

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

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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 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

During the last decade, active labour market policies have been increasingly under review. 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 has been a major vehicle used by many governments to stimulate employment; examples for the latter are the “Earned Income Tax Credit” in the US and the “Working Families Tax Credit” in Great Britain. Marginal wage subsidies concern only a firm’s additional employment exceeding some reference level (Knabe et al. 2006). Targeted wage subsidies – or hiring subsidies as Orzag/Snowder (2003) use the term – are tailored to particular groups of unemployed and typically granted for a limited period. Although sometimes a wider definition is used, we follow Fay (1996) in interpreting wage subsidies as payments to employers.

This paper analyses the effectiveness of targeted wage subsidies – covering a share of labour costs and paid to employers for a fixed period of time – for hard-to-place workers in Germany, who took up subsidized employment during the second quarter of 2002. The programme will be described in detail in Section 2, which also discusses recent results from the literature. Section 3 presents the econometric approach, the evaluation strategy and describes data and variables underlying the empirical analysis. 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 a subsidy on the treated compared to non-participation: We also estimate the effect of receiving a subsidy 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 (Hamer-mesh 1993, Chapter 5): They obviously reduce labour costs of a given employee for a firm. The subsidy 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. The size of the effect is larger, when the wage elasticities of labour supply and labour demand are small. 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).

However, Fay (1996) makes the point that substitution effects may not be considered that important from a policy perspective since the targeted subsidy schemes are intended to “shuffle the queue” of job-seekers. Subsidies targeted at the long-term unemployed may lead employers to hire them instead of short-term unemployed, who would have been recruited in the absence of the subsidy.

As will be discussed in detail below, wage subsidies differ from other labour market instruments in the sense that participation also depends on the employer's hiring behaviour. For consistency, we will nevertheless use some expressions commonly used in the literature on the evaluation of social programmes. Thus, in what follows, the wage subsidy analysed will often be called a programme or a treatment, with the persons supported called participants or treated persons.

2 Programme features and empirical evidence

2.1 Characteristics of the programme

The programme we are dealing with in this paper is one of three variants of a wage subsidy to employers – called “Eingliederungszuschuss“ (EGZ) – that were in place during the period 1998 to 2003. We concentrate on the variant for hard-to-place workers (“EGZ bei erschwelter Vermittlung“), whose target group are unemployed with severe problems of reintegration, like e.g. long-term unemployed or disabled persons. Of the other two variants, one is characterized by a rather low level of targeting; to get the subsidy, the employer must give reasons for special training requirements. The other is targeted at workers of age 50 and older; in Germany unemployment rates are high and reemployment-chances are low for the over 50s.

The so-called Hartz-reforms, which were initiated in 2002, caused a fundamental revision of active and passive labour market policies in Germany. A legal reform of the

EGZ was enacted in 2004, when the three former variants were collapsed into a single wage subsidy for hard-to-place workers, with a looser definition of target groups and less generous financial support. In the past, partly also in the period under study, there had been very similar wage subsidy programmes also administered by the Federal Employment Agency (see Jaenichen 2000; ZEW et al. 2006). Since 2004, however, the EGZ and a weakly targeted wage subsidy for hires in newly founded firms are the most important wage subsidy programmes in Germany. Among smaller programmes, there is a subsidy for the support of severely disabled individuals and another for the promotion of job rotation.

The decision to support an unemployed with an EGZ has to be reasoned in each individual case. Case managers in local employment agencies have latitude in the allowance decision as well as in the fixing of the amount and duration of the subsidy. The EGZ for hard-to-place persons could regularly account for as much as 50 percent of the monthly wage or salary and continue for at most 12 months. These limits could be exceeded in exceptional cases. As a special feature of the EGZ, a follow-up period of further employment is obligatory after the expiration of the subsidy. If a person hired with an EGZ is dismissed within this period for reasons attributable to the employer, the employer can be asked to reimburse part of the subsidy.

2.2 Importance of the programme

The EGZ and more generally 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 and job creation in the public sector 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. The EGZ is characterized by fairly high numbers of participants from 2001 to 2003, while other wage subsidy programmes phased out in this period. A peak was reached in 2002 with roughly 190,000 entries into one of the three variants of the EGZ and 80,000 entries into the EGZ for hard-to-place workers. As a consequence of the high stock of participants, total expenditure for the EGZ reached a maximum of 1.3 billion Euro in 2003. After 2002, the expenditures for active labour market policies decreased quite drastically. The budget for the most important programmes (“Eingliederungstitel“) dropped from 13.5 billion in 2002 to 9.1 billion in 2004.

Several shifts in the relative importance of different programmes accompanied this period: The previously most important programme for job creation in the public sector nearly disappeared. On the other hand, two programmes offering financial support for

unemployed persons founding their own businesses grew in numbers. While the expenditure for EGZ dropped as well, the EGZ share of the total budget experienced a slight increase from 9.1 percent in 2002 to 10.0 percent in 2004 (Bernhard et al. 2006). There was a further structural break in German labour market policies in early 2005, when the former unemployment assistance for long-term unemployed persons was integrated with the former social assistance.

Table 1: 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).

