Assignment Mechanisms, Selection Criteria, and the Effectiveness of Training Programmes*

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Abstract

We analyse the effectiveness of vocational training under two different assignment mechanisms. The direct assignment mechanism is characterised by the strong influence of caseworkers who can directly assign the unemployed to vocational training courses. Under the voucher assignment mechanism unemployed have more freedom to choose among different courses and training providers. Simultaneously with the assignment mechanism the selection criteria for potential training participants is changed. Unemployed awarded with a voucher are supposed to have higher employment probabilities after training than unemployed directly assigned to a training programme. We find that the voucher assignment system reduces the returns to vocational training over the short term. These negative effects fade and eventually, after seven years, become positive. The stricter selection rules appear to be poorly constructed and to reduce the effectiveness of training.

JEL-Classification: J68, H43, C21

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1 Introduction

Numerous countries implement active labour market policies (ALMPs) to reintegrate unemployed individuals at the labour market. The effectiveness of these programmes is evaluated in a large body of literature that presents mixed empirical evidence (see Card, Kluve, and Weber, 2010, for a recent review). Instead of evaluating the effectiveness of a programme itself, we investigate the different mechanisms that may influence the success of ALMP programmes. In particular, in this paper, we focus on the effectiveness of different assignment systems for allocating unemployed individuals to ALMP programmes.

Assignment systems may vary substantially between programmes and across countries. They can range from systems in which unemployed persons are directly assigned to courses by caseworkers to systems in which the recipients make individual course choices. The degree of the freedom of choice ranges substantially by assignment system. Surprisingly, the effect of the assignment system on the effectiveness of ALMP programmes is not well studied in the literature. Although it is possible to draw some conclusions from the literature on schooling vouchers and the effect of choice with respect to educational decisions (e.g., Cullen, Jacob, and Levitt, 2006, Attanasio, Meghir, and Santiago, 2012), the results from this literature are not transferable to adult education in all dimensions. The effectiveness of providing more choices through, e.g., a voucher system, depends critically on alignment between the consumer preferences and efficiency goals of the society, even under perfect information and markets (Rothenberg, 1962). When parents make educational choices for their children, these choices are usually in line with the goals of the society. Both are interested in the transmission of knowledge to the younger generation. For adult education, however, the consumer preferences and efficiency goals of the society do not necessarily coincide. For example, training courses for unemployed persons have typically two major goals: rapid reintegration into the labour market and human capital accumulation. These goals may conflict because fast reintegration might not allow human capital to accumulate. It is unclear whether the relative importance of these two goals is the same for training participants and the society. Possibly, unemployed individuals increase their preference for human capital accumulation when they are allowed to choose the course that is most convenient for them. They might become more patient in finding a new job. In this scenario, an assignment system that allows for more freedom of choice may lead to reduced job search intensity and increased reservation wages of participants compared with a system that provides fewer choices. Accordingly, the mechanisms affecting adult and child education vouchers differ in important dimensions. This underlines the relevance of this study for closing the research gap on the impact of assignment mechanisms on the effectiveness of ALMPs.

We exploit a substantial reform of allocation in vocational training programmes for unemployed individuals in Germany. A voucher system replaced the direct assignment of unemployed persons to specific courses by caseworkers in January 2003. So-called training vouchers were introduced to introduce market mechanisms to the training market and to increase the freedom of choice and self-responsibility of programme participants. Coupled with the introduction of training vouchers, stricter selection criteria for programme participants were implemented. Under the pre-2003 regime, caseworkers assigned training based on subjective measures. According to the new selection criteria, at least 70% of all course participants should be re-employed within six months after completing training.

