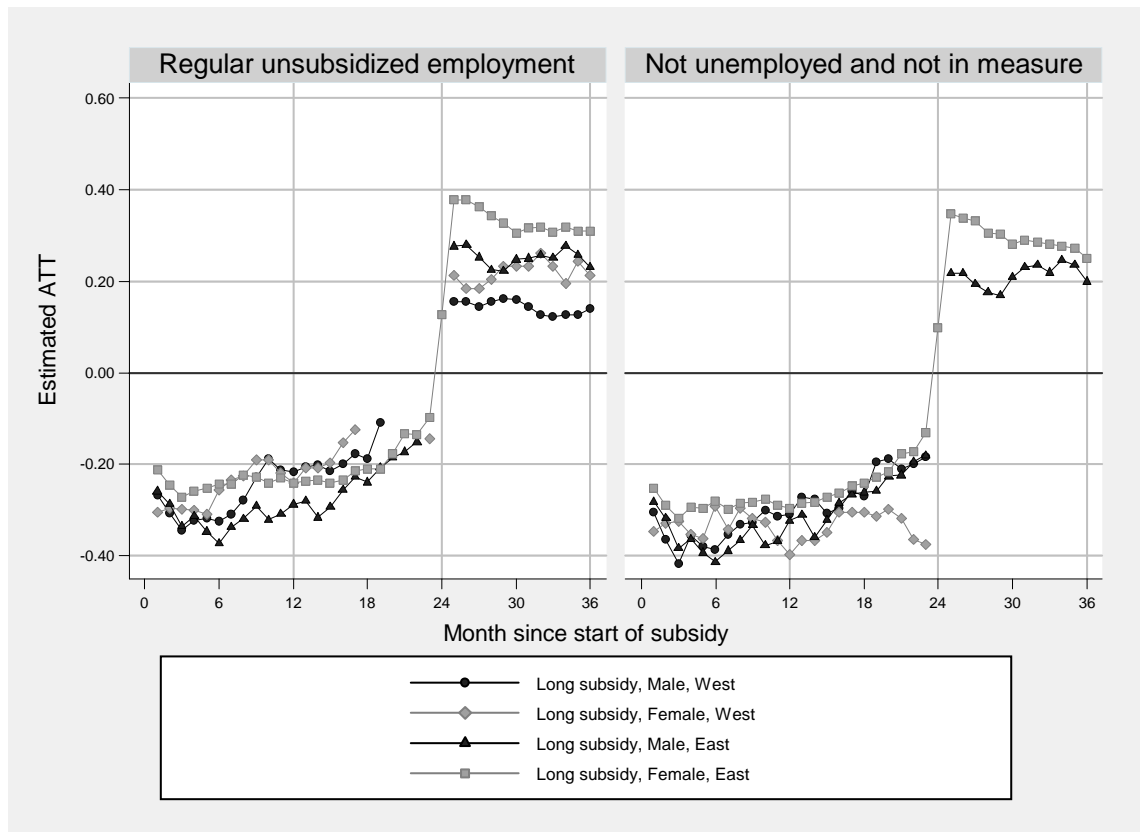
Note: Displayed effects are significant at $\alpha = 0.05$. Short-term subsidies are paid for 4 to 6 months, while long-term subsidies are paid for 7 to 12 months.

Figure 2: Estimated average treatment effects on individuals receiving a subsidy conditional on taking up a job (ATT) during the 2nd quarter of 2002



Note: Displayed effects are significant at $\alpha = 0.05$. Short subsidies are paid for 4 to 6 months, while long subsidies are paid for 7 to 12 months.

Figure 3: Estimated average treatment effects on individuals taking up a subsidised job conditional on having participated in a short-term training measure (ATT) during the 2nd quarter of 2002



Note: Displayed effects are significant at $\alpha = 0.05$. Long subsidies are paid for 7 to 12 months.