How important are active labour market programmes and especially wage subsidies for transitions out of unemployment? First, long-term unemployment is still a huge problem in Germany. According to OECD Data, long-term unemployment was equal to or above 50 percent of total unemployment in every year from 2003 to 2005 (OECD 2006). Second, the most frequent transition out of unemployment is still into regular employment. However, as Table 1 shows, transitions from unemployment into various labour market programmes accounted for nearly 30 percent of all unemployment exits and thus came close to exits into regular employment, which accounted for 32 percent of all unemployment exits in 2004 (Rothe 2007). The large share of exits into programmes is mostly due to the short-term training measures and to start-up programmes that promote self-employment of formerly unemployed. In contrast, transitions into (temporarily) subsidised employment like the EGZ accounted for only 2.6 percent of all unemployment exits in 2004. Thus, looking at aggregate figures, exits into a wage subsidy programme are of limited importance for leaving unemployment in Germany. Furthermore, compared to non-subsidised jobs, subsidised jobs only account for a small fraction of transitions to employment.

2.3 Selected review of the literature

A number of studies have estimated the impact of targeted wage subsidies on participants. This requires knowledge of the potential that labour market prospects participants would have had without 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 and helped to avoid further unemployment.

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.

Furthermore, 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 nonparametric survival function matching estimators to demonstrate the importance of the timing of programme entry. Their result was again that longer run effects are positive. While the studies cited so far rely on the large num-

ber of observable characteristics available in their data sets to estimate treatment effects, Forslund et al. (2004) additionally used instrumental variable difference-in-difference techniques. However, 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 to two wage subsidy programmes on employment, comparing those who are eligible with those who are nearly eligible. 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. Both programmes were subject to an upper bound of earnings subsidised. Hamersma used a difference-in-differences matching estimator. She 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). An early social experiment of the effectiveness of targeted wage subsidies dates back to Burtless (1985), who investigated a programme 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 less likely to find employment than job-seekers without vouchers. He speculated that vouchers had a stigmatizing effect and were used by employers as a screening device.

For Germany, Boockmann et al. (2007) investigated changes in the legislation regarding the EGZ for workers of 50 or older. The regular subsidy used to amount to 50 percent of the wage rate, paid in monthly rates for up to two years. In exceptional cases, the subsidy could be granted at up to 70 percent for up to five years. Two changes took place in 2002 and 2004: In 2002, eligibility for the programme – which earlier had covered only

the long-term unemployed – was extended to all workers of 50 or older. In 2004, under the new EGZ, eligibility was again confined to hard-to-place persons, which implies a stricter definition of the target group than the age restriction alone. The special conditions for older workers with respect to the duration of the subsidy were cut down to a new maximum duration of three years. The authors used a difference-in-differences estimator to compare changes in transition probabilities between the treatment group (defined as all workers of 50) and the control group (comprised of slightly younger workers). Significant effects of the changes in conditions were found only for the subgroup of female workers in East Germany. The authors concluded that increases in subsidised employment for all other groups investigated are absorbed by deadweight losses. However, in interpreting the results, one has to take into account that only a comparatively small percentage of individuals in the age groups investigated actually received the subsidy and that changes in legislation affected mainly the duration of the subsidy.

Finally, a comparison of subsidised and non-subsidised individuals taking-up a job has been conducted by Cockx et al. (1998). They analysed temporary wage subsidies that have been paid to employers in Belgium during 1991 and 1992. The subsidy was granted for 12 to 24 months and covered 10 to 50 percent of the wage rate; it was often targeted at particular groups. The authors utilized data from firms on their last five recruitments and estimate a duration model. They found positive, but insignificant effects of the subsidy on job tenure. They found positive, but insignificant effects of the subsidy on job tenure. Similarly, in the already mentioned study for the State of Wisconsin, Hamersma (2005) estimated the effect of a subsidy on wages and tenure using propensity score matching. She found significantly positive effects on wages in the subsidized job (around 40 percent of the tax credit were passed through to workers in the form of a wage premium), but insignificant effects on tenure.

Summing up, a number of studies based on non-experimental data obtain the result that targeted wage subsidies improve the labour market chances of the unemployed. The few findings from social and natural experiments are more ambiguous. However, one has to be careful in comparing results from different countries: There are large differences in programme sizes as well as in the amount and duration of wage subsidies. The implementation of such measures differs across countries. Finally, the institutional framework – for instance replacement rates and the importance of activation strategies – will also have an impact on the success of a labour market programme.

3 Evaluation approach, data and variables

3.1 Econometric strategy

We are interested in the impact of the subsidy on the labour market outcomes of participants, in particular on their chances to remain employed or to become unemployed again. The fundamental evaluation problem is caused by the fact that participants in labour market programmes will sort themselves (or will be sorted) into programmes on the basis of their current as well as of their expected labour market prospects.

In the absence of an experimental design, a simple comparison of the outcomes of participants with the outcomes of a sample of persons, who did not participate, will usually lead to biased evaluation results: The selection of participants into the programme on the basis of individual characteristics induces a correlation of these characteristics and the observed post-programme outcomes. The idea of constructing control groups is to find a very similar group of individuals, who have not participated (or participated only later), such that the outcomes of this group can be interpreted as counterfactual outcomes of the group of participants.

The formal notation follows the potential outcome approach (Rubin 1974, Lechner 1999, see also Heckman et al. 1999). We think of every person having two potential (post-programme) outcomes, so let Y_1 be the potential outcome for the case that a person participates in a programme and Y_0 the potential outcome in the case of non-participation. In our evaluation, participation stands for an entry into the EGZ programme during the second quarter (q2) of 2002. We utilize a binary variable D^{q2} to distinguish between participants ($D^{q2} = 1$) und non-participants ($D^{q2} = 0$). Let us furthermore assume that the programme does not have effects on the labour market outcomes of non-participants; this is the stable unit treatment value assumption.

