Do low-skilled workers benefit from further training subsidies?

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September 10, 2015

– very preliminary, please do not cite –

Abstract

I analyse the effects of further training subsidies for low-skilled employed workers on individual labor market outcomes in Germany for the period 2007 to 2012. Using detailed administrative data, I exploit cross-regional variation in conditional policy styles of local employment agencies, and use this fuzzy discontinuity as an instrument for program participation. I find that the subsidies caused significant gains in cumulative employment duration and earnings in the short run for the subgroup of compliers. These gains are particularly pronounced for women, younger workers and workers participating more than six months.

JEL-Classification: J18, J24, J31, I21

Keywords: Further training for employees, low-skilled workers, instrumental variables

Acknowledgements: I am grateful for generous advice from Gesine Stephan. I also thank Øivind Anti Nilsen, Núria Rodríguez-Planas, Yolanda Rebollo Sanz, Wolfgang Dauth, Bastian Stockinger, Daniel Hamermesh and members of the FBC1 unit at IAB for helpful remarks. Specific thanks goes to the ITM unit at IAB for providing the data. I also thank participants of the 2015 BGPE Research Workshop in Regensburg, the 2015 IWQW PhD Research Semainar at the University of Erlangen-Nuremberg, and the 2015 Spring Meeting of Young Economists in Ghent for helpful comments.

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1 Introduction

In the last decades, one of the most remarkable changes in Western societies caused by globalization were structural changes in employment (Goos et al. 2009, Goos & Manning 2007, Autor et al. 2006). The international integration of product and labor markets affected the composition of industries and increased the demand for workers with occupations of greater complexity. Spitz-Oener (2006, p. 263) finds that "occupations have experienced a shift towards analytical and interactive activities and away from cognitive and manual routine tasks" in the past decades. This implies that the structural change in employment occurs particularly to the detriment of the low-skilled and to the advantage of highly skilled workers, as tasks of lower complexity are increasingly relocated to countries in the East. This enduring situation of economic change requires a flexible and suitably skilled workforce, which necessitates investments in education and training.

This study provides findings on the poorly understood impact of public subsidies for the further training of *employed* low-skilled workers. I focus on unravelling the causal effects of government subsidies on cumulated earnings and cumulated employment outcomes exploiting substantial exogenous variation in conditional policy styles at the regional level of unemployment agencies, which provide the subsidy in Germany. I thereby contribute to a literature that has, so far, seen little quantitative analysis. The bulk of literature is concerned with subsidized training programs for unemployed workers (for an overview see Card et al. 2010, 2015) and mostly concludes that training for the unemployed yields positive gains in the long run.

Concerning the returns of training subsidies for employed workers, the literature is much scarcer. Existing studies focus on heterogeneous programs using differing methods and data and therefore also reporting mixed results. Current studies by Abramovsky et al. (2011) and Görlitz (2010) look at firm-level outcomes of government subsidies for the further training of low-skilled employed workers. While Abramovsky et al. (2011) do not find any effect of UK subsidies for low-skilled training on firm and training take-up rates, Görlitz (2010) finds positive effects (+10 to 15 percent) of German firm vouchers covering a share of direct

training costs on the share of firms investing in training. The same holds for the impact on individual-level outcomes. While Hidalgo et al. (2014) and Görlitz & Tamm (2015) fail to find any significant impacts of training vouchers for low-skilled (low-income) workers on wages, Stenberg (2011) shows that an additional year of adult education in Sweden increases annual earnings by 4.4 percent. However, these programmes differ from the subsidy scheme evaluated here because they are not directly linked to on-the-job training and unlikely to contain any firm-specific components. The paper by Singer & Toomet (2013), which analyzes the impact of subsidized training on the employment duration of workers older than 45 years in small- and medium-sized firms, is closest to this study. They find improved employment for participants of the subsidy scheme, which they attribute to an increase in non-pecuniary returns rather than to the accumulation of new human capital.

The contribution of this study is to use high-quality register data to evaluate the effects of a government subsidy scheme that provides further training for low-skilled employed workers in Germany. Exploiting exogenous regional variation of treatment probabilities following studies by Frölich & Lechner (2010) or Doerr et al. (2014), I can tackle problems of selectivity, that is, workers selecting into the scheme based on unobservable characteristics, and identify a causal effect. As I am particularly interested in heterogenous treatment effects, I lay special focus on the treatment effects of workers in subgroups, thereby taking into account that there is evidence showing disparate effects of further training by age, gender, or intensity of the program (Grund & Martin 2012).

The variation in conditional policy styles of local employment agencies affects the implementation of training subsidies for low-skilled employed workers. Policy styles vary between agencies due to differences in the organizational structure, problem-solving mechanisms, and concepts (for a detailed discussion see Doerr & Kruppe 2014). While this affects the propensity to be treated, that is, to participate in the subsidy scheme, policy styles are exogenous to employment durations and wages of low-skilled employed workers. Employed workers have in general no contact whatsoever with employment agencies. Therefore, assuming that the labor market outcomes of workers are influenced by the region-corrected policy styles other than

via the underlying subsidy scheme–which is the only federal subsidy directed at employed workers–is very credible.

The remainder of this paper is organized as follows. Section 2 outlines the institutional background. Section 3 describes the empirical approach and section 4 the data. Section 5 is devoted to the analysis of treatment effects for compliers on employment and earnings. Section 6 concludes.

2 The German labor market and the training subsidy scheme

Germany has experienced a very striking case of structural changes in the workforce. Following the example of other OECD countries which fund training supply with loan and subsidy schemes or tax deductions (Bassanini et al. 2005), the German government decided on the introduction of further training subsidies for employed workers in 2007 in addition to existing programs for unemployed workers. The intention of these subsidies is to meet potential reservations from the low-skilled workers' side, who show disproportionately low interest in further training, and to raise employers' (in particular of small and middle-sized firms) propensity to further train their low-skilled personnel.

To this day, this subsidy scheme is the only program by the German Federal Employment Agency (FEA) that provides training subsidies for employed workers, while this has been a common tool for unemployed workers. Another attempt to boost further training was made by a federal training voucher program implemented by local educational centers (see for example Görlitz & Tamm 2015, Görlitz 2010). This program differs from the one of interest here as it deals with off-the-job training rather than on-the-job training. The underlying subsidy scheme is handled by the FEA, whose major responsibilities are the integration of unemployed workers and the distribution of unemployment benefits. The FEA operated 176 local employment agencies, which own a certain competency over the applied ALMP mix, all over Germany during the period of subsidy entrants (2007-2010) of this study.

In the relevant period for this study, workers entered the program via the employer. Firms received information about possibilities of subsidizing further training from caseworkers who

promote the scheme. Then potential participants meet with the responsible caseworkers at the local employment office who advise them individually. In agreement with the firm, workers can either pick a training course offered by a private provider, for example via training voucher, or occupational re-training. Courses are typically conducted by private providers but must be certified by the local employment agency.

Potential participants are considered low-skilled if they lack a vocational qualification or pursued a helper job which did not require any qualification at least for the previous four years.[1] Further criteria require that the employer must release the employee from work but continues wage payments irrespective of the worker's absence from work during training participation. The training course must predominantly focus on general rather than firm-specific learning contents, as the objective is to improve knowledge that is, applicable on the general labor market. A subsidized training course is supposed to terminate with the receipt of an acknowledged degree.

Once these criteria are met, the worker-employer duo can qualify for two different types of subsidy: first, training costs may be covered up to 100 percent by the FEA. Second, the employer may in addition receive a wage subsidy. The wage subsidy covers up to 100 percent of the full wage if training measures take place outside the firm. If training takes place inside the firm, the employer is expected to take a share in the costs. Thus, in this case the FEA covers only up to 50 percent of the wage subsidy because firm-specific elements of training are more likely.

Table 1: Inflows to the subsidy scheme and per capita costs for low-skilled workers

	Cost reimbursement (§81 (2) SGB III)		**Wage subsidy** (§81 (5) SGB III)	
Year	Inflows	Costs in EUR	Inflows	Costs in EUR
2007	10,458	1,425.88	14,527	1,681.51
2008	23,007	2,279.19	28,571	2,977.99
2009	38,426	2,647.63	36,579	4,851.24
2010	17,374	3,961.28	14,809	6,832.72

Source: Statistics of the Federal Employment Agency.

[1] Another target group are workers in small and middle-sized firms (Singer & Toomet 2013).

Table 1 shows the inflows into the scheme by year and kind of subsidy. Compared to other programs of active labor market policy, the number of subsidy recipients is rather low. Because the existence of such subsidies was rather unknown in its introductory phase, there were only few entries. Due to increasing advertising efforts by employment agencies, the introduction of external further training counselors, and opening the program for workers just leaving unemployment, numbers increased. At the tip of the economic crisis in Germany in 2009 numbers peaked. The program costs displayed in Table 1 reflect this development. At about the same time, in April 2009, for the first time official rules of procedure were published by the FEA, that is, a written document containing the standards by which the program is to be implemented. This document caused a shift towards longer-lasting training episodes. Moreover, the award of the subsidies became more and more restrictive as local employment agencies were instructed to follow the eligibility criteria more strictly and to accurately document the allocation of subsidies. This process was emphasized after an inspection of the program's monetary allocation by the German Federal Court of Auditors (Bundesrechnungshof 2009). This again yielded an abrupt reduction of inflows in 2010 while the per capita spending increased substantially, in particular spending for wage subsidies. This might be primarily attributable to the increase in longer-lasting subsidized training periods.

3 Theoretical considerations

Reasons for a legitimate intervention of official institutions into further training activities can be derived from the literature (for a summary see Booth & Bryan 2005). According to conventional human capital theory, investments in general human capital should be fully covered by the workers themselves (Becker 1964). If these costs are directly subtracted from earnings, workers experience an upward-sloping wage profile over time because they receive wages below productivity during training participation and wages equivalent to their higher productivity afterwards. If such a wage cut is not possible, for example due to minimum wage regulations, workers will underinvest in general training (Leuven 2005). In a situation of liquidity constraints, the employer can step in as a lender and offer the worker wages above

productivity during training participation and below productivity afterwards. However, this scenario is likely to be applied only in cases when workers can be bound to the firm until the loan is repaid. Thus, if such a contract cannot be enforced, this yields an underinvestment in training. In the case of firm-specific further training, when neither firms nor workers have an incentive to invest in training because long-term contracting cannot be enforced (hold-up), the labor market equilibrium of further training is inefficient, unless firms and workers agree to share the costs and benefits (Garibaldi 2006).

