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Abstract

This paper investigates the link between variation in the supply of workers who participate in specific types of active labour market policies (ALMPs) and firm performance using a new exceptionally informative German employer-employee data base. For identification we exploit that German local employment agencies (LEAs) have a high degree of autonomy in determining their own mix of ALMPs and that firms' hiring regions overlap only imperfectly with the areas of responsibility of the LEAs. Our results indicate that in general firms do not benefit from ALMPs and in some cases may even be harmed by certain programs, in particular by subsidized employment and longer training programs. These findings complement the negative assessment of the cost-effectiveness of ALMPs from the empirical literature on the effects for participants.

JEL-Code: J680.

Keywords: subsidized employment programs, training programs, regional variation, program evaluation.

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1. Introduction

Does it matter for firms when different active labour policies (ALMPs) are used in the regions in which they typically hire their workers? So far, the vast literature on the effects of ALMPs, which emerged in the last decade, focuses on the question whether the unemployed benefit from participating in the programs. However, firms may benefit (or lose) as well: Better-targeted applications from participants in job search assistance programs may reduce hiring costs and improve match quality. Training programs may reduce the mismatch between the skills demanded by firms and the skills of the unemployed workers the firm may potentially hire. Subsidized employment may directly reduce firms' wage costs and may lead to a competitive advantage over firms for which this program is not available. Thus, ALMPs may increase the profitability of firms via these channels. This may lead to positive long-run effects if firms prosper and create new jobs. If these positive effects materialize, they become part of the justification for the typically large expenditures on ALMPs on top of potential individual effects. From the point of view of the protagonists of those policies, such additional justification may be particularly called for, because much of the literature on the individual effects of ALMPs concludes that most of the programs do not increase the employment chances of their participants sufficiently to pass a cost-benefit test.[1]

In light of the largely non-positive individual evidence, can we expect any effects on firms? We have three answers to this question. Firstly, given that most programs are rather short, the estimated individual effects for the unemployed will be small as well - in many cases too small to be detectable with the sample sizes usually available. Firms, however, may benefit from the cumulated effects in the pool of unemployed workers in which they hire, which could be much larger. Secondly, there is almost no reliable evidence on the effects of

[1] See for example the meta analysis by Card, Kluve, and Weber (2010).

ALMPs on job match quality. The reason is a methodological problem: match quality can only be measured for unemployed eventually finding a job. This creates as selection problem into employment that is hard to solve even if program participation is randomized. Finally, there may be other effects the literature has neglected so far. Examples are so-called pre-program or threat effects: Unemployed workers who expect negative utility from a program they have been assigned to may increase their job search efforts in order to avoid participation. This results in higher exit rates to employment but may come at the cost of reduced job match quality. Several recent studies show that such effects exist (e.g. Black, Smith, Berger, and Noel, 2003, Geerdsen, 2006, Geerdsen and Holm, 2007, Graversen and van Ours, 2008, Rosholm and Svarer, 2008, Van den Berg, Bergemann and Caliendo, 2009), but the effects on match quality remain unclear.

In this paper we study the effects of the availability of different types and intensities of ALMPs in the regions in which the firms hire (called their 'hiring regions' from now on).[2] Such an analysis faces two main challenges: Firstly, informative data are needed measuring in which regions individual firms hire, how these regions differ with respect to the mix of ALMPs used, and how firms perform. Secondly, classical endogeneity and selectivity issues have to be resolved to allow for the intended ceteris paribus comparison of the outcomes of 'otherwise similar' firms which exogenously face different regimes of ALMPs.

The first problem is solved using a newly available German linked employer-employee data base that combines firm survey data with several administrative data sources. This data includes exceptionally detailed regional and program information. It allows for community level measurements of hirings and of the composition of ALMPs. To solve the endogeneity problem between the respective policy and measures for firms' economic success, we ex-

[2] Thus, we do not directly investigate the effects of a firm 'using' for example a wage subsidy, i.e. the direct channel.

ploit three institutional features: Firstly, the local employment agencies (LEAs) have a high degree of autonomy in defining the mix of ALMPs they are implementing. Secondly, the LEAs' responsibility is strictly limited to the workforce living in the communities assigned to the LEA. Finally, firms' hiring regions do not perfectly overlap with the areas of responsibility of one or multiple LEAs. This induces exogenous variation in the level and mix of ALMPs firms face in their hiring regions. This variation is induced by preference-related variation in strategies across LEAs as well as by a substantial part of the LEAs' policy being determined outside the firm's hiring region.

Although, there are several papers using regional information to analyse the effects of ALMPs on the unemployed,[3] our intended contribution is – in that respect – most closely related to the small literature using aggregate regional data to gauge the macroeconomic impact of different ALMPs (e.g. Dahlberg and Forslund, 2005, for Sweden, and Hujer, Blien, Caliendo, and Zeiss, 2006, for Germany). However, a key difference to that literature, which also leads to different identification strategies, is that our target is the economic performance of individual firms, not of a particular region. Blasko and Pertold-Gebicka (2013), to our knowledge, is the only study that addresses a similar question as our paper using a somewhat similar set-up. In their observational study based on administrative data they exploit an increase of job search assistance and monitoring in one Danish region to estimate firm effects based on a parametric difference-in-difference set-up. They do not find positive effects.

Our results mainly support the pessimistic assessment of the cost-effectiveness of ALMPs. This assessment is consistent with the large empirical literature on the effects for participants for a wide range of programmes that are investigated here as well, as well as with

[3] E.g. Blundell, Costa-Dias, Meghir, and van Reenen, 2002, who exploit the regional variation in the introduction of the New Deal for Young People in the UK, and Frölich and Lechner, 2010, who exploit an exogenous regional variation in the participation probabilities of ALMP in Switzerland.

the firm based results obtained by Blasko and Pertold-Gebicka (2013) for job search assistance. Using a semiparametric econometric approach, we find that in general firms do not benefit from local ALMPs and in some cases may even be harmed by them. In particular, the extensive use of subsidized employment and long further vocational training programs in a firm's hiring region has on average negative effects on those firms. This complements the existing literature in an important way because the absence of positive effects on firm growth and survival makes it seem unlikely that positive effects on the macro level exist that are large enough to justify the huge expenditures on ALMPs. Our results are somewhat more optimistic for two specific types of training: intensive on-the-job training in a simulated work environment and training that awards a formal vocational degree. These programs are however small (but costly).

The remainder of the paper is organized as follows: In the next section we discuss the potential links between different exposures to regional ALMPs and firm performance in some detail. Section 3 provides the institutional background on the implementation of different ALMPs. In Section 4 we describe the data, and discuss identification and estimation of the effects of interest. Section 5 contains the results and sensitivity checks. Section 6 concludes. An appendix contains supplementary material. Additional information relating to the technical implementation of the estimation and the data, as well as further results and sensitivity checks are relegated to an internet appendix.[4]

2 How firms may benefit from active labour market programs

In the empirical analysis below we will consider three broad categories of active labour market programs, namely job search assistance, training and subsidized employment.

[4] Currently, the internet appendix is available on request from the authors. Later on, it will be posted on the web.

Before describing in detail the specific programs and our empirical strategy, we review some theoretical arguments for a potential link between these programs and the performance of firms that could potentially hire their participants.

Job search assistance programs aim at increasing jobseekers' search effectiveness by reducing information asymmetries regarding open vacancies, by achieving a better targeting of applications, and by improving job search skills. This program may affect firms may be affected by this program via two channels, both of which increase the firms' profitability: Firstly, firms save hiring costs because more effective job search of workers leads to faster hiring by firms. Secondly, the quality of the job match should be improved as well which in turn reduces turnover and thus turnover-over related costs, like a loss of firm specific human capital and future hiring costs.

The objective of training programs is to improve workers' skills and thus remove or reduce skill mismatch in the labour market. By training the pool of applicants such that it is more suitable to the firms' requirements, job match quality improves. Moreover, firms save the cost of training new hires by themselves.

Subsidized employment can have two opposing effects. Firms that hire workers for whom they receive the subsidy save wage cost. These savings improve profitability if the potential deficits in the productivity of the workers eligible for the subsidy were overcompensated. In contrast, firms that do not receive such subsidies may be harmed because of a comparatively less competitive cost structure. However, if subsidized employment positively affects the employability of subsidized workers in the long run, other firms hiring from the same regional skill pool may also benefit from a more suitable pool of applicants (leading to similar effects as training).

ALMPs may also affect firms through two other channels. On the one hand, participation in ALMPs can lead to sizeable lock-in effects, i.e. periods in which unemployed workers

search with lower intensity while participating in a program (see Wunsch and Lechner, 2008, Lechner, Miquel and Wunsch, 2011, for evidence for Germany). This may prolong the time until a vacancy is filled with a suitable match, and hence increase firms' hiring costs (or decrease match quality). On the other hand, there is a growing literature providing evidence for so-called pre-program or threat effects that occur after assignment to a program but before actual participation (e.g., Black, Smith, Berger, and Noel, 2003, Geerdsen, 2006, Geerdsen and Holm, 2007, Graversen and van Ours, 2008, Rosholm and Svarer, 2008, Van den Berg, Bergemann, and Caliendo, 2009): Unemployed workers who expect negative utility from a measure they have been assigned to may increase their job search efforts and reduce their reservation wage in order to avoid participation. This effect leads to vacancies being filled faster, but the quality of the job match may be lower.