The parameter we want to estimate, the mean effect of treatment on the treated, is given by the expected difference in an individual's two potential outcomes:

$$(1) \quad E[Y_1 - Y_0 | (D^{q2} = 1)] = E[Y_1 | (D^{q2} = 1)] - E[Y_0 | (D^{q2} = 1)].$$

The first term on the right-hand side can easily be estimated by the mean of the observed outcomes of participants. However, there is no such simple empirical equivalent for the second term on the right-hand side, representing the potential outcomes of participants in the case of non-participation. The outcomes of an arbitrary group of non-participants do not provide a good estimate of the participants' counterfactuals, if access to the pro-

programme is correlated with individual characteristics. In our wage subsidy problem, the participants will have better labour market prospects than the typical long-term unemployed, because at least they managed to get a subsidised job. At the same time, they will have worse labour market prospects than the average unemployed moving to a regular job without the support of a subsidy. Thus $E[Y_0 | (D^{q2} = 0)]$ does not provide an estimator of $E[Y_0 | (D^{q2} = 1)]$.

Matching methods solve this problem by choosing a control group with a distribution of characteristics similar to the distribution observed for the group of participants. They rely on the conditional independence assumption (Rosenbaum/Rubin 1983, Lechner 1999), which requires $Y_0 \perp D^{q2} | X$, where \perp denotes statistical independence. The vector X contains all variables that jointly influence selection into the programme as well as post-programme outcomes. The interpretation is that – conditional on X – the outcome when non-participating does not differ between participants and non-participants. Hence it is legitimate to simulate the non-participation outcomes of participants using the outcomes of non-participants.

The estimator for the mean effect of treatment is then given by

$$(2) \quad E[Y_1 - Y_0 | (D^{q2} = 1)] = E[Y_1 | (X, D^{q2} = 1) - Y_0 | (X, D^{q2} = 0)].$$

The estimation of (2) using the outcomes of a control group is valid only if there are non-participants with characteristics similar to those of participants over the whole X - space. This is the condition of common support and implies that the distributions of the X -variables must be overlapping for all values of X (Heckman et al. 1999). With targeted programmes, the fulfilment of this condition cannot be assumed a priori.

Propensity score matching is a two-stage method suggested by Rosenbaum/Rubin (1983): Instead of conditioning on every single element of X in estimating the treatment effect, it suffices to condition on the participation probability, respectively the propensity score $P(X) = P(D^{q2} = 1 | X)$. In a first step, the propensity score for participants and non-participants is estimated – for instance by a probit model – using the vector X as exogenous variables. Common support here implies that the evaluation is confined to the interval in which there are values of the propensity score for both groups. The second step consists of a selection of a control group such that the distributions of the propensity scores are similar (balanced) for participants and controls.

In this paper, we use nearest-neighbour matching with replacement and apply the STATA-module *psmatch2* (Sianesi/Leuven 2003). Matching is performed by choosing a non-participant for every participant, such that the distance between their propensity scores is minimized. Replacement implies that a non-participant can be used more than once in the matching procedure. The programme impact is estimated as the mean difference in the outcomes of both groups. To compute the variance of the estimator, we use a simple variance formula (Lechner 2001), which accounts for the possible variance-inflating effect of the multiple uses of non-participants. However, the formula neglects the uncertainty involved in the estimation of the propensity score.

To assess the quality of the matching we compute – before and after the matching took place – the mean standardized bias (MSB, Rosenbaum/Rubin 1983) of the explaining variables between each treated group and its matched control group. The standardized bias of a covariate is defined as the difference of means in the treated and matched control sample, divided by the square root of the average sample variance. A lower value of the MSB indicates more similarity between both groups.

3.2 Evaluation strategy

The aim of constructing control groups is to impute the counterfactual outcomes for participants, if the latter had not taken up the subsidised job at the time they actually did. While matching methods help to balance the distribution of individual characteristics between the groups of participants and non-participants, it is nevertheless important to be precise about what is the counterfactual situation of interest.

The evaluation period usually starts with the entry into the programme and covers the time interval for which the outcomes of participants and control group are evaluated. Prevailing examples of control group definitions are:

- a) Persons, who do not participate in any labour market programme during the whole evaluation period (e.g. Gerfin/Lechner 2002).
- b) Persons, who did not participate in a labour market programme until the beginning of the evaluation period, but may participate later on (e.g. Sianesi 2002, 2004).
- c) Persons, who entered a different labour market programme at the beginning of the evaluation period (e.g. Gerfin/Lechner 2002, Sianesi 2002).

The idea underlying the first approach is to imitate a social experiment, in which a randomly chosen group of the population is assigned to treatment, while another random

group, whose members will not receive treatment, acts as a control group. In such an experimental design and under certain ideal conditions – everybody in the treatment group receives treatment and members of the control group will not get another kind of treatment – a simple comparison of the outcomes of the treated group and the control group can give a consistent estimate of the programme impact (see Heckman et al. 1999 for a discussion of problems with experimental evaluation). With non-experimental data, however, one problem consists in the non-randomness of the timing of programme participation in an unemployment spell. If people enter a programme because they did not find a job before, or they do not enter a programme because they expect to find a job quickly, this creates a relationship between the probability of programme participation and the probability of finding a job. Therefore, if the timing of programme entry is not taken into account, matching conditional on individual characteristics might not remove selectivity (Sianesi 2001, Fredriksson/Johansson 2004). This is an argument for using the second definition – applied as well in our empirical strategy – for comparisons with non-participants.