In the new training literature, labor markets are no longer assumed to be perfectly competitive due to an oligopolistic market structure. In the light of the associated compressed wage distribution, firms invest in general human capital given that productivity after training increases at a faster rate than wages (Acemoglu & Pischke 1999*a*,*b*). The model implies that wages will be below the workers' marginal productivity. From a society's point of view, this yields an underprovision of training. Thus, there is multiple evidence that privately provided investments in further training might be insufficient.

A large and growing body of literature has investigated the determinants of (privately funded) further training (for an overview see Brunello et al. 2007). The decision of training participation is made by both employer and employee by weighing the costs and benefits of such training. For the employer, benefits are, for example, increased productivity, the enhancement of the workers' organizational commitment, and the saving of screening new employees, while costs may contain direct costs or continuing wage payments for workers in training. For the employees, benefits can be expressed in higher wages after training or a higher chance to be promoted, while costs might arise due to the costs of reduced wage payments during training or additional working time. As a result, trained workers are usually younger, male, better-educated fulltime-employed workers, and training firms are rather large, while low-skilled workers participate less often in further training (Grund & Martin 2012, Bassanini et al. 2005, Albert et al. 2010, Fouarge et al. 2010, Leuven & Oosterbeek 1999). Public interventions, such as further training subsidies, might therefore alter the decisions for training on the employer's and the employee's side by lowering the marginal costs of further training.

Compared to the literature on the returns of an additional year of schooling, the returns of further training seem comparably large (for overviews see Brunello et al. 2007, Haelermans & Borghans 2012). In a meta-study Haelermans & Borghans (2012) estimate an average return of a training course of about three percent. With respect to earnings, one can expect a constant or increasing profile of subsidized further training over time. This is due to the fact that the employer has no need to decrease wages during subsidized training caused by a lower productivity. Over time, productivity might even increase yielding higher wages. With regard to employment durations, there are two opposing forces: on the one hand, employment duration might increase due to a higher productivity and a higher commitment to the firm. On the other hand, employment duration might decrease, because subsidized training costs are lower and require a shorter period to amortize.

4 Empirical approach

In this study, I consider participants entering the subsidy scheme between January 2007 and December 2010 and follow them over a period of up to five years. Exploiting regional variation in the conditional award intensity of the training subsidy as an exogenous instrument, I apply a fuzzy regression discontinuity design (RDD) and estimate the effect of compliers' program participation on cumulated earnings and cumulated employment duration (see also Angrist & Pischke 2009). To identify the causal effect of the subsidy on the outcome, the preferred model to be estimated is

$$Y_{itq} = \alpha_0 + \alpha_1 T_{itq} + \alpha_2 X_{itq} + \alpha_3 F_{ft} + \alpha_4 A_{at} + \eta_{itq} \tag{1}$$

where T_{it} is a dummy that indicates subsidy recipience, X_{it} controls for individual characteristics, and F_{ft} for employer characteristics. A_{at} are labor market characteristics, that is, the composition of the population and the workforce, which allow me to correct for regional, structural, and economic differences. i indicates observations on the individual, f on the firm, and a on the agency district level. q indicate observations by quarter and t by year. There remains $\eta_i = M'_{it}\gamma + \nu_i$, unobservable variables such as M_{it}, which might indicate a worker's

ambitions which I cannot control for. The effect of M_{it} influences both the outcome Y_{it} as well as the treatment indicator T_{it}, such that $E(T_{it}\eta_{it}) \neq 0$. In order to be able to estimate the causal effect of T_{it} on Y_{it}, I thus need an instrument for subsidy scheme participation. To instrument treatment, I construct the variable

$$\text{A}award \quad intensity_{atq} = \frac{\sum_i SLS_{iatq}}{\sum_i LS_{iat}} \times 100 \tag{2}$$

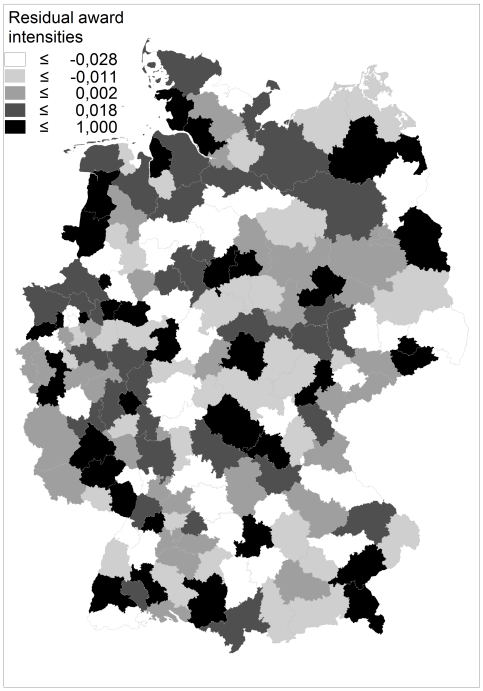
for every German local employment agency district a per quarter q in year t. SLS_{iatq} are low-skilled employees who start subsidized further training per quarter and agency district, LS_{iat} are all low-skilled employees per agency district and year. $Award \quad intensity_{atq}$ corresponds to the unconditional local award intensity by quarter and agency district. Differences in the award intensity of employment agencies are partially driven by regional conditions, which are likely to impact both an individual's probability to participate in the subsidy scheme, as well as the labor market outcomes of participants. Therefore, I only exploit remaining variation in the probability to be treated between different agency districts after purging the variation in the award intensity from regional, establishment-, and individual confounders. I refer to this residual variation with *conditional policy styles*. Figure 1 displays the corresponding variation of these average residual award intensities across German employment agency districts. Obviously, even after controlling for regional and other conditions, there are clear differences in the award intensities between local employment offices.

The implementation of the instrumental variable is accomplished per two stage least squares (2SLS) estimation. For the first stage in 2SLS, I regress the treatment indicator T_{itq}, which takes the value 1 if a worker participates in the subsidy program within a certain quarter, on the instrument $\text{A}ward \quad intensity_{atq}$ and all further control variables X_{itq}, F_{ft}, and A_{at}.

$$T_{itq} = \beta_0 + \beta_1 \text{A}ward \quad intensity_{atq} + \beta_2 X_{itq} + \beta_3 F_{ft} + \beta_4 A_{at} + \epsilon_{itq} = \hat{T_{itq}} + \epsilon_{itq} \tag{3}$$

β_1 captures the effect of conditional policy styles on the treatment probability, which corresponds to residual $Award \quad intensity_{atq}$ after controlling for potential confounders. Substi-

Figure 1: Average residual award intensities (policy styles) from 2007 to 2010 by employment agency district



Source: IEB V11.00 - 131009. Own calculations.

tuting equation 3 into equation 1 in a second step yields a regression of the outcome of choice Y_{itq} on the predicted treatment probability $\hat{T_{itq}}$ and the same set of control variables as in the first stage

$$Y_{itq} = \gamma_0 + \gamma_1 \hat{T_{itq}} + \gamma_2 X_{itq} + \gamma_3 F_{ft} + \gamma_4 A_{at} + (\eta_{itq} + \alpha_1 \epsilon_{itq}) \tag{4}$$

Thus, $\hat{T_{itq}}$ from equation 3 is initially by construction correlated with regional and other characteristics, but once put into the second stage equation 4, the additional controls purge $\hat{T_{itq}}$ from this correlation. Therefore, the resulting coefficient γ_1 reports the local average treatment effect (LATE) of participation for all compliers. One can distinguish between four different groups in the LATE framework: *always-takers* are participating in the program irrespective of the policy style. *Compliers* participate only in agency districts that conditionally are more prone to grant further training subsidies. *Never-takers* participate never,

whereas *defiers* participate only in districts less prone to grant subsidies.[2] Therefore, when interpreting the results, it should be kept in mind that the effect I estimate for compliers is different from the usually obtained average treatment effect on the treated (ATET), which is the common effect of always-takers and compliers.

Regional variation has been exploited in previous studies for the evaluation of ALMP instruments (Frölich & Lechner 2010, Doerr et al. 2014). In the underlying case, policy styles refer to the implementation of subsidized further training for employed low-skilled workers. The chosen strategy is close in spirit to the approach by Doerr et al. (2014) who exploit the residual variation of the local award intensities of training vouchers for unemployed workers to check the sensitivity of the vouchers' individual labor market effects. Agency-specific policy styles reflect the part of the implementation of specific instruments that is, solely due to unique features of the local employment agencies and which are independent from structural or economic specifics. Doerr & Kruppe (2014) elaborate in detail on the exogeneity of conditional policy styles. Combining unique survey data of caseworkers' and managers' assessment of a voucher program with register data, they find for Germany that in particular cooperative behavior and a high degree of communication within employment agencies and with firms determine policy styles. The literature claims moreover, that diversity in problem-solving mechanisms as well as in agency-specific paradigms generate this variation (Knodt 1998).

In particular after the first-time introduction of the subsidy scheme in 2007, it is possible that the degree of communication with employment agencies induced an irregular understanding and implementation of the program among caseworkers, thus affecting the promotion and the speed of the subsidy implementation. With respect to agency-specific paradigms, managers might differ in their preferences and sentiments towards subsidies directed at employed workers. Irrespective of structural differences of unemployment, some managers might con-

[2]The monotonicity assumption excludes the existence of defiers. Monotonicity implies that employed workers within that employment agency district react in the same way when the instrument takes a higher value. Thus, a higher conditional award intensity makes workers strictly more prone to participate, while it does not cause any worker to change the status from participant to non-participant. Another basic underlying assumption of my approach is the stable unit treatment value assumption (SUTVA). This assumption requires that individual decisions to participate in the program do not impact other individuals' labor market chances. Since the training subsidy scheme is rather small compared to other ALMP instruments, and the mean share of treated per participating firm in the sample is about 3.5 percent, this assumption very likely holds in the underlying case.

sider re-including the unemployed as more important than training already employed individuals. As the program became more known to caseworkers and the FEA re-regulated the content and the eligibility criteria over the years, policy styles potentially changed.

Conditional policy styles are positively correlated with the probability to participate in the program but not directly connected to employment or earnings of participants. Thus, the instrument purges the treatment effect of confounders that might simultaneously affect individual outcomes as well as the probability to be subsidized. However, the quality of the instrument hinges on several important conditions. First, to avoid the problem of weak instruments, the instrument should have sufficient explanatory power. Second, unobservable confounding factors on the agency, establishment, or worker level should not be strongly correlated with conditional policy styles. If this were the case, the instrument would not purge the program effect of these confounders, and the resulting coefficient would still be biased. Third, there should not be any effect of conditional policy styles on employment or earnings, as this would violate the exclusion restriction.