Appendix A: Method

The potential outcome approach (Rubin 1974, Lechner 1999, Heckman et al. 1999) assumes two hypothetical outcomes for every person. Y_1 is the potential outcome if a person participates in a programme, Y_0 is the potential outcome in the case of non-participation. The binary variable D^t distinguishes participants ($D^t = 1$) from non-participants ($D^t = 0$). Participants have entered the EGZ programme during a month t of the second quarter of 2002. The stable unit treatment value assumption requires that no spillover effects take place (Rosenbaum/Rubin 1983).

The parameter we estimate is the mean effect of taking up subsidized employment during the analyzed quarter on the employment prospects of participants. It is given by the expected difference in an individual's two potential outcomes in $t + h$, where $h = 0 \dots 36$ is measured in months since entry into the subsidized job:

$$(1) \quad E[Y_1^{t+h} - Y_0^{t+h} | (D^t = 1)] = E[Y_1^{t+h} | (D^t = 1)] - E[Y_0^{t+h} | (D^t = 1)].$$

To estimate $E[Y_0^{t+h} | (D^t = 1)]$ – the counterfactual average outcome of participants in the case of non-participation – matching methods choose a comparison group of non-participants under the assumption of unconfoundedness (Rosenbaum/Rubin 1983) or conditional independence (Lechner 1999): Conditional on a vector X – containing all variables that jointly influence participation and outcomes – the outcome in the case of non-participation should not differ between participants and comparison persons. An estimator for (1) is then given by

$$(2) \quad E[Y_1^{t+h} - Y_0^{t+h} | (D^t = 1)] = E[Y_1^{t+h} | (X, D^t = 1) - Y_0^{t+h} | (X, D^t = 0)].$$

Propensity score matching additionally uses the fundamental result from Rosenbaum/Rubin (1983), and conditions simply on participation probabilities $P(X) = P(D^t = 1 | X)$: In a first step, propensity scores for participants and non-participants are estimated – for instance by a probit model – using the vector X as exogenous variables. The second step consists of a selection of a comparison group such that the distributions of the propensity scores are similar (balanced) for participants and controls. “Common support” requires that the realizations of X (and the values

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2
3
4 of the estimated propensity scores) can be observed for both participants and non-participants
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6 (Heckman et al. 1999).
7

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9 In this paper, we use nearest-neighbour matching with replacement and apply the STATA-
10 module *psmatch2* (Sianesi/Leuven 2003). The matching procedure chooses for each participant
11 the non-participant with the most similar propensity score as a comparison person. Replacement
12 implies that a non-participant can be used more than once in the matching procedure. Following
13 (2), the programme impact is estimated as the mean difference in the outcomes of both groups.
14 To obtain an estimate for the variance of the estimator a formula suggested by Lechner (2001)
15 is used, which accounts for the possible variance-inflating effect of the multiple uses of non-
16 participants. As an indicator for matching quality, we compute the mean standardized bias
17 (MSB, Rosenbaum/Rubin 1983) between each treated group and its matched comparison group
18 for each variable from X . The standardized bias of a covariate is defined as the difference of
19 means in the treated and matched control sample, divided by the square root of the average
20 sample variance. A lower value of the MSB indicates more similarity between both groups.
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Appendix B: Additional tables

Table B.1: Variable means of selected attributes (0 = no, 1 = yes) for individuals taking up short-term (S) or long-term (L) subsidised employment, the sample of unemployed persons (U), the sample of individuals taking up unsubsidised employment (E), individuals taking up long-term subsidised employment following a short-term training measure (LM) and the sample of individuals that have participated in a short-term training measure (M)