The third definition of the control group permits a comparison of the impact of one programme with that of another programme, given that there is sufficient overlap in the distributions of the characteristics of the two groups of participants. Larsson (2003) and Sianesi (2001) make the point in their evaluations of Swedish active labour market policies that there is no no-treatment group among the unemployed in Sweden: Every unemployed person will either leave unemployment without assistance or he/she will sooner or later participate in some kind of labour market programme. In such an institutional setting, it makes more sense to compare participants in different programmes.

In our analysis, we will look at three different counterfactuals for persons entering wage subsidy programmes:

- 1) The effect of taking up a subsidised job versus remaining unemployed.
- 2) The effect of taking up a subsidised job versus taking up non-subsidised employment.
- 3) The effect of taking up a subsidised job after a short period of on-the-job training versus participation in on-the-job training only, where the subsidised job may or may not be in the same firm.

The **first** scenario is adequate, if the job and programme allocation process is such that – for any group of unemployed persons with identical characteristics – the selection into

subsidised jobs is random and not driven by systematic factors. A strong argument against this is that the treatment in the case of the EGZ is not solely the subsidy, but the subsidy in combination with a new job. In other words, wage subsidies like the EGZ cannot be "prescribed" and there must be an employer willing to offer a job to the person in question. Thus, the access to EGZ might be driven by factors observable to employers, but not identified in the data, as for instance the motivation of unemployed individuals and the assessment of these skills through caseworkers and firms.

Accordingly, an argument against the matching approach in the context of wage subsidies is that the fact that someone has been able to find at least a subsidised job is as such a hint on unobserved individual heterogeneity. However, unobserved heterogeneity will be strongly correlated with the observed explaining variables; this should hold particularly for information on an individual's labour market history (Heckman et. al 1999). Thus, we are rather confident that this problem will not be a major one for the data at hand, as we have information on previous employment history, which should capture most of the effects of unobserved individual factors.

What we do not observe, however, are employer characteristics. It is very plausible that labour demand side factors will influence access to subsidised jobs as well as the probability of further employment. A worker with given characteristics might be able to get and keep an unsubsidised job at some employers, "only" a subsidised job at others and no job at all in the remaining firms. The matching of workers to heterogeneous firms, together with the lack of information on employer characteristics (at least in our data), may therefore produce a bias in our impact estimates which is similar to selection bias resulting from individual heterogeneity.

Another problem inherent in the data construction for this first scenario is an initial advantage of participants over non-participants. This initial advantage results because the participants are sampled conditional on their unemployment exit, while the non-participants, by definition, are unemployed at the beginning of the evaluation period (Jaenichen 2002). This last problem should however vanish over time, as more and more non-participants leave unemployment. Furthermore, in our outcome variables for evaluation, we will include subsidised employment as well as the follow-up period of compulsory employment in the category "not employed". This also should lead to a more conservative valuation of the participant's outcomes.

The **second** scenario tries to avoid some of the problems of the first scenario. Here, we compare participants supported with EGZ to persons who have left unemployment for

regular employment at the same time. Within this scenario, there might again be problems of unobserved individual heterogeneity or unobserved employer-side influences. Individual heterogeneity could exist, since treated individuals have been able to find only a subsidised job. One could think of the local placement officers as having more knowledge about an individual's reemployment chances than we have on the basis of the variables in our data. However, the argument given above applies again: Information on the previous employment history should absorb most of individual heterogeneity. With respect to employer-side influences, we can argue with some plausibility that both participants and controls in this scenario are subject to such factors. So, if there is no systematic heterogeneity between firms utilizing subsidies and firms who do not, there is no reason to assume a bias provoked by the lack of information on firms. Existing studies for German firms do indeed identify variables influencing the probability that firms make use of wage subsidies. However, the results are not unambiguous (Hartmann 2004, Jaenichen 1999).

The **third** scenario is suggested by the observation that there has been combined use of the EGZ with other labour market programmes and especially with short-term training measures in firms ("betriebliche Trainingsmaßnahmen"). The question we ask is, whether there is an additional advantage of an EGZ after such a short-term training measure in comparison to short-term training only. To the degree that short-term training serves as a filter for employment, there might be a selection problem, if those workers later participating in the EGZ programme had performed better during their training period and thus were employable. However, it may also be the case that employers, knowing about the "true" productivity after the end of the training period, have better arguments to bargain for a further period of subsidised employment. In this case, the workers with combined participation of training and EGZ would be those who really need a subsidy for integration. Summarizing, in this third scenario, the possibility to screen the worker during the training period might give rise to selectivity and this should be kept in mind for the interpretation of the results.

3.3 Data and variables

We utilize administrative data collected and provided by the German Federal Employment Agency. The newly constructed research data set **Integrated Employment Biographies** (IEB) contains detailed information on socio-demographic characteristics, programme participation, employment and unemployment histories. Hummel et al. (2005) describe a sample of the database that is open for public use through the Re-

search Data Center of the Federal Employment Agency. The IEB is composed of four databases, which are all organised in the form of episodes:

- a) The IAB employment history (BeH) includes compulsory reports of companies on dependent employment relationships, for which social security contributions have been paid.
- b) Data on job search originating from the applicants pool database (BewA) encloses detailed information on jobseekers' characteristics.
- c) The participants-in-measures data (MTG) merges information on participation in active labour market programmes.
- d) The IAB benefit recipient history (LeH) covers information on times during which unemployment benefits or unemployment assistance has been paid.