In order to address concerns regarding the independence of the instrument, the final regressions include a range of control variables that are discussed in detail in section 5.2. Regarding the validity of the exclusion restriction, one must remember that I exploit policy styles concerning a program that addresses employed instead of unemployed workers. Continuously employed individuals have no contact with employment agencies whose prior task is to integrate unemployed workers. Moreover, the underlying subsidy scheme is the only further training program by the FEA directed at employed workers. Therefore, any actions or policies of the FEA should not have any direct effects on employment durations or earnings of employed workers, except through the incidence and frequency of subsidized training.

5 Data and variables

5.1 Sample selection

The analyses are based on administrative records drawn from the Integrated Employment Biographies (IEB) V11.00.00. The IEB data are provided by the German Institute for Em-

ployment Research and contain the employment careers of all individuals liable to social security contributions (about 80 percent of the German workforce). They provide information on benefit receipt, job search, and participation in active labor market policies. These data are process-generated and highly reliable (see also Dorner et al. 2010).

The IEB data can be merged with data from three other sources. The Establishment History Panel (EHP) includes the universe of all German establishments employing at least one worker liable to social security contributions (Hethey-Maier & Seth 2009). As the data are of the same administrative origin as the IEB data, they have the same high reliability. From these records I draw information about firm age, size, and the composition of the workforce in terms of gender, age, and qualification. Moreover, I add regional data on the population (density, share of women, age structure) from the Federal Statistical Office and data on the composition of employees (age and skill structure, the employer's establishment size, industry structure) on the agency level from the Labor Placement Statistics.

For the analysis, I identify all participants picking up subsidized further training during the years 2007 to 2010 and the reason for program eligibility, either through wage subsidy or reimbursement of training costs as a low-skilled participant. Based on the IEB data, the respective numbers are displayed in Table 2 by the corresponding year. I only consider the first treatment spell. As nearly 60 percent of all cases received a combination of both measures, I adjust for parallel treatment spells by counting workers receiving both measures only once. The potential bias from subsequent treatment spells should be low, as only approximately 9 percent of all participants had a second treatment at some later point in time. I further straighten the sample by dropping all workers who received training subsidies in another target group of the scheme. From the resulting sample, I drop apprentices and parttime workers as well as workers outside the age range 20 to 65. Finally, I drop the upper and the lower 1 percent of the distribution of conditional policy styles to exclude outliers.

I divide the sample into quarterly strata and I calculate all control and outcome variables relative to the first day of each quarter. A quarterly treatment group consists of workers who start participation in the subsidy scheme within that quarter. The quarterly potential comparison group consists of workers who never participate in the subsidy scheme within our

observation period. I use random draws of workers who were at least one day employed in the quarter of counterfactual treatment between 2007 and 2010. Following the literature by Sianesi (2004), Fredriksson & Johansson (2008), and Crépon et al. (2009), statically defining treatment and comparison group and neglecting the dynamics of going into the program might bias the estimates due to conditioning on the outcome. This bias is particularly important, if a large fraction of workers from the comparison group participate to some extent themselves in the subsidy scheme sooner or later. As I am able to pick a large comparison group due to the rich register data, I am confident that this kind of bias is negligible in this study.

Table 2: Inflows to the subsidy scheme for the adjusted sample by legal basis

Year	Overall	Among those in %:	**Cost reimbursement**	**Wage subsidy**
2007	13,974		59.67	90.60
2008	21,174		78.03	89.75
2009	34,312		85.40	76.20
2010	13,155		91.30	67.41
Total	82,615		80.10	80.71

Source: IEB V11.00 - 131009. Own calculations.

5.2 Independent variables

In general, when evaluating an instrument of active labor market policies (ALMP), there is the problem of potential unobservable selection, that is, one cannot control for all confounders that impact individual participation. Assumably, there are at least three different sources of potential selection: regions, firms (establishments), and workers.[3] The allocation of the subsidy scheme is also very likely correlated with a number of these confounding variables.

5.2.1 Worker level

Table A.1 shows that there is likely selection on the level of workers. Men, slightly younger workers, and immigrants are over-represented in the treatment group. There are more than twice as many workers without a vocational degree (40 percent) in the treatment group and

[3]See also Grund & Martin (2012) for a discussion of further training determinants on the level of individuals, jobs, and firms.

workers in that group have also lower schooling. Moreover, the treatment group consists of about twice as many unskilled blue collar workers (58 percent). One year prior to treatment start, about twice as many workers (13 percent) in the treatment group were unskilled helpers compared to non-participants. Concerning occupational groups, subsidized workers are over-represented in manufacturing and transportation & logistics but under-represented in management & organisation. With respect to employment careers, treated people have less stable employment histories than those of the control group (shorter tenure and employment, more employment spells) as well as lower earnings (74 vs. 86 Euros). They have received more unemployment and welfare benefits in the prior three years and were also longer in uninsured job search unemployment.

Besides these observable factors, a varying distribution of unobservable factors might impact the local allocation of the subsidy. Unobserved worker characteristics such as motivation might drive the award intensity. Moreover, there is the possibility that firms encourage ambitious workers, who they want to keep anyway, to participate, which again drives the award intensity. By contrast, the subsidy might also be understood as a means of providing those workers with general training who firms plan to lay off due to economic difficulties. Following suggestions by Caliendo et al. (2014) for propensity score matching, personality traits such as motivation–which increase a worker's value for the firm–should be captured by including variables into the regression which reflect the employment careers and tenure.

5.2.2 Firm level

As a worker's participation in the program includes the involvement of the employer, it is important to account also for potential selection bias that she might cause. As mentioned above, the subsidy scheme was at first rather unknown to eligible firms and employees. Therefore, caseworkers in the employer service of local employment agencies promoted the program–in particular to firms in industries reporting skill shortages–making use of a data base containing address information on all establishments within an employment agency district.

Establishments which make use of the subsidy scheme might differ from other firms, in particular by firm size, the industry code, the workforce, and the economic situation.

For example, larger firms have HR departments which might regularly collect information about opportunities for subsidies. At the same time, it is easier for large firms to spare workers who are in a training course. Moreover, in certain industries there is a higher level of human capital depreciation, while in other sectors the marginal returns to training might be particularly large, thus driving selection. Moreover, firms in economic difficulties might increasingly demand public support, selecting also into the subsidy scheme. Accounting for the distribution of firms in terms of industry, size, worker composition, and the economic situation, which is here expressed in growth of the workforce, should be sufficient to purge the treatment probability and the policy style of unobservable factors. Table A.2 illustrates the distribution of the characteristics of participating and non-participating firms on the firm level. Unlike on the individual level (Table A.3), which shows that individuals participating are by tendency employed in larger establishments, there is no difference in terms of firm size on the firm level. Moreover, there is no difference between participating and non-participating firms in the economic situation. However, there are differences with respect to the industry. Firms prone to use the subsidy operate more likely in transportation and economic services.

5.2.3 Regional level

Another crucial gateway is the implementation on the level of employment agencies. Structural differences in unemployment rates and the local economic situation determine the size and the general strategy of a local employment agency. Thus, I consider the distribution of employed workers among agency districts and how the overall population between districts compares. Participants work in less densely populated regions, with a slightly older working population, and slightly lower unemployment rates. Moreover, I consider the distribution of firms and their workforce between employment agencies. However, there exist hardly any economic differences as Table A.4 shows.

After purging all of these factors, I am confident that the remaining differences in the allocation of the subsidy scheme depend on the local agencies' individual strategies and tastes and are unrelated to the economic situation of the region. These policy styles are exogenous

to individual labor market outcomes, but impact the treatment propensity, and can therefore be used as an instrument for program participation.

5.3 Outcome variables

Table 3 presents descriptive results on the durations of cumulated employment and unemployment between treated participants and workers without subsidies as well as cumulated earnings over a period of up to three years.[4]

On average, workers have very stable employment relationships, because they are employed for more than 90 percent of the observation period. Over a three year period, workers making use of the training subsidies were about six days more employed (difference < 1 percent). At the same time, participants receive on average benefits for 11 additional days within three years. Within the first three years after training start, earnings for subsidy recipients are significantly lower than for the comparison workers, a finding which must be related to worse-paying jobs. The difference is about 9 percent.

Table 3: Statistics of the outcome variables for treated (T) and potential comparisons (C)

	T	C	Diff.	p-Value	N (T)	N (C)	Min	Max
Cumulative Employment								
1st year	348.32	343.19	5.13	0.00	82,615	813,779	0.00	366
2nd year	675.20	667.27	7.93	0.00	82,615	813,779	0.00	731
3rd year	991.12	980.24	10.88	0.00	69,460	659,388	0.00	1,096
Cumulative Unemployment								
1st year	8.49	7.31	1.18	0.00	82,615	813,779	0.00	366
2nd year	24.96	18.15	6.81	0.00	82,615	813,779	0.00	731
3rd year	41.37	29.29	12.08	0.00	69,460	659,388	0.00	1,095
log Earnings								
1st year	4.22	4.32	-0.09	0.00	82,615	813,728	-5.29	5.75
2nd year	4.19	4.28	-0.09	0.00	82,615	813,743	-5.98	5.53
3rd year	4.17	4.26	-0.08	0.00	69,460	659,363	-6.39	5.43

Source: IEB V11.00 - 131009. Own calculations.

Notes T: treated, C: potential comparisons

[4]Treatment refers to treatment start, which is measured at the first of the quarter of potential treatment start. As I deal with employed instead of unemployed workers, this solves the problem of finding a counterfactual moment of treatment for non-subsidized workers. This is described in more detail in Section 5.1.

6 Results

6.1 The baseline model

This section discusses the results of the econometric analysis. The first column of Table 4 shows the most parsimonious specification controlling only for quarter dummies that account for the timing of treatment and regional characteristics.[5] Those include the district unemployment rate–in order to roughly control for regional labor market characteristics and regional labor supply and demand–the composition of all individuals living within a regional employment agency district in terms of density, gender and age, and variables which control for the composition of the workforce within an agency district. The first stage results reported in the bottom panel imply that the award intensity of employment agency districts is a strong instrument. Working in an employment agency district with a twice as high subsidy award intensity than another district increases the unconditional probability to be treated by about 56 percentage points. The F-test statistic of excluded instruments is also well above conventional threshold levels. Turning to the second stage results, I find that participation in the subsidy scheme has a positive significant impact of approximately four weeks on compliers' cumulated employment duration within a time period of two years.

