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Regional discontinuities and the effectiveness of further training subsidies for low-skilled employees

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Contents

Abstract

I analyze the effects of further training subsidies for low-skilled employees on individual labor market outcomes in Germany for the period from 2007 to 2012. Using detailed administrative data, I exploit cross-regional variation in the conditional policy styles of local employment agencies and use this fuzzy discontinuity as an instrument for program participation. I find that subsidies significantly increase cumulative employment duration and earnings for the subgroup of compliers. These gains are particularly pronounced for workers who are women, younger than 35 years old, non-German citizens and participated before the economic crisis of 2009.

Zusammenfassung

In diesem Beitrag untersuche ich die Wirkung von Weiterbildungssubventionen auf die Arbeitsmarktchancen von gering Qualifizierten zwischen 2007 und 2012. Mit Hilfe detaillierter Prozessdaten bestimme ich Unterschiede in den konditionalen Politikstilen zwischen den lokalen Arbeitsagenturen und nutze diese unscharfe Diskontinuität als Instrument um kausale Effekte zu identifizieren. Für „complier“, d.h. Personen, die die Subventionen nur aufgrund eines bestimmten Politikstils einer Arbeitsagentur erhalten, erhöhen sich aggregierte Beschäftigung und Einkommen. Besonders stark profitieren dabei Frauen, unter 35-jährige, Personen ohne deutsche Staatsbürgerschaft und Teilnehmer in den Jahren vor der Wirtschaftskrise von 2009.

JEL classification: J18, J24, J31, I21

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

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1 Introduction

Among the most remarkable changes in Western societies over the last decades have been the structural changes in employment caused by globalization (Goos et al., 2009; Goos and Manning, 2007; Autor et al., 2006). The international integration of product and labor markets has affected the composition of industries and increased demand for workers in more-complex occupations. For example, 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" over the past decades. This implies that the structural change in employment is particularly detrimental to low-skilled workers and advantageous for highly skilled workers, as less-complex tasks are increasingly relocated to countries in the East (Goos et al., 2009; Goos and Manning, 2007). This continuing economic change requires a flexible and suitably skilled workforce, which necessitates investments in education and training.

This study examines the poorly understood impact of public subsidies for further training of *employed* low-skilled workers. I focus on unravelling the causal effects of government subsidies on cumulative earnings and employment outcomes exploiting substantial exogenous variation in award intensity, which I call the conditional policy style, at the regional level among unemployment agencies that provide these subsidies in Germany. I thereby contribute to a literature that has, so far, seen little quantitative analysis. The bulk of the 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.

In contrast, the literature on the returns of training subsidies for employed workers is relatively scarce. Existing studies focus on heterogeneous programs using differing methods and data and, therefore, report mixed results. Abramovsky et al. (2011) and Görlitz (2010) look at the firm-level outcomes of government subsidies for further training for low-skilled employed workers. While Abramovsky et al. (2011) do not find effects of UK subsidies for low-skilled training on firm or training take-up rates, Görlitz (2010) finds positive effects, ranging from 10 to 15 percent, for the share of firms investing in training through German firm vouchers partially covering direct training costs. By contrast, providing additional information about these vouchers to German workers does not increase training take-up at the worker level (Görlitz and Tamm, 2016*a*), and there is no significant impact on wages for low-skilled workers (Görlitz and Tamm, 2016*b*). Consistently, Hidalgo et al. (2014) do not find wage effects of training vouchers for low-skilled workers in the Netherlands. By contrast, Stenberg (2011) shows that an additional year of adult education in Sweden increases annual earnings by 4.4 percent. However, these programs differ from the subsidy program evaluated here because they are not directly linked to on-the-job training. The paper by Dauth and Toomet (2016), which analyzes the impact of subsidized training on the employment duration of workers older than 45 years in small and medium-sized firms, is the most similar to this study. They find improved employment among participants of the subsidy program, which they attribute to the postponement of retirement due to increased job satisfaction.

This study contributes to the literature by using high-quality register data to evaluate the effects of a government subsidy program that provides further training for low-skilled employed workers in Germany. By exploiting exogenous regional variation in the treatment probabilities, as in similar studies by, e.g., Frölich and Lechner (2010) or Doerr et al. (2014), I can address problems of selectivity, i.e., workers selecting into the program based on unobservable characteristics, and identify causal effects. As I am particularly interested in heterogeneous treatment effects, I focus on the treatment effects of workers in subgroups, thereby taking into account evidence of disparate effects of further training by age or gender (Grund and Martin, 2012). Several robustness checks confirm the validity of the results.

Variation in the conditional policy styles of local employment agencies affects the implementation of training subsidies for low-skilled employed workers. Policy styles vary across agencies due to differences in their organizational structures, problem-solving mechanisms, and concepts (for a detailed discussion, see Doerr and Kruppe, 2014). While this affects the propensity to be treated, i.e., participation in the subsidy program, policy styles are exogenous to the employment durations and wages of low-skilled employed workers. Employed workers generally have no contact whatsoever with employment agencies. Therefore, the assumption that region-corrected policy styles influence workers' labor market outcomes only via the underlying subsidy program is very credible.

The remainder of this paper is organized as follows. Section 2 outlines the institutional background. Section 3 describes the theoretical considerations and Section 4 the empirical approach. Section 5 characterizes the data, and Section 6 provides the treatment effects on the employment and earnings of compliers. Section 7 analyzes the costs and benefits of the program, and Section 8 concludes.

2 The German labor market and the training subsidy program

Germany has experienced striking structural changes in its workforce. Following other OECD countries, which fund training through loans, subsidy programs or tax deductions (Bassanini et al., 2007), the German government introduced further training subsidies for employed workers in 2007, supplementing existing programs for unemployed workers. These subsidies are intended to encourage low-skilled workers, who show disproportionately low interest in further training, to participate and to raise employers' (particularly small and medium-sized firms) propensity to further train their low-skilled personnel.

To this day, this subsidy program is the only German Federal Employment Agency (FEA) program that targets employed workers. The major responsibilities of the FEA are the reintegration of unemployed workers and the distribution of unemployment benefits. Thus, in contrast to other FEA programs, which are directed at unemployed workers, the subsidy program considered in this study is unique. Although another federal training voucher program (Bildungsprämie) has been implemented by local educational centers (see, for example, Görlitz and Tamm, 2016*b*; Görlitz, 2010), that program provides off-the-job training. By contrast, the FEA subsidy program concerns on-the-job training. The FEA operated 176 local employment agencies—which own certain competencies in the active labor market policy (ALMP) mix—during the relevant 2007–2010 study period.

During this period, workers entered the program primarily via their employers, although there was no explicit implementation approach. Commonly, caseworkers promoted the program to firms, which then identified potential participants who met with caseworkers at the local employment office. With the firm's support, workers could then receive a training voucher for either a training course offered by a private provider or occupational re-training. These courses are typically offered by private providers, which must be certified by the local employment agency.

The program is primarily targeted at low-skilled workers.[1] Potential participants are considered low-skilled if they lack a vocational qualification or worked in a helper job, which did not require a qualification, for at least four years. Further eligibility criteria are that the employer releases the employee from work to participate in training and continues wage payments during this absence. Subsidized training courses must focus on general rather than firm-specific learning, as the objective is to improve knowledge that is applicable in the general labor market. A subsidized training course is supposed to terminate with the receipt of a certificate.

Once these criteria are met, the worker-employer dyad qualifies for two different types of subsidies: first, the FEA may cover up to 100 percent of the training costs. Second, employers may receive wage subsidies to compensate for the workers' reduced productivity during training. If training takes place outside the firm, these wage subsidies may cover up to 100 percent of the full wage. If training takes place inside the firm, the employer is expected to share these costs. The FEA covers up to 50 percent of the wage because firm-specific elements of training are more likely.

Table 1: Inflows to the subsidy program 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.

Table 1 shows the inflows into the program by year and kind of subsidy. Compared to other German ALMP programs, the number of subsidy recipients is rather low (Büttner et al., 2015). Because awareness of such subsidies was low during the introductory phase, there were few entries. The number of participants increased with the advertising efforts of employment agencies, the introduction of external training counselors, and the expansion of the program to workers just leaving unemployment. Enrollment peaked in 2009 with the financial and economic crisis in Germany. The program costs displayed in Table 1 reflect this development. An inspection of the program's monetary allocations by the German Federal Court of Auditors (Bundesrechnungshof, 2009) revealed an abrupt reduction of inflows in 2010, as local employment agencies were instructed to apply the eligibility criteria more strictly and to accurately document the allocation of subsidies.

[1] The program also targets the employees of small and medium-sized firms (Dauth and Toomet, 2016).

3 Theoretical considerations

The training literature offers several reasons for legitimate interventions in further training activities by official institutions (for a summary, see Booth and 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 lower wages with their reduced productivity during training and subsequent wage increases based on their increased productivity. If such a wage reduction is not possible, e.g., due to minimum wage regulations, workers will underinvest in general training (Leuven, 2005). In a situation of liquidity constraints, employers can lend workers money by paying wages above productivity during training and below productivity afterwards. However, this scenario probably only applies when firms are able to bind workers until the loan is repaid. Thus, if such a contract cannot be enforced, there is underinvestment in training.

In the case of firm-specific further training, neither firms nor workers have incentives to invest in training because long-term contracting cannot be enforced (hold-up), and 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, as productivity increases at a faster rate than wages after training (Acemoglu and Pischke, 1999*a*,*b*). The model implies that wages will be below workers' marginal productivity. From society's perspective, this yields underprovision of training. Thus, there is evidence that privately provided investments in further training is 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). Both employers and employees determine training participation by weighing the costs and benefits of such training. For employers, the benefits include increased productivity, enhanced worker commitment to the organization, and reduced labor turnover, while the costs may include direct costs or continued wage payments for workers in training. For employees, the benefits can be expressed as higher wages after training and higher chances of promotion, while the costs include reduced wage payments during training or additional working time. As a consequence, trained workers are usually younger, male, better-educated, and employed full-time. The training firms are relatively large. By contrast, low-skilled workers participate in further training less often because they cannot easily handle training costs (Albert et al., 2010; Bassanini et al., 2007; Fouarge et al., 2013; Grund and Martin, 2012; Leuven and Oosterbeek, 1999). Public interventions, such as further training subsidies, might therefore alter the training decisions on both the employer and the employee sides by reducing the marginal costs of further training.

The literature indicates that compared to the returns of an additional year of schooling, the returns to further training are comparably large (for overviews, see Brunello et al., 2007;

Haelermans and Borghans, 2012). In a meta-study, Haelermans and Borghans (2012) estimate that the average return to a training course is approximate three percent. Regarding the expected effects of the programs on earnings, one can expect a constant or increasing wage profile over time. Initially, employers have no need to decrease wages during subsidized training, as wage subsidies compensate them for workers' lower productivity. After training, productivity increases, yielding higher wages. Regarding employment duration, there are two opposing forces: on the one hand, employment duration increases due to workers' higher productivity and commitment to the firm. On the other hand, employment duration decreases, because employers face lower training costs that are amortized over a shorter period.