These data were supplemented by the most current information on labour market status available from the data warehouse of the Federal Employment Agency. The combination of information on programme episodes and on employment episodes allows us to distinguish between times in subsidised employment (through a wage subsidy or in a public job creation measure) and regular employment. Since the data stem from different sources, we had to correct several inconsistencies.

For our first and second scenario, the treatment sample consists of all individuals taking up subsidised employment during the second quarter of 2002, who have been unemployed beforehand (these were about 85 percent of all individuals receiving an EGZ during this period). Thus, our analysis avoids the problem of dynamic programme entry (Sianesi 2004, Frederiksson/Johansson 2004) by sampling only persons, who started their subsidised employment during a short interval of time. The analysis of the third scenario is based on a sample of all individuals that have participated in a firm-related short-term training measure (with a maximum duration of three months) during the first half-year of 2002 and have taken up a subsidised job within three months after the training measure has ended. From those receiving an EGZ in the second quarter of 2002, about 20 percent had previously participated in a short-term training measure; 64 percent of these measures had taken place within establishments.

Special data sets were built up to get a pool of potential control persons for each scenario. For the first scenario, control groups were taken from monthly 2.5-percent-samples of entries into unemployment since 2000. For the second scenario, we utilize a 75-percent-sample of entries into employment from the first half-year of 2002. Finally, a

sample of 310,000 exits from short-term training measures between October 2001 and June 2002 forms the basis for the construction of control groups for the third scenario.

Our analysis distinguishes between several subgroups of participants. First, all analyses were conducted separately by gender and region (West vs. East Germany). Furthermore, we distinguish short-term subsidies with a duration of four to six months from long-term subsidies lasting from seven to twelve months. Because of small case-loads for short-term subsidies, the impact of the EGZ in the third scenario is only estimated for long-term subsidies. The actual durations are clustered at the upper bounds of the chosen intervals: Among all EGZ for hard-to-place workers that were granted for no longer than a year, 68 percent had a length of one year and 17 percent a length of half a year.

We consider two binary **outcome variables** to describe the labour market status. Our first outcome variable indicates whether an individual is in unsubsidised employment at the beginning of a month. The second outcome variable shows whether a person has successfully avoided unemployment at the beginning of a month (he/she is not registered as unemployed or as participating in a labour market measure). Beyond the period of subsidization itself, we interpret the follow-up period, during which the employer is obliged to sustain the employment relationship, as a further period of programme participation.

If we observe differences in estimated treatment effects between the two outcome variables, than the share of individuals in the treated group and in the control group switching to an “unobserved” state differs. In fact, there are many gaps between intervals of employment, unemployment and participation in measures, without any information on the labour market state during these periods. In most cases, these gaps probably result from the fact that individuals have withdrawn from the labour market for a shorter or longer period of time (examples are home-time or early retirement). However, self-employed individuals also do not appear in the register.

To measure the outcomes of control group members, hypothetical starting dates are required. The problem is solved differently for each evaluation scenario. In the first scenario, each matched control person is assigned the starting date of his/her treated counterpart. If a matched control person is no longer unemployed at this hypothetical starting date (which is the case for relatively few observations), the matched pair is deleted from the sample. In the second scenario, outcomes of control persons are measured beginning with the month of entry into employment. In the third scenario, a hypothetical starting date for matched controls is obtained by adding the time span between the participant's

training programme exit and his/her entry into the EGZ to the control's end date of the training programme.

The non-testable conditional independence assumption requires the observation of all **explaining variables** that determine selection into the programme as well as the outcome in the case of non-participation. The selection into the programme is the result of individual choice, of caseworkers' assessments and negotiation strategies as well as of firms' choices and behaviour. We model selection using the following observable information on individuals:

- a) socio-demographic characteristics (nationality, age, education, health and – for female workers – information on the family status),
- b) variables on the five-year-history prior to the respective unemployment spell (participation in measures and years in unsubsidised regular employment),
- c) the timing of entry into unemployment,
- d) information on the regional labour market situation (performance cluster suggested by Blien et al. 2004).

All variables are categorized as dummy variables and measured at the beginning of the subsidised employment spell. For members of the control groups, time-varying variables are measured in the mid of the first quarter 2002. Table A.1 in the Appendix shows variable means of selected explanatory variables for subsidised workers as well as for our samples of potential control persons. Subsidised persons might be regarded to be a positive selection compared to all unemployed, but as a negative selection compared to those who have been hired into a regular unsubsidised job. The sample of individuals, who do not take up a subsidised job after having completed a training measure, might have some labour market advantages compared to those who receive a subsidy later. Comparing both regions of Germany, participants in East Germany have on average a better qualification, less health problems and more often experience in previous programme participation

As we have already discussed in detail in Section 3.2, we are confident that unobserved individual heterogeneity should not be a major problem for our analysis. Note furthermore, that – due to the large size of the samples of potential controls – common support is achieved for all treated individuals in our sample. The stable unit treatment value assumption can be supposed to hold due to the rather low importance of inflows into subsidised jobs (Section 2.2).

4 Empirical Results

4.1 Effect of a subsidy on the treated

In the first scenario, we estimate the average effect of the EGZ on the employment prospects of previously unemployed persons. As a counterfactual, we have the option to participate never or only later in a labour market programme. Table 2 displays in Panel IV mean standardized biases before and after matching. The bias reduction obtained through the matching procedure is on average slightly less than 80 percent, the absolute value of the mean standardized bias after matching never exceeds the value of 5. Thus the quality of the matching seems quite satisfactory.