3 Institutional background

3.1 Active labour market policies in Germany 2000-2004

In this study, we analyze the effects of ALMPs between the two major reforms of German labour market policy that occurred in 1998 and in 2005. Facing an average stock of about four million unemployed workers, the Federal Employment Agency (FEA) spent around 15 billion EUR per year on ALMPs in that period (see Table 3.1). The FEA relied on five main groups of ALMPs. Table 3.1 displays expenditures and number of participants (entries) for these ALMPs for the period 2000-2004.

Despite a steady increase in unemployment, expenditures on ALMPs have gradually been reduced over this period, leading to a substantial reduction of 3.5 billion EUR in 2004 compared to 2000. The main reason is a shift from long intense and costly programs to more inexpensive ones. With more than one million participants in 2004 ‘training measures’ (TM) have become the most important measure in terms of the head count. Usually TM’s combine

weak forms of monitoring like availability checks with basic job search assistance such as teaching on how to locate relevant vacancies, on how to write a good application and on how to behave in a job interview. There are also programs that provide some job-relevant skills, like training for using computer software. Durations are 1-2 months on average and are limited to 3 months at maximum. Therefore, expenditures are low. Despite the large number of participants, they make up only 4% of the total budget. Another program that has gained importance is the support of unemployed who want to become self-employed. Facing a persistent labour demand deficit, the FEA increasingly encourages unemployed workers to set-up small businesses, mainly by providing income support to bridge the time until the business becomes profitable. Due to lack of data we do not consider this program. It is also of little relevance to firms with respect to their hiring opportunities.

Table 3.1: Active labour market policies in Germany 2000-2004

	2000	2001	2002	2003	2004
Total budget in million EUR	16'131	15'636	15'346	13'796	12'531
Share in % spent on:					
Training measures	2	2	3	4	4
Further vocational training	42	45	44	36	29
Employment programs	31	26	20	17	13
Wage subsidies	7	9	11	14	11
Support of self-employment	5	5	7	12	22
Average number of unemployed	3'888'652	3'851'636	4'060'317	4'376'769	4'381'281
Entries into:					
Training measures	476'672	565'132	877'038	1'070'137	1'189'599
Further vocational training	551'534	449'622	456'301	246'245	186'624
Employment programs	361'073	273'356	228'839	184'714	169'241
Wage subsidies	204'948	192'555	225'732	203'824	192'174
Support of self-employment	92'604	95'656	123'268	135'774	360'559

Source: http://www.pub.arbeitsagentur.de/hst/services/statistik/detail_2004/a.html, 30.01.2012.

'Further vocational training' (FVT) is the most important program in Germany in terms of expenditures, although its role is diminishing. In the period we consider, expenditures have been reduced by more than 45%, and the number of participants declined by about two thirds. This program provides intense job-related training. It aims to reduce the mismatch

between the skills of unemployed workers and what is demanded by the market. With durations of up to two years, these programs can be very costly. There is however substantial variation in both contents and durations. In the empirical analysis we therefore distinguish four different types of FVT: The first group combines class-room and on-the job training. Here, we distinguish programs with planned durations of up to and above six months in order to account for differences in the amount of human capital added. We separately analyse the most intense form of FVT which provides a formal vocational degree equivalent to a German apprenticeship degree and takes 1-2 years to complete (so-called degree courses). Finally, we consider FVT conducted in so-called practice firms. They provide occupation-specific on-the-job training by simulating either the commercial part of a company (administration, accounting, customer relations, etc.) or the manufacturing part.[5]

The last group of programs considered is 'subsidized employment'. It comprises two distinct subtypes. The first type is a time-limited wage subsidy which employers can receive when hiring unemployed workers for a regular job in order to compensate for the training investments required due to initial deficits in productivity. The use of these subsidies has been quite stable over the period 2000-2004. The second program type consists of so-called employment programs. They offer subsidized non-market jobs with the aim of both providing some additional income, and maintaining the employability of unemployed workers through daily routines, social contacts, on-the-job learning, etc. Durations are usually around one year and up to 100% of the remuneration the workers receive is subsidized. This makes these programs the second most expensive. They were mainly used in East Germany and over time

[5] For the commercial part, practice firms trade *virtual* goods and services with each other to provide realistic conditions for participants, who are the practice firm's employees. The skills so obtained correspond to what is required for the specific job held within the practice firm (e.g., that of an accountant). For the manufacturing part, courses in practice firms are heterogeneous and range from specialist training in technical professions to obtaining a driver's license for special vehicles to simply practicing the craft of a carpenter.

their importance has declined substantially. This is reflected in the number of participants, which has more than halved from 2000 to 2004.

3.2 Regional implementation of ALMPs

To identify the effects of the supply of participants in different types of ALMPs on firms' outcomes we exploit that in the period we consider (2001-2003) there is a variation in the use of ALMPs that is exogenous to firm performance. This variation stems from the fact that the hiring regions of firms do not coincide with the administrative regions for which the decisions on the local mix of ALMPs are made. In the following we describe this decision process based on Blien (1998), Mosley, Schütz, Schmid, and Müller (2003), Schütz and Mosley (2005), and Yankova (2010).

In the second half of each year the Federal Employment Agency decides on the total budget available for ALMPs in the next calendar year. The FEA also defines some overall policy objectives and corresponding guidelines for the use of different types of ALMPs, e.g. whether the focus should be on qualification or subsidized employment. The FEA then decides on which share of the total budget will be distributed to the 10 regional headquarters. This decision is based on the size of the region and local labour market conditions, in particular on employment growth, unemployment and long-term unemployment ($\geq$ 1 year) rates, as well as the share of exits from unemployment. The regional headquarters then decide on the budget for each local employment agency (LEA) based on similar criteria. They also define overall policy objectives and targets for the coming year and issue guidelines for the use of ALMPs to reach these goals. In December, the budgets, policy objectives and general guidelines for the coming year are determined for each LEA. At the beginning of the following year, the LEAs decide on their individual strategies and on which share of their budget to spend on the different types of ALMPs. Since most services have to be purchased in advance from external providers, adjustments in the use of ALMPs only happen with some slack.

Within the overall guidelines issued by the FEA and the regional headquarters, each of the 176 LEAs decides autonomously on the use of different activation measures. The population the LEA serves is limited to those unemployed workers who live in the area of their responsibility. This is helpful for our identification strategy because due to clustering of households with similar socio-economic status in certain areas, neighbouring LEAs may differ substantially in their use of ALMPs due to the differences in their clientele (as well as their preferences). In Table 3.2, we provide exemplary evidence for this. The neighbouring cities of Berlin and Potsdam form one local labour market. It is served by 6 different LEAs. People living in this region can easily commute to any place within this area. Table 3.2 shows the share of unemployed workers participating in the four types of ALMPs for the period we will consider in the empirical analysis (2001-2003).

Table 3.2: Regional variation in ALMPs for the Berlin/Potsdam labour market 2001-2003

	Training measures			Further vocational training			Employment programs			Wage subsidies		
	2001	2002	2003	2001	2002	2003	2001	2002	2003	2001	2002	2003
Potsdam	1.8	1.5	2.8	8.2	7.5	4.6	5.7	5.1	4.6	3.6	5.3	4.7
Berlin South	1.1	1.3	2.1	8.1	7.1	4.7	7.0	5.9	5.5	3.3	3.0	3.4
Berlin South-West	0.7	1.2	1.7	7.3	6.8	5.2	6.0	6.1	5.2	3.3	4.3	3.8
Berlin North	0.7	1.0	1.1	7.4	6.7	3.7	4.4	4.2	3.5	1.9	2.8	3.1
Berlin Centre	1.1	0.8	1.7	8.1	6.0	4.2	7.2	5.1	4.6	2.1	2.0	1.6
Berlin East	1.0	0.9	1.4	7.5	7.3	4.7	7.3	5.5	4.3	2.1	1.8	2.2

Note: Participants as a fraction of the unemployed calculated as (average number of participants * 100) / (average number of participants + average number of unemployed). Source: *Eingliederungsbilanzen* published on http://www.pub.arbeitsagentur.de/hst/services/statistik/detail_2004/a.html, 30.01.2012.