4 Empirical approach

4.1 The LATE framework

In this study, I consider participants entering the subsidy program 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 training subsidies as an exogenous instrument, I apply a fuzzy regression discontinuity design (RDD) and estimate the effect of compliers' program participation on cumulative earnings and employment duration (see also, Angrist and Pischke, 2009). To identify the causal effect of the subsidy on the outcome, the preferred model to be estimated is

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

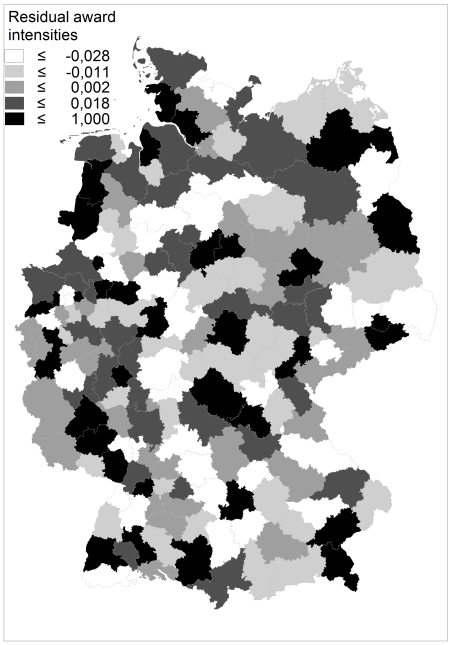
where T_{itq} is a dummy that indicates subsidy recipience, X_{itq} controls for individual characteristics, and F_{ft} for employer characteristics. A_{at} denotes the labor market characteristics, including the composition of the population and the workforce, which allow correction for regional, structural, and economic differences. *i* indicates observations on the individual level, *f* on the firm level, and *a* on the agency district level; *q* indicates observations by quarter and *t* by year. There remain $\eta_{iq} = M'_{itq}\gamma + \nu_i$ unobservable variables, such as M_{itq}, which might indicate workers' ambitions for which I cannot control. The effect of M_{itq} influences both the outcome Y_{itq} and the treatment indicator T_{itq}, such that $E(T_{itq}\eta_{itq}) \neq 0$. In order to estimate the causal effect of T_{itq} on Y_{itq}, I need an instrument for subsidy program participation. To instrument treatment, I construct the following variable

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

for every German local employment agency district *a* per quarter *q* in year *t*. SLS_{iatq} denotes the low-skilled employees who start subsidized training in each quarter and agency district, LS_{iat} indicates all low-skilled employees per agency district and year, and $Award\ intensity_{atq}$ corresponds to the unconditional local award intensity by quarter and agency district.

Differences in employment agencies' award intensities are partially driven by regional conditions, which are likely to impact both an individual's probability of participating in the subsidy program and participants' labor market outcomes. Therefore, I only exploit the remaining variation in the probability of treatment between different agency districts after purging variation in award intensity from region-, establishment-, and individual-level confounders. I refer to this residual variation as the *conditional policy style*. Figure 1 displays the corresponding variation of these average residual award intensities across German employment agency districts. Even after controlling for regional and other conditions, there are clear differences in the award intensities across local employment offices.

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



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

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

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

where β_1 captures the effect of conditional policy style on the treatment probability, which corresponds to residual $Award \quad intensity_{atq}$ after controlling for potential confounders. Substituting Equation (3) into Equation (1) in the 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

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

Thus, $\hat{T_{itq}}$ from Equation (3) is, by construction, initially correlated with regional and other characteristics, but once included in the second-stage Equation (4), the additional controls purge $\hat{T_{itq}}$ from this correlation.

The resulting coefficient γ_1 reports the local average treatment effect (LATE) of participation for all compliers.[2] 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 combined effect for always-takers and compliers.

4.2 Instrumenting with conditional policy styles

Regional variation in employment offices' policy styles has been exploited to evaluate ALMP instruments in recent studies (Boockmann et al., 2014; Doerr et al., 2014; Dean et al., 2015; Eppel, 2016; Frölich and Lechner, 2010; Lechner et al., 2013; Markussen and Roed, 2014). In this study, agency-specific policy styles reflect the part of the implementation of subsidized further training for employed low-skilled workers that is solely due to local employment agencies' unique features, which are independent from structural or economic specifics. Knodt (1998) defines policy styles as persisting differences in paradigms, problem-solving mechanisms, and cooperative behavior between agents. Adopting this idea, Doerr and Kruppe (2014) elaborate on the exogeneity of conditional policy styles. By combining unique survey data of caseworkers and managers' assessments of a training voucher program with German register data, they find that, in particular, cooperative behaviors and high degrees of communication within employment agencies and with firms determine policy styles.

I make a similar argument. Regarding agency-specific paradigms, managers might differ in their preferences and sentiments towards subsidies directed at employed workers. Irrespective of structural differences in unemployment across employment agencies, some managers might consider including the unemployed to be more important than training employed individuals. Particularly after the subsidy program's introduction in 2007, information on the implementation of the program was not yet specific. Therefore, it is not unlikely that employment agencies had different interpretation of which firms and workers were eligible for the program and which types of training were subsidizable. This also affected how caseworkers promoted the program to firms and organizations, such as chambers of commerce, and the type and speed of program implementation. As caseworkers became more familiar with the program and the FEA clarified the content and the eligibility criteria, policy styles may have changed over time.

4.3 LATE assumptions

Conditional policy styles are positively correlated with the probability of participating in the program but are not directly connected to participants' employment or earnings. Thus, the

[2] One can distinguish among four different groups in the LATE framework: *Always-takers* participate in the program irrespective of the policy style. *Compliers* participate only in agency districts that are conditionally more prone to grant further training subsidies. *Never-takers* never participate, whereas *defiers* participate only in districts less prone to grant subsidies. The monotonicity assumption (see Section 4.3) excludes the existence of defiers.

instrument purges the treatment effects of confounders that might simultaneously affect both the individual outcomes and the probability of subsidization. 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. I will show in Section 6 that this assumption is fully met.

Second, the instrument (conditional policy styles) must be independent of unobservable confounding factors on the agency, establishment, and worker levels. Such a correlation might arise if the instrument attracts certain workers or firms or if certain workers or firms drive the instrument. The former scenario can be excluded, as it is unlikely that workers or firms relocate to regions with specific policy styles (which are likely unknown to them). The latter scenario is more likely because very motivated workers or firms might actively seek further training subsidies from local employment agencies. This would then drive local award intensity. Thanks to rich data, I can account for both of these cases. Worker motivation or ambition is captured by extensive controls for the labor market career and firm tenure, which reflects a worker's motivation and labor force attachment. Firms demanding subsidies probably have particular firm characteristics, such as firm size or workforce structure. Moreover, such firms might operate in industries where further training and skill updating are generally important. Thanks to the rich register data, I can control for these confounders.

Third, there should not be any correlation between conditional policy styles and employment or earnings, as this would violate the exclusion restriction. 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. The underlying subsidy program is the only FEA training program directed at employed workers. Continuously employed individuals have no contact with employment agencies whose primary task is the integration of unemployed workers. Therefore, any FEA actions or policies should not have any direct effects on the employment durations or earnings of employed workers, except through the incidence and frequency of subsidized training.

Fourth, monotonicity requires that employed workers within an 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 likely to participate, and it does not cause any worker who wants to participate to change her mind. This assumption cannot be tested; however, there is no reason to assume that it is violated.

Fifth, the stable unit treatment value assumption (SUTVA) requires that individual decisions to participate in the program do not impact other individuals' labor market chances. The training subsidy program is rather small compared to other ALMP instruments, and the mean share of treated workers in the workforce per participating firm in the sample is approximately 3.5 percent; thus, this assumption very likely holds in the underlying case.

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 Employment Research and document the employment careers of all individuals liable for social security contributions (approximately 80 percent of the German workforce). They provide information on benefit receipts, job searches, and participation in active labor market policies. These data are process generated and highly reliable (see also, Dorner et al., 2010).

The IEB data are merged with data from three other sources. The Establishment History Panel (EHP) includes the universe of German establishments employing at least one worker liable for social security contributions (Hethey-Maier and Seth, 2009). As these 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 workforce composition 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, employer's size, industry structure) on the agency level from the Labor Placement Statistics.

For the analysis, I identify all low-skilled participants entering subsidized further training between 2007 and 2010 and the type of subsidy, i.e., wage subsidies or reimbursement of training costs. The corresponding numbers based on the final sample are displayed in Table 2. As nearly 60 percent of all participants received a combination of wage subsidies and reimbursements, I adjust for parallel treatment spells in the empirical analyses 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 spell at some later point. From the resulting sample, I drop apprentices, part-time workers and workers outside the age range of 20 to 65 years. Finally, I drop all workers from the Bochum employment agency district, as this agency reported an extremely high share of low-skilled participants relative to all low-skilled employees in that district in the fourth quarter of 2007 for no obvious reason.

I divide the sample into quarterly strata and calculate all control variables relative to the first day of each quarter. Outcome variables are calculated relative to the last day of each quarter. A quarterly treatment group consists of workers who start participation in the subsidy program in that quarter. The quarterly potential comparison group consists of workers who were employed at least one day in that quarter. For the main analyses, I consider one random draw of control workers who do not participate in the subsidy program during the observation period. This restriction is relaxed in Section 6.5.

5.2 Independent variables

In general, when evaluating an ALMP instrument, unobservable selection is a potential problem, i.e., one cannot control for all confounders that impact individual participation.

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

Year	Overall	Among those in %:	**Cost reimbursement**	**Wage subsidy**
2007	12,205		60.20	90.29
2008	21,453		77.04	89.90
2009	32,870		85.33	76.13
2010	12,397		91.27	66.77
Total	78,925		80.13	80.59

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

Presumably, there are at least three different sources of potential selection: regions, firms (establishments), and workers.[3] The allocation of the subsidy program is probably correlated with a number of these confounding variables.

5.2.1 Worker level

Table A.4.1 shows that worker-level selection into the program is likely. 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 (38 percent) and with lower schooling levels in the treatment group. Moreover, the treatment group consists of more than twice as many unskilled blue-collar workers (59 percent). One year prior to the start of the treatment, nearly three times as many workers (14 percent) in the treatment group were unskilled helpers compared to non-participants. Concerning occupational groups, subsidized workers are over-represented in manufacturing and transportation and logistics but under-represented in management and organization. With respect to employment careers, treated people have slightly stabler employment histories than those of the control group (longer tenures and employment durations, less employment spells), but lower earnings (74 vs. 83 euros per day). Treated workers received less unemployment benefits in the prior three years, but more welfare benefits.

Besides these observable factors, varying distributions of unobservable factors might impact local subsidy allocation. Unobserved worker characteristics, such as motivation, might drive award intensity. Moreover, it is possible that firms encourage ambitious workers, whom they want to keep, to participate, which in turn drives award intensity. By contrast, subsidies might also be a means of providing training for workers whom firms plan to layoff, e.g., due to economic difficulties. Following the suggestion of Caliendo et al. (2014) for propensity score matching, personality traits such as motivation–which increase a worker's value to the firm–should be captured by including variables into the regression that reflect the employment career and tenure.

