Do Firms Benefit from Active Labour Market Policies?

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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 to determine 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 by subsidized employment and long further vocational training programs. These findings complement the negative assessment of the cost-effectiveness of ALMPs from the empirical literature on the effects for participants.

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

JEL classification: J68.

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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? This question has so far been neglected by the vast literature on the effects of ALMPs. This literature almost entirely focuses on the question whether the unemployed benefit from participating in the various 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 the literature on the individual effects of ALMPs quite consistently concludes that most types of programs do not increase the employment chances of their participants sufficiently to pass a cost-benefit test.[1] Hence, the natural question is whether we can expect any effects on firms in the light of this evidence.

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 ALMPs on job match quality. The reason is a methodological problem: match quality can only be measured conditional on having 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. One example are so-called pre-program or threat effects: Unemployed workers who expect negative utility from a program

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

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, 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 required to measure in which regions individual firms hire, how these regions differ with respect to the particular components of ALMPs used, as well as firm performance. 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 that include exceptionally detailed regional and program information. This data base allows us to measure hiring by firms as well as the level and mix of ALMPs on the community level. To solve the endogeneity problem between the respective policy and measures for firms' economic success, we exploit 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 that 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 analyze the effects of ALMPs on the unemployed (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

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

UK, and Frölich and Lechner, 2010, who exploit an exogenous regional variation in the participation probabilities of ALMP in Switzerland), our intended contribution is probably 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.

Our results mainly support the pessimistic assessment of the cost-effectiveness of ALMPs from the empirical literature on the effects for participants. We find that in general firms do not benefit from local ALMPs and in some cases may even be harmed. In particular, extensive use of subsidized employment or long further vocational training programs in a firm's hiring region has 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.

The remainder of the paper is organized as follows: In the next section we discuss the potential links between different exposure 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 that can be downloaded from www.sew.unisg.ch.

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. Before describing the specific programs we look at and our empirical strategy in detail, we

review some theoretical arguments for a potential link between the size of 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. 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 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 potential deficits in productivity the particular group of workers eligible for the subsidy may have are 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 positively 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. 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 for which Table 3.1 displays expenditures and the number of participants (entries) for the period 2000-2004.

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.

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 measures to more inexpensive ones. With more than one million participants in 2004 so-called training measures (TM) have become by far the most important measure in terms of the head count. Usually TM's combine basic job search assistance by learning how to locate relevant vacancies, how to write a good application and how to behave in a job interview with weak forms of monitoring like availability checks. But there are also programs that provide some job-rele-

vant skills, like training for using computer software. Durations are short with 1-2 months on average and are limited to 3 months at maximum. This is why expenditures are low and make up only 4% of the total budget despite the large number of participants. 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 foundation of small businesses by unemployed workers, mainly by providing income support to bridge the time until the business becomes profitable. Due to lack of data we do not consider this measure in our analysis. It appears also to be of little relevance to firms with respect to their hiring opportunities.

Further vocational training (FVT) is the most important measure 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. The aim is to remove or reduce the mismatch between the skills of unemployed workers and what is demanded by the market. With durations of up to two years, the programs can be very costly. There is however substantial variation in both contents and durations. In the analysis we therefore distinguish four different types of FVT: For the first group of training programs which comprises the classical type of FVT combining class-room and varying amounts of on-the job training 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 analyze 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.[3]

The last major group of programs is subsidized employment which comprises two distinct subtypes. Using the first type, employers who hire unemployed workers for a regular job

[3] 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.

may receive time-limited wage subsidies to compensate for the training investments required due to the initial deficits in the productivity of these former unemployed workers. 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 which makes these programs the second most expensive. Over time the importance of this measure has declined substantially which is reflected in the number of participants, which has more than halved from 2000 to 2004.

3.2 Regional implementation of ALMPs

In order to identify the effects of the supply of participants in different types of ALMPs on firm outcomes we exploit that there is a variation in the use of ALMPs that is exogenous to firm performance. This variation is coming 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. It 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 in order to reach these goals. In December the budgets, policy objectives and general guidelines for the coming year are fixed for each local employment agency. At the beginning of the following year the local employment agencies 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, the single LEA 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 that is served by 6 different LEAs. People living in this region can easily commute to any place within this area. In Table 3.2, we report the share of unemployed workers participating in the four types of ALMPs and 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 Middle	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.