We investigate the effectiveness of the voucher assignment system in terms of post-participation employment probabilities and earnings. We exploit rich administrative data for *all* individuals who participated in vocational training programmes during the 2001-2004 period. Numerous studies evaluate the effectiveness of vocational training programmes under a specific assignment system. For example, Doerr et al. (2013) and Heinrich et al. (2010) investigate the returns to vocational training programmes under a voucher assignment system. They find that vocational training vouchers increase the labour market opportunities of programme participants over the long run. In contrast to these studies, we evaluate the effectiveness of the assignment system. This means that we compare the effectiveness of vocational training programmes under the voucher and the direct assignment systems. To date, this comparison has only been considered in Rinne, Uhlendorff, and Zhao (2013). They exploit the same reform but only consider vocational training programmes with durations of up to one year and follow individuals for 1.5 years after the courses start. They mainly find insignificant positive effects of the new allocation system.

Our first contribution is the investigation of the channels through which the reform of the allocation system influences the effectiveness of vocational training programmes. We apply decomposition methods to distinguish the effects that can be associated with the voucher assignment system and with the new selection rules. Furthermore, we apply the mediation framework discussed in Baron and Kenny (1986) and Imai, Keele, and Yamamoto (2010) to separate the direct and indirect effects of the assignment mechanism. Programme composition and duration differ considerably before and after the reform. We argue that these are intermediate variables on the causal path between the voucher assignment mechanism and employment outcomes. However, programme composition and duration may be adjusted without changing the entire assignment system. To address this, we present additional results for the direct effect of the assignment system after controlling for programme composition and duration (the so-called controlled direct effect).

Second, we consider the long-term effects. We follow all individuals for seven years after the courses start. Especially for vocational training programmes of long duration, the associated increase in human capital needs some time to unfold. To date, no evidence of the effectiveness of assignment mechanisms or selection criteria is available for such long durations. Lechner, Miquel, and Wunsch (2011) note the importance of considering the long-term impacts of ALMP.

Third, we consider all vocational training programmes. Particular retraining courses provide participants with the opportunity to obtain a new vocational degree. They reflect a major component of vocational training programmes, accounting for more than 20% of all programmes. We also show effect heterogeneity with respect to these program types.

Fourth, we have access to an extremely large and rich data set, which enables inferences with high precision.[1] Data of this quality, that is, containing the full sample of training participants, was not available for previous studies.

Finally, we develop methodological extensions to the evaluation framework. We combine a multiple treatment framework with classic matching and difference-in-difference methods.

We find that the voucher assignment system negatively affects re-employment probabilities and monthly earnings between the first and second year after the start of training. A possible explanation is lower job search intensity under the voucher system. An increase in the freedom of choice and a more accommodating counselling style under the voucher system give the unemployed the possibility to participate in their preferred courses. They might be less impatient to find a job (see the discussions in Behncke, Frölich, and Lechner, 2010, Huber, Lechner, and Mellace, 2014). DellaVigna and Paserman (2005) provide evidence of a negative relationship between the patience of unemployed persons and their re-employment probability. Our results suggest that the negative effects disappear three years after course start. After seven years, we even find positive effects of the voucher assignment system on employment and earnings. This suggests that the unemployed accumulate more human capital during training, which pays off over the long run. The voucher assignment system is, over the long run, more effective for vocational training programmes with short durations than for those with long durations. We observe large changes in programme duration after the reform, which lead to positive effects over the short run. These positive effects are comparable to those in Rinne, Uhlendorff, and Zhao (2013). However, we argue that the changes in programme durations are not necessarily related to the voucher assignment system.

Furthermore, we find that the new selection criteria for programme participants are poorly designed. Caseworkers have an incentive to allocate unemployed individuals with good labour market opportunities to vocational training programmes with shorter durations. This strategy helps caseworkers to conform to the 70% rule but does not increase

[1] We observe 30,982 (74,180) training participants after (before) the reform. In contrast, Rinne, Uhlendorff, and Zhao (2013) include 1,319 (25,223) training participants after (before) the reform in their main specification. This large sample overcomes a shortcoming of Rinne, Uhlendorff, and Zhao (2013), who are rarely ever able to distinguish their estimated parameters from zero at conventional significance levels.

the efficiency of vocational training. This reiterates the concern of Heckman, Heinrich, and Smith (2002) that providing caseworkers with misaligned performance incentives can conflict with the intentions of political reform. Selection rules based on impacts rather than on outcome levels may improve the efficiency of training programmes. On a positive note, certain programme types are more frequently allocated to local employment agency districts with high unemployment rates. As argued in Lechner and Wunsch (2009), counter-cyclical allocation of vocational training can improve its effectiveness because of the low opportunity cost.