[3] See also, Grund and Martin (2012) for a discussion of further training determinants at the individual, job, and firm levels.

5.2.2 Firm level

As a worker's program participation involves the employer, it is important to account for potential selection bias due to the employer. As mentioned above, the subsidy program was initially unknown to eligible firms and employees. Caseworkers with the employer service of local employment agencies promoted the program—in particular, to firms in industries reporting skill shortages—using a database that contains the addresses of all establishments within an employment agency district.

Thus, establishments that make use of the subsidy program might differ from other firms, particularly in terms of firm size, industry code, workforce, and economic situation. For example, larger firms have HR departments that might regularly collect information about existing subsidies. It is also easier for large firms to continue to function while workers are at training. Moreover, in some industries, such as the IT sector, human capital depreciates more quickly than in others, making further training necessary. In other industries, e.g., in the geriatric care sector, the marginal returns to training are particularly large (Dauth and Lang, 2017), making training more attractive. Furthermore, firms facing economic difficulties might increasingly demand public support.

Accounting for the distribution of firms in terms of industry, size, worker composition, and economic situation—which is reflected in the growth of the workforce—should sufficiently purge the treatment probability and establishment policy style confounders. Table A.4.2 shows that participating individuals tend to be employed in larger establishments with workforces characterized by higher shares of older and low-skilled people. Moreover, there are differences with respect to the industry. Firms in transportation, production, and economic services are more likely to use the subsidy.

5.2.3 Regional level

Another crucial factor is implementation at the employment agency level. Structural differences in unemployment rates and local economic situations determine the size of the agencies' staff and their general strategies. Thus, I account for the distribution of the employed population and the overall population of districts. I find that participants work in less densely populated regions with slightly lower unemployment rates. Moreover, I consider the distribution of the population regarding age and of firms and workforces across employment agencies. However, there are few economic differences, as Table A.4.3 shows.

After controlling for all of these factors, I am confident that the remaining differences in the allocation of the subsidy program depend on the local agencies' strategies and preferences 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 to instrument for program participation.

5.3 Outcome variables

Table 3 presents the descriptive results for cumulative employment, unemployment, and earnings for treated and comparison workers who did not receive subsidies over a period of three years.[4]

On average, the high values for cumulative employment suggest that workers have very stable employment relationships. Over a three year period, treated workers are employed for approximately 68 more days. Within the same period, they have approximately five fewer days in benefit receipts. Within the first three years of starting training, the earnings of subsidy recipients are significantly lower, by three percent, than those of comparison workers.

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
Cumulated employment								
1st year	342.44	322.52	19.91	0.00	78,925	325,571	0.00	366
2nd year	667.97	627.37	40.60	0.00	78,925	325,571	0.00	731
3rd year	954.49	886.48	68.00	0.00	78,925	325,571	0.00	1,096
Cumulated unemployment								
1st year	11.15	14.27	-3.12	0.00	78,925	325,571	0.00	365
2nd year	26.90	30.98	-4.08	0.00	78,925	325,571	0.00	730
3rd year	38.89	44.20	-5.31	0.00	78,925	325,571	0.00	1,095
log Earnings (daily average)								
1st year	4.20	4.21	-0.01	0.03	78,648	315,874	-3.94	5.79
2nd year	4.18	4.17	0.01	0.00	78,765	318,825	-5.30	5.53
3rd year	4.14	4.10	0.03	0.00	78,799	319,761	-5.40	5.41

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

Notes T: treated, C: potential comparisons

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] The latter include the district unemployment rate—in order to roughly control for regional labor market characteristics

[4] Treatment refers to treatment starting in a given quarter of potential treatment.

[5] For the purposes of organized presentation, I indicate the variables included in the regressions but do not display their coefficients.

Table 4: LATE for participation in the subsidy program on cumulative employment within two years

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) OLS
Treated	57.290***	35.468***	22.945***	34.675***
	(5.47)	(4.75)	(5.56)	(1.48)
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	0.892***	0.777***	0.601***	
	(0.21)	(0.19)	(0.18)	
F-test of excl. instr.	17.392	16.283	9.749	
R-squared	0.014	0.062	0.172	
N	404,496	404,496	404,496	404,496
N (participants)	78,925	78,925	78,925	78,925

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%.

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 that 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 subsidy award intensity that is twice as high as that of another district increases the unconditional probability of being treated by approximately 90 percentage points. The F-test statistic of the excluded instruments is well above conventional threshold levels. Turning to the second-stage results, I find that participation in the subsidy program has a positive and significant impact of approximately 57 days (8 weeks) on compliers' cumulative employment duration over a period of two years.

In column (2), I include firm characteristics, because factors such as firm size, workforce composition, and industry are important determinants of average employment duration and 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 significantly. Finally, I add control variables for individual characteristics, in particular, socio-demographic controls and variables that reflect both the employment career and any unobservable personal traits, as suggested by Caliendo et al. (2014) in a similar setup. The effect on employment decreases to three weeks more employment over two years for compliers.

The lower panel suggests that the first stage remains highly significant. Model (3) is the preferred or benchmark specification in the following analysis, as it contains the most important control variables. For this model, I also report the results of a corresponding OLS regression in column (4). Comparing the coefficients of (3) and (4) shows that the OLS regression generates a significant positive effect of approximately one more month of employment for participants over a two-year period, which is probably an overestimate of the true effect.

6.2 Additional labor market outcomes

The previous section has shown that participation in the subsidy program affects employment liable for social security contributions two years after treatment. Table 5 summarizes the results for the baseline specification.

Table 5: LATE and OLS estimates for participation in the subsidy program on cumulative employment, cumulative unemployment and log earnings (daily average)

	(1)		(2)		(3)	
	Employment		**Unemployment**		**log Earnings**	
	2SLS	OLS	2SLS	OLS	2SLS	OLS
1st year	14.831***	19.813***	-3.413***	-5.332***	0.072***	0.090***
	(2.61)	(0.67)	(1.27)	(0.42)	(0.02)	(0.00)
2nd year	22.945***	34.675***	-3.397	-6.704***	0.065***	0.093***
	(5.56)	(1.48)	(3.43)	(0.88)	(0.02)	(0.01)
3rd year	28.127***	50.001***	-1.287	-8.489***	0.062***	0.098***
	(9.89)	(2.29)	(5.68)	(1.30)	(0.02)	(0.01)
First-stage results, dependent variable: treatment dummy						
1st and 2nd year						
Con. policy style	0.601***		0.601***		0.602***	
	(0.18)		(0.18)		(0.18)	
N	404,496	404,496	404,496	404,496	394,533	394,533
N(Participants)	78,925	78,925	78,925	78,925	78,648	78,648
3rd year						
Con. policy style	0.567***		0.567***		0.566***	
	(0.17)		(0.17)		(0.17)	
N	324,620	324,620	324,620	324,620	320,059	320,059
N(Participants)	66,528	66,528	66,528	66,528	66,418	66,418

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%.

The effect on employment is steadily increasing over time to approximately 28 more days of employment for treated workers. The training period has a median duration of 73 days. Thus, employment effects that persist long beyond the end of training duration do not result from a lock-in effect. A second outcome considered in column (2) is cumulative days of unemployment. I define unemployment as a period during which unemployment insurance benefits are received. Compared to employment, the effect on unemployment is less

pronounced. There is a significant negative effect on unemployment benefit recipiency in the first year, indicating that complying program participants claim benefits from approximately 3 fewer days during the first year. Thus, more employment for compliers does not automatically translate into less insured unemployment. Finally, to see how the economic situations of compliers translate into earnings, I consider cumulative earnings as an outcome. Participants' earnings from the program are six percent higher in the three years after treatment.

A direct comparison of the OLS and IV estimates is only possible if the treatment effects are constant across all participants and if the average treatment effect on the treated is similar to the LATE. Here, the IV and OLS estimates, which reveal that OLS coefficients are higher in most cases, implies that OLS either cannot fully control for the positive selection of workers into the subsidy program or that compliers gain less from the subsidy program.

6.3 Results by selected subgroups

6.3.1 Results by cohort

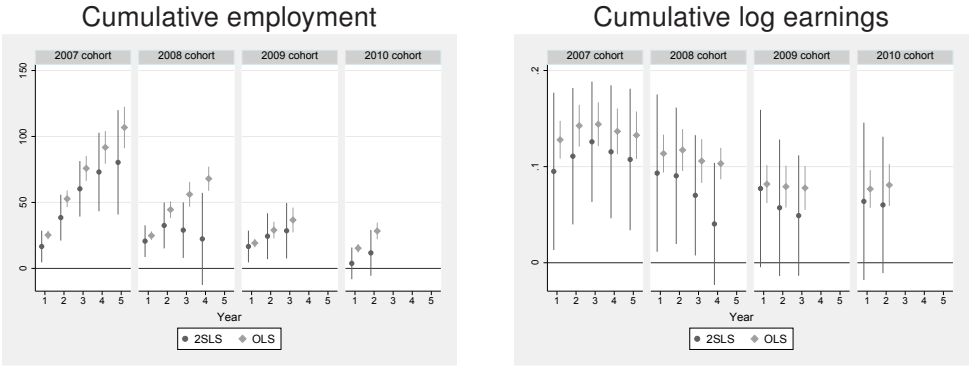
During the observation period from 2007 to 2010, the rules regarding the implementation of the program were adjusted several times. Therefore, examining the effects by year cohort might offer valuable clues as to whether changes in regulations affected the economic outcomes of participants. I focus on employment and earnings in the following analysis. As the data end on December 31, 2012, I can observe individuals over different periods: the longest period is for workers who started participating in 2007 (up to five years) and the shortest period began in 2010 (two years). The first-stage results support the validity of the instrument for all strata. 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.

Using an IV approach, I find quite large effects on employment and earnings for participants in 2007 (see Figure 2). In the two years after treatment, compliers are employed for approximately forty more days and earn 11 percent more than non-participants. This effect on employment increases to approximately 80 more days of employment within five years, while the effect on earnings remains relatively stable. The remaining cohort effects are smaller and insignificant in the longer run.

The difference in these effects can be attributed to the compositional differences of participating workers, as Table A.4.4 shows. In particular in 2007 and 2008, subsidized workers had the least favorable characteristics. They were less attached to the firm, which is reflected in approximately twice as much unemployment compared to participants in 2009 and 2010 and substantially less employment and shorter tenures. Thus, training might be particularly beneficiary for compliers with low skills and high marginal returns to training.

This shift in the composition of the participating workforce can be explained by changes in legislation: during the introductory phase of the program, implementation was ambiguous to caseworkers, for example, it was unclear which courses were fundable. Moreover, in

Figure 2: LATE and OLS estimates for participation in the subsidy scheme by cohort of treatment start



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. First-stage [SE] (F-test) values for employment within one year: cohort 2007: 1.448 [0.14] (119.614), cohort 2008: 1.042 [0.15] (45.031), cohort 2009: 0.388 [0.11] (10.980), cohort 2010: 1.483 [0.20] (48.839). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

2008, the FEA expanded the program to include recently re-employed workers who had left unemployment directly before entering the program. As a result, from 2007 to 2009 funded training courses were shorter than later on, making it easier for very low-skilled workers to obtain subsidized training.