We find substantial variation in the use of different ALMPs. Consider, for example, the year 2003: the share of unemployed participating in training measures is 1.5 times higher in Potsdam than in Berlin North. For wage subsidies, the share is 3 times higher in Potsdam than in Berlin Centre. Berlin South strongly focuses on the use of employment programs, while Berlin South-West emphasizes further vocational training. Since all 6 LEAs are serving the same labour market, this variation is induced by the different clientele each LEA serves and by LEA-specific strategies and preferences. Thus, conditional on the composition and

performance of the local labour market, this variation in the use of ALMPs can be regarded as exogenous to the performance of a firm operating in this labour market, as long as it does not employ a significant share of the local work force and thus influences indirectly (or directly) the decisions of the LEA. It is important to note at this point that - in contrast, for example, to the U.S. - private sector representatives are not involved in determining the strategy of the LEAs. In the next section, we describe in detail how we exploit this variation for the identification of the effects of ALMP on the economic outcomes of the firms.

4 Empirical strategy

4.1 Basic idea for identification

The identification strategy is based on the following intuition: Suppose that there are two firms. They are located in different communities. Therefore, they have different 'hiring regions' (i.e. the regions from which firms draw their work force; to be defined exactly below). Suppose further that these firms are comparable in terms of their characteristics (size, age, industry, composition of employees, etc.) as well as the characteristics of the local labour market in which they are hiring (GDP, unemployment, industry structure, composition of workforce, etc.). We exploit the fact that despite their similarity and comparable labour market conditions in their (potentially overlapping) hiring regions, both firms face different supplies of participants in different types of ALMPs for reasons that are exogenous to the firms' performances. Exogeneity is coming from three sources which are all related to the fact that the firms' hiring regions do not perfectly coincide with the area of responsibility of a single LEA: Firstly, the workers living in the firms' hiring regions are served by different LEAs. This induces variation in ALMPs of the hiring regions due to differences in activation strategies that are unrelated to local labour market conditions. These differences are for example related to the preferences of the LEA's directors, or originate from different regional cluster-

ing of the type of clientele each LEA serves despite comparable composition of the workforce in both hiring regions (see Section 3.2). Secondly, a non-negligible fraction of the workers served by a single LEA lives outside a firms hiring regions. This implies that the LEA's policy is at least partially determined by factors outside a firm's hiring region. Thirdly, the firms' employees make up only a small part of the workforce served by the relevant LEAs. This implies that a single firm should have a negligible impact on the LEAs' active labour market policy.

4.2 Data

The main data used in this study is a multi-source linked employer-employee dataset. It covers all establishments[6] in non-seasonal sectors that have at least 100 employees on June 30, 2000, and that participated in the IAB Establishment Panel (EP) in 2000. To limit the potential influence of a single firm we exclude firms with more than 2000 employees. In total, we observe 2979 establishments in our baseline year 2000. For each of those establishments there is yearly aggregate information about all employees (so-called IAB Establishment History Panel, EHP) for the period 1990-2008 which is constructed from the social insurance records of the employees. For each establishment there is information about the composition of its work force as of June 30, each year, in terms of gender, age, nationality, education and type of job, as well as measures of earnings, tenure, and turnover. Furthermore, for all employees of the sampled establishments the individual data from the IAB's Integrated Employment Histories (IEB) for the period 1990-2008 is available as well. This administrative database combines the social insurance records for employed workers, the unemployment

[6] We use the terms establishment and firms interchangeably in the following as the definition in the IAB Establishment Panel (EP) is, in fact, a mixture of the two. In this data, an establishment is defined as the union of all establishments of a firm in the same community and industry. Thus, an "establishment" in the EP can be a firm with a single or multiple establishments, or one or more establishments of a larger firm. Note that, because an establishment in the EP is defined based on a community, if the firm relocates to another community this is observed as closure of the establishment.

insurance records, the program participation register, and the jobseeker registers of the local employment agencies. Finally, this database is merged with the IAB Establishment Panel (EP), a large yearly representative panel of establishments in Germany that started in 1993 for West Germany and in 1996 for East Germany. It includes rich information about the characteristics, policies, and performance of the participating establishments.

To characterize local labour markets we also use two further datasets. Firstly, from INKAR (2004) we merge to our data detailed county-level information on population density, rurality/urbanity, migration, commuting, public transport, infrastructure, economic performance like GDP growth, and earnings. Secondly, we use the Integrated Employment Histories (IEB 1990-2008) containing a large representative sample of employed and unemployed German workers. This dataset allows characterizing the local workforce of each community in a detailed way. In particular, since the IEB includes the administrative records from the LEAs, unemployment insurance and social insurance, in fact we observe the same information about the local workforce as the LEAs when they decide on the use of different ALMPs. This is crucial for identification, because it allows controlling for the characteristics of the local workforce that determine both local ALMPs and firm performance.

The time frame used for the analysis is as follows: The baseline year for our analysis is 2000, the year when firms are sampled. Data from this year as well as from earlier years is used to measure control variables and 'pre-treatment' outcomes. The 'treatments', i.e. the firms' exposure to different ALMPs, are measured for the period 2001-2003.[7] Firm outcomes are measured from 2004 onwards. Available outcome variables are recorded in two different sources. Based on the EHP it is possible to measure firm survival, firm size and growth, tenure, turnover, and temporary contracts annually for the period 2004-2008. Based on

[7] Later years are not used because the decision process for the regional implementation of ALMPs changed in 2004.

administrative records, the reliability of the EHP is high and there are no attrition problems.[8] All other outcome variables are calculated from the EP survey. Based on the 2004 survey we measure the firm's economic development over the last year, the current composition of the workforce and current hiring in the year 2004, as well as expected personnel problems in the following two years. The 2005 survey contains measurements of profitability and investments for the year 2004, as well as of the state of the firm's technical equipment. All information coming from the EP is self-assessed. Moreover, there is survey attrition. All 2979 firms in our sample answered the survey in 2000. For, respectively, 47% and 44% of them we observe the outcomes in the 2004 and 2005 surveys. This includes some item non-response, which is negligible, though (0-5%). Thus, attrition is substantial. However, in Section 5.2 we will provide evidence showing that survey non-response is unrelated to the ALMPs considered.

4.3 Hiring regions and firm-specific exposure to different ALMPs

As discussed in Section 4.1, the firm's hiring regions and how they overlap with the areas of responsibility of the LEAs play an important role in our identification strategy. In the following we describe how these regions are defined and how they are used to construct measures of the firm-specific supply of participants in different ALMPs. We exploit that the IEB contains information on both the community where a person works and the community where that person lives. Hence, it is known from which communities firms hire.

What we would like to measure is the region from which a firm potentially hires by assigning a firm-specific weight to each community by dividing the number of potential hires of the firm from the community by the total number of potential hires by the firm. There are two ways to measure this with the data at hand. On the one hand, we could use pre-treatment

[8] A record ends when a firm closes. However, since this event may be influenced by ALMPs, it is an interesting outcome in itself (and recorded as one of the outcome variables used in the estimation).

information about the workers a firm actually hired in the past (i.e. before 2001). However, the community information is only available from 1999 onwards which implies that we could only use information from 1999-2000 to construct the hiring regions based on the firm's actual hiring decisions. Because the number of employees per firm is not very large (minimum: 100, median: 245, 75th percentile: 499), and the time period that can be used is short, this procedure yields a very imprecise measurement of the community weights. Moreover, the discrepancy between actual and potential hiring regions could be quite large in this case.

To get a more precise measurement of the community weights and to better capture *potential* hiring opportunities, the following approach is implemented: Consider a firm that is located in community *i*. We use the IEB data, which is representative for German employees for the period 1999-2008, to check where all employees who work in community *i* live. We construct a weight w_{ij} for each community *j* that is equal to the number of workers hired from community *j* by firms located in community *i*, divided by the total number of hires by firms located in community *i*. Consequently, all firms that are located in the same community *i* are supposed to have the same potential hiring region.[9] The weights sum up to one for each firm and can be interpreted as the long-run likelihood of obtaining job applications from a particular community. Although there will be some measurement error, this approach better captures the idea of how far a worker is willing to commute to work.[10] Note that from the individual perspective, sending applications to particular firms should not be influenced directly by the firms' reaction to the local ALMP. Thus, in this sense the weights are exogenous. However, there might still be concerns regarding the way we define the hiring regions with respect to

[9] It might be desirable to split all workers in a given community at least by, for example, industry. However, this again introduces a lot of imprecision which is why we decided not to implement it.

[10] Our approach does not capture relocation in order to start a new job. However, since this is much more common for high-skilled jobs, and since we are interested in hiring from the pool of - on average low-skilled - unemployed workers, the resulting measurement error should be negligible.

the use of post-treatment periods (2004-2008) and potentially endogenous relocation of workers. We address these issues in various sensitivity checks in Section 5.3. As a preview of those results, note that there is no reason to believe they play any important role.

Since it is known which LEA each community belongs to, the above weights are used to measure firm-specific exposure to different ALMPs. Because ALMPs are only relevant for applications received from the pool of unemployed workers we modify the community weights to account for skill differences in the pool of employed and unemployed workers. First, we calculate the weights conditional on the education level of the employees, i.e. education-specific community weights, w_{ije}. We distinguish three education levels: without vocational degree, with vocational degree and with college or university degree. We then calculate the share of *unemployed* workers with the corresponding education level in community j: u_{je}. The modified community weights are given by $\tilde{w}_{ij} = \sum_{e=1}^{3} w_{ije} u_{je}$. They take into account that for example low-skilled workers are over-represented among the unemployed. So if community j has a higher weight among low-skilled employees than overall, it is more relevant for hiring from the pool of unemployed workers. Hence, we will have $\tilde{w}_{ij} > w_{ij}$.