Table 4: LATE for participation in the subsidy scheme on cumulated employment within two years

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) OLS
Treated	31.886***	30.456***	15.062***	21.389***
	(5.00)	(6.70)	(3.38)	(1.02)
Unemployment rate	Yes	Yes	Yes	Yes
Distribution of employees	Yes	Yes	Yes	Yes
Population characteristics	Yes	Yes	Yes	Yes
Firm characteristics & occupation	-	Yes	Yes	Yes
Socio-demographic characteristics & employment history	-	-	Yes	Yes
First-stage results, dependent variable: treatment dummy				
Conditional policy style	0.564***	0.525***	0.424***	
	(0.08)	(0.08)	(0.07)	
F-test of excl. instr.	54.158	48.618	34.491	
R-squared	0.080	0.141	0.234	

Continued on next page...

[5]For the purpose of a well-arranged presentation, I only indicate the existence of the variables in the regressions but do not display coefficients.

... table 4 continued

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) OLS
nobs	896,394	896,394	896,394	

Source: IEB V11.00 - 131009. Own calculations.

Notes: All regressions include a constant. The first stage regressions include the same set of control variables as the corresponding second stage in the upper panel. To control for the timing of program start, all regressions include interaction terms of quarter and year dummies. *Distribution of employees* is reported by age, skill, firm size, and industry. *Population char.* comprise the population density (also by gender/ age groups) on the level of local employment agency districts and the state. *Firm characteristics* comprise firm size and age, industry and workforce skill composition. *Socio-demographic char.* include gender, age, nationality, schooling degree, vocational degree, and job position. *Employment history* includes indicators for past employment, tenure, benefit and welfare periods, unemployed job search periods, average daily wage, and benefits. Standard errors (in parentheses) are clustered at the level of 176 local employment agency districts. Significance level: *** 1%, ** 5%, * 10%.

In column (2), I include firm characteristics because factors such as firm size, workforce composition, and industry are important determinants of average employment durations as well as of firms' further training investments. Thus, these variables potentially affect both the treatment probability and the outcome. As a consequence of including these characteristics, the positive effect decreases slightly. Finally, I add control variables for individual characteristics, in particular socio-demographic controls and variables that reflect both the previous employment career as well as unobservable personal traits as suggested by Caliendo et al. (2014) in a similar setup. The central variable of interest reduces significantly to two weeks ($\approx$ 2 percent).

The lower panel suggests that the first stage remains highly significant and stable throughout and decreases only when controlling for individual characteristics. This suggests that the instrument is not in the least correlated with firm characteristics. As it contains the most important control variables, model (3) will be the preferred benchmark specification in the following. For this model, I also report the estimate of a corresponding OLS regression in column (4). The comparison of the coefficients from (3) and (4) shows that the OLS regression generates a significant effect of larger size for participants over a two year period ($\approx$ 3 percent), thus probably overestimating the true effect.

6.2 Further labor market outcomes

The previous section has shown that participation in the subsidy scheme affects employment liable to social security contributions two years after treatment. Table 5 summarizes the results for the baseline specification. The effect on employment is steadily increasing over time indicating that additional employment is not solely due to more employment during the training period, which has a median duration of 73 days, thus unlike a pseudo lock-in effect. The effects on employment raise the question if this result also translates to unemployment. Therefore, I consider cumulated days of unemployment as a second outcome in column (2). I define unemployment as periods with the receipt of unemployment insurance benefits. Compared to employment, the effect on unemployment is less pronounced. There is a significant negative effect on unemployment benefit recipience in the first year, indicating that program participants claim about 3 days less benefits during the first year compared to non-participants. In economic terms, the effect is rather small ($\approx$ 1 percent) and vanishes later on. Thus, more employment for compliers does not automatically translate into less insured unemployment. Finally, in order to see how the economic situation of compliers translates into earnings, I consider cumulated earnings as a final outcome. Participants initially seem to profit from the program in financial terms. This effect seems to be both driven by additional employment and by slightly higher gross wages because the effect on employment (12 / 365 * 100 = 3.3 %) is lower than the one on earnings (6 %). However, two years after treatment, this gain reduces to approximately 3.5 percent higher earnings and becomes insignificant.

A direct comparison of OLS and IV estimates is only possible if treatment effects are constant across all participants and the average treatment effect on the treated is similar to the LATE. Therefore, a comparison of IV and OLS estimates which shows that OLS coefficients are in most cases higher–except for unemployment, where OLS is smaller–implies that OLS either cannot fully control for the positive selection of workers into the subsidy scheme or that compliers have less favorable characteristics and therefore profit less from the subsidy scheme.

Table 5: LATE and OLS estimates for participation in the subsidy scheme on cumulated employment, cumulated unemployment and log average earnings

	(1)		(2)		(3)	
	Employment		**Unemployment**		**log Earnings**	
	2SLS	OLS	2SLS	OLS	2SLS	OLS
1st year	11.699***	13.599***	-3.041***	-3.362***	0.061***	0.094***
	(1.33)	(0.40)	(0.94)	(0.28)	(0.02)	(0.00)
2nd year	15.118***	21.389***	1.441	-1.053	0.037	0.088***
	(3.38)	(1.02)	(2.48)	(0.70)	(0.03)	(0.00)
3rd year	22.812***	27.308***	2.740	1.418	0.036	0.086***
	(6.57)	(1.73)	(4.20)	(1.06)	(0.03)	(0.00)
First-stage results, dependent variable: treatment dummy						
1st and 2nd year						
Con. policy style	0.424***		0.424***		0.424***	
	(0.07)		(0.07)		(0.07)	
F-test of excl. instr.	35.768		35.768		35.768	
N	896,394	896,394	896,394	896,394	896,394	896,394
N(Participants)	82,615	82,615	82,615	82,615	82,615	82,615
3rd year						
Con. policy style	0.404***		0.404***		0.404***	
	(0.07)		(0.07)		(0.07)	
F-test of excl. instr.	38.259		38.259		38.323	
N	728,848	728,848	728,848	728,848	728,823	728,823
N(Participants)	69,460	69,460	69,460	69,460	69,460	69,460

Source: IEB V11.00 - 131009. Own calculations.

Notes: All outcome variables are calculated over 365 days (1st year), 730 days (2nd year), and 1095 days (3rd year) after the first of the quarter of the potential treatment start. Outcome *employment* comprises cumulated days in employment liable to social security contributions. *Unemployment* comprises cumulated days of being registered as unemployed and receiving unemployment benefits. *Earnings* is the average of daily cumulated wages. All regressions include the same set of control variables as in Table 4. The first stage regressions include the same set of control variables as the corresponding second stage regressions. Standard errors clustered at the level of 176 local employment agency districts in parentheses. Significance level: *** 1%, ** 5%, * 10%.

6.3 Results by selected subgroups

6.3.1 Results by cohort

During the observation period from 2007 to 2010, the rules of procedure regarding the implementation of the program had been adjusted several times. Therefore, a look at separate effects by yearly cohort might offer valuable clues whether changes in regulations affected the economic outcomes of participants. I focus on employment and earnings in the following. As the data end on 31 December 2012, I can observe individuals over differing periods of time: the longest for workers starting participation in 2007 (up to five years) and the shortest for participants in 2010 (two years). The first stage results show that the validity of the instrument holds over all split samples. The F-test statistics are above the conventional threshold of 10, and there is a strong positive correlation between the conditional policy styles and the propensity to receive treatment.

As described above, the 2SLS estimates report the effect for additionally recruited workers, that is, those workers who participated in the program only because they worked in an agency district that was more prone to provide the subsidy. This implies that the treatment effect on the treated, which is most commonly estimated, can differ if the group of compliers differs from other participants (always-takers).

Using IV, I find quite large effects on employment and earnings for participants in 2008 (see Table 6). After two years, compliers are approximately four weeks longer employed than non-participants and earn 16 percent more. This effect is substantially smaller for 2007 and the later cohorts, where the maximum gain in employment is approximately 18 days and gains in earnings vary between 6 and 8 percent. The difference in these effects can be attributed to compositional differences of participating workers, as Table A.5 shows. In particular in 2008, subsidized workers had the least favorable characteristics. They were less attached to the firm, which is reflected in about twice as much unemployment experience compared to participants in 2009 and 2010, and substantially less tenure and employment. Moreover, there was an exceptionally high share of participants from the occupational field of logistics and few workers from the manufacturing branch. Perhaps, the program was particularly effective in

2008 because at that time the FEA had opened the program for just re-employed workers who had left unemployment directly before entering the program. Thus, compliers in 2008 might have characteristics closer to those of unemployed workers than employed workers, because for those workers, training might not have occurred without the subsidy due to financial restrictions. By contrast, as Table A.5 shows, the latter cohorts consisted of workers with more favorable characteristics.

This shift in the composition of the participating workforce can be explained by changes in legislation: in the introductory phase of the program, caseworkers tried to attract as many workers as possible because the program was still unknown to workers and firms. Additionally, employment agencies had the chance to hire external training counsellors starting in 10/2007 whose task was to spread information about the program in firms and chambers. Moreover, it was still unclear to caseworkers what the exact implementation was to look like, for example which courses were fundable and which not. As a result of this, in 2007 and 2008 funded training courses were shorter than later on, making training for very low-skilled workers who might not have been trained without the subsidy and in long-lasting courses more likely. Firms might be hesitant to further train very low-skilled workers because after investing in costly training, they promise relatively low returns due to a low productivity. This changed in 2009 due to several reasons: in 4/2009 the FEA introduced for the first time a set of written rules of procedures that gave a summary of how to implement the subsidy. Moreover, the Federal Court of Auditors requested a profound documentation of the allocation of funds. These steps resulted in a trend for longer-lasting training courses with a certificate at the end. This caused a steep decline of program entries in 2010 due to more costly training for employers (indirect costs). The start of the economic crises in 9/2008 might also have contributed to differing effects.