Variables (0 = no, 1 = yes)	Male						Female					
	S	L	U	E	LM	M	S	L	U	E	LM	M
West Germany												
<i>Individual Characteristics</i>												
Foreign nationality	0.15	0.16	0.21	0.16	0.13	0.12	0.07	0.10	0.16	0.10	0.14	0.08
Age 25 to 29	0.20	0.16	0.16	0.19	0.19	0.21	0.13	0.10	0.12	0.15	0.13	0.16
Age 30 to 34	0.19	0.19	0.17	0.21	0.16	0.21	0.16	0.20	0.15	0.18	0.24	0.20
Age 35 to 39	0.23	0.21	0.17	0.20	0.20	0.21	0.26	0.23	0.17	0.20	0.21	0.23
Age 40 to 44	0.18	0.22	0.15	0.16	0.25	0.16	0.20	0.22	0.16	0.18	0.18	0.18
Age 45 to 49	0.15	0.16	0.12	0.11	0.17	0.11	0.19	0.20	0.14	0.14	0.21	0.13
Age 50 to 54	0.04	0.04	0.10	0.08	0.02	0.08	0.05	0.05	0.12	0.09	0.03	0.08
Age 55 to 59	0.01	0.01	0.08	0.04	0.01	0.03	0.01	0.01	0.10	0.04	-	0.02
Age 60 to 64	0.00	0.00	0.04	0.01	-	-	-	0.00	0.03	0.01	-	-
Lower secondary degree (9 years) or less, no vocational training	0.37	0.36	0.38	0.30	0.34	0.28	0.24	0.22	0.35	0.25	0.21	0.20
Medium secondary degree (10 years), no vocational training	0.02	0.03	0.03	0.02	0.03	0.03	0.04	0.05	0.05	0.04	0.03	0.05
Vocational training	0.51	0.50	0.47	0.57	0.53	0.57	0.59	0.55	0.45	0.52	0.62	0.54
Highest secondary degree (12-13 years)	0.06	0.07	0.07	0.06	0.06	0.09	0.09	0.11	0.09	0.11	0.08	0.14
University degree	0.04	0.04	0.05	0.05	0.04	0.04	0.04	0.06	0.07	0.07	0.07	0.07
Health problems	0.18	0.19	0.21	0.10	0.21	0.16	0.15	0.13	0.19	0.10	0.14	0.14
Disabled at least 50%	0.01	0.02	0.04	0.02	0.04	0.08	0.02	0.02	0.04	0.02	0.03	0.08
<i>Five-year-history</i>												
Measure of active labour market policy	0.54	0.57	0.38	0.25	1.00	1.00	0.56	0.61	0.34	0.31	1.00	1.00
No unsubsidised regular employment	0.09	0.13	0.17	0.05	0.15	0.10	0.16	0.17	0.23	0.10	0.17	0.16
Up to 1 year in regular employment	0.16	0.21	0.15	0.07	0.25	0.15	0.16	0.20	0.12	0.09	0.22	0.15
1-2 years in regular employment	0.18	0.21	0.15	0.10	0.19	0.16	0.14	0.17	0.13	0.13	0.15	0.15
2-3 years in regular employment	0.20	0.19	0.16	0.14	0.19	0.16	0.19	0.17	0.15	0.15	0.14	0.15
3-4 years in regular employment	0.24	0.22	0.19	0.27	0.18	0.20	0.24	0.23	0.17	0.21	0.26	0.18
4-5 years in regular employment	0.13	0.05	0.18	0.36	0.05	0.22	0.11	0.05	0.19	0.32	0.06	0.22
Number of observations	1398	2044	69393	113584	285	10321	657	1064	45610	58798	117	5561
East Germany												
<i>Individual Characteristics</i>												
Foreign nationality	0.03	0.04	0.05	0.03	0.01	0.01	0.02	0.02	0.04	0.02	0.01	0.01
Age 25 to 29	0.16	0.17	0.13	0.14	0.20	0.17	0.11	0.10	0.09	0.11	0.08	0.12
Age 30 to 34	0.17	0.20	0.14	0.17	0.18	0.17	0.21	0.22	0.14	0.15	0.21	0.19
Age 35 to 39	0.23	0.21	0.16	0.19	0.20	0.19	0.25	0.24	0.16	0.20	0.29	0.22
Age 40 to 44	0.24	0.22	0.17	0.18	0.25	0.18	0.23	0.23	0.17	0.20	0.23	0.20