Denote by s_j the supply of participants in a certain type of ALMP in community j, and by J the total number of communities. The supply in a given community, s_j, is measured by using the number of completed courses in the period 2001-2003, or - if there is large variation in durations - by the number of participants weighted by the share of time within the treatment period 2001-2003 which they spent in the respective programs (see Table 4.1 for which measurement concept is used for the particular treatment). To obtain program shares we divide this number by the number of unemployed workers weighted by which share of the period 2001-2003 they have been unemployed. Using program shares ensures that we measure the relative importance of a given type of program independent of the level of unemployment in a community.

To move from the community-specific shares, s_j, to the firm-specific shares, $\tilde{d}_i$, the modified weights described above are applied: $\tilde{d}_i = \Sigma_{j=1}^{J} \tilde{w}_{ij} s_j, i = 1,...,N$. Unfortunately, the sample of 2979 firms is too small to exploit the total variation in $\tilde{d}_i$ to estimate dose-response relations (like in Imbens, 2000). Therefore, we split the firms into two groups with high exposure to $\tilde{d}_i$ ($d_i = 1$ *if* $\tilde{d}_i > a$) and low exposure to $\tilde{d}_i$ ($d_i = 0$, *if* $\tilde{d}_i \leq b, a \geq b$). In total, we analyse the effect of high versus low exposure to 9 different types of ALMPs that are listed in Table 4.1 and have already been described in Section 3.1. As cut-offs for $d_i = 1$ we use $\Pr(\tilde{d}_i > a) = 1/3$ and for $d_i = 0$ we use $\Pr(\tilde{d}_i \leq b) = 1/3$. Firms in the middle third of the distribution of $\tilde{d}_i$ are excluded from the analysis. We additionally contrast high exposure to long training (TR) and low exposure to short TR with the reverse combination. In this case, due to sample size requirements, we define high (low) exposure as firms above (below) the median of the distribution of $\tilde{d}_i$.

Table 4.1: Treatments

No.	Acronym	Description	Measurement
1	SE	Subsidized employment	Participants weighted by duration
2	TM	Training measures	Number of completed courses
3	Short FVT	Classical further vocational training (FVT) with planned duration of up to 6 months	Number of completed courses
4	Long FVT	Classical FVT with planned duration of more than 6 months	Number of completed courses
5	DC	Degree course (FVT that awards a vocational degree)	Participants weighted by duration
6	PF	FVT in practice firms	Number of completed courses
7	Short TR	2, 3 and 6 with planned duration of up to 6 months	Number of completed courses
8	Long TR	4, 5 and 6 with planned duration of more than 6 months	Number of completed courses
9	TR	TM, FVT, DC, PF	Number of completed courses
10	Long/Short	7 vs. 8	Number of completed courses

In Table A.1 in the Appendix, we describe the treatments considered in more detail. Here, we report separately for the firms coded as *d=1* and *d=0* in a given contrast the average program shares in the six distinct types of programs considered (treatments 1-6 in Table 4.1). This is informative about correlations in the use of different types of ALMPs and hence im-

portant for the interpretation of the treatments. The main message from Table A.1 is that except for some correlations with training measures (TM) and sometimes with subsidized employment (SE), the other dimensions of the ALMPs that are not used to define the respective treatment are very well balanced between treated and untreated firms. This means that the treatments we define have a relatively clear interpretation, because most of the other dimensions of the ALMPs are implicitly held constant. Correlations can only be found in the following cases: The use of TM is positively correlated with SE, short FVT, DC and PF but negatively correlated with long FVT. The use of SE does not vary much but there is some negative correlation with short FVT and DC in the latter treatments but not in the SE treatment itself.

Table 4.2 reports sample sizes and descriptive statistics for selected firm and regional characteristic by treatment status for each of the 11 treatments (for a full set of descriptive statistics see Internet Appendix I.3). Selectivity in terms of the composition of a firm's workforce is very small. We only report the share of female but differences in terms of age, education and type of shop are similarly small (see Internet Appendix I.3). With the exception of degree courses (DC) selectivity in terms of pre-treatment outcomes (firm size, turnover) and earnings is also generally small. Sometimes larger differences occur for firm size but especially the turnover measures are usually very similar for treated and untreated firms. For DC there are larger differences in firm size, tenure and earnings. Here, selectivity is also highest with respect to the unemployment rate in the firms' hiring regions which shows a 5 %-point lower rate for treated than for untreated firms. A similar difference can be observed for Short FVT. In contrast, the hiring regions of firms exposed to a high share of subsidized employment (SE) or training (TR) exhibit higher unemployment rates than those with a low exposure. For the other treatments, the unemployment rates are quite similar. The differences in the characteristics of the employed and unemployed workforce in the firms' hiring regions are similar to those in the unemployment rates (see Internet Appendix I.3).

Table 4.2: Descriptive statistics by treatment and treatment status

Treatment	*d*	*N*	Employees	Females	Tenure	Entries	Exits	Temp	Earnings	UE rate	Rural	City
(1) SE*	0	593	444	.37	5.8	.16	.14	.06	2790	.13	.24	.12
	1	612	405	.37	5.5	.19	.15	.08	2700	.17	.28	.25
(2) TM	0	853	457	.38	6.3	.18	.16	.06	2640	.19	.24	.25
	1	880	377	.40	6.2	.18	.16	.07	2520	.21	.36	.16
(3) Short FVT	0	736	403	.39	6.2	.17	.17	.05	2490	.22	.27	.24
	1	878	417	.40	6.4	.18	.16	.08	2640	.18	.34	.17
(4) Long FVT*	0	591	410	.36	7.2	.15	.14	.06	2700	.15	.35	.07
	1	609	490	.37	7.1	.17	.16	.06	2790	.16	.19	.20
(5) DC	0	846	359	.41	5.9	.17	.17	.07	2460	.23	.35	.22
	1	875	431	.39	6.6	.17	.16	.07	2670	.18	.30	.21
(6) PF	0	854	408	.40	6.0	.18	.18	.07	2580	.21	.26	.33
	1	880	378	.40	6.2	.18	.17	.08	2520	.20	.38	.18
(7) Short TR	0	758	457	.38	6.3	.18	.17	.06	2640	.19	.21	.29
	1	878	369	.41	6.2	.18	.16	.07	2520	.21	.36	.14
(8) Long TR*	0	592	401	.37	7.0	.16	.14	.06	2730	.15	.30	.09
	1	546	463	.38	7.1	.17	.16	.06	2760	.16	.19	.18
(9) TR	0	793	456	.38	6.4	.17	.17	.06	2640	.18	.23	.27
	1	878	371	.41	6.1	.18	.16	.07	2490	.22	.38	.14
(10) Long/Short*	0	461	422	.37	7.1	.16	.16	.06	2730	.16	.26	.09
	1	451	511	.38	6.8	.18	.17	.06	2850	.16	.12	.29

Note: *Treatments 1, 4, 8 and 10 only include West German firms due to lack of common support for East German firms. SE subsidized employment (mainly wage subsidies), TM training measures, FVT further vocational training, DC degree courses, PF practice firms, TR training. N = number of observations (establishments). All variables are measured in 2000 and calculated from the IEB or the EHP data. Tenure is measured in years, earnings in EUR per month. Females, entries, exits and temporary workers (Temp) are shares of employees. UE rate = number of unemployed workers / number of employed workers weighted by the spell duration in 2000 for each community and then aggregated using the firm-specific weights. Rural and City are community-specific dummies aggregated using the firm-specific weights.

However, selectivity is strong for all treatments with respect to the (weighted) share of rural and urban communities in the firms' hiring regions. It is important to note though that the small differences in pre-treatment outcomes indicate that the link between those regional differences and firm performance seems to be weak. This supports our argument that the differences in the local use of ALMPs are to a large extent driven by factors that are unrelated to firm performance, especially since the differences in the composition of the local workforce and labour market performance are also moderate to small.

Another lesson from Table 4.2 is that the firms for which the effects are estimated differ for each treatment (both in their number and their characteristics). Thus, if effects are heterogeneous, the effects obtained below are not directly comparable across treatments. In

particular, for some treatments we only use West German firms (see note to Table 4.2). The reason is that the number of East German firms in one of the treatment groups is too small to allow capturing the relevant selectivity (lack of overlap in the covariate distributions, i.e. no common support). The full set of descriptive statistics in Internet Appendix I.3 shows how the firms considered in each treatment differ in detail.