Table 3.2 shows substantial variation in the use of different ALMPs. Taking the 2003 figures, the share of unemployed participating in training measures for example is 1.5 times higher in Potsdam than in Berlin North. For wage subsidies, the share is 3 times higher in Potsdam than in the LEA with the lowest share, Berlin Middle. Berlin South strongly focuses on the use of employment programs, while Berlin South-West emphasizes further vocational training. Since all 6 LEAs are operating in the same labour market, this variation is induced by the different clientele each LEA serves and by different strategies (preferences) followed by each LEA. 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. In the

next section we describe in detail how we exploit this variation of regional ALMP for the identification of the effect 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 that are located in different communities and therefore 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' performance. Exogeneity is coming from three sources which are 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, e.g. the preferences of the LEA's directors, or originate from different regional clustering 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 linked employer-employee dataset that combines information from several sources. It covers all establishments[4] in non-seasonal sectors that have at least 100 employees on June 30, 2000, and that participated in the IAB Establishment Panel in 2000. 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. Based on the social insurance records of the employees for each establishment there is information about the composition of its employees 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 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 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 additional datasets. Firstly, we merge to our data with detailed county-level information on population density, rurality/urbanity, migration, commuting, public transport, infrastructure, economic performance like GDP growth, and earnings from INKAR (2004). 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 very 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 make their decisions 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.

[4] We use the terms establishment and firms interchangeably in the following.

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.[5] 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. Since the EHP is based on administrative records, the reliability of the information is high and there are no attrition problems except when a firm closes, an event that be influenced by ALMPs and is thus an interesting outcome in itself. 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 allows the measurement 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 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 ALMP 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 in 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

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

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 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. 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.[6] 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.

Since it is known to which LEA each community belongs to, these 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

[6] 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 measurement error our approach might be subject to because of this should be negligible.

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 analyze 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 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 important 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.2). 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.2). With the exception of degree courses (DC) selectivity in terms of pre-treatment outcomes (firm size, turnover) and earnings is also 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 percentage 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 that those with low exposure. For the other treatments, 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.2).

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. SE subsidized employment, 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 seem 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.2 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 as well as the firm's performance, and exploit that there is 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, or by overall differences in LEA strategies. As mentioned before, 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.[7] (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.[8]

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, 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 in each community that is available to the LEAs when

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

[8] 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.

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.2 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 treatment 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 5.2 we discuss the role of pre-treatment outcomes for selection correction in more detail.

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

Number of LEAs*	Number of firms	Percent	Cumulative percent	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 Table 4.3 we address the issue of 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. 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 and 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 firms. For the small share of firms for whom the hiring region has a relative large weight we provide a sensitivity check in Section 5.2 where we exclude these firms. Column 7 of Table 4.3 finally shows that all firms have a negligible impact on local ALMPs, even inside the hiring regions. More than 90% of all firms employ less than 1% of workers in their hiring region.

In summary, Table 4.3 provides strong supporting evidence that both the firms and their hiring regions play no dominating role in determining the ALMPs of single LEAs. Table 4.3 also indicates 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 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.3 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 confirm the conclusions from the descriptive statistics in Section 4.3.

In a recent extensive Monte Carlo study Huber, Lechner, and Wunsch (2010) 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 IA.1. For inference we use the bootstrap by independently drawing firms and then bootstrapping the p-value of the t-statistic (1999 replications). Again, all details are relegated to Internet Appendix IA.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 depending on attrition. Moreover, as discussed in Section 4.3, the firms for which the effects are estimated differ across treatments. Consequently, 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.