Table 2: 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

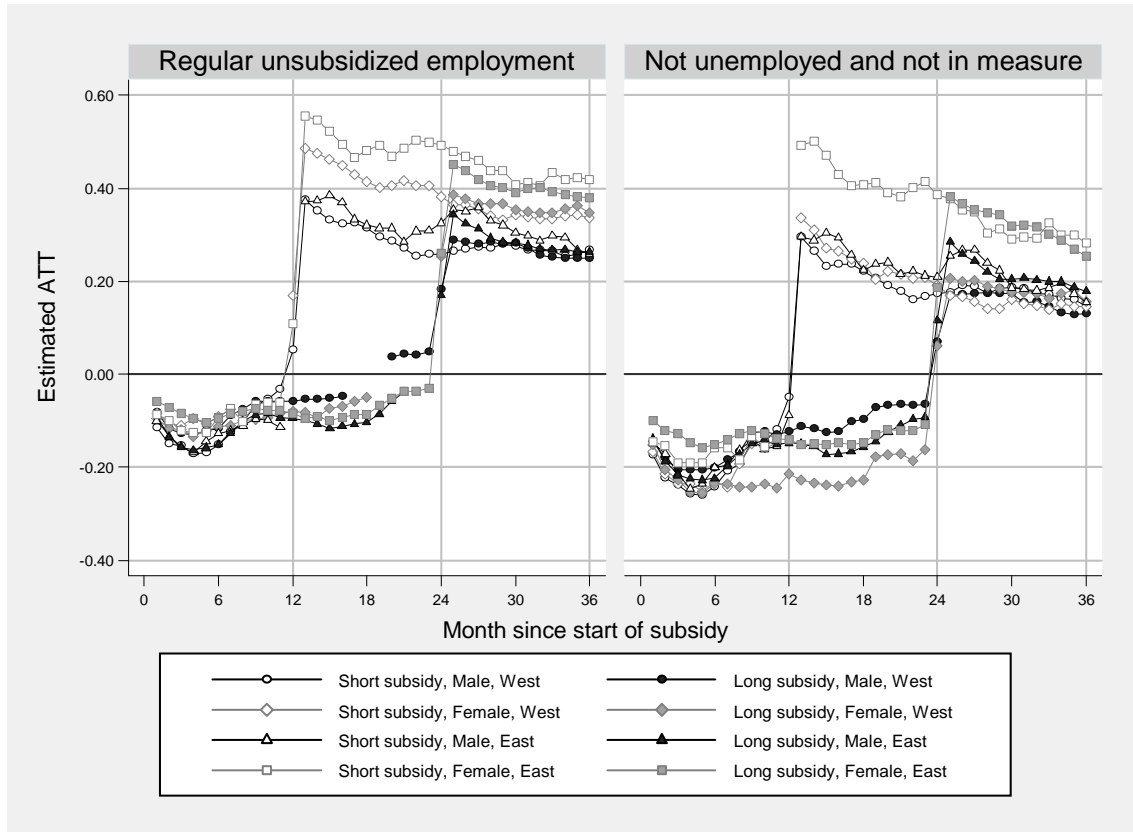
			Short-term subsidy				Long-term subsidy			
			Male West	Fe- male West	Male East	Fe- male East	Male West	Fe- male West	Male East	Fe- male East
I.	Share in regular unsubsidised employment	Treated	0.50	0.56	0.50	0.65	0.44	0.55	0.48	0.60
		Controls	0.23	0.23	0.24	0.24	0.19	0.21	0.22	0.22
		ATT	0.27	0.34	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 A.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 “hypothetical” starting date, the matched pair was dropped from further analysis.

Panel I and II of Table 2 summarize the results three years after programme start, documenting the share of treated and of matched control persons in regular unsubsidised employment, the share that is not unemployed (or in a labour market measure) as well as average treatment effects on the treated. The evolution of the estimated average treatment effects over time is displayed in Figure 1. Plots above the abscissa have to be interpreted as a “success” of the particular programme. Remember that the period of subsidization as well as the following period, during which the employer is obliged to

sustain the employment relationship, is not interpreted as a “labour market success” in the sense of both outcome variables.

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 1 shows for the period of subsidization and for the compulsory period of further employment that treated individuals were less often in unsubsidised employment and have less often avoided unemployment. These locking-in effects are a necessary side-effect of the construction of the outcome variables. We find large and significant positive effects of the treatment on the treated immediately after the end of the support by the programme, which, however, decline slightly over time.

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 control group. In other words, we estimate that without treatment 25 to 42 percentage points of the treated would not have been regular employed. Turning to our second outcome variable, the difference in the shares “not unemployed and not in measure” varies between 0.14

and 0.28; thus 14 to 28 additional percentage points of the treated would have been unemployed without treatment.

The striking difference between both outcome variables results from a different share of individuals with an unknown state in the treated and the matched control group (Panel III of Table 2); the share is on average half as high in the treated group. This implies that a higher percentage of untreated individuals withdraw from the labour market as discouraged workers. Thus the subsidies help to activate hard-to-place individuals, who might otherwise have withdrawn from the labour market.