4.4 Plausibility of the identification strategy

As discussed in Section 4.1, the basic idea for identification is that we condition on all variables that jointly determine the ALMPs conducted *inside* the firm's hiring regions and the firm's performance, and then exploit that there is still variation in these local ALMPs. This variation is induced by characteristics of areas *outside* the firm's hiring region which are administered by the same LEAs, and/or by overall differences in LEA strategies. This implies that we impose the following three specific assumptions: (1) All characteristics of the firm and the firm's hiring region that are related to both firm performance and community-specific ALMPs *within* the firm's hiring region are observed.[11] (2) The firm's hiring region does not completely coincide with the area of responsibility of a single LEA. (3) The firms' employees are a negligible part of the workforce served by the relevant LEAs.[12]

The plausibility of assumption (1) hinges on the ability to capture the relevant differences in the economic performance of the firms' hiring regions as they are related to both firm performance and local ALMPs. We argue that this is possible with our data. Firstly, we observe a rich set of indicators for and predictors of economic performance on the county level: GDP growth, jobs per inhabitant, earnings, population density, rurality/urbanity, migration,

[11] We also need common support in all these characteristics.

[12] Implicitly, we also assume that firms do not strategically choose their location to maximize the benefits from local ALMPs. This is plausible since location choices are long-term because of the fixed cost involved, while autonomous local policy variations are only possible very recently.

commuting, public transport, travel time to next bigger city, and child care facilities. Second and most importantly, we observe the same administrative data for employed and unemployed workers that are available to the LEAs when making their decision on local ALMPs. This allows us to construct a large set of control variables that capture differences in the composition and evolution of both the employed and unemployed workforce of each community, in particular in terms of gender, age, nationality, education, occupation, industry, earnings and type of (last) job, unemployment rate, type and amount of income support during unemployment (see Internet Appendix I.3 for a full list of variables constructed from the different data sources).

We also control for firm characteristics, in particular for industry and the composition of the work force in terms of gender, age, education, nationality, earnings, and the type of job. The reason is that the general economic performance of the hiring region may have different effects on firm performance depending on the characteristics of the firm, for example, if the sector of the firm differs from the sector that dominates in the hiring region. A priori it is not clear whether one should also condition on pre-treatment outcomes like firm size and turnover. For example firm size is strongly related to a firm's ability to cope with adverse economic conditions and therefore to firm performance. Hence, it is an important confounder. However, if local ALMPs are correlated over time, pre-treatment outcomes may not be exogenous to future treatments. In the previous section, we have shown that for some treatments there are sizeable differences in firm size for treated and control firms. We therefore condition on firm size in 2000 but not on other pre-treatment outcomes. In Section 5.3, we discuss the role of pre-treatment outcomes for selection correction in more detail.

Table 4.3 documents the role of the firm's hiring region for the LEAs' decisions regarding the use of ALMPs (assumption 2) as well as the firm's impact on local ALMPs (assumption 3). In the first four columns, we report summary statistics on how many LEAs

overlap with a firm's hiring region. Only 36 firms or 1.2% of the sample overlap with only one LEA. More than 50% of the sample overlaps with at least 10 LEAs. Thus, local ALMPs inside the hiring regions are far from being dominated by the strategy of a single LEA.

Table 4.3: Overlap of a firm's hiring region with different LEAs

Number of LEAs*	Number of firms	Share in %	Cumulative share in %	Percentile	Weight of hiring region in LEA region**	Weight of firm in hiring region***
1	36	1.2	1.2	10	.04	.0004
2	79	2.7	3.9	20	.06	.0007
3	140	4.7	8.6	30	.08	.0010
4	173	5.8	14.4	40	.10	.0012
5	195	6.5	20.9	50	.14	.0016
6	211	7.1	28.0	60	.20	.0021
7	204	6.8	34.8	70	.28	.0029
8	182	6.1	41.0	80	.40	.0042
9	139	4.7	45.6	90	.74	.0066
10	128	4.3	49.9	Mean	.25	.0030
>10	1492	50.1	50.1	Maximum	.84	.0592

Note: * Number of LEAs with which a firm's hiring region overlaps. ** Sum of employees in communities in a firm's hiring region divided by the sum of all employees covered by the corresponding LEAs, calculated for each community in a firm's hiring region and then aggregated using the firm-specific weights. *** Sum of employees in firm divided by sum of employees in hiring region.

In column 6 of Table 4.3 we report summary statistics on how many employees live in communities inside the hiring region of a firm relative to all employees living in the area of responsibility of the LEAs that overlap with the hiring region. This provides a measure for how much of the ALMP of a LEA is determined inside rather than outside a firm's hiring region. For more than 80% of the firms this share is less than 50%. The mean is 25% and the maximum is 84%. Hence, there is no hiring region that completely coincides with a single LEA region. Furthermore, for the large majority of firms hiring regions do not dominate the workforce of the overlapping LEAs. Consequently, a large part of the LEAs' ALMPs is determined outside the hiring regions of the respective firm. For the small share of firms for whom the hiring region has a relative large weight we provide a sensitivity check in Section 5.3, which is based on excluding these firms. The final column of Table 4.3 shows that all

firms have a negligible impact on local ALMPs, even inside their hiring regions. More than 90% of all firms employ less than 1% of workers in their hiring region.

Table 4.4: Determinants of the program shares in a firm's hiring region

	SE	TM	Short FVT	Long FVT	DC	PF	Short TR	Long TR	TR
P-value of F-test for joint significance									
Characteristics of firm	0.55	0.40	0.18	0.63	0.55	0.83	0.30	0.87	0.27
Regional characteristics	0.00	0.02	0.06	0.02	0.13	0.00	0.01	0.00	0.02
Characteristics of workforce inside hiring region	0.00	0.00	0.03	0.00	0.42	0.01	0.00	0.00	0.00
Characteristics of workforce outside hiring region	0.03	0.00	0.04	0.04	0.05	0.00	0.00	0.03	0.02
LEA shares	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Partial R-squared in % of total R-squared									
Characteristics of firm	0.0	0.5	1.0	0.7	1.4	0.4	0.5	0.5	0.5
Regional characteristics	0.9	3.3	2.5	3.3	3.6	5.6	3.0	6.0	2.9
Characteristics of workforce inside hiring region	1.3	8.9	8.7	8.1	14.0	10.0	8.0	9.3	8.4
Characteristics of workforce outside hiring region	0.6	3.5	6.3	3.3	12.7	10.5	3.0	6.5	3.0
LEA shares	2.3	36.1	44.2	14.6	42.9	27.1	34.3	25.3	35.2
Total R-squared	0.87	0.54	0.35	0.43	0.21	0.38	0.57	0.34	0.56

Note: SE subsidized employment, TM training measures, FVT further vocational training, DC degree courses, PF practice firms, TR training. Tests are based on the regressions presented in Table I.2 in Internet Appendix I.2.

To further asses the type of variation we are exploiting, Table 4.4 summarizes information about the determinants of the program shares we use to define treatments. Table 4.5 shows how they are related to firm survival as one major outcome of interest. In Table 4.4, we report summary statistics from firm-level regressions of the respective program shares the firm faces, on, firstly, the variables we control for (firm characteristics, county-level regional characteristics, and the characteristics of the workforce *inside* the firm's hiring region), secondly, the characteristics of the workforce *outside* the firm's hiring region, and, thirdly, the shares of the LEAs in the firm's hiring region.[13] The second set of variables measures which factors *outside* the firm's hiring region determine the ALMPs firms face. The LEA shares measure to what extent ALMPs are driven by LEA-specific factors such as preferences that cannot be explained by the composition of the LEA's workforce or other local labour market

[13] We group the 176 LEAs into 90 groups by combining small neighbouring LEAs.

conditions. These two blocks of variables are informative about the exogenous variation we use for identification. We report the p-value of an F-test of the joint significance of these five blocks of variables to see whether they matter at all, as well as their partial R^2 in % of the total R^2 to assess their relative importance. Table 4.5 reports the same statistics for similar regressions of an indicator for firm exit in the years 2004-2008 on the same blocks of variables.

Table 4.5: Correlation of firm exit 2004-2008 with baseline covariates

	2004	2005	2006	2007	2008
P-value of F-test for joint significance					
Characteristics of firm	0.00	0.00	0.00	0.00	0.00
Regional characteristics	0.32	0.47	0.17	0.04	0.06
Characteristics of workforce inside hiring region	0.01	0.03	0.01	0.03	0.03
Characteristics of workforce outside hiring region	0.58	0.89	0.57	0.54	0.65
LEA shares	0.07	0.38	0.09	0.14	0.06
Partial R-squared in % of total R-squared					
Characteristics of firm	46	52	47	45	48
Regional characteristics	8	8	9	11	10
Characteristics of workforce inside hiring region	15	13	14	13	11
Characteristics of workforce outside hiring region	9	6	8	8	8
LEA shares	14	12	17	19	17
Total R-squared	0.07	0.09	0.09	0.08	0.09

Note: Based on regressions of the outcome variable of interest (firm exit in a given year) on the same blocks of variables as the regressions on which Table 4.4 is based (see Table I.2 in Internet Appendix I.2).