In April 2009, the FEA introduced written rules and procedures for the implementation of the subsidy program. Moreover, the Federal Court of Auditors requested detailed documentation of the allocation of funds that same year. These steps resulted in a trend of longer training courses that granted a certificate at the end. This caused a steep decline in program entries in 2010, as training became more costly for employers (indirect costs). Compared to the introductory phase, this drew more employable workers to the program in 2009 and 2010 (see Table A.4.4).

The economic and financial crisis, which started in September 2008, induced firms to hoard labor, e.g., by further training workers (Möller, 2010). Thus, assuming decreasing returns to training and skills, compositional differences over time explain the larger program effects in the introductory phase and the smaller effects later on.

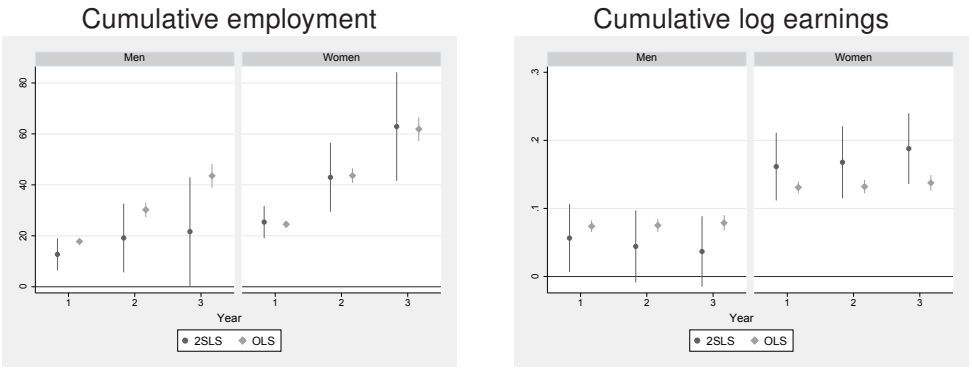
6.3.2 Results by individual characteristics

In the next step, I conduct subgroup analyses, pooling data for all cohorts from 2007 to 2010. This yields a sufficiently large sample of participants. I include interactions with quarter and year dummies to control for the timing of the (counterfactual) treatment start. I consider the same outcome variables as in the previous section. Again, the first-stage values and F-test statistics confirm the high quality of the instrument in most cases.

Looking at the effects on employment and earnings by gender (Figure 3), I find significantly higher returns to training on employment for women working full-time (+62 days) than for men working full-time (+22 days). This is even more pronounced in terms of earnings, with complying women earning approximately 19 percent more (men: no effect). This is likely attributable to different distributions of women and men across sectors of the economy. Table A.4.4 shows that women are concentrated in professions in health care, management and organization, and humanities and arts, while men are concentrated in manufacturing, construction, and transportation and logistics. In these sectors, women receive training courses that are, on average, 60 percent longer, which probably impart deeper knowledge.

In contrast to the previous estimates, for women, the 2SLS coefficients are larger than the OLS coefficients. This suggests that compared to men, participating women reflect the negative selection of all women. The IV specification can better control for this selection. Furthermore, this suggests that complying women, who participate due to an excessive local policy style, profit more than females who are always-takers. Thus, drawing more women into the program might increase its efficiency.

Figure 3: LATE and OLS estimates for participation in the subsidy scheme by gender

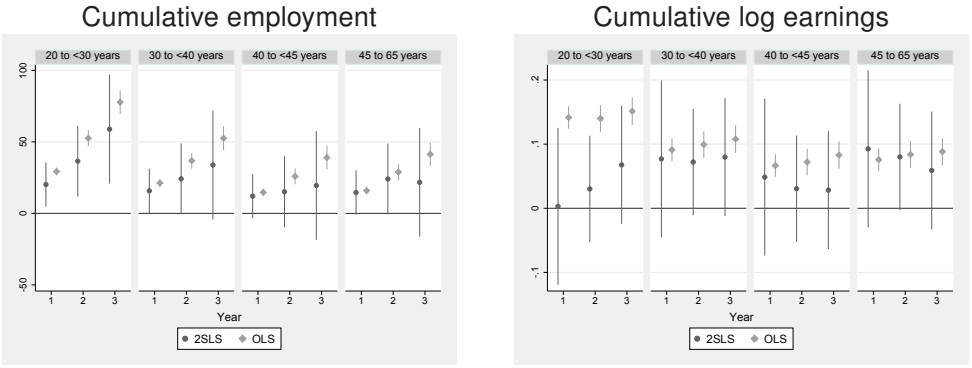


Source: IEB V11.00 - 131009. Own calculations.
Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first-stage regressions include the same set of control variables as the corresponding second-stage regressions. First-stage (F-test) values for employment within one year: men: 0.573 [0.18] (8.801), women: 0.650 [0.15] (19.163). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

Separating the program effect by age group (Figure 4) shows that the youngest participants, aged 20 to 30 years, profit the most and that the treatment effect declines with age. Thus, 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.

Figure 5 shows that the LATEs on employment do not differ significantly between workers with German citizenship and workers without German citizenship. Regarding earnings, non-Germans benefit more than Germans in the long run. In the three years after treatment, complying participants receive earnings that are approximately 11 percent higher earnings than those of other workers. Given that the marginal returns are decreasing in the

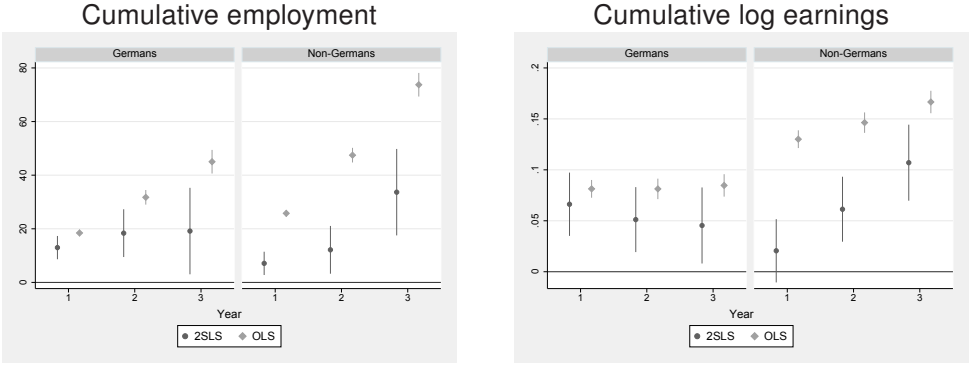
Figure 4: LATE and OLS estimates for participation in the subsidy scheme by age



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. First-stage (F-test) values for employment within one year: 20-<30 years: 0.707 [0.14] (24.865), 30-<40 years: 0.560 [0.19] (8.353), 40-<45 years: 0.599 [0.20] (8.010), 45-65 years: 0.578 [0.16] (10.459). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

skill level, the lower skill levels of non-Germans compared to Germans (Table A.4.4) imply higher marginal returns for non-Germans and explain the differing treatment effects[6]

Figure 5: LATE and OLS estimates for participation in the subsidy scheme for Germans and non-Germans



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. First-stage (F-test) values for employment within one year: Germans: 0.668 [0.17] (14.929), Non-Germans: 0.422 [0.15] (7.690). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

[6] I conduct additional subgroup analyses; however, they do not provide further insights. There are no differences in terms of the degree conferred at the end of a training course (Figure A.4.1). Firm size does also not seem to matter much for the effectiveness of the program (Figure A.4.2). There is no clear pattern. If anything, complying workers of small firms with up to 25 workers seem to benefit more than others. This effect holds only for earning, and only within the first two years of starting treatment. Thus, in the short run, small firms profit more from training subsidies than larger firms, which likely rely on their own training funds.

6.4 Robustness

6.4.1 Local labor markets

Some researcher might be concerned that the regional controls for agency districts insufficiently account for structural and economic differences. If so, unobservable confounding factors of program participation at the agency level are correlated with the conditional policy styles. In an alternative specification, I therefore control for labor market fixed effects, exploiting only variations in policy styles that occur within the same labor market. Following the classification by Kosfeld and Werner (2012), I identify 141 local labor markets for Germany. Those markets are characterized by close commuter links and high seclusion from other regional labour markets. Local labor markets are based on aggregations of 402 counties. As agency districts are not based on aggregated counties, labor markets and agency districts are not nested but overlapping, enabling the fixed effects estimation.

Compared to Table 5, the LATE estimates in Table 6 are generally larger, but the overall effects nearly identical. This suggests that the applied regional controls sufficiently purge the instrument of economic and structural components. Table A.4.6 supports this conclusion. The instrument remains stable with the subsequent addition of regional controls. This confirms that the remaining local confounding variation in the baseline model, which might drive unobserved selection into treatment, is irrelevant.

Table 6: Robustness: LATE and OLS estimates with labor market fixed effects for participation in the subsidy program on cumulative employment, cumulative unemployment, and log earnings (daily average)

	(1)		(2)		(3)	
	Employment		**Unemployment**		**log Earnings**	
	2SLS	OLS	2SLS	OLS	2SLS	OLS
1st year	16.225***	19.989***	-3.252**	-5.337***	0.070***	0.091***
	(2.73)	(0.66)	(1.36)	(0.43)	(0.02)	(0.00)
2nd year	25.393***	34.943***	-2.929	-6.654***	0.069***	0.094***
	(6.06)	(1.46)	(3.67)	(0.88)	(0.02)	(0.01)
3rd year	33.582***	50.472***	-2.197	-8.559***	0.076***	0.100***
	(10.00)	(2.25)	(5.52)	(1.28)	(0.02)	(0.01)
First-stage results for observations included in the 1st and 2nd year, dependent variable: treatment dummy						
Con. policy style	0.637***		0.637***		0.639***	
	(0.20)		(0.20)		(0.20)	
F-test of excl. instr.	10.656		10.656		10.526	
N(Participants)	78,925	78,925	78,925	78,925	78,648	78,648
N	404,496	404,496	404,496	404,496	394,533	394,533

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%.

6.4.2 Benchmark estimates

As previously mentioned, the extent to which the difference between LATE and OLS is due to the selection bias of OLS or the compliers' characteristics is unclear. Assuming that the remaining selection bias can substantially be reduced by applying propensity score matching, which controls for observable unobserved heterogeneity, I can determine whether the effects for compliers differ from those for all participants by comparing the matching coefficients and the LATE coefficients of Table 5. The ATET obtained with propensity score matching, combines the program effects for compliers and always-takers. 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 are presented in Table 7. They are larger than those obtained with OLS or 2SLS. The average participant is employed for approximately 55 more days and earns 15 percent more than non-participants within three years. As the LATEs are smaller, this implies that always-participants profit more from the subsidy program than compliers. Thus, further extending the program might be costly for the FEA, as additional workers who would receive the subsidies gain less than those already receiving the subsidy.