Comparisons of estimates of the treatment effect between the different groups investigated must be interpreted with caution, since characteristics of group members differ for each group (see Table 2). Nonetheless, some results should be mentioned:

- Across control groups, the average outcome “unsubsidised employment” (Panel I of Table 2) does not differ remarkably by gender or region, but is slightly smaller for the long-term subsidy. However, across treated groups the grant of an EGZ seems to have had a particularly large effect on employment opportunities of female workers, which was even stronger in East Germany. Accordingly, estimated treatment effects are much higher for female than for male workers.
- For the outcome “not unemployed and not in measure” (Panel II of Table 2), we observe a striking result for the matched control sample of females from West Germany – they are less often unemployed than females from East Germany or males. An obvious explanation is that housework has always been a rather accepted alternative for women in West Germany, which facilitates withdrawing from the labour market. Looking at the treatment groups, the outcome is again more advantageous for female workers. The net result is that the estimated treatment effect is highest for the group of females in East Germany and does not differ much among the other groups.
- Comparing the recipients of short-term and long-term EGZ, labour market outcomes are always more favourable for treated individuals who received a short EGZ as well as for their matched control persons. This is not surprising, since the duration of the subsidy should be a function of placement difficulties. However, estimated average treatment effects are rather similar for short- and long-term subsidies at each point of time after the employment promotion had expired (Figure 1).

A deadweight loss in a narrower sense only occurs when the same individual would have got the same job without the help of the EGZ. Data and technique applied do not

provide a measure for this effect. Some studies for other labour market measures interpret the share of the matched control group that has successfully found an unsubsidised job as a measure of the deadweight loss in a wider sense (Winterhager et al. 2006, 513). In this sense, the deadweight accompanying EGZ would be around 20 percentage points (Panel I of Table 2).

4.2 Effect of a subsidy conditional on taking up a job

The second scenario analyses the average effect of the EGZ on the treated, conditional on having taken a job. Thus the questions analysed here and in the former section are fundamentally different: The previous estimates referred to the combined effect of receiving a subsidy and taking a job. This section investigates solely the effect of the subsidy, conditional on having found a (subsidised or unsubsidised) job. The results of the estimates can be found in Table 3 and Figure 2. The information on the mean standardized bias in Panel IV of Table 3 indicates again a satisfactory matching quality, with a mean bias reduction of more than 80 percent through the matching procedure.

Table 3: 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

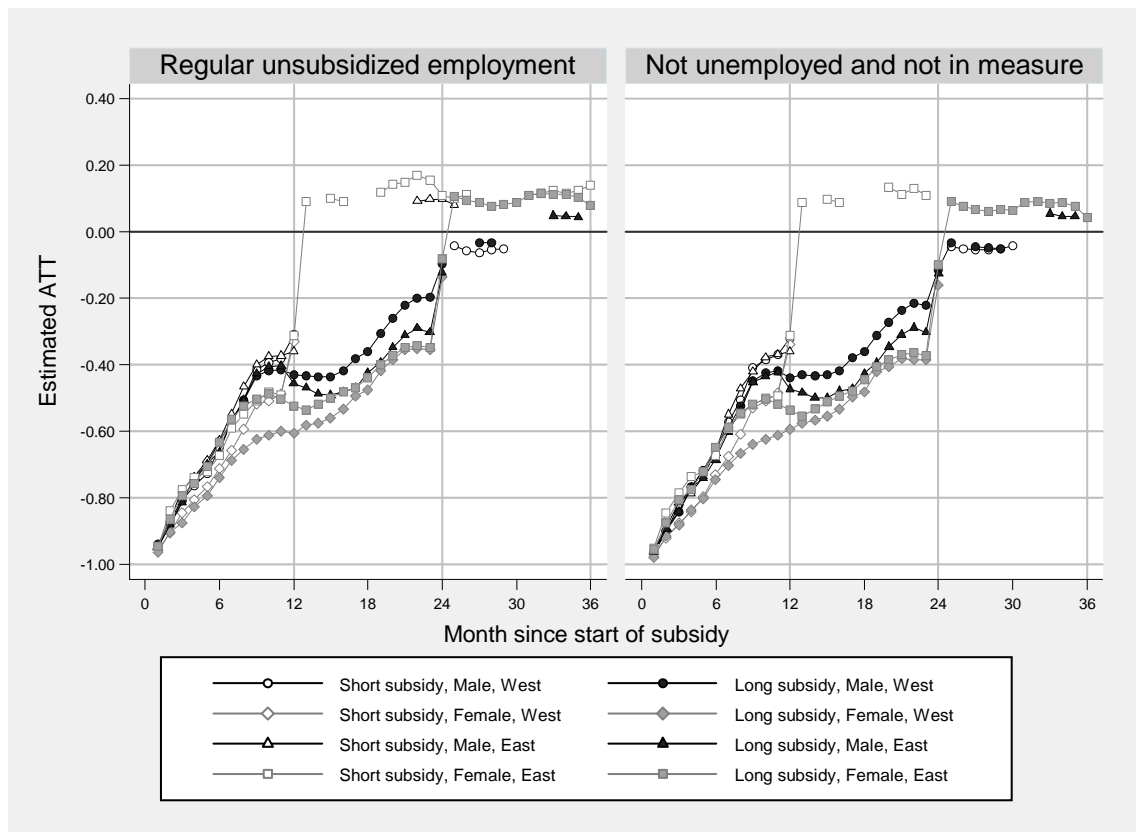
			Short-term subsidy				Long-term subsidy			
			Male West	Fe-male West	Male East	Fe-male East	Male West	Fe-male West	Male East	Fe-male East
I.	Share in regular unsubsidised employment	Treated	0.50	0.57	0.48	0.64	0.44	0.56	0.49	0.60
		Controls	0.49	0.60	0.47	0.50	0.46	0.56	0.45	0.52
		ATT	n.s.	n.s.	n.s.	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.

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 3). 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.

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.