Tables 4.4 and 4.5 show the importance of controlling for county-level regional characteristics as well as of controlling for the characteristics of the workforce *inside* the firm's hiring region. The reason is that both groups of variables are related to both the program shares and firm performance. Firm characteristics have, as expected, an important impact on firm performance but they do not seem to matter for local ALMPs. This supports our claim that individual firms do not affect policy. From Table 4.4 we furthermore see that there is substantial variation in local ALMPs induced by factors *outside* the firm's hiring region. These are in particular LEA-specific factors such as preferences that cannot be explained by the composition of the LEA's workforce or other local labour market conditions. Hence, conditioning on firm characteristics, county-level regional characteristics, and the characteristics of the workforce *inside* the firm's hiring region, there is considerable variation left which

we can use for estimating the effects of interest. Table 4.5 finally indicates that the characteristics of the workforce *outside* the firm's hiring region are not significantly related to firm performance, which supports our claim that these factors are exogenous. For the LEA shares we find no or at most a weak relation. As regards the latter, note that in these auxiliary linear regressions the LEA shares might also pick up functional form misspecification in the regional characteristics and the characteristics of the LEA's workforce. In our semi-parametric estimator for the firm effects of ALMP we do not restrict the relation between control variables and outcomes, hence such spurious correlations will not be picked up.

In summary, Tables 4.3-4.5 provide strong supporting evidence that both the firms and their hiring regions play no dominating role in determining the ALMPs of single LEAs. Moreover, they indicate that, conditional on the firm-performance-related determinants of local ALMPs inside the hiring regions, there is room for sufficient exogenous variation that is induced mainly by strategy differences due to overlap with multiple LEAs as well as by the workforce covered by the LEAs that lives outside the hiring region.

4.5 Estimation

Estimation is straightforward because we face a standard so-called binary treatment framework where we condition on a large number of observables. For implementing the latter, matching on the propensity score is a standard method because its semi-parametric nature leads to desirable robustness properties and allows for effect heterogeneity (for a recent survey see Imbens and Wooldridge, 2009). We obtain the propensity scores for each treatment by estimating probit models using the respective treatment dummy as dependent variable and characteristics of the firms and their hiring regions discussed in the previous sections as control variables. See Internet Appendix I.4 for the exact specifications and results. The models have been tested extensively against misspecification in terms of omitted variables, non-

normality and heteroscedasticity. The estimation results for the probits confirm the conclusions drawn from the descriptive statistics in Section 4.3.

In a recent extensive Monte Carlo study Huber, Lechner, and Wunsch (2013) find that one particular estimator that combines weighted radius matching with bias-adjustment regressions performs particularly well. This estimator is used in this paper as well. Its details are provided in Internet Appendix I.1. For inference we use the (block-) bootstrap by independently drawing firms (and all their employees) and then bootstrapping the t-statistic to obtain p-values (1999 replications). Again, all details are relegated to Internet Appendix I.1.

5 Results

In this section, we present the average effects of the ten different treatments defined in Section 4.3 on various firm outcomes for the firms under investigation. As discussed in Section 4.2, outcomes are calculated from three different data sources and refer to different subsamples of firms which result from outcome-specific attrition rates.[14]

5.1 Outcomes from administrative data

In this section we present the main results. They are based on the outcomes measured in the administrative EHP data. This data source has the advantage that the information is available for all firms (no attrition) in the sample and has a high degree of reliability. Table 5.1 shows the effects of c.p. moving a form from low to high exposure to different types of ALMPs on firm growth, firm survival and turnover. The effects on firm growth are measured both in absolute (effect on firm size) and relative terms (change in firm size relative to 2000). Firm survival is measured by a dummy variable that is equal to one if the establishment no

[14] As discussed in Section 4.3, the firms for which the effects are estimated differ across treatments. Consequently, if there is effect heterogeneity, the effects are not directly comparable across data sources and time periods for which outcomes are measured, as well as across treatments. This has to be kept in mind when interpreting the results.

longer exists on June, 30, in the respective year. To measure turnover, we use different quantiles of the firm's tenure distribution as well as the share of temporary workers. For the sake of brevity, in Table 5.1 we focus on the short-run effects measured in 2004 and the longer-run effects of 2008. Results including the intermediate years as well are reported in Table I.5 in Internet Appendix I.5. They confirm the findings discussed below.

Table 5.1: Short and long-run outcomes from the EHP for the full sample

Outcome	Year	SE	TM	Short FVT	Long FVT	DC	PF	Short TR	Long TR	TR	Long vs. Short
Firm size [a]	2004	-32.7	17.0	-9.9	-148.3	-9.5	14.5	14.2	-57.4	9.9	3.2
	2008	-53.4	29.3	-21.4	*-196.3*	-13.6	33.3	25.6	-96.1	1.9	-24.0
Growth in levels [b]	2004	-17.2	-17.5	-3.3	-46.7	1.8	6.4	-1.3	7.5	-9.1	35.1
	2008	**-37.9**	-5.2	-14.8	**-94.7**	-2.2	25.2	10.1	-31.3	-17.2	7.8
Firm closure	2004	1.7	-1.1	2.1	0.6	2.0	0.5	0.8	0.1	-2.3	-1.7
% points*100	2008	*4.6*	0.0	2.4	5.1	5.9	-0.5	1.9	1.8	5.1	-1.8
Share exits [c]	2004	0.0	1.8	-1.3	2.2	1.6	0.3	1.9	0.2	*3.8*	-1.5
	2008	2.2	-5.9	-1.3	**6.6**	2.6	-3.3	-1.6	0.8	0.5	7.1
Share temporary	2004	-5.4	18.3	-3.7	19.8	15.7	-8.0	16.0	13.2	35.1	7.2
workers*100 [d]	2008	*58.8*	-59.4	0.5	**40.2**	15.0	-35.8	-11.7	-14.4	3.7	53.6
Tenure in days:	2004	1.2	15.7	1.9	-86.6	19.2	2.1	44.0	-32.7	*88.9*	**-164.7**
25% quantile	2008	-71.0	32.7	20.5	-63.4	5.0	96.5	3.6	131.0	*135.3*	-63.4
Tenure in days	2004	-36.9	-94.1	4.2	-209.3	146.0	117.3	46.3	-6.5	77.6	-275.4
median	2008	-68.7	-16.5	-1.1	-226.5	110.7	*309.1*	84.1	131.5	**243.4**	-144.4
Tenure in days	2004	-192.6	-54.9	41.6	-347.8	164.1	-43.6	108.2	94.9	0.0	-602.3
75% quantile	2008	-83.6	-5.7	8.1	-352.4	169.9	276.0	89.3	147.8	159.9	-477.1

Note: SE subsidized employment, TM training measures, FVT further vocational training, DC degree courses, PF practice firms, TR training. [a] Number of employees. [b] Difference in number of employees relative to the year 2000. Number of exits in last year. [d] Workers not employed in firm on June, 30, of particular year but in year before and in year after. *Italics* indicate significance at the 10% level, **bold** numbers indicate significance at the 5% level, and ***bold numbers in italics*** indicate significance at the 1% level.

We find that firms facing a higher share of subsidized employment are hurt in the long run. They shrink, employ a higher share of temporary workers and go out of business with higher probability. It is important to note that subsidized employment for those firms mainly comprises wage subsidies. Due to a lack of common support this treatment only includes West German firms. The other type of subsidized employment, employment programs, is used very rarely in West Germany. The results suggest that subsidized workers do not seem to be a good match. This is in contrast to empirical studies of the effects of wage subsidies on subsi-

dized employees that use matching methods (e.g. Sianesi, 2008; Bernhard, Gartner and Stephan, 2008). However, these studies have been criticised on methodological grounds by Schünemann, Lechner and Wunsch (2013), who, using a more credible identification strategy, find no positive effects of wage subsidies for subsidized employees.

For short training programs, which provide job search assistance or moderate human capital improvements (training measures, short further vocational training, short training in practice firms), we do not find any effects on firm performance. Hence, neither is there evidence for open vacancies being filled faster, nor for improvements in the quality of the pool of potential applicants and match quality. The former confirms the findings from the empirical literature on effects for participants (Card, Kluve and Weber, 2010). The latter complements this literature and supports its pessimistic assessment of the cost-effectiveness of these measures.

Firms facing a large supply of participants in long further vocational training (FVT) are harmed in the longer run. The share of temporary workers and exits increases significantly, and the firms are smaller and shrink faster. (There are also sizeable negative effects on tenure and firm survival but they are not statistically significant.) There are two possible explanations for this finding. On the one hand, intensive use of long FVT may affect the pool of applicants in an undesirable way from the firms' point of view. This may be an indication that LEAs may misjudge which skills are demanded by the market.[15] On the other hand, the large share of these programs might be evidence for sizeable threat effects which negatively affect job match quality: unemployed workers have a strong incentive to accept any job offer to avoid being locked in such a long program that they might consider mainly as a leisure tax.

[15] There is evidence for this for long training programs used in the 1990s in East Germany, see Lechner, Miquel, and Wunsch (2007).