Table 7: Nearest-neighbor propensity score matching estimates for participation in the subsidy program on cumulated employment, cumulated unemployment, and log earnings (daily average)

	(1) **Employment**		(2) **Unemployment**		(3) **log Earnings**			
	Coef.	SE	Coef.	SE	Coef.	SE	N(Part.)	N(Contr.)
1st year	21.611***	0.599	-6.374***	0.351	0.139	0.005	78,925	325,571
2nd year	40.082***	1.216	-9.426***	0.599	0.149	0.005	78,925	325,571
3rd year	55.336***	2.102	-10.859	0.898	0.147	0.006	66,528	258,092

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%.

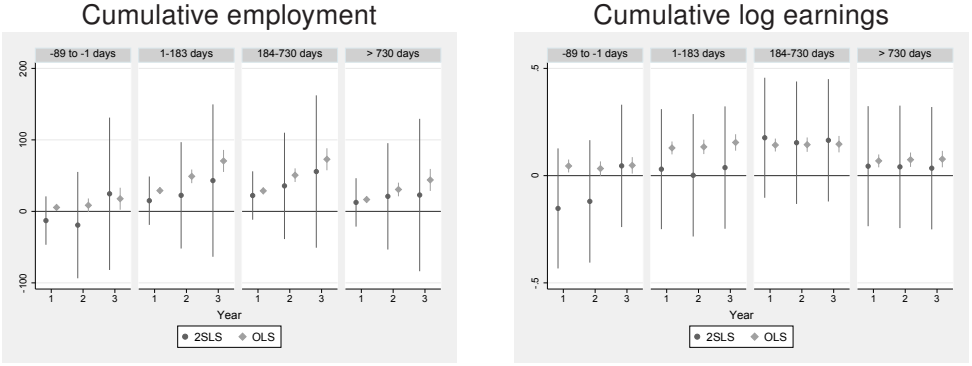
6.5 The dynamics of treatment

6.5.1 Tenure and the probability to be treated

In the literature on the evaluation of ALMPs for unemployed workers, the timing of participation during the unemployment spell matters (Fredriksson and Johansson, 2008; Sianesi, 2004). Thus, researchers often apply dynamic approaches to account for the correlation between the probability to be treated and unemployment duration.

Adapting timing in my setting, I examine whether treatment differs by tenure, i.e., for new hires versus long-serving workers. Tenure is measured at the beginning of the quarter of potential treatment. Thus, I assign negative tenure to workers starting employment that quarter. Figure 6 shows that all existing effects cancel out. All the effects are non-significant and do not reveal a meaningful pattern.

Figure 6: LATE and OLS estimates for participation in the subsidy scheme by tenure



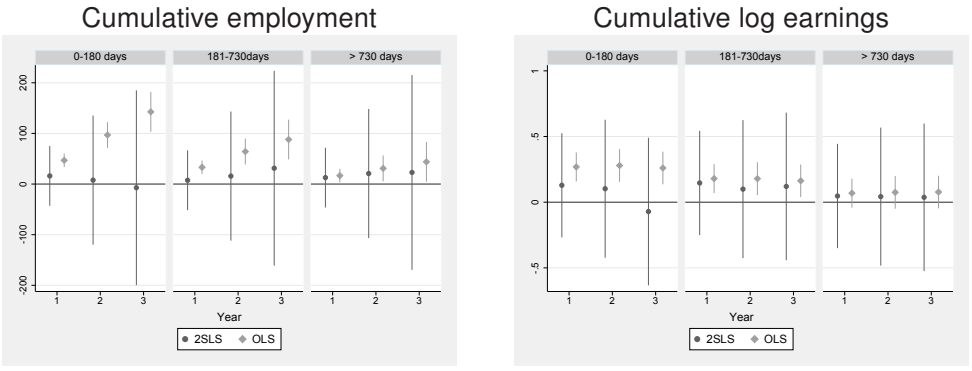
Source: IEB V11.00 - 131009. Own calculations.
Notes: Outcome variables as in Table 5. All regressions include the same set of control variables as in Table 4. The first-stage regressions include the same set of control variables as the corresponding second-stage regressions. First-stage (F-test) values for employment within one year: -89 to -1 days: 0.694 [0.11] (37.918), 1-183 days: 0.699 [0.13] (28.964), 184 to 730 days: 0.771 [0.16] (23.162), >730 days: 0.533 [0.17] (7.653). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

I conduct further in-depth analyses that account more specifically for the particular setting of the subsidy program and the ambiguous role of tenure. As outlined earlier, the program analyzed in this study was introduced on January 1, 2007. Theoretically, one can distinguish between two types of workers: On the one hand, there are workers who are already employed in January 2007 for whom the risk of treatment starts with the introduction of the program. I call these workers "incumbents". On the other hand, there are workers who begin employment after that date for whom the risk of treatment starts with employment in a given firm. I call these workers "entrants". Thus, unlike a dynamic setting in which workers enter unemployment and start an ALMP program during that unemployment spell, there is no direct link between employment duration and the probability of being treated (Fredriksson and Johansson, 2008).

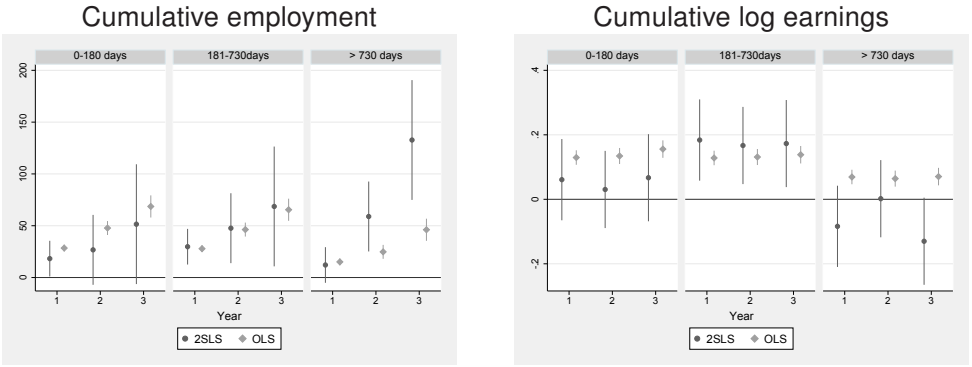
Distinguishing between incumbents and entrants, I repeat the analyses for tenure.[7] Figure 7 shows that tenure is not a driving factor for incumbents, 93 percent of whom had been employed for at least two years. Only seven percent of incumbents were included in the first two tenure categories. I find significant differences (Figure 7) for entrants whose overall tenure is, by construction, lower (Table A.4.5). The employment effects are largest (+135 days) for entrants who had worked for at least two years at the beginning of the quarter of potential treatment, but there are no effects on earnings. I find significant earnings effects only for entrants employed from 184 to 730 days at the beginning of a quarter, i.e., workers who had passed the probationary period but who are still within the two-year period when temporary contracts can legally be concluded. Thus, training seems most financially beneficial for compliers who are still temporarily employed, giving them the opportunity to distinguish themselves from other workers.

[7] The results for negative tenure durations are, by construction, only based on observations of entrants. Therefore, Figure 7 does not explicitly depict them again.

Figure 7: LATE and OLS estimates for participation in the subsidy scheme by tenure and worker type



a) Workers already employed before program introduction (incumbents)



b) Workers employed after program introduction (entrants)

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. First-stage (F-test) values for employment within one year: *Incumbents:* 1-183 days: 1.597 [0.21] (59.604), 184 to 730 days: 1.318 [0.14] (93.321), >730 days: 0.527 [0.17] (7.836). *Entrants:* 1-183 days: 0.648 [0.13] (26.737), 184 to 730 days: 0.659 [0.15] (18.821), >730 days: 0.711 [0.21] (11.119). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

6.5.2 Dynamic treatment effects

Having observed some heterogeneous effects by tenure, I test whether accounting for the dynamics of treatment alters the overall results in the next step (Table 8). Therefore, I create a dataset that enables me to estimate LATEs for every quarter of potential treatment since the first risk exposure. Similarly to Sianesi (2004), I ensure that control workers in a given quarter can become participants any time unless their employment spell with a given employer ends. I conduct 16 separate estimations for each quarter after the exposure to the treatment risk for both incumbents and entrants. The reported aggregated estimates are weighted averages of these 16 treatment effects, where the weight of each quarterly estimate is the fraction of treated workers per quarter of all treated workers: $\hat{w_q} = \frac{\sum treated_q}{\sum treated} \quad with \quad q = quarter$. Standard errors are bootstrapped.

Compared with the previous findings, I must now consider two factors. First, if using the previous, i.e., static, approach I were to condition on shorter employment durations for workers in the control group, the estimates in the static framework would be overstated and exceed those of the dynamic framework. Second, estimating dynamic treatment effects implies that I must interpret the coefficients as the effect of treatment versus waiting for compliers, which differs from the interpretation of the previous coefficients. Dynamic coefficients are more difficult to interpret, as what they measure is unclear. Control workers can be future participants and such treatment would affect their future outcomes, attenuating the treatment effect. If the effectiveness of the program increases (decreases) over time, this attenuation bias is amplified (reduced).

Comparing the dynamic estimates of Table 8 with the static ones of Table 5 reveals that regarding employment, the dynamic estimates are slightly larger for both incumbents and entrants. Thus, all approaches yield very similar results; however, the static approach provides more conservative estimates. Regarding earnings, the static approach generates an earnings effect of six percent. In the dynamic framework, I find an earnings effect of 8 percent in the three years after treatment for incumbents. I find an effect of +5 percent for entrants, but these coefficients are statistically insignificant. Thus, on the one hand, the results of the static framework might be slightly overstated for entrants. On the other hand, the dynamic effects might be attenuated because training is more efficient for workers who participate later in their employment spell. This is supported by the previous findings that program efficiency varies with tenure and is particularly high for workers who have been employed between 0.5 and 2 years.

Table 8: Dynamic LATE and OLS estimates for participation in the subsidy program on cumulative employment and log earnings (daily average)

	(1)		(2)	
	Employment		**log Earnings**	
	2SLS	OLS	2SLS	OLS
Workers already employed before program introduction (incumbents)				
1st year	13.279***	18.645***	0.088***	0.083***
	(5.23)	(0.67)	(0.03)	(0.004)
2nd year	26.727***	34.102***	0.086***	0.087***
	(9.87)	(1.66)	(0.04)	(0.01)
3rd year	36.081*	46.914***	0.079**	0.085***
	(18.73)	(2.77)	(0.04)	(0.01)
First-stage results for observations included in the 1st and 2nd year, dependent variable: treatment dummy				
Con. policy style	1.188***		1.194***	
	(0.08)		(0.08)	
N	255,774	255,774	251,088	251,088
N(Participants)	52,862	52,862	52,755	52,755
Workers employed after program introduction (entrants)				
1st year	15.101***	23.046***	0.040	0.110***
	(6.07)	(1.09)	(0.04)	(0.008)
2nd year	23.645***	38.173***	0.008	0.109***
	(10.63)	(2.86)	(0.04)	(0.01)
3rd year	43.128**	55.338***	0.054	0.122***
	(20.34)	(4.88)	(0.05)	(0.01)
First-stage results for observations included in the 1st and 2nd year, dependent variable: treatment dummy				
Con. policy style	0.649***		0.666***	
	(0.13)		(0.13)	
N	142,864	142,864	137,728	137,728
N(Participants)	24,813	24,813	24,645	24,645

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. *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 in parentheses are bootstrapped (50 repetitions) and clustered at the level of 176 local employment agency districts. Significance level: *** 1%, ** 5%, * 10%.