Age 45 to 49	0.20	0.19	0.15	0.16	0.16	0.15	0.18	0.20	0.16	0.17	0.18	0.16
Age 50 to 54	0.01	0.01	0.12	0.10	0.01	0.10	0.02	0.01	0.15	0.11	0.01	0.09
Age 55 to 59	-	0.00	0.09	0.05	0.00	0.03	-	0.00	0.12	0.05	-	0.03
Age 60 to 64	-	0.00	0.04	0.01	-	-	-	-	0.02	0.00	-	-
Lower secondary degree (9 years) or less, no vocational training	0.08	0.10	0.14	0.07	0.07	0.07	0.04	0.05	0.14	0.08	0.04	0.06
Medium secondary degree (10 years), no vocational training	0.03	0.03	0.03	0.02	0.02	0.02	0.02	0.04	0.04	0.04	0.03	0.03
Vocational training	0.82	0.80	0.74	0.84	0.83	0.82	0.83	0.81	0.72	0.77	0.86	0.80
Highest secondary degree (12-13 years)	0.04	0.03	0.04	0.03	0.04	0.04	0.07	0.06	0.05	0.06	0.04	0.06
University degree	0.04	0.03	0.05	0.03	0.03	0.05	0.04	0.04	0.05	0.06	0.03	0.05
Health problems	0.10	0.11	0.16	0.07	0.11	0.09	0.11	0.08	0.15	0.07	0.05	0.08
Disabled at least 50%	0.01	0.00	0.02	0.01	0.00	0.03	0.01	0.00	0.02	0.02	0.01	0.04
<i>Five-year-history</i>												
Measure of active labour market policy	0.58	0.57	0.55	0.37	1.00	1.00	0.72	0.70	0.64	0.53	1.00	1.00
No unsubsidised regular employment	0.07	0.08	0.14	0.04	0.09	0.07	0.19	0.19	0.27	0.12	0.21	0.18
Up to 1 year in regular employment	0.16	0.15	0.20	0.08	0.14	0.16	0.31	0.26	0.26	0.17	0.28	0.26
1-2 years in regular employment	0.13	0.19	0.17	0.11	0.21	0.16	0.14	0.18	0.15	0.17	0.17	0.18
2-3 years in regular employment	0.22	0.23	0.16	0.15	0.20	0.17	0.13	0.17	0.12	0.18	0.18	0.13
3-4 years in regular employment	0.31	0.30	0.18	0.26	0.30	0.22	0.16	0.16	0.11	0.17	0.13	0.12
4-5 years in regular employment	0.11	0.06	0.16	0.37	0.06	0.22	0.07	0.03	0.09	0.18	0.03	0.12
Number of observations	372	3580	42832	58803	537	7900	253	3379	31829	25958	551	4918

Note: Short-term subsidies are paid for 4 to 6 months, while long-term subsidies are paid for 7 to 12 months.

Table B.2: Transitions from unemployment during the year 2004:
Employment states following unemployment exits in 2004

	Transition to ...	Germany		West		East	
		in 1000	in %	in 1000	in %	in 1000	in %
I.	Labour Market Programmes	1678	29	1000	25	678	39
	• Wage Subsidy	151	3	78	2	73	4
	• Self-Employment (with assistance)	272	5	186	5	87	5
	• Training Programme (short- and long-term measures)	987	17	650	16	337	19
	• Public Job Creation	267	5	86	2	181	10
II.	Employment	2105	37	1517	38	588	34
	• Regular Employment	1830	32	1305	32	525	30
	• Mini-Jobs (max. 360 €/month)	252	4	192	5	60	3
	• Self-Employment (without assistance)	23	0	20	0	4	0
III.	Other (educational system, non-participation)	1972	34	1510	37	462	27
IV.	Number of Spells	5754	100	4026	100	1728	100

Source: Own calculations based on Rothe (2007).