6.3.2 Results by duration, gender, and age

In a next step, I look at the impact of training subsidies for different subgroups pooling data of all cohorts from 2007 to 2010. This yields a sufficiently large sample size of participants. I include interactions of quarter and year dummies to control for the timing of the (counter-

Table 6: LATE and OLS estimates for participation in the subsidy scheme by cohort of treatment start

	(1)		(2)		First stage results	
	Employment		**log Earnings**		F-test	Cond. policy
	2SLS	OLS	2SLS	OLS		styles
Results for the 2010 cohort						
1st year	6.612*	15.202***	0.086***	0.108***		
	(3.60)	(0.72)	(0.03)	(0.01)		
2nd year	7.279	28.667***	0.066**	0.110***	144.139	1.221***
	(7.23)	(1.85)	(0.03)	(0.01)		(0.10)
N (total)	167,546		167,546			
N (participants)	13,155		13,155			
Results for the 2009 cohort						
1st year	10.287***	13.494***	0.081***	0.086***		
	(1.61)	(0.58)	(0.01)	(0.00)		
2nd year	17.548***	20.766***	0.076***	0.080***	29.505	0.391***
	(3.70)	(1.49)	(0.01)	(0.01)		(0.07)
3rd year	22.895***	27.712***	0.071***	0.081***		
	(7.24)	(2.21)	(0.02)	(0.01)		
N (total)	343,250		343,250			
N (participants)	34,312		34,312			
Results for the 2008 cohort						
1st year	13.830***	13.654***	0.143***	0.102***		
	(3.59)	(0.89)	(0.03)	(0.01)		
2nd year	30.779***	21.641***	0.160***	0.100***	1120.199	0.994***
	(7.02)	(2.07)	(0.03)	(0.01)		(0.03)
3rd year	44.431***	29.291***	0.163***	0.100***		
	(11.45)	(3.11)	(0.03)	(0.01)		
4th year	46.242***	38.833***	0.147***	0.103***		
	(16.63)	(4.18)	(0.03)	(0.01)		
N (total)	252,689		252,689			
N (participants)	21,174		21,174			
Results for the 2007 cohort						
1st year	13.772***	14.012***	0.014	0.103***		
	(3.63)	(1.11)	(0.04)	(0.01)		
2nd year	12.531**	22.209***	-0.028	0.095***	21.324	0.416***
	(6.38)	(2.44)	(0.04)	(0.01)		(0.09)
3rd year	15.190	29.388***	-0.044	0.093***		
	(9.75)	(3.98)	(0.04)	(0.01)		
4th year	21.258	38.708***	-0.056	0.094***		
	(13.79)	(5.38)	(0.04)	(0.01)		
5th year	30.016	49.739***	-0.059	0.097***		
	(18.79)	(6.88)	(0.04)	(0.01)		
N (total)	132,909		132,909			
N (participants)	13,974		13,974			

Source: IEB V11.00 - 131009. Own calculations.

Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first stage regressions include the same set of control variables as the corresponding second stage regressions. Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Significance level: *** 1%, ** 5%, * 10%.

factual) treatment start. I consider the same outcome variables as in the previous section. Again, first stage and F-test statistics confirm a good quality of the instrument.

As the data do not contain any information about the exact type of subsidized training, I have to rely on other information that reveal the intensity of subsidized training courses. Therefore, I distinguish the sample of participants by subsidy period. Table 7 shows that participants from the last Panel (longest subsidy periods) succeed significantly better than other participants in terms of employment (+ 43 days) and earnings (+ 14 percent). This might be related to the fact that this type of training is aligned with the largest gains in productivity, resulting in wage gains, making the worker more valuable to the current firm, and increasing the hiring changes at other firms. Looking at the effects on employment and earnings by gender (Table 8), I find significantly larger returns to training on employment for fulltime working women (+52 days) than for fulltime working men (+18 days). This is even more pronounced in terms of earnings with participating women earning about 20 percent more than non-participating women. This is likely attributable to women and men operating in different sectors of the economy. Quite often women receive longer-lasting and more profound training in the elderly care sector with the objective of a recognized degree. Moreover, the larger effect for women might be due a lower absolute number of female training participants. Given that marginal returns are decreasing with the number of trained workers, fewer female participants imply higher marginal returns to training for women than for men from a firm's perspective. 2SLS estimates exceeding OLS estimates suggest that complying women profit more from the program than the average participant, perhaps because they would not have been trained without the subsidy.

Finally, separating the program effect by age group (Table 9) shows that the youngest participants aged 20 to 30 years profit the most and that the treatment effect declines with age. Thus, it seems as if the marginal returns to training for compliers are particularly high at the beginning of the employment career due to a lower initial level of knowledge, lower opportunity costs, and better cognitive skills.

Table 7: LATE and OLS estimates for participation in the subsidy scheme by duration

	(1)		(2)		First stage results	
	Employment		**log Earnings**		F-test	Award
	2SLS	OLS	2SLS	OLS		intensity
Results for workers subsidized for 1 to 19 days (1st quartile)						
1st year	8.409***	11.123***	0.074***	0.086***	119.065	0.333***
	(2.26)	(0.72)	(0.02)	(0.01)		(0.03)
2nd year	15.938**	19.386***	0.069***	0.083***		
	(7.28)	(1.82)	(0.02)	(0.01)		
3rd year	26.486**	27.729***	0.080***	0.084***	113.725	0.325***
	(13.47)	(3.25)	(0.03)	(0.01)		(0.03)
N (total)	834,871		834,820			
N (participants)	21,092		21,092			
Results for workers subsidized for 20 to 73 days (2nd quartile)						
1st year	15.507***	10.493***	0.063***	0.071***	32.951	0.302***
	(2.48)	(0.80)	(0.02)	(0.01)		(0.05)
2nd year	20.228***	15.965***	0.025	0.067***		
	(4.89)	(1.83)	(0.02)	(0.01)		
3rd year	32.329***	21.123***	0.029	0.064***	29.155	0.295***
	(7.05)	(3.13)	(0.03)	(0.01)		(0.05)
N (total)	834,076		834,025			
N (participants)	20,297		20,297			
Results for workers subsidized for 74 to 177 days (3rd quartile)						
1st year	11.338***	12.190***	0.070**	0.087***	16.872	0.207***
	(4.20)	(0.78)	(0.03)	(0.01)		(0.05)
2nd year	10.821	15.636***	0.054*	0.076***		
	(8.65)	(1.57)	(0.03)	(0.01)		
3rd year	22.560	20.545***	0.083**	0.076***	15.286	0.196***
	(15.24)	(2.77)	(0.03)	(0.01)		(0.05)
N (total)	834,510		834,459			
N (participants)	20,731		20,731			
Results for workers subsidized for $\geq$ 178 days (4th quartile)						
1st year	15.841***	22.112***	0.118***	0.143***	14.262	0.182***
	(4.07)	(0.60)	(0.04)	(0.00)		(0.05)
2nd year	23.624***	38.774***	0.105**	0.140***		
	(8.87)	(1.67)	(0.04)	(0.01)		
3rd year	43.333**	49.963***	0.142***	0.140***	12.833	0.165***
	(17.57)	(2.68)	(0.05)	(0.01)		(0.05)
N (total)	834,274		834,223			
N (participants)	20,495		20,495			

Source: IEB V11.00 - 131009. Own calculations.
Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first stage regressions include the same set of control variables as the corresponding second stage regressions. Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Significance level: *** 1%, ** 5%, * 10%.

Table 8: LATE and OLS estimates for participation in the subsidy scheme by gender

	(1)		(2)		First stage results	
	Employment		**log Earnings**		F-test	Award
	2SLS	OLS	2SLS	OLS		intensity
Results for men						
1st year	10.515***	12.452***	0.053***	0.076***	34.384	0.424***
	(1.47)	(0.43)	(0.01)	(0.00)		(0.07)
2nd year	12.016***	17.860***	0.025	0.067***		
	(3.33)	(1.07)	(0.02)	(0.00)		
3rd year	18.450***	21.355***	0.019	0.062***	37.216	0.405***
	(6.68)	(1.86)	(0.02)	(0.00)		(0.07)
N (total)	595,839		595,805			
N (participants)	65,347		65,347			
Results for women						
1st year	16.977***	16.270***	0.147***	0.142***	22.922	0.376***
	(3.89)	(0.85)	(0.03)	(0.01)		(0.08)
2nd year	32.036***	29.013***	0.160***	0.141***		
	(10.03)	(1.97)	(0.04)	(0.01)		
3rd year	52.047***	38.724***	0.197***	0.143***	0.348***	0.348***
	(18.83)	(3.38)	(0.05)	(0.01)	(0.07)	(0.07)
N (total)	300,555		300,538			
N (participants)	17,268		17,268			

Source: IEB V11.00 - 131009. Own calculations.
Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first stage regressions include the same set of control variables as the corresponding second stage regressions. Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Significance level: *** 1%, ** 5%, * 10%.

Table 9: LATE and OLS estimates for participation in the subsidy scheme by age

	(1)		(2)		First stage results	
	Employment		**log Earnings**		F-test	Award
	2SLS	OLS	2SLS	OLS		intensity
Age 20 to <30 years						
1st year	20.135***	20.656***	0.115***	0.145***	36.238	0.423***
	(3.88)	(0.90)	(0.03)	(0.01)		(0.07)
2nd year	46.157***	33.697***	0.131***	0.135***		
	(10.30)	(2.21)	(0.03)	(0.01)		
3rd year	81.448***	46.585***	0.164***	0.135***	35.153	0.409***
	(18.71)	(3.44)	(0.03)	(0.01)		((0.07)
N (total)	165,484		165,468			
N (participants)	16,710		16,710			
Age 30 to <40 years						
1st year	8.812***	13.928***	0.044**	0.097***	26.397	0.431***
	(1.69)	(0.57)	(0.02)	(0.00)		(0.08)
2nd year	14.315***	21.924***	0.024	0.093***		
	(4.40)	(1.44)	(0.03)	(0.01)		
3rd year	23.773***	27.435***	0.029	0.090***	26.702	0.408***
	(7.58)	(2.52)	(0.03)	(0.01)		(0.08)
N (total)	227,999		227,987			
N (participants)	26,340		26,340			
Age 40 to <45 years						
1st year	9.596***	9.824***	0.047	0.077***	26.531	0.391***
	(1.91)	(0.73)	(0.03)	(0.01)		(0.08)
2nd year	15.430***	15.702***	0.029	0.073***		
	(4.71)	(1.79)	(0.04)	(0.01)		
3rd year	19.831**	20.084***	0.015	0.072***	28.205	0.370***
	(8.85)	(2.76)	(0.05)	(0.01)		(0.07)
N (total)	152,263		152,256			
N (participants)	16,461		16,461			
Age 45 to 65 years						
1st year	11.142***	11.232***	0.050**	0.088***	50.529	0.421***
	(2.56)	(0.60)	(0.02)	(0.00)		(0.06)
2nd year	4.268	18.171***	0.006	0.087***		
	(4.94)	(1.39)	(0.03)	(0.01)		
3rd year	-0.737	22.762***	-0.014	0.084***	56.401	0.400***
	(11.31)	(2.67)	(0.04)	(0.01)		(0.05)
N (total)	350,648		350,632			
N (participants)	23,104		23,104			

Source: IEB V11.00 - 131009. Own calculations.
Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first stage regressions include the same set of control variables as the corresponding second stage regressions. Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Significance level: *** 1%, ** 5%, * 10%.