7 Cost-benefit considerations

In this section, I provide a rough cost-benefit analysis at the participant level for the first three years after entry into the program. According to statistics published by the FEA, the program registered approximately 183,751 entries into the program (cost reimbursement and wage subsidy) (Table 9, Panel A, Line 1). However, as approximately 60 percent of workers received both benefits, and 40 percent received either cost reimbursement or a wage subsidy, the number of actual participants is approximately 30 percent smaller than the number of inflows (Panel A, Line 2).

The only cost information available are the program costs, i.e., actual expenses of all employment agencies each year provided by the FEA controller. Between 2007 and 2010, total expenses for the subsidy program, i.e., expenses due to wage subsidies and reimbursements for training costs, amounted to approximately 626 million euros (Table 9, Panel B), which corresponds to per capita costs of approximately 4,867 euros. Over a two-year period, this amounts to a daily cost of 6.67 euros per person.

Table 9: 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	72	68	79	74	74
Effect on earnings after two years	0.11	0.09	0.06	0.06	0.07
Average additional earnings per day over two years	7.90	6.12	4.74	4.44	5.18

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.

On the benefit side, I assume that workers receive the same average daily wage of approximately 74 euros as during the three years before participation (Table 9, Panel C). Multiplying these daily wages by the estimated LATE on earnings from Table 5 and Figure 2 yields an increase in daily per capita earnings of approximately 5.18 euros. Thus, comparing daily per capita costs (Table 9, Panel B, last Line) and daily per capita benefits (Table 9, Panel C, last Line) over a two-year period, on average, the program does not seem to pay off. However, this differs by year such that for 2007 and 2008 benefits might actually exceed costs. Moreover, assuming that the positive effect on earnings is permanent and persists beyond two years, e.g., five or ten years, the subsidy program 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 for 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

approach those costs, except for 2007 and 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.[8]

It is difficult to judge the adequacy of these estimates. Further gains in employment or earnings beyond a two-year horizon imply an improvement of the results. Moreover, from the social perspective, one has to add potential gains to employers and public gains through reduced benefit transfers. However, I have information on neither the administrative costs of employing caseworkers to implement the subsidy program nor the cost of the drop in firm productivity during the training period.

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 workers rather than unemployed workers. For identification, I rely on an IV approach, exploiting conditional regional variation in the intensity of subsidy awards 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 LATEs.

The evidence suggests that the subsidy improved the labor market outcomes (employment and earnings) of subsidy recipients. For compliers, I find positive effects of 28 more days of employment, an increase in earnings by 6.2 percent, and no effect on the receipt of unemployment benefits over a period of three years. Given that Haelermans and Borghans (2012) report an average return to privately funded training of 3.5 percent, these estimates are nearly twice as large as those reported in the literature. The effect on cumulative employment is negligible in economic terms. However, there is substantial heterogeneity across groups of compliers. In particular, workers starting program participation in 2007 profit more in terms of employment and earnings than do later cohorts. This is related to a compositional change in the participants, which was triggered by the economic crisis and adjustments in FEA regulations. As a consequence, low-skilled workers who entered the program later had more favorable characteristics and therefore gained relatively little from the subsidy program. Further beneficiaries of the program include women, younger workers, and non-Germans. Complementing the analysis with several robustness checks, I conduct additional dynamic estimations in the spirit of dynamic matching approaches (Fredriksson and Johansson, 2008; Sianesi, 2004). The resulting estimates support the robustness of the previous findings.

From a political perspective, the results of this study suggest that targeting females, younger workers, and non-Germans might increase the subsidy program's efficiency. In fact, recent adjustments by the FEA emphasize these groups (training in the female-dominated occupation of elderly care and focusing on younger workers since April 2012. Currently, drawing

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

refugees into the program is being carefully considered. In this sense, my study provides ex post justification for these adjustments.

The program has the potential to improve the labor market chances of low-skilled workers who usually face high unemployment risk, particularly for long-term unemployment (Bundesagentur für Arbeit, 2015). Thus, complementing programs for unemployed low-skilled workers, this subsidy program may efficiently prevent unemployment among low-skilled rather than stepping in when the damage is already done. Thus, saved benefit transfers and potential positive spillover effects on coworkers (De Griep and Sauermann, 2012) should also be taken into account should data that allow for deeper cost-benefit analysis become available.

Evaluations of training subsidies for employed workers exist for the German training premium (Bildungsprämie), which subsidizes general training courses for employees in the form of vouchers, as well as for programs in Sweden (Schwerdt et al., 2012), and the Netherlands (Hidalgo et al., 2014). In contrast to these studies, which 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, as training is a rather general term. Moreover, these programs have no direct link to participants' jobs, and these newly acquired skills might be less applicable to that job.

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Appendix

Table A.4.1: 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	39.28	40.48	-1.20	0.00
Age^2	1,629.70	1,758.43	-128.73	0.00
Immigrant	0.16	0.09	0.07	0.00
Degree info missing	0.01	0.05	-0.04	0.00
No degree	0.38	0.14	0.24	0.00
Vocational degree	0.54	0.59	-0.04	0.00
A-levels	0.02	0.02	-0.00	0.00
A-levels and vocational degree	0.03	0.08	-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	0.08	0.20	0.00
No school degree	0.07	0.04	0.03	0.00
Lower secondary degree (Hauptschule)	0.36	0.33	0.03	0.00
Medium secondary degree (Realschule)	0.22	0.33	-0.11	0.00
Higher secondary degree (Fachhochschulreife)	0.03	0.07	-0.04	0.00
Abitur	0.04	0.15	-0.11	0.00
Unskilled blue collar worker	0.59	0.25	0.35	0.00
Skilled blue collar worker	0.23	0.22	0.02	0.00
Master craftsman	0.00	0.01	-0.01	0.00
White collar worker	0.17	0.53	-0.35	0.00
No unskilled worker (1 year prior)	0.78	0.82	-0.04	0.00
Unskilled worker (1 year prior)	0.14	0.06	0.07	0.00
Info if unskilled worker missing (1 year prior)	0.09	0.12	-0.03	0.00
Employed before	0.93	0.93	0.01	0.00
Tenure without interruption since 1/2000	1,627.81	1,428.12	199.69	0.00
Tenure without interruption since 1/2000 ^{2}	4,300,487.43	3,639,209.75	661,277.69	0.00
Employment in prior 3 years	950.51	920.41	30.09	0.00
Employment in prior 3 years^2	977,442.42	937,659.28	39,783.15	0.00
Number of employment spells in prior 3 years	1.26	1.28	-0.02	0.00
Benefit receipt in prior 3 years	34.00	38.64	-4.64	0.00
Benefit receipt in prior 3 years^2	10,772.49	15,294.56	-4,522.07	0.00
Number of benefit periods in prior 3 years	0.29	0.29	0.00	0.72
Unemployed job search in prior 3 years	56.98	56.22	0.76	0.18
Unemployed job search in prior 3 years^2	22,659.94	23,572.57	-912.63	0.01
Number of unemployed job search periods in prior 3 years	0.55	0.51	0.04	0.00
Welfare receipt in prior 3 years	56.29	52.18	4.11	0.00
Welfare receipt in prior 3 years^2	35,527.67	37,174.55	-1,646.87	0.02
Number of welfare periods in prior 3 years	0.22	0.19	0.04	0.00
Average daily benefits in prior 3 years	4.06	4.07	-0.01	0.79
Average daily wage conditional on employment in prior 3 years	73.98	82.65	-8.67	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	0.10	0.13	0.00
Technical profession in manufacturing	0.16	0.14	0.01	0.00
Profession in construction	0.06	0.07	-0.01	0.00
Profession in food, hotel/ restaurant industry	0.03	0.05	-0.02	0.00
Profession in medical and non-medical health care	0.03	0.07	-0.03	0.00
Profession in humanities and arts	0.04	0.05	-0.02	0.00
Profession in retail and trade	0.03	0.08	-0.04	0.00
Profession in management and organisation	0.05	0.17	-0.12	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	0.11	0.16	0.00

Continued on next page...

... table A.4.1 continued

	Treated	Comparisons	Difference	p-Value
Profession in cleaning	0.01	0.01	-0.00	0.00
N	78,925	325,571		

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

Table A.4.2: Establishment characteristics for the aggregate sample

	Treated	Comparisons	Difference	p-Value
Firmsize 1-25 workers	0.16	0.30	-0.14	0.00
Firmsize 26-100 workers	0.24	0.24	0.00	0.07
Firmsize 101-500 workers	0.34	0.26	0.08	0.00
Firmsize >500 workers	0.26	0.19	0.06	0.00
Missing industry	0.00	0.00	-0.00	0.00
Farming, forestry, fishing	0.01	0.01	-0.01	0.00
Mining	0.00	0.00	-0.00	0.00
Production	0.41	0.25	0.16	0.00
Energy	0.00	0.01	-0.01	0.00
Watery	0.02	0.01	0.01	0.00
Construction	0.05	0.07	-0.02	0.00
Trade	0.08	0.14	-0.06	0.00
Transportation and storage	0.13	0.05	0.08	0.00
Hotel and restaurant industry	0.01	0.03	-0.02	0.00
Information and communication	0.02	0.04	-0.02	0.00
Financial and insurance services	0.00	0.04	-0.03	0.00
Estate and housing	0.04	0.05	-0.01	0.00
Freelance, scientific and technical services	0.01	0.05	-0.04	0.00
Other economic services	0.13	0.07	0.06	0.00
Public administration, social insurance	0.01	0.04	-0.04	0.00
Education	0.01	0.02	-0.02	0.00
Health and welfare	0.06	0.09	-0.03	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
Exterritorial organizations	0.00	0.00	0.00	0.00
0-10 years	0.03	0.04	-0.01	0.00
>10-20 years	0.24	0.27	-0.03	0.00
>20-30 years	0.11	0.11	-0.00	0.28
>30 years	0.63	0.58	0.04	0.00
0-0.05 % lowskilled workers in firm	0.26	0.45	-0.20	0.00
0.05-0.1 % lowskilled workers in firm	0.13	0.16	-0.02	0.00
0.1-0.2 % lowskilled workers in firm	0.24	0.19	0.05	0.00
>0.2 % lowskilled workers in firm	0.37	0.20	0.17	0.00
N	78,925	325,571		