5.1 Outcomes from administrative data

In this section we present the main set of 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 high or low exposure to different types of ALMPs on firm growth, form survival and turnover. For the sake of brevity, we focus on the short-run effects measured in 2004 and the longer-run effects of 2008. Results for all years from 2004 to 2008 are reported in Table I.5 in the Internet Appendix I.4. 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 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 programs are hurt in the long run. They shrink, employ a higher share of temporary workers and go out of business with higher probability. The results negate positive effects of these programs concerning the employability of local unemployed work force. This finding is in line with the literature on the (non-positive or even negative) effects of these programs for their participants (again see the meta study by Card, Kluve, and Weber, 2010; for Germany see Wunsch and Lechner, 2008, among others). These findings are more in line with theories that predict negative effects on the competitiveness of unsubsidized firms.[9]

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, there is neither evidence for open vacancies being filled faster, nor for improvements in the quality of the pool of potential applicants and match quality. The former again confirms the findings from the

[9] Note again that this is not the effect for firms using the program, but for firms in hiring regions with a disproportionally high occurrence of the subsidized employment program.

empirical literature on effects for participants cited above. 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) seem to be 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 which may be an indication that LEAs may misjudge which skills are demanded by the market.[10] 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 is seen by them mainly as a tax on their free time.

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 must 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. But there is 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. As the firms considered for the contrast look very similar to those for short TR, this seems 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).

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

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 smaller number of observations.

5.2 Outcomes from survey data

For, respectively, 47% and 44% of the original sample (who responded to the EP 2000 survey) we observe outcomes in the EP survey in 2004 and 2005. In Section 5.3 we show that survey non-response is unrelated to the treatments we defined after selection correction using matching. 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.2 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 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.4. 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 on self-assessed firm performance and firms' human resource policies as measured in the EP survey. 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. However, less treated than non-treated firms report shortage of young qualified workers and there are less 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.[11]

For the short training programs we confirm the more pessimistic assessment in the responding subsample with a higher share of East German firms. For TM and short TR we find negative effects on growth and positive effects on partial firm closure in 2004. 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, with a higher share of workers that earn at most 400 EUR per month (so-called mini jobs) and that are 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 for 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.4), we see a higher share of exposed firms employing temporary workers but a lower number of interns and helps working in these 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. Moreover, a smaller share of exposed firms seems to employ workers that earn at most 401-800 EUR per month (so-called midi jobs). 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.

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

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
Development since last year (EP 2004):										
Growth rate * 100	-2.5	-2.7	0.8	1.2	-0.9	-0.1	*-2.5*	1.1	*-2.4*	*3.5*
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
Share of exits in first half of 2004	4.6	2.9	-0.4	-1.2	-1.8	1.6	2.1	-0.2	1.9	-0.8
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
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
Composition of workforce 2004 (EP 2004):										
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
401-800 EUR workers (y/n)	4.7	7.4	2.5	-0.6	-4.1	*-11.7*	6.7	3.2	8.5	-5.3
Share of workers earning less than 400 EUR	-0.4	-1.1	*1.0*	0.0	-2.6	-1.0	0.0	-1.2	0.2	1.0
Share of workers earning 401-800 EUR	0.4	0.0	0.5	0.0	0.1	-0.1	0.5	-0.3	*0.9*	-0.3
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
High turnover (y/n)	1.7	1.2	-1.9	1.4	-0.8	-1.2	1.9	-1.1	1.5	-1.8
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
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
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
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

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.

The evidence for all training programs together is mixed. High exposure induces negative effects on growth and a higher share of exits in 2004. However, we also find positive effects on hiring of both temporary and high-skilled workers as well as on employment of workers in midi jobs. We also obtain mixed results for the contrast long versus short training. There seem to be some positive short-run effects on growth and the state of the firms' technical equipment. But we also see negative effects on hiring.

In summary, the results for the survey outcomes are largely in line with those from the administrative outcomes. 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.5. 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 selection 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.

Third, since the main results are based on conditioning on firm size, Tables I.9, I.10 and I.11 in Internet Appendix I.5 show the results without conditioning on firm size. Although there are some differences in the significance levels, magnitudes are fairly similar and the conclusions are robust.

Finally, we assess another issue 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.12 of Internet Appendix I.5. 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 we no longer see such effects for all training programs taken together. It needs to be emphasized however that the power of the last sensitivity check is clearly limited by the considerable estimation noise in the smaller sample.

6 Conclusions

Exploiting unique linked employer-employee data and institutional features of the implementation of ALMPs in Germany that induce exogenous variation in the level and mix of ALMPs in firms' hiring regions we investigate whether firms benefit from ALMPs. 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 use of subsidized employment 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 for positive effects of an extensive use of these programs on match quality and turnover measured in terms of tenure in the exposed firms.

For future research it would be interesting to analyze 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 adverse effects on participants 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.