6.4 Robustness

6.4.1 Local labor markets

It might be of concern that the regional controls for agency districts account insufficiently for structural and economic differences. As a consequence, unobservable confounding factors of program participation on the agency level might be correlated with conditional policy styles. In an alternative specification, I therefore control for labor-market fixed effects, exploiting only variation in policy styles that occurs within the same labor markets. Following the classification by Kosfeld & Werner (2012), I distinguish 141 local labor markets for Germany. Those are marked by close commuter links and a high seclusion toward other regional labour markets. Local labor markets are based on an aggregation of 402 counties. As this is not the case for agency districts, labor markets and agency districts are not nested but overlapping, enabling fixed-effects estimations.

The LATE estimates of Table 10 are mostly slightly smaller, but overall nearly identical to those of Table 5. This confirms that there is no remaining local confounding variation in the baseline model that might drive unobserved selection into treatment.

6.4.2 Benchmark estimates

As mentioned before, it is unclear to which extend the difference between LATE and OLS is due to the selection bias of OLS or due to specifics of compliers. Assuming that remaining selection bias can substantially be reduced by applying difference-in-differences propensity score matching, which controls for observable and time-invariant unobserved heterogeneity, I can learn if the effects for compliers differ from those for all participants by comparing matching coefficients and LATE coefficients from Table 5. The propensity score regressions are based on nearest neighbor matching with 25 neighbors and include the same set of control variables as the previous estimations. The matching results–which deliver a more precise estimation by construction–are presented in Table 11.

The effects obtained with propensity score matching from Table 11 are smaller than the ones obtained with OLS or 2SLS, but closer to the latter. The average participant is about

Table 10: Robustness: LATE and OLS estimates with labor market fixed effects for participation in the subsidy scheme on cumulated employment, cumulated unemployment, and log average earnings

	(1)		(2)		(3)	
	Employment		**Unemployment**		**log Earnings**	
	2SLS	OLS	2SLS	OLS	2SLS	OLS
1st year	10.821***	13.612***	-2.528***	-3.352***	0.037*	0.094***
	(1.34)	(0.40)	(0.79)	(0.28)	(0.02)	(0.00)
2nd year	13.315***	21.403***	2.994	-0.999	0.012	0.088***
	(2.83)	(1.03)	(2.56)	(0.70)	(0.03)	(0.00)
3rd year	21.024***	27.330***	4.040	1.504	0.009	0.085***
	(6.19)	(1.74)	(4.66)	(1.06)	(0.04)	(0.00)
First-stage results, dependent variable: treatment dummy						
1st and 2nd year						
Con. policy style	0.420***		0.420***		0.424***	
	(0.07)		(0.07)		(0.07)	
F-test of excl. instr.	32.541		32.541		35.768	
N	896,394	896,394	896,394	896,394	896,343	896,343
3rd year						
Con. policy style	0.401***		0.401***		0.401***	
	(0.07)		(0.07)		(0.07)	
F-test of excl. instr.	34.386		34.386		34.384	
N	728,848	728,848	728,848	728,848	728,823	728,823

Source: IEB V11.00 - 131009. Own calculations.

Notes: All regressions include a constant. The first stage regressions include the same set of control variables as the corresponding second stage in the upper panel. To control for the timing of program start, all regressions include interaction terms of quarter and year dummies. Control variables include labor market fixed effects as well as firm characteristics, socio-demographic characteristics, and employment history as in Table 4. Standard errors (in parentheses) are clustered at the level of 176 local employment agency districts. Significance level: *** 1%, ** 5%, * 10%.

Table 11: Difference-in-difference nearest neighbor propensity score matching estimates for participation in the subsidy scheme on cumulated employment, cumulated unemployment, and log average earnings

	(1)		(2)		(3)			
	Employment		**Unemployment**		**log Earnings**			
	Coef.	SE	Coef.	SE	Coef.	SE	N(Part.)	N(Contr.)
1st year	8.014***	0.431	-3.887***	0.250	0.010	0.009	82,615	813,779
2nd year	11.103***	0.926	-1.701***	0.457	-0.007	0.008	82,615	813,779
3rd year	10.659***	1.686	0.040	0.711	-0.008	0.007	69,455	659,388

Source: IEB V11.00 - 131009. Own calculations.

Notes: Outcome variables as in Table 5. The propensity score regressions are based on nearest neighbor matching with 25 neighbors and include the same set of control variables as in Table 4. Significance level: *** 1%, ** 5%, * 10%.

10 days more employed after three years as non-participants. This might imply that always-participants profit less from the subsidy scheme than compliers. After three years, the LATE and the ATET on earnings and unemployment are both indistinguishable from zero. This indicates that compliers and always-takers might have very common features in this respect. Irrespective of the applied methodology for the identification of a causal effect, all estimates point into a similar direction.

7 Cost-benefit considerations

In this section I provide a rough cost-benefit analysis on the level of participants over the first three years after entry into the program. According to the statistics published by the FEA, the program registered about 183,751 entries into the program (cost reimbursement and wage subsidy) (Table 12, Panel A, Line 1). However, as about 60 percent of the workers received a combination of both, and 40 percent received either cost reimbursement or a wage subsidy, the number of participants is approximately 30 percent smaller than the number of inflows (Panel A, Line 2).

The only cost information available are the programme costs, that is, actual expenses of all employment agencies within each year provided by the FEA controlling department.[6] Between 2007 and 2010, total expenses for the subsidy scheme, that is, expenses due to wage subsidies and the reimbursement of training costs, amounted to approximately 626 million Euros (Table 12, Panel B), which corresponds to per capita costs of about 4,867 Euros. Over a two year period, this amounts to additional daily costs of 6.67 Euros per person.

On the benefit side, I assume that workers receive the same average daily wage of approximately 74 Euros as during the previous three years before participation (Table 12, Panel C). Multiplying these daily wages with the estimated LATE on earnings from Tables 5 and 6 yields an increase in daily per capita earnings of about 2.66 Euros. Thus, comparing daily per capita costs (Table 12, Panel B, last Line) and daily per capita benefits (Table 12, Panel C, last Line) over a two-year period, the program on average does not seem to pay off. However,

[6] I do not have information on administrative costs for employing caseworkers to implement the subsidy scheme. However, these costs might be negligible because the subsidy scheme is rather small compared to other instruments of active labor market policies and might therefore not require additional staff.

Table 12: Costs and benefits of the program

Year	2007	2008	2009	2010	Total
Panel A: Inflows and participants					
Total inflows	24,985	51,578	75,005	32,183	183,751
Approximate number of participants	17,490	36,105	52,504	22,528	128,626
Panel B: Costs					
Total costs in MIO	39.33	137.52	279.19	170.01	626.06
Average per capita cost in T	2,249.24	3,808.93	5,317.53	7,546.57	4,867.30
Average per capita cost per day over two years	3.08	5.22	7.28	10.34	6.67
Panel C: Benefits					
Average earnings during last three years	75	67	78	73	74
Effect on earnings after two years	0	0.16	0.076	0.066	0.036
Average additional earnings per day over two years	0	10.72	5.93	4.82	2.66

Source: IEB V11.00 - 131009. Own calculations.
Notes: As 60 percent of all inflows are double-counts by person, the approximate number of participants is about 30 percent lower than the number of inflows.

this differs by year such that for 2008 benefits might actually exceed costs. Moreover, assuming that the positive effect on earnings is permanent and remains longer than two years, for example five or ten years, the subsidy scheme could be beneficial.

From the fiscal point of view, it is important to include additional tax revenues. In order to pay off, the program should pay the daily average cost of 6.67 Euros. As the additional daily earnings are lower than that in most years, it is impossible that additional tax revenues come close to those costs, except for the year 2008. Overall, the numbers suggests that in budgetary terms, the program might not pay off for the government, at least over a two-year period.[7]

The estimates are conservative, as I assume that there is no future effect of the subsidy scheme beyond the two year time horizon. Further gains in employment or earnings would therefore imply an improvement of the results. Moreover, from the social perspective, one has to add potential gains by employers and public gains through reduced benefit transfers. However, one can only speculate how to assess these benefits properly, which I refrain from doing here.

[7] I ignore any general equilibrium effects that might arise.

8 Conclusion

In this paper, I have analyzed the impact of further training subsidies targeted at low-skilled employed workers between 2007 and 2010. Thereby, I contribute to the scarce literature on the effects of subsidized further training for low-skilled employed rather than unemployed workers. For identification, I rely on an instrumental variables approach, exploiting conditional regional variation in the intensity of awarding the subsidy by local employment agencies. This conditional intensity is exogenous to the labor market outcomes of employed workers, hence enabling me to predict program participation and to obtain local average treatment effects.

The evidence suggests that the subsidy improved labor market outcomes (employment and earnings) of subsidy recipients positively. For compliers, I find positive effects of 22 days more employment, an insignificant increase in earnings by 3.5 percent, and no effect on the receipt of UI benefits after three years. Given that Haelermans & Borghans (2012) report an average return to privately funded training of 3.5 percent, these estimates were comparable to those reported in the literature if they were significant. The effect on cumulated employment in economic terms is rather negligible. However, there is substantial heterogeneity between different groups of compliers. In particular workers starting participation in the scheme during the year 2008 profit more in terms of employment and earnings than the first or later cohorts. This is related to a compositional change of participants which was triggered by the start of the economic crises and adjustments in regulations by the FEA. As a consequence, low-skilled workers in the later period of the program had overall more favorable characteristics and therefore profited relatively little from the subsidy scheme. Further beneficiaries of the scheme are in particular women, younger workers, and participants in relatively longer (more intensive) training measures.

From a political perspective, these results suggest that a further targeting of the subsidy scheme to females, younger workers, and longer-lasting training programs might increase the program's efficiency. In fact, recent adjustments by the FEA put more emphasis on these groups (focus on training in the female occupation field of "elderly care", focus on younger

workers since 04/2012, opening the possibility of more profound re-training in modules since 04/2015). In this sense, my study provides an ex-post justification for these adjustments.

The importance of the program is given further leverage when considering that it improves the labor market chances of a group of individuals (low-skilled workers) that usually face a high risk of becoming unemployed, in particular long-term unemployed (Bundesagentur für Arbeit 2015). Thus, complementing programs for unemployed low-skilled workers, this subsidy scheme can be efficient in preventing unemployment for low-skilled in the first place rather than stepping in when the damage is already done. Thus, saved benefit transfers and potential positive spillover effects on coworkers (De Griep & Sauermann 2012) should also be taken into account if data become available that allow for a profound cost-benefit analysis.

Evaluations of training subsidies for employed workers in the form of vouchers exist for the German training premium (Bildungsprämie), which subsidizes general training courses for employees, as well as programs in Sweden (Schwerdt et al. 2012), and the Netherlands (Hidalgo et al. 2014). In contrast to these studies that do not find any significant effects on employment and earnings, I find positive returns to training. This might be related to differences in the type of training, which is a general problem in the training literature. Moreover, these programs have no direct link to the job of the participant and do therefore not promote firm-specific knowledge, which might potentially be to some extend the case with the underlying subsidy scheme.