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

Table A.4.3: 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	552.61	732.30	-179.69	0.00
Female population density per square km	50.88	50.96	-0.09	0.00
% of population aged 0-2 years	2.49	2.53	-0.03	0.00
% of population aged 3-5 years	2.56	2.56	0.01	0.00
% of population aged 6-9 years	3.68	3.60	0.08	0.00
% of population aged 10-14 years	5.00	4.80	0.20	0.00
% of population aged 15-17 years	3.15	3.00	0.16	0.00
% of population aged 18-19 years	2.34	2.26	0.08	0.00
% of population aged 20-24 years	6.02	6.04	-0.02	0.00
% of population aged 25-29 years	6.00	6.21	-0.21	0.00
% of population aged 30-34 years	5.72	5.93	-0.21	0.00
% of population aged 35-39 years	6.53	6.63	-0.09	0.00
% of population aged 40-44 years	8.43	8.45	-0.02	0.00
% of population aged 45-49 years	8.54	8.48	0.06	0.00
% of population aged 50-54 years	7.37	7.29	0.08	0.00
% of population aged 55-59 years	6.57	6.54	0.03	0.00
% of population aged 60-64 years	5.24	5.29	-0.06	0.00
% of population aged 65-74 years	11.53	11.65	-0.11	0.00
% of population aged >75 years	8.83	8.76	0.07	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	0.86
Bremen	0.01	0.01	-0.00	0.00
Nordrhein-Westfalen	0.20	0.21	-0.01	0.00
Hessen	0.06	0.08	-0.01	0.00
Rheinland-Pfalz	0.06	0.04	0.01	0.00
Baden-Württemberg	0.17	0.14	0.03	0.00
Bayern	0.21	0.16	0.05	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	0.59
Unemployment rate	8.66	9.38	-0.72	0.00
Unemployment rate^2	90.61	104.67	-14.06	0.00
% of workforce aged 15-19 years	4.02	3.71	0.30	0.00
% of workforce aged 20-24 years	8.93	8.87	0.06	0.00
% of workforce aged 25-29 years	9.61	9.87	-0.26	0.00
% of workforce aged 30-34 years	9.22	9.48	-0.26	0.00
% of workforce aged 35-39 years	11.35	11.54	-0.19	0.00
% of workforce aged 40-44 years	14.80	14.84	-0.04	0.00
% of workforce aged 45-49 years	14.48	14.33	0.15	0.00
% of workforce aged 50-54 years	11.67	11.55	0.12	0.00
% of workforce aged 55-59 years	9.12	9.11	0.01	0.03
% of workforce aged 60-64 years	4.08	4.09	-0.01	0.00
% of workforce aged >=65 years	2.68	2.56	0.12	0.00
% of workforce unskilled	24.60	25.49	-0.88	0.00
% of workforce highly skilled	7.68	8.82	-1.15	0.00
% of workforce medium-skilled	54.57	53.62	0.95	0.00
% of workforce low-skilled	13.15	12.07	1.08	0.00
% in firms size 1-5 workers	11.25	11.03	0.22	0.00
% in firms size 6-9 workers	6.52	6.31	0.21	0.00
% in firms size 10-19 workers	9.67	9.34	0.33	0.00
% in firms size 20-49 workers	14.13	13.78	0.35	0.00
% in firms size 50-99 workers	11.94	11.82	0.12	0.00
% in firms size 100-499 workers	26.36	26.36	-0.00	0.89
% in firms size <500 workers	20.12	21.35	-1.23	0.00
% in sector farming, forestry, fishing	0.82	0.79	0.03	0.00
% in sector mining	0.37	0.33	0.05	0.00

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... table A.4.3 continued

	Treated	Comparisons	Difference	p-Value
% in sector production	25.87	23.01	2.86	0.00
% in sector energy	0.82	0.85	-0.04	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	14.43	14.40	0.03	0.00
% in sector transportation and storage	4.81	5.10	-0.30	0.00
% in sector hotel and restaurant industry	2.84	2.94	-0.10	0.00
% in sector information and communication	2.41	3.02	-0.61	0.00
% in sector financial and insurance services	3.24	3.67	-0.43	0.00
% in sector estate and housing	0.62	0.77	-0.15	0.00
% in sector freelance, scientific and technical services	4.75	5.56	-0.81	0.00
% in sector other economic services	5.66	6.22	-0.56	0.00
% in sector public administration, social insurance	5.82	6.02	-0.20	0.00
% in sector education	3.61	3.78	-0.17	0.00
% in sector health and welfare	13.48	13.08	0.40	0.00
% in sector art, entertainment and recreation	0.73	0.81	-0.08	0.00
% in sector other services	2.71	2.93	-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	0.05	-0.04	0.00
2007Q2	0.07	0.07	-0.00	0.00
2007Q3	0.12	0.08	0.05	0.00
2007Q4	0.04	0.05	-0.01	0.00
2008Q1	0.02	0.06	-0.03	0.00
2008Q2	0.06	0.07	-0.01	0.00
2008Q3	0.14	0.08	0.07	0.00
2008Q4	0.03	0.05	-0.02	0.00
2009Q1	0.04	0.06	-0.01	0.00
2009Q2	0.06	0.07	-0.01	0.00
2009Q3	0.09	0.07	0.01	0.00
2009Q4	0.03	0.05	-0.02	0.00
2010Q1	0.07	0.05	0.02	0.00
2010Q2	0.08	0.06	0.02	0.00
2010Q3	0.06	0.07	-0.01	0.00
2010Q4	0.05	0.05	0.00	0.05
N	78,925	325,571		

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

Table A.4.4: Means of selected individual characteristics for subgroups of participants

	By year of participation				By gender		By age				By nationality	
	2007	2008	2009	2010	Men	Women	20-30 years	30-40 years	40-45 years	45-65 years	German	Non-German
Female	0.23	0.20	0.20	0.26	0.00	1.00	0.17	0.18	0.23	0.27	0.22	0.18
Age	39.01	38.58	39.82	39.34	38.81	41.05	26.43	35.21	42.52	50.78	39.56	37.83
Immigrant	0.18	0.17	0.15	0.17	0.17	0.14	0.16	0.21	0.15	0.11	0.00	1.00
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
Degree info missing	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.01
No degree	0.45	0.41	0.35	0.36	0.38	0.39	0.39	0.36	0.38	0.41	0.33	0.67
Vocational degree	0.48	0.53	0.57	0.55	0.55	0.52	0.54	0.55	0.56	0.52	0.60	0.25
A-levels	0.01	0.02	0.01	0.03	0.02	0.02	0.02	0.02	0.01	0.01	0.01	0.03
A-levels and vocational degree	0.02	0.03	0.03	0.03	0.03	0.04	0.03	0.04	0.02	0.02	0.03	0.02
Polytechnical degree	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.02	0.01	0.01
University degree	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.01
School degree info missing	0.27	0.25	0.33	0.26	0.29	0.29	0.08	0.26	0.36	0.42	0.28	0.30
No school degree	0.06	0.08	0.06	0.07	0.07	0.05	0.10	0.07	0.06	0.05	0.05	0.16
Lower secondary degree (Hauptschule)	0.38	0.39	0.33	0.37	0.38	0.29	0.49	0.37	0.32	0.29	0.36	0.34
Medium secondary degree (Realschule)	0.23	0.23	0.22	0.22	0.21	0.29	0.28	0.23	0.21	0.19	0.24	0.12
Higher secondary degree (Fachhochschulreife)	0.02	0.02	0.03	0.03	0.03	0.04	0.03	0.03	0.02	0.02	0.03	0.02
Abitur	0.03	0.03	0.03	0.05	0.03	0.05	0.03	0.04	0.03	0.03	0.03	0.05
Unskilled blue collar worker	0.62	0.65	0.55	0.56	0.61	0.52	0.64	0.60	0.58	0.55	0.56	0.76
Skilled blue collar worker	0.24	0.21	0.27	0.18	0.27	0.09	0.22	0.23	0.23	0.25	0.25	0.15
Master craftsman	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.00	0.00
White collar worker	0.14	0.14	0.17	0.25	0.11	0.39	0.14	0.17	0.18	0.19	0.19	0.09
Employed before	0.91	0.91	0.96	0.94	0.94	0.92	0.89	0.94	0.95	0.95	0.94	0.93
Employment in prior 3 years	902.77	908.50	990.26	964.79	957.44	924.89	841.31	951.30	989.96	999.06	955.80	923.07
Tenure since 1/2000	1,343.52	1,425.81	1,826.56	1,730.30	1,633.29	1,607.59	873.88	1,557.46	1,872.95	2,069.33	1,641.95	1,554.54
Unemployed job search in prior 3 years	85.52	76.75	37.81	45.49	57.50	55.03	88.48	59.06	44.34	41.22	55.41	65.08
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	0.02
Profession in farming/ gardening	0.01	0.02	0.01	0.02	0.02	0.00	0.02	0.02	0.01	0.01	0.01	0.02
Profession in manufacturing	0.25	0.24	0.26	0.16	0.27	0.12	0.21	0.22	0.24	0.27	0.23	0.30
Technical profession in manufacturing	0.17	0.13	0.18	0.12	0.15	0.18	0.14	0.15	0.16	0.17	0.16	0.15
Profession in construction	0.05	0.06	0.06	0.06	0.07	0.00	0.08	0.06	0.05	0.04	0.06	0.05
Profession in food, hotel/ restaurant industry	0.02	0.03	0.02	0.04	0.02	0.05	0.03	0.02	0.02	0.03	0.02	0.04
Profession in medical and non-medical health care	0.03	0.03	0.03	0.07	0.01	0.11	0.04	0.03	0.03	0.03	0.03	0.03
Profession in humanities and arts	0.03	0.03	0.03	0.06	0.01	0.13	0.03	0.03	0.04	0.04	0.04	0.02
Profession in retail and trade	0.02	0.03	0.03	0.04	0.02	0.07	0.03	0.03	0.03	0.04	0.03	0.03
Profession in management and organisation	0.05	0.05	0.05	0.06	0.03	0.13	0.05	0.05	0.05	0.06	0.06	0.02
Profession in business-related services	0.01	0.01	0.01	0.01	0.00	0.02	0.01	0.01	0.01	0.01	0.01	0.00
Profession in IT and natural science services	0.03	0.02	0.04	0.04	0.03	0.03	0.03	0.04	0.03	0.03	0.04	0.02
Profession in security	0.04	0.03	0.02	0.03	0.03	0.02	0.04	0.03	0.03	0.03	0.03	0.02
Profession in transportation and logistics	0.26	0.31	0.24	0.25	0.31	0.10	0.27	0.29	0.26	0.23	0.26	0.27
Profession in cleaning	0.01	0.01	0.01	0.02	0.01	0.02	0.01	0.01	0.01	0.01	0.01	0.02
Training duration	138.05	123.99	139.07	188.18	125.29	206.17	157.45	156.62	141.05	116.84	142.95	140.33
N	12,205	21,453	32,870	12,397	62,106	16,819	15,727	25,274	15,831	22,093	66,162	12,763

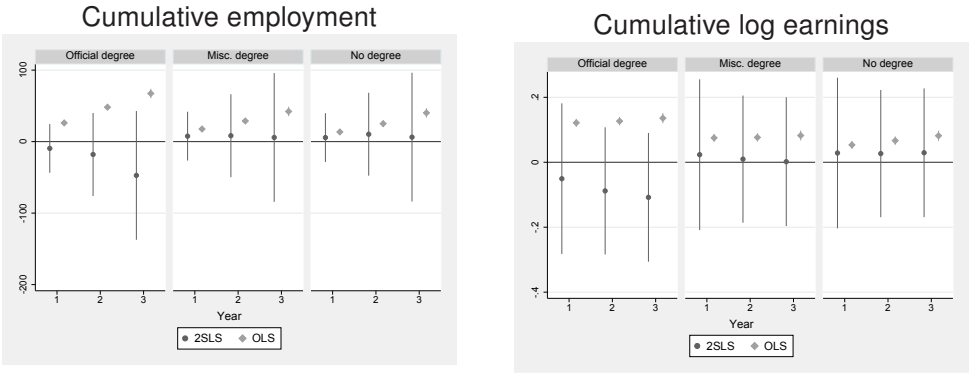
Source: IEB V11.00 - 131009. Own calculations.