In choosing the members of the matched control 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 the discussion of the conditional independence assumption in Section 3.3). However, the matched control 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. Table 4 and Figure 3 display the main results. The mean standardized bias (Panel IV of Table 4) is reduced considerably through matching for three of four groups; the bias reduction for female unemployed in West Germany is below 50 percent, however.

Table 4: 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	Fe-male West	Male East	Fe-male 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

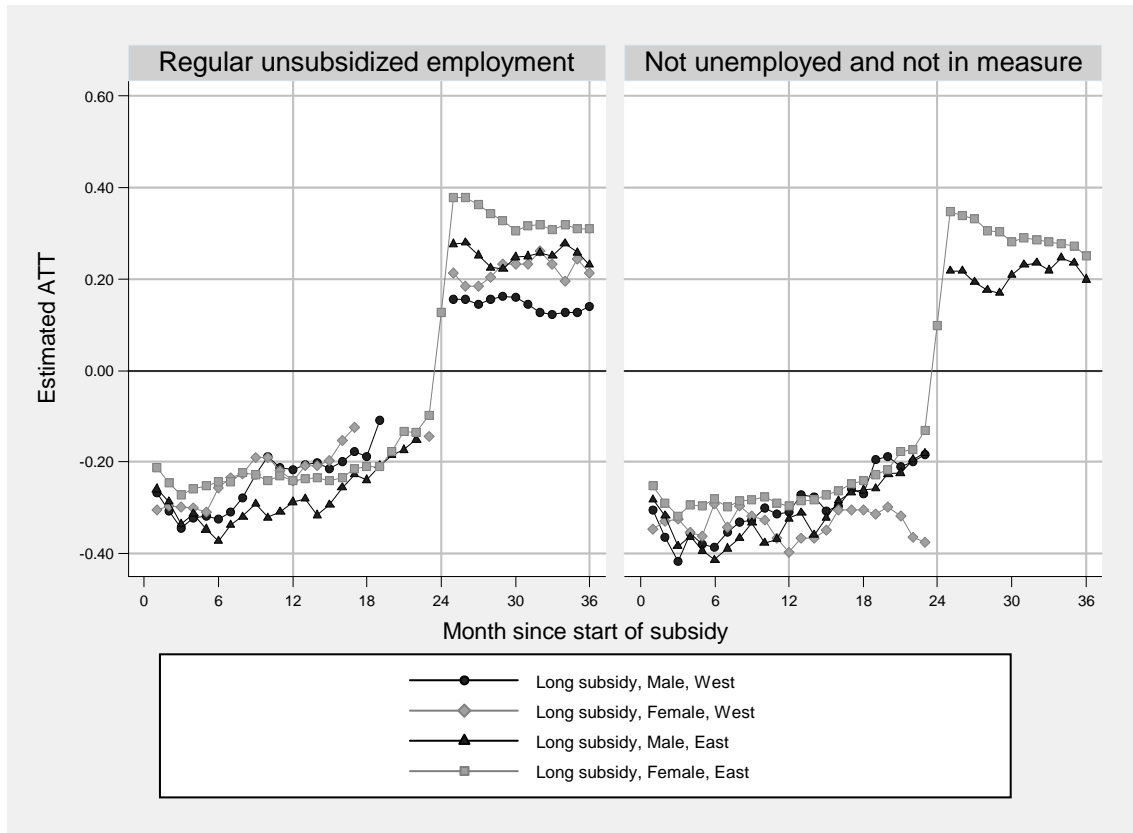
Note: n.s. = not significant at $\alpha = 0.05$. Long-term subsidies are paid for 7 to 12 months.

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 4, Panel I and II), are rather similar to the results of all subsidised individuals (Table 2, 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 4 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 2, which were not conditional on the participation in a short-term training measure.
- Panel II in Table 4 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.

Thus, might short-term training serve as a substitute for wage subsidies? Our results might be explained by the fact 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. However, as has already been argued, it is also possible that the weaker impact is due to a larger share of individuals, who proved less productive during the training period and thus required additional support for their integration. Finally, individuals participating in these training measures are already a selected sample, and results for them may not be extended readily to the entire sample of unemployed.

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.

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 to a considerable amount. For previously unemployed individuals, three years after the start of the programme, the share in regular employment is from 25 to 42 percent higher in the treatment group than in the matched control group. A comparison between groups of unemployed persons taking subsidised employment with matched control 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 control 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? The possibility of deadweight effects notwithstanding, our impression 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).

The results of the third scenario – even if the estimated impact is positive – raise the question to what degree short training measures within firms might obtain results similar to an EGZ (at much lower costs). In addition to the previously raised argument of heterogeneity in the job matches, selectivity with respect to further EGZ support might occur, since employers learn about a participant's productivity during the training period. It is also plausible that case managers utilize short training measures in a kind of trial and error process. If a cheaper training measure suffices to integrate a previously unemployed person into a firm, than the more time-consuming decision for or against an EGZ may readily be postponed. There is some evidence from case studies in selected agencies that short training measures in firms and the EGZ were sometimes seen as substitutes, thus the choice between them was rather an incidental matter (ZEW et al. 2006, 61).

To conclude, our findings are in line with results from the literature; most studies that estimate effects of targeted wage subsidy programmes on the treated find positive effects on individual employment probabilities. For any assessment of the benefits of wage subsidies, it should be noted that the methods applied in this paper do not identify deadweight loss or potential displacement and substitution effects. In this sense Fay (1996) emphasises that careful controls are an important part of designing wage subsidy programmes. Otherwise, there is a risk that firms use schemes as a permanent subsidy to their workforce.

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Appendix

Table A.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.