A remaining unsolved question is how many of the awarded subsidies promoted training that would not have taken place without the subsidy, that is, the importance of deadweight. It is up to further research to account for this in order to allow more accurate conclusions about the efficacy of the program.

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Appendix

Table A.1: Average socio-demographic and employment characteristics for the aggregate sample

	Treated	Comparisons	Difference	p-Value
Female	0.21	0.35	-0.14	0.00
Age	39.22	41.53	-2.31	0.00
Age^2	1,625.98	1,837.91	-211.92	0.00
Immigrant	0.16	0.08	0.08	0.00
Degree info missing	0.01	0.05	-0.04	0.00
No degree	0.40	0.14	0.26	0.00
Vocational degree	0.53	0.60	-0.07	0.00
A-levels	0.02	0.02	-0.00	0.03
A-levels and vocational degree	0.03	0.07	-0.05	0.00
Polytechnical degree	0.01	0.05	-0.04	0.00
University degree	0.01	0.07	-0.06	0.00
School degree info missing	0.29	0.09	0.20	0.00
No school degree	0.07	0.03	0.04	0.00
Lower secondary degree (Hauptschule)	0.36	0.33	0.03	0.00
Medium secondary degree (Realschule)	0.22	0.34	-0.12	0.00
Higher secondary degree (Fachhochschulreife)	0.03	0.07	-0.04	0.00
Abitur	0.03	0.15	-0.11	0.00
Unskilled blue collar worker	0.58	0.23	0.35	0.00
Skilled blue collar worker	0.25	0.22	0.02	0.00
Master craftsman	0.00	0.01	-0.01	0.00
White collar worker	0.17	0.53	-0.37	0.00
No unskilled worker (1 year prior)	0.78	0.86	-0.09	0.00
Unskilled worker (1 year prior)	0.13	0.06	0.08	0.00
Info if unskilled worker missing (1 year prior)	0.09	0.08	0.01	0.00
Employed before	0.93	0.95	-0.02	0.00
Employment in prior 3 years	947.38	969.46	-22.08	0.00
Employment in prior 3 years^2	973,107.81	1,010,280.12	-37,172.31	0.00
Number of employment spells in prior 3 years	1.26	1.19	0.07	0.00
Tenure with interruptions in prior 3 years	762.83	791.17	-28.34	0.00
Tenure with interruptions in prior 3 years^2	759,374.46	794,850.55	-35,476.09	0.00
Benefit receipt in prior 3 years	34.49	27.76	6.73	0.00
Benefit receipt in prior 3 years^2	11,024.52	11,480.74	-456.22	0.13
Number of benefit periods in prior 3 years	0.29	0.21	0.08	0.00
Unemployed job search in prior 3 years	58.21	38.66	19.55	0.00
Unemployed job search in prior 3 years^2	23,355.66	15,867.16	7,488.50	0.00
Number of unemployed job search periods in prior 3 years	0.57	0.36	0.21	0.00
Welfare receipt in prior 3 years	57.49	38.16	19.33	0.00
Welfare receipt in prior 3 years^2	36,455.29	27,542.32	8,912.96	0.00
Number of welfare periods in prior 3 years	0.23	0.13	0.10	0.00
Average daily benefits in prior 3 years	4.07	3.01	1.07	0.00
Average daily wage conditional on employment in prior 3 years	74.10	87.79	-13.68	0.00
Helper/ no profession	0.01	0.01	0.00	0.00
Profession in farming/ gardening	0.01	0.02	-0.00	0.00
Profession in manufacturing	0.24	0.11	0.13	0.00
Technical profession in manufacturing	0.16	0.15	0.01	0.00
Profession in construction	0.06	0.07	-0.01	0.00

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... table A.1 continued

	Treated	Comparisons	Difference	p-Value
Profession in food, hotel/ restaurant industry	0.02	0.04	-0.02	0.00
Profession in medical and non-medical health care	0.03	0.07	-0.04	0.00
Profession in humanities and arts	0.04	0.05	-0.02	0.00
Profession in retail and trade	0.03	0.07	-0.04	0.00
Profession in management and organisation	0.05	0.18	-0.13	0.00
Profession in business-related services	0.01	0.06	-0.05	0.00
Profession in IT and natural science services	0.03	0.04	-0.01	0.00
Profession in security	0.03	0.02	0.01	0.00
Profession in transportation and logistics	0.26	0.10	0.16	0.00
Profession in cleaning	0.01	0.01	-0.00	0.00
N	82,615	813,779		

Source: IEB V11.00 - 131009. Own calculations.

Table A.2: Average characteristics of firms using the subsidy scheme and firms not using the subsidy scheme

	Treated firms	Comparison firms	Difference	p-Value
Firm size	186.62	182.87	3.75	0.42
Share of low-skilled workers	0.0015	0.0012	0.04	0.00
Growth since t-1	0.27	0.37	-0.10	0.52
Growth since t-3	0.89	0.83	0.06	0.75
Growth since t-5	1.29	1.06	0.23	0.21
Missing industry	0.11	0.02	0.09	0.00
Farming, forestry, fishing	0.01	0.01	0.00	0.62
Mining	0.00	0.00	-0.00	0.79
Production	0.21	0.21	-0.01	0.02
Energy	0.00	0.01	-0.01	0.00
Watery	0.01	0.01	0.00	0.00
Construction	0.07	0.08	-0.01	0.00
Trade	0.09	0.16	-0.06	0.00
Transportation and storage	0.12	0.05	0.06	0.00
Hotel and restaurant industry	0.02	0.04	-0.02	0.00
Information and communication	0.02	0.03	-0.01	0.00
Financial and insurance services	0.00	0.03	-0.03	0.00
Estate and housing	0.03	0.03	-0.00	0.02
Freelance, scientific and technical services	0.02	0.06	-0.04	0.00
Other economic services	0.13	0.07	0.06	0.00
Public administration, social insurance	0.01	0.04	-0.03	0.00
Education	0.01	0.02	-0.01	0.00
Health and welfare	0.11	0.09	0.02	0.00
Art, entertainment and recreation	0.00	0.01	-0.00	0.00
Other services	0.01	0.02	-0.01	0.00
Private households	0.00	0.00	-0.00	0.19
Exterritorial organizations	0.00	0.00	-0.00	0.10
N	22,790	170,938		

Source: IEB V11.00 - 131009. Own calculations.

Table A.3: Establishment characteristics for the aggregate sample

	Treated	Comparisons	Difference	p-Value
Firmsize Missing	0.14	0.07	0.07	0.00
Firmsize 1-25 workers	0.13	0.26	-0.13	0.00
Firmsize 26-100 workers	0.20	0.22	-0.02	0.00
Firmsize 101-500 workers	0.29	0.25	0.04	0.00
Firmsize >500 workers	0.24	0.20	0.04	0.00
Missing industry	0.19	0.06	0.13	0.00
Farming, forestry, fishing	0.00	0.01	-0.00	0.00
Mining	0.00	0.00	-0.00	0.00
Production	0.34	0.26	0.08	0.00
Energy	0.00	0.01	-0.01	0.00
Watery	0.02	0.01	0.01	0.00
Construction	0.04	0.06	-0.02	0.00
Trade	0.07	0.13	-0.06	0.00
Transportation and storage	0.10	0.05	0.05	0.00
Hotel and restaurant industry	0.01	0.03	-0.01	0.00
Information and communication	0.02	0.03	-0.02	0.00
Financial and insurance services	0.00	0.03	-0.03	0.00
Estate and housing	0.03	0.03	-0.00	0.04
Freelance, scientific and technical services	0.01	0.05	-0.04	0.00
Other economic services	0.10	0.05	0.05	0.00
Public administration, social insurance	0.01	0.05	-0.04	0.00
Education	0.00	0.02	-0.02	0.00
Health and welfare	0.05	0.08	-0.04	0.00
Art, entertainment and recreation	0.00	0.01	-0.01	0.00
Other services	0.01	0.02	-0.01	0.00
Private households	0.00	0.00	-0.00	0.00
Exteritoral organizations	0.00	0.00	0.00	0.41
0-10 years	0.03	0.03	-0.01	0.00
>10-20 years	0.23	0.27	-0.04	0.00
>20-30 years	0.11	0.11	-0.00	0.72
>30 years	0.63	0.59	0.05	0.00
Missing	0.14	0.07	0.07	0.00
0-0.05 % lowskilled workers in firm	0.21	0.41	-0.19	0.00
0.05-0.1 % lowskilled workers in firm	0.12	0.16	-0.04	0.00
0.1-0.2 % lowskilled workers in firm	0.21	0.19	0.03	0.00
>0.2 % lowskilled workers in firm	0.32	0.18	0.14	0.00
N	82,615	813,779		

Source: IEB V11.00 - 131009. Own calculations.

Table A.4: Regional characteristics at the level of agency districts for the aggregate sample at the level of agency districts