Table A.4.5: Means of selected individual characteristics for subgroups of participants

	By firm size				By tenure									
					All				Employed before 2007			Employed after 2007		
	1-25	26-100	101-500	> 500	-89 - -1	1 - 183	184 - 730	> 730	1 - 183	184 - 730	> 730	1 - 183	184 - 730	> 730
Female	0.19	0.21	0.22	0.22	0.24	0.22	0.22	0.21	0.33	0.22	0.21	0.22	0.22	0.27
Age	37.11	38.52	39.81	40.67	35.35	36.09	36.60	40.93	34.63	36.35	41.11	36.13	36.69	36.96
Immigrant	0.13	0.15	0.17	0.19	0.18	0.18	0.17	0.15	0.21	0.18	0.15	0.18	0.16	0.18
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	0.00	0.00	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.01
No degree	0.32	0.35	0.40	0.43	0.38	0.35	0.34	0.40	0.40	0.37	0.41	0.35	0.33	0.32
Vocational degree	0.58	0.56	0.53	0.52	0.51	0.55	0.56	0.54	0.50	0.55	0.54	0.56	0.57	0.56
A-levels	0.02	0.02	0.02	0.01	0.03	0.02	0.03	0.01	0.03	0.02	0.01	0.02	0.03	0.04
A-levels and vocational degree	0.04	0.03	0.03	0.02	0.05	0.04	0.04	0.02	0.03	0.03	0.02	0.04	0.04	0.05
Polytechnical degree	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	0.02
University degree	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	0.02
School degree info missing	0.14	0.20	0.30	0.44	0.03	0.09	0.10	0.39	0.10	0.10	0.41	0.09	0.10	0.11
No school degree	0.08	0.08	0.07	0.05	0.11	0.10	0.09	0.05	0.08	0.09	0.05	0.10	0.09	0.09
Lower secondary degree (Hauptschule)	0.42	0.41	0.35	0.29	0.50	0.46	0.45	0.31	0.47	0.47	0.30	0.46	0.45	0.41
Medium secondary degree (Realschule)	0.28	0.24	0.22	0.18	0.27	0.27	0.27	0.20	0.26	0.27	0.19	0.27	0.27	0.29
Higher secondary degree (Fachhochschulreife)	0.04	0.03	0.03	0.02	0.04	0.04	0.03	0.02	0.04	0.03	0.02	0.04	0.04	0.04
Abitur	0.05	0.04	0.03	0.02	0.06	0.05	0.05	0.03	0.05	0.04	0.03	0.05	0.05	0.07
Unskilled blue collar worker	0.48	0.53	0.62	0.68	0.62	0.59	0.60	0.59	0.63	0.65	0.59	0.59	0.59	0.54
Skilled blue collar worker	0.29	0.26	0.20	0.22	0.19	0.22	0.22	0.24	0.16	0.21	0.25	0.23	0.22	0.20
Master craftsman	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
White collar worker	0.23	0.20	0.17	0.10	0.19	0.18	0.17	0.17	0.21	0.13	0.16	0.18	0.19	0.26
Employed before	0.86	0.92	0.94	0.98	0.39	0.86	0.96	0.99	0.93	0.96	0.99	0.86	0.96	0.99
Employment in prior 3 years	844.01	906.08	960.60	1,046.27	585.41	688.73	794.34	1,072.79	601.82	787.39	1,076.34	691.36	796.61	992.84
Tenure since 1/2000	1,044.64	1,304.96	1,709.07	2,193.18	-41.68	80.59	429.74	2,375.75	122.04	499.75	2,440.89	79.33	406.85	909.07
Unemployed job search in prior 3 years	99.88	76.90	53.65	15.48	206.64	157.58	119.46	8.36	200.42	135.58	7.25	156.28	114.19	33.37
Helper/ no profession	0.00	0.01	0.01	0.02	0.00	0.00	0.01	0.01	0.00	0.01	0.01	0.00	0.01	0.00
Profession in farming/ gardening	0.05	0.02	0.01	0.00	0.01	0.02	0.02	0.01	0.00	0.02	0.01	0.02	0.02	0.02
Profession in manufacturing	0.13	0.17	0.26	0.34	0.20	0.18	0.17	0.27	0.15	0.16	0.28	0.18	0.18	0.14
Technical profession in manufacturing	0.10	0.11	0.14	0.26	0.11	0.13	0.13	0.17	0.26	0.14	0.17	0.13	0.12	0.14
Profession in construction	0.12	0.08	0.04	0.02	0.05	0.07	0.07	0.05	0.06	0.08	0.05	0.07	0.07	0.06
Profession in food, hotel/ restaurant industry	0.03	0.02	0.03	0.02	0.03	0.03	0.03	0.02	0.02	0.02	0.02	0.03	0.03	0.05
Profession in medical and non-medical health care	0.04	0.05	0.03	0.01	0.05	0.05	0.04	0.03	0.02	0.03	0.03	0.05	0.04	0.05
Profession in humanities and arts	0.06	0.07	0.03	0.01	0.05	0.06	0.05	0.03	0.05	0.04	0.02	0.06	0.06	0.06
Profession in retail and trade	0.04	0.03	0.04	0.02	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.05
Profession in management and organisation	0.08	0.05	0.05	0.04	0.09	0.06	0.05	0.05	0.12	0.05	0.05	0.05	0.05	0.07
Profession in business-related services	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.02
Profession in IT and natural science services	0.03	0.02	0.03	0.05	0.02	0.02	0.03	0.04	0.00	0.02	0.04	0.02	0.03	0.04
Profession in security	0.02	0.03	0.04	0.03	0.03	0.04	0.04	0.02	0.02	0.05	0.02	0.05	0.04	0.03
Profession in transportation and logistics	0.29	0.32	0.28	0.18	0.30	0.29	0.31	0.24	0.27	0.32	0.24	0.29	0.30	0.24
Profession in cleaning	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.00	0.01	0.01	0.01	0.02	0.02
Training duration	147.46	147.23	140.29	137.93	183.71	141.03	144.70	137.99	59.67	122.81	135.57	143.49	151.85	192.57
N	12,688	19,169	26,900	20,168	5,218	8,379	13,970	51,358	246	3,442	49,174	8,133	10,528	2,184

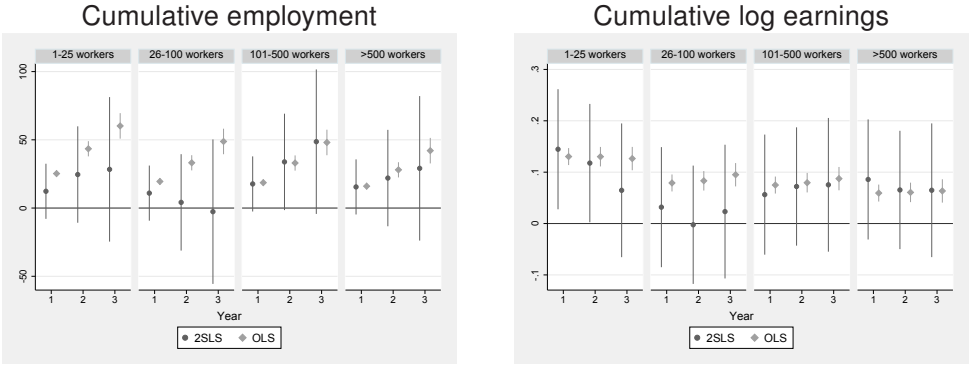
Source: IEB V11.00 - 131009. Own calculations.

Figure A.4.1: LATE and OLS estimates for participation in the subsidy scheme by aspired degree



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. First-stage (F-test) values for employment within one year: No degree: 0.199 [0.04] (19.794), Misc. degree: 0.671 [0.12] (31.760) No degree: 0.479 [0.09] (27.584). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

Figure A.4.2: LATE and OLS estimates for participation in the subsidy scheme by firm size



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. First-stage (F-test) values for employment within one year: 1-25 workers: 0.463 [0.11] (19.568), 26-100 workers: 0.641 [0.13] (26.565), 101-500 workers: 0.718 [0.24] (8.366), >500 workers: 0.532 [0.14] (10.145). Standard errors clustered at the level of 176 local employment agency districts and individuals in parentheses. Confidence intervals indicate significance at the 5% level.

Table A.4.6: LATE for participation in the subsidy program on cumulated employment within two years controlling for regional characteristics

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS
Treated	60.899***	59.423***	59.450***	61.422***	59.050***	60.368***	60.658***	57.290***
	(6.04)	(5.34)	(4.84)	(5.46)	(5.03)	(5.23)	(5.58)	(5.46)
Quarter-year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment rate	-	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bundesland	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Population characteristics	-	-	-	Yes	Yes	Yes	Yes	Yes
Age distribution of workforce	-	-	-	-	Yes	Yes	Yes	Yes
Skill distribution of workforce	-	-	-	-	-	Yes	Yes	Yes
Firm size distribution of workforce	-	-	-	-	-	-	Yes	Yes
Industry distribution of workforce	-	-	-	-	-	-	-	Yes
First-stage results, dependent variable: treatment dummy								
Conditional policy style	0.939***	0.929***	0.923***	0.877***	0.890***	0.891***	0.894***	0.892***
	(0.24)	(0.24)	(0.24)	(0.23)	(0.23)	(0.22)	(0.22)	(0.21)
F-test of excl. instr.	15.209	14.808	14.405	14.988	15.641	15.881	16.309	17.392
R-squared	0.011	0.012	0.012	0.013	0.013	0.013	0.013	0.014
N	404,496	404,496	404,496	404,496	404,496	404,496	404,496	404,496
N (participants)	78,925	78,925	78,925	78,925	78,925	78,925	78,925	78,925

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. All variables are measured on the level of local employment agency districts. *Population char.* comprise the population density (also by gender/ age groups). Standard errors (in parentheses) are clustered at the level of 176 local employment agency districts. Significance level: *** 1%, ** 5%, * 10%.

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