Interestingly, we do not find such negative effects for all long training programs taken together (long FVT, degree courses and long training in practice firms) although long FVT dominates this combined treatment. As the characteristics of the firms and their hiring regions (see Section 4.3) as well as the shares of subsidized employment and short training (see Table A.1) are very similar to those for long FVT, the differences in the effects are likely to come from degree courses and practice firms. We do not find any significant effects of a high share of degree courses in the hiring region although tenure seems to increase somewhat. There is, however, some evidence for positive effects of practice firms. In the long run, median tenure increases by almost one year. There is also some indication of positive effects on growth and a reduction in the share of temporary workers, but these effects are not statistically significant.

When all types of training are grouped together, i.e. we contrast more or less intensive use of training in general, we find some evidence for more exits in the short run but also a shift of the tenure distribution to the right in the longer run. The latter again seem to be driven by degree courses and long training in practice firms. This finding clearly stands against the overall time trend and training being used more intensely in regions with higher unemployment rate (see Table 4.2). Thus, there is some indication for a possible improvement in match quality due to intense training of unemployed workers in degree courses and practice firms.

For the contrast of intense use of long training versus intense use of short training we find negative effects on tenure which seem to be driven by the negative effects of high exposure to long FVT. This also shows in terms of a sizeable increase in exits and temporary workers but the effects are not significant, probably due to the relatively small number of observations.

5.2 Outcomes from survey data

In the years 2004 and 2005 we observe additional outcomes in the EP survey for, respectively, 47% and 44% of the firms who responded to the EP 2000 survey. In Section 5.3,

we show that survey non-response is unrelated to the treatments we defined after selection correction. However, the population for which we estimate the effects using survey outcomes may still differ from the one using the full sample. Thus, results may differ due to effect heterogeneity. In Tables I.3a and I.3b in the Internet Appendix I.3 we therefore present descriptive statistics for the full sample and the two subsamples for which we observe the outcomes in the EP survey 2004 and 2005, respectively. All characteristics are very similar across subsamples with two exceptions: survey response rates are somewhat lower for firms in big cities and notably higher for East German firms.

To assess whether this heterogeneity affects results, we re-estimate the effects for the EHP outcomes in the two subsamples that responded to the EP survey 2004 and 2005, respectively. The results are presented in Tables I.6 and I.7 in Internet Appendix I.5. Our findings are similar for subsidized employment, degree courses, practice firms, and the contrast long versus short training, although we sometimes lose precision, and hence significance, due to the smaller sample size. Different results are obtained for TM and short FVT, for which we find negative effects on firm survival and tenure (significant for short FVT in the 2005 sample). This also shows up in the effect for all short training programs, which are not significant, though. It also affects the results for all training programs taken together, for which we no longer find positive effects on tenure. For long training programs, the negative effects on long FVT now show up significantly for some outcomes. Unfortunately, sample sizes are too small to investigate this potential source of effect heterogeneity in more detail.

In Table 5.2, we present short-run effects for the survey outcomes. (To avoid even higher attrition rates, we only use the 2004 and 2005 waves of the BP.) Moreover, all variables are self-assessed with the usual drawbacks of such information. We focus on outcome variables that provide information on firm performance which complement the ones from the administrative BHP data.

Table 5.2: Short-run outcomes from EP surveys

Outcome	SE	TM	Short FVT	Long FVT	DC	PF	Short TR	Long TR	TR	Long/ Short
Profitability in 2004 (EP 2005) (1 of 5 categories)*:*										
Very good	-1.9	0.1	-0.3	0.8	0.3	0.9	-1.7	-2.8	0.4	2.0
Good	-1.2	6.5	-2.1	2.7	-1.3	0.4	2.6	-5.6	-2.8	-1.9
Reasonable	-11.9	-0.2	-3.3	0.3	2.1	-6.8	-8.2	8.0	-0.3	2.1
Sufficient	7.9	-3.9	5.1	-1.9	0.5	7.5	4.2	9.1	3.4	0.0
Insufficient	3.0	-3.6	0.5	-3.1	-1.9	-1.0	3.9	-1.3	1.4	2.3
Investments in 2004 (EP 2005):										
No investments (y/n)	-6.2	5.7	7.7	-5.4	4.5	*8.5*	5.5	-3.5	2.7	5.4
Technical equipment 2005 (EP 2005):										
New	1.9	0.3	4.8	0.2	6.3	2.2	-0.9	2.6	2.1	-0.9
Relatively new	-7.8	-1.4	*-8.6*	4.7	-15.0	-8.2	-6.8	1.5	-6.3	15.7
About average	7.8	2.4	2.4	-3.8	*8.0*	5.8	*9.1*	-3.1	5.9	-14.7
Somewhat outdated	-1.8	-1.7	1.6	-0.4	0.6	0.4	-0.3	-0.7	-1.9	0.2
Firm closure (EP 2004):										
Partial closure in last year (y/n)	3.2	*2.8*	-2.2	-1.9	-1.6	2.0	*1.6*	-7.0	0.9	-0.9
Planned relocation of production to Eastern Europe (y/n)	1.7	2.0	-8.9	3.5	-2.4	1.2	0.1	-2.0	0.4	-2.2
Stability of employment 2004 (EP 2004):										
Hired temporary workers in first half of 2004 (y/n)	-9.5	9.6	-3.1	-0.9	-6.2	3.7	*13.1*	-4.4	*11.7*	-9.2
Temporary workers (y/n)	-3.1	-10.4	-0.3	2.7	*7.2*	2.7	-1.8	0.3	-0.3	-2.3
Leased workers (y/n)	1.7	8.4	-3.4	-6.4	-3.9	2.5	14.0	*-9.4*	7.4	-8.4
Number of interns/helps	-0.6	0.7	0.7	0.2	*-1.1*	0.7	0.8	-0.4	0.8	-3.0
Current hiring 2004 (EP 2004):										
Currently hiring (y/n)	1.1	5.8	*-7.9*	-14.4	0.6	*11.6*	0.4	*-10.6*	13.1	-13.9
Number of vacancies overall	-0.1	0.3	-0.2	-0.2	0.0	0.4	-0.2	*-0.6*	0.4	-1.2
for unskilled workers	0.1	0.2	-0.1	0.2	0.1	0.0	0.0	-0.1	0.0	0.0
for skilled workers	-0.1	0.1	0.0	0.0	0.0	0.1	-0.2	-0.3	0.1	-0.3
for unskilled clerks	0.0	-0.1	-0.1	0.0	-0.2	-0.1	-0.1	0.0	0.0	-0.2
for skilled clerks	-0.1	0.1	0.1	-0.1	0.0	*0.2*	0.1	-0.2	0.1	*-0.3*
for high-skilled clerks	0.0	0.0	-0.1	-0.3	0.0	*0.2*	0.0	0.1	0.2	*-0.5*
Expected personnel problems for 2005-2006 (EP 2004):										
Too many employees (y/n)	-1.0	-13.8	0.5	3.5	5.7	6.2	-5.6	6.0	-4.3	0.7
Too few employees (y/n)	-3.6	0.4	-0.7	-1.7	-1.3	1.4	1.6	-3.2	-4.3	0.3
High turnover (y/n)	1.7	1.2	-1.9	1.4	-0.8	-1.2	1.9	-1.1	1.5	-1.8
Shortage of young qualified workers (y/n)	*-7.0*	-6.7	-7.8	1.2	9.9	0.1	-2.9	-3.1	-2.9	-3.6
Skilled workers leave firm (y/n)	3.5	1.7	-4.4	1.7	-2.5	-3.4	*5.0*	2.6	7.3	-6.9
Difficulties to hire skilled workers (y/n)	-6.4	6.3	-5.9	-4.9	-1.4	3.8	1.7	-4.3	-0.9	-3.6
High training needs (y/n)	-0.1	6.4	0.5	-4.5	-3.7	-0.7	5.2	-1.5	7.6	1.6

Note: SE subsidized employment, TM training measures, FVT further vocational training, DC degree courses, PF practice firms, TR training. Effects for binary outcomes (y/n) and shares are in percentage points*100. *Italics* indicate significance at the 10% level, **bold** numbers indicate significance at the 5% level, and ***bold numbers in italics*** indicate significance at the 1% level.

Firstly, we report effects on self-assessed profitability (dummy variables for five categories from very good to insufficient) as a direct measure of firm performance. Secondly, as predictors of longer-run performance, we look at whether any investments have been made

and at the state of the technical equipment (dummy variables for four categories from 'new' to 'somewhat outdated'). Thirdly, we directly supplement the information on firm survival and turnover from the BHP. In the BHP, we only measure *complete* closure of an establishment that was part of the BP in 2000. However, the BP contains information on whether *parts* of the establishment have been closed, or whether a relocation of production to Eastern Europe is planned for the coming year. As regards turnover, the BP contains more detailed information on workers with temporary contracts. The administrative data does not contain information on the type of employment contracts. It is only observed whether workers return to the same firm after an interruption. In the BP, there is information whether worker on temporary contracts have recently been hired or are currently employed, whether leased workers are currently employed in the firm, and how many interns or helps are working in the firm. The third set of survey outcomes provides information on current hiring: whether the firm is hiring at all and how many vacancies there are by type of job (worker versus clerk) and skill level. The final set of variables concerns expected personnel problems for the coming two years such as shortage of skilled workers of high turnover. They may be a predictor of future firm performance.