	Treated	Comparisons	Difference	p-Value
Population density per square km	603.86	736.54	-132.67	0.00
Female population density per square km	50.88	50.96	-0.08	0.00
% of population aged 0-2 years	2.49	2.52	-0.04	0.00
% of population aged 3-5 years	2.56	2.56	0.00	0.06
% of population aged 6-9 years	3.67	3.60	0.07	0.00
% of population aged 10-14 years	4.98	4.81	0.18	0.00
% of population aged 15-17 years	3.15	3.00	0.15	0.00
% of population aged 18-19 years	2.33	2.26	0.07	0.00
% of population aged 20-24 years	6.02	6.04	-0.02	0.00
% of population aged 25-29 years	6.01	6.20	-0.19	0.00
% of population aged 30-34 years	5.72	5.92	-0.19	0.00
% of population aged 35-39 years	6.54	6.60	-0.07	0.00
% of population aged 40-44 years	8.43	8.44	-0.01	0.00
% of population aged 45-49 years	8.53	8.49	0.04	0.00
% of population aged 50-54 years	7.37	7.30	0.07	0.00
% of population aged 55-59 years	6.58	6.55	0.03	0.00
% of population aged 60-64 years	5.24	5.29	-0.05	0.00
% of population aged 65-74 years	11.53	11.64	-0.11	0.00
% of population aged >75 years	8.86	8.78	0.08	0.00
Schleswig-Holstein	0.02	0.03	-0.00	0.00
Hamburg	0.02	0.03	-0.01	0.00
Niedersachsen	0.09	0.09	-0.00	0.95
Bremen	0.01	0.01	-0.00	0.00
Nordrhein-Westfalen	0.22	0.21	0.00	0.21
Hessen	0.07	0.08	-0.01	0.00
Rheinland-Pfalz	0.05	0.04	0.01	0.00
Baden-Württemberg	0.16	0.14	0.02	0.00
Bayern	0.20	0.16	0.04	0.00
Saarland	0.01	0.01	0.00	0.00
Berlin	0.02	0.04	-0.02	0.00
Brandenburg	0.02	0.03	-0.01	0.00
Mecklenburg-Vorpommern	0.03	0.02	0.01	0.00
Sachsen	0.03	0.05	-0.02	0.00
Sachsen-Anhalt	0.02	0.03	-0.01	0.00
Thüringen	0.03	0.03	-0.00	0.01
Unemployment rate	0.09	0.09	-0.01	0.00
Unemployment rate^2	0.01	0.01	-0.00	0.00
% of workforce aged 15-19 years	3.97	3.71	0.27	0.00
% of workforce aged 20-24 years	8.93	8.87	0.06	0.00
% of workforce aged 25-29 years	9.62	9.86	-0.24	0.00
% of workforce aged 30-34 years	9.22	9.46	-0.24	0.00
% of workforce aged 35-39 years	11.36	11.42	-0.06	0.00
% of workforce aged 40-44 years	14.81	14.81	-0.00	0.17
% of workforce aged 45-49 years	14.50	14.39	0.10	0.00
% of workforce aged 50-54 years	11.69	11.60	0.09	0.00
% of workforce aged 55-59 years	9.11	9.15	-0.04	0.00
% of workforce aged 60-64 years	4.07	4.12	-0.05	0.00
% of workforce aged >=65 years	2.67	2.57	0.10	0.00
% of workforce unskilled	24.73	25.54	-0.82	0.00
% of workforce highly skilled	7.69	8.81	-1.12	0.00
% of workforce medium-skilled	54.47	53.58	0.89	0.00
% of workforce low-skilled	13.11	12.06	1.04	0.00

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... table A.4 continued

	Treated	Comparisons	Difference	p-Value
% in firms size 1-5 workers	11.20	11.01	0.19	0.00
% in firms size 6-9 workers	6.48	6.30	0.18	0.00
% in firms size 10-19 workers	9.61	9.34	0.27	0.00
% in firms size 20-49 workers	14.05	13.79	0.26	0.00
% in firms size 50-99 workers	11.92	11.83	0.10	0.00
% in firms size 100-499 workers	26.36	26.34	0.02	0.07
% in firms size <500 workers	20.37	21.39	-1.02	0.00
% in sector farming, forestry, fishing	0.80	0.78	0.02	0.00
% in sector mining	0.43	0.34	0.10	0.00
% in sector production	25.60	23.12	2.48	0.00
% in sector energy	0.83	0.85	-0.03	0.00
% in sector watery	0.76	0.79	-0.03	0.00
% in sector construction	5.95	5.69	0.26	0.00
% in sector trade	14.42	14.40	0.02	0.05
% in sector transportation and storage	4.81	5.08	-0.26	0.00
% in sector hotel and restaurant industry	2.83	2.93	-0.10	0.00
% in sector information and communication	2.41	3.00	-0.58	0.00
% in sector financial and insurance services	3.20	3.64	-0.44	0.00
% in sector estate and housing	0.64	0.77	-0.13	0.00
% in sector freelance, scientific and technical services	4.81	5.56	-0.75	0.00
% in sector other economic services	5.71	6.17	-0.46	0.00
% in sector public administration, social insurance	5.87	6.01	-0.14	0.00
% in sector education	3.64	3.79	-0.14	0.00
% in sector health and welfare	13.55	13.13	0.42	0.00
% in sector art, entertainment and recreation	0.74	0.80	-0.07	0.00
% in sector other services	2.71	2.92	-0.22	0.00
% in sector private households	0.13	0.13	0.00	0.00
% in sector exterritorial organizations	0.16	0.11	0.05	0.00
2007Q1	0.01	0.04	-0.02	0.00
2007Q2	0.07	0.07	-0.01	0.00
2007Q3	0.13	0.10	0.03	0.00
2007Q4	0.04	0.05	-0.01	0.00
2008Q1	0.02	0.04	-0.01	0.00
2008Q2	0.06	0.07	-0.01	0.00
2008Q3	0.14	0.09	0.05	0.00
2008Q4	0.03	0.05	-0.02	0.00
2009Q1	0.04	0.04	0.01	0.00
2009Q2	0.06	0.07	-0.01	0.00
2009Q3	0.09	0.09	-0.01	0.00
2009Q4	0.03	0.05	-0.01	0.00
2010Q1	0.09	0.04	0.05	0.00
2010Q2	0.07	0.07	0.00	0.22
2010Q3	0.06	0.09	-0.03	0.00
2010Q4	0.06	0.05	0.01	0.00
N	82,615	813,779		

Source: IEB V11.00 - 131009. Own calculations.

Table A.5: Means of selected individual characteristics for subgroups of participants

	By year of participation				By duration (quartiles)				By gender		By age			
	2007	2008	2009	2010	1-19 days	20-73 days	74-177 days	$\geq$ 178 days	Men	Women	20-30 years	30-40 years	40-45 years	45-65 years
Female	0.21	0.19	0.20	0.26	0.17	0.17	0.21	0.30	0.00	1.00	0.17	0.17	0.23	0.26
Age	39.34	38.31	39.71	39.27	40.77	39.07	38.48	38.54	38.76	40.95	26.34	35.20	42.51	50.78
Immigrant	0.17	0.17	0.15	0.17	0.16	0.16	0.17	0.16	0.17	0.14	0.16	0.21	0.15	0.11
No vocational degree	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Degree info missing	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.00	0.01	0.01	0.01
No degree	0.50	0.42	0.35	0.36	0.42	0.41	0.39	0.37	0.40	0.39	0.40	0.37	0.39	0.43
Vocational degree	0.44	0.51	0.57	0.54	0.52	0.53	0.54	0.54	0.53	0.51	0.53	0.54	0.54	0.50
A-levels	0.01	0.02	0.01	0.03	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.01
A-levels and vocational degree	0.02	0.03	0.03	0.03	0.02	0.03	0.03	0.04	0.03	0.04	0.03	0.04	0.02	0.02
Polytechnical degree	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01
University degree	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01
School degree info missing	0.31	0.24	0.32	0.26	0.36	0.27	0.24	0.29	0.29	0.29	0.08	0.26	0.37	0.42
No school degree	0.06	0.08	0.06	0.07	0.05	0.08	0.08	0.05	0.07	0.05	0.10	0.07	0.06	0.05
Lower secondary degree (Hauptschule)	0.36	0.40	0.33	0.37	0.34	0.38	0.39	0.34	0.38	0.29	0.48	0.37	0.31	0.29
Medium secondary degree (Realschule)	0.21	0.22	0.22	0.23	0.20	0.21	0.22	0.24	0.20	0.29	0.28	0.23	0.20	0.18
Higher secondary degree (Fachhochschulreife)	0.02	0.02	0.03	0.03	0.02	0.02	0.03	0.03	0.03	0.04	0.03	0.03	0.02	0.02
Abitur	0.03	0.03	0.03	0.05	0.03	0.03	0.04	0.05	0.03	0.05	0.03	0.04	0.03	0.03
Unskilled blue collar worker	0.55	0.64	0.56	0.56	0.61	0.56	0.61	0.53	0.59	0.52	0.63	0.59	0.56	0.53
Skilled blue collar worker	0.32	0.22	0.26	0.18	0.27	0.31	0.22	0.19	0.29	0.09	0.23	0.24	0.25	0.27
Master craftsman	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
White collar worker	0.12	0.14	0.17	0.25	0.10	0.13	0.17	0.28	0.11	0.39	0.14	0.17	0.18	0.19
Employed before	0.91	0.90	0.96	0.94	0.96	0.93	0.91	0.92	0.93	0.92	0.88	0.93	0.95	0.95
Employment in prior 3 years	914.06	895.04	986.43	963.79	976.26	939.55	918.49	954.65	954.18	920.54	836.29	947.97	987.56	997.71
Tenure with interruptions in prior 3 years	747.04	686.73	815.72	762.26	816.27	748.10	711.02	774.82	766.23	748.43	570.74	754.09	823.21	867.77
Unemployed job search in prior 3 years	80.05	82.65	39.02	46.14	46.96	64.31	72.08	49.70	58.77	56.41	89.64	60.90	45.44	41.73
Helper/ no profession	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Profession in farming/ gardening	0.01	0.02	0.01	0.02	0.02	0.02	0.02	0.01	0.02	0.00	0.02	0.02	0.01	0.01
Profession in manufacturing	0.27	0.22	0.26	0.17	0.28	0.23	0.21	0.22	0.27	0.12	0.21	0.22	0.24	0.27
Technical profession in manufacturing	0.21	0.13	0.18	0.12	0.21	0.16	0.14	0.13	0.16	0.18	0.14	0.15	0.17	0.18
Profession in construction	0.04	0.07	0.06	0.06	0.06	0.08	0.06	0.03	0.07	0.00	0.09	0.06	0.05	0.04
Profession in food, hotel/ restaurant industry	0.02	0.03	0.02	0.04	0.02	0.02	0.03	0.03	0.02	0.05	0.03	0.02	0.02	0.03
Profession in medical and non-medical health care	0.02	0.03	0.02	0.07	0.01	0.02	0.03	0.08	0.01	0.11	0.04	0.03	0.03	0.03
Profession in humanities and arts	0.03	0.03	0.03	0.06	0.01	0.03	0.03	0.08	0.01	0.13	0.03	0.03	0.04	0.04
Profession in retail and trade	0.02	0.03	0.03	0.04	0.02	0.02	0.03	0.06	0.02	0.07	0.03	0.03	0.03	0.04
Profession in management and organisation	0.05	0.05	0.05	0.06	0.04	0.05	0.06	0.06	0.03	0.13	0.05	0.05	0.05	0.05
Profession in business-related services	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.00	0.02	0.01	0.01	0.01	0.01
Profession in IT and natural science services	0.03	0.02	0.04	0.04	0.02	0.02	0.03	0.05	0.03	0.03	0.03	0.04	0.03	0.03
Profession in security	0.04	0.03	0.02	0.03	0.03	0.04	0.03	0.03	0.03	0.02	0.04	0.03	0.03	0.03
Profession in transportation and logistics	0.24	0.32	0.24	0.25	0.26	0.29	0.30	0.20	0.31	0.10	0.27	0.29	0.26	0.23
Profession in cleaning	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01

Source: IEB V11.00 - 131009. Own calculations.