In line with the results from the administrative data, we find negative effects of high exposure to subsidized employment on self-assessed profitability in 2004. This is despite less treated than non-treated firms reporting shortage of young qualified workers and fewer treated firms without any investments. Yet, the more outdated state of treated firms' technical equipment in 2005 suggests that total investments, which we do not observe, might have been reduced.[16]

[16] The effect is relatively large but not significant in this relatively small sample.

For TM and short TR, we see an increase in partial firm closure in 2004, thus confirming the more pessimistic assessment in the responding subsample with a higher share of East German firms. For short TR we also see a higher share of firms hiring temporary workers and employing leased workers in 2004, more outdated equipment in 2005, and more firms facing the problem of skilled workers leaving the firm in 2005-2006. For short FVT, we find significantly more treated firms without any investments and currently not hiring (in 2004). However, we also see a lower share of treated firms expecting a shortage of young qualified workers or planning a relocation of production to Eastern Europe in 2005-2006.

For firms facing a high share of participants in long FVT, we find negative effects on hiring in 2004, which is in line with negative long-run effects on growth and survival, based on the administrative outcomes. For degree courses, which seem to have some positive long-run effects on tenure based on the EHP (see Table I.6 in Internet Appendix I.5), we see a higher share of exposed firms employing temporary workers but a lower number of interns and helps working in these firms. We also find more outdated equipment among treated firms. The results for practice firms are somewhat mixed. In line with the positive effects obtained from the administrative outcomes, we find positive effects on hiring in 2004. However, we also see a significantly higher share of firms not investing in 2004. For all long training programs taken together we confirm the more negative picture in the responding subsample with a higher share of East German firms. We find negative effects on self-assessed profitability and hiring in 2004. We also see a lower share of firms making use of leased workers who are usually the first to get laid off, and fewer vacancies for unskilled workers.

For all training programs together, we find an increased hiring of temporary workers. When comparing high exposure to long training to high exposure to short training, we find negative effects on hiring, in particular of unskilled workers but also of higher-skilled clerks.

However, there is also some evidence for better technical equipment in firms that hire in regions with intensive use of long training.

In summary, the results for the survey outcomes are largely in line with those from the administrative data. Perhaps with the exception of practice firms and degree courses, we obtain a rather pessimistic assessment of the potential benefits firms may have from a large supply of potential applicants that participated in different types of ALMPs.

5.3 Sensitivity analysis

In the sensitivity analysis we address several potential problems of the empirical design used: First, all estimates for the outcome variables coming from the EP surveys are based on smaller samples than for those outcome variables measured in the administrative data (EHP). Therefore, we estimate the effect of the different treatments on responding in the respective survey. The results are presented in Table I.8 in Internet Appendix I.6.1. The effects are small and none of them is statistically significant.

Second, for the reasons detailed in Section 4.4, we do not condition on pre-treatment outcomes other than firm size. Therefore, we estimate the effects of the different policies (measured 2001-2003) on the EHP outcomes in 2000 to get an idea whether this might cause any problem. The results are presented in the lower part of Table I.8. Again, the effects are small and are not statistically significant. The exception is one single coefficient which is significant at the 10% level (long versus short training on 25% quantile of tenure). Thus, our matching procedure balances all pre-treatment outcomes well implying that this issue does not seem to be a concern either. In Tables I.9, I.10 and I.11 in Internet Appendix I.6.2, we furthermore show that our results are not sensitive to conditioning on firm size. Although there are some differences in the significance levels, magnitudes are similar and the conclusions are robust.

Although the pre-treatment outcomes are well-balanced, one may be worried that treated and untreated firms are facing different growth trends and that this is driving our results. We address this issue by including pre-treatment firm growth 1999-2000 as control variable in Table I.12 in Internet Appendix I.6.3. In many places precision declines but the negative results for subsidized employment and long FVT, as well as indications of positive effects on tenure for practice firms and training in general, are confirmed.

Next, we assess an issue that is related to the credibility of our identification strategy. Identification requires that the ALMPs in a firm's hiring region are determined to a large extent by factors outside this region. Therefore, another sensitivity check is based on smaller samples in which firms are removed that have hiring regions that may have a larger impact on the local ALMPs. Specifically, we exclude firms that only overlap with one LEA, or whose hiring region has a weight of 50% or more in the overlapping LEA regions, or whose weight in its hiring region exceeds 1%. This reduces the sample by 702 firms (24%). The results for the EHP outcomes are presented in Table I.13 of Internet Appendix I.6.4. Although precision declines due to smaller sample sizes, our main findings are generally confirmed, or least not contradicted: Extensive use of subsidized employment and long FVT has negative effects on firm performance in the long-run, while for degree courses and practice firms there is evidence for positive effects on tenure. However, in two cases we find somewhat different results: For TM there is also some (noisy) evidence for positive effects on tenure, but the effects for all training programs taken together vanish. Note however that the power of the last sensitivity check is limited by the considerable estimation noise in the smaller sample.

The final two sensitivity checks are concerned with the measurement of firms' hiring regions. The first one addresses potential relocation of workers before starting a new job. In the period we use to construct the hiring regions (1999-2008), 10.3% of unemployment-to-job transitions and 4.3% of job-to-job transitions involved relocation to a different community

(which may just be a neighbouring community to the previous one, though). Although these figures are quite low, we investigate whether it matters that we use the community where workers live once they have started a job for construction of the hiring regions. In Table I.14 in Internet Appendix I.6.5, we present the results when we instead use the community where people lived before starting the current job. The negative effects of long versus short training on tenure become more pronounced and in some cases precision declines, but qualitatively the results are unchanged. Finally, Tables I.15 and I.16 in Internet Appendix I.6.6 show that using the outcome period 2004-2008 when constructing the community weights also does not drive our results. Both excluding this period and just using it does not change any conclusion.

6 Conclusions

We investigate whether firms benefit from ALMPs by exploiting unique linked employer-employee data together with institutional features of the implementation of ALMPs in Germany that induce exogenous variation in the level and mix of ALMPs firms' are faced with. Our results mainly support the pessimistic assessment of the cost-effectiveness of ALMPs from the empirical literature on the effects for individual participants of these programs. We do not find any effects of job search assistance and short training programs in general. Moreover, extensive supply of subsidized employment, in particular in the form of wage subsidies, or long further vocational training programs, in a firm's hiring region has negative effects on firms. These are important findings because the absence of positive effects on firm growth and survival also speaks against positive effects on the macro level which may justify the large expenditures on ALMPs but are hard to estimate empirically.

Our results are somewhat less pessimistic for two specific types of training: intensive on-the-job training in practice firms and training that leads to a formal vocational degree. We

find weak evidence that extensive use of these programs improves match quality and reduces turnover in the exposed firms.

For future research it would be interesting to analyse the channels through which the negative effects we find come about. Understanding the channels is important from a policy perspective because they may call for very different strategies to counteract adverse effects on firms. For example, the negative effects for subsidized employment may be due to bad matching or the result of firms using unsubsidized workers suffering from a less competitive cost structure. However, identifying and estimating channel-specific effects is a challenging task and requires better data than we currently have at our disposal.

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Appendix

Table A.1: Interactions between treatments

Treatment	d	SE	TM	Short FVT	Long FVT	DC	PF
(1) SE*	0	.03	.54	.07	.07	.02	.03
	1	.06	.71	.08	.07	.02	.04
(2) TM	0	.06	.36	.06	.09	.02	.03
	1	.08	.91	.08	.09	.02	.05
(3) Short FVT	0	.08	.53	.03	.09	.02	.04
	1	.06	.71	.11	.08	.02	.04
(4) Long FVT*	0	.04	.60	.08	.04	.02	.04
	1	.04	.51	.07	.11	.02	.03
(5) DC	0	.09	.58	.07	.09	.01	.04
	1	.06	.68	.08	.08	.03	.04
(6) PF	0	.07	.51	.07	.10	.02	.01
	1	.08	.72	.07	.08	.02	.07
(7) Short TR	0	.06	.36	.05	.09	.02	.02
	1	.08	.90	.09	.09	.02	.05
(8) Long TR*	0	.04	.59	.08	.05	.02	.03
	1	.04	.54	.07	.10	.03	.03
(9) TR	0	.06	.37	.05	.08	.02	.02
	1	.08	.90	.09	.10	.02	.05
(10) Long/Short*	0	.04	.76	.09	.05	.02	.04
	1	.04	.41	.06	.09	.03	.02

Note: Shaded cells indicate the program shares that have been used to define the treatment dummies. *Treatments 1, 4, 8 and 10 only include West German firms. This is the reason for the lower shares of SE in these treatments. SE subsidized employment, TM training measures, FVT further vocational training, DC degree courses, PF practice firms, TR training.