These results point to the following three policy implications. First, assignment systems in which caseworkers have authority and control over course assignment appear to improve the re-employment chances and earnings possibilities of participants over the short run. Second, voucher assignment schemes should especially be used for programmes with short durations when the society has a high preference for long-term employment opportunities. Programme participants will suffer from lower employment during the first period after beginning training. Third, selection rules can, in principle, improve the effectiveness of training programmes. However, these rules should be designed to select participants with the largest returns to training.

The remainder of this paper is structured as follows. An overview of the institutional background and a description of the expected results based on the existing literature are provided in Section 2. A detailed data description can be found in Section 3. The parameter of interest, identification, and estimation are presented in Section 4. A discussion of the results follows in Section 5. The final section concludes. Additional information is provided in Appendices A-D.

2 Background

2.1 Institutions

Vocational training programmes are a major aspect of ALMPs in Germany. Between 2000 and 2002, average annual expenditures exceeded seven billion Euros (Labour Market Reports, Federal Employment Agency of Germany). The primary objective of vocational training for the unemployed is to adjust their skills to changing requirements in the labour market and/or changed individual conditions (due to health problems, for example). The obtained certificates or vocational degrees serve as important signalling devices for potential employers. Vocational training primarily comprises three types of programmes: practice firm training, classic vocational training, and retraining. Classic vocational training courses are categorised by their planned durations. We distinguish between short training (a maximum duration of 6 months) and long training (a minimum duration of 6 months).

Table 1: Vocational training programmes

Programme type	Description	Examples
Practice firm training	Courses that took place in practice firms to simulate a work environment.	Training in commercial software, for office clerks, in data processing
Short training	Provision of occupation specific skills (duration $\leq$ 6 months).	Training courses for medical assistants, office clerks, draftsman, hairdressers, lawyers
Long training	Provision of occupation specific skills (duration > 6 months).	Training for tax accountants, elderly care nurses, office clerks, physical therapists
Retraining	Courses to obtain a first/new vocational degree.	Apprenticeship as elderly care nurses, physical therapists, hotel and catering assistants
Others	e.g., courses for career improvement	

Note: We use the categorisation of programmes proposed by Lechner, Miquel, and Wunsch (2011). Additionally, the information on the training voucher with regard to the contents of the training courses is analysed. The presented examples refer to training goals that are often denoted on the training voucher. The category "Others" contains different types of training programs with very few participants.

Teaching takes place in classrooms or on the job. Typical examples of vocational training schemes are courses in IT-based accounting or customer orientation and sales approach. Practice firm training simulates a work environment in a practice firm. Retraining (also called degree courses) is of long durations of up to three years. They lead to the completion of a (new) vocational degree within the German apprenticeship system. They cover, for example, the full curriculum of a vocational training for an elderly care nurse or office clerk. Further descriptions and examples of courses can be found in Table 1.

Before 2003, the assignment process for vocational training was characterised by caseworkers with strong authority and control regarding the choice of training providers and courses. Caseworkers directly assigned the unemployed to courses based on subjective measures. Consequently, close cooperation was established between the local employment agencies and training providers. This was heavily criticised by federal institutions and in media coverage. The pre-reform assignment process was determined by the supply of courses and socio-political factors.

In January 2003, a voucher allocation system was introduced with the intention of increasing the responsibility of training participants and introducing market mechanisms for training providers. Potential training participants receive a vocational training voucher, which allows them to select the training provider and course. The choice is subject to the following restrictions: First, the voucher specifies the objective, content, and maximum duration of the course. Second, it can be redeemed within a one-day commuting zone. Third, the validity of training vouchers varies between one week and a maximum of three months. Fourth, no sanctions are imposed if a voucher is not redeemed.

Stricter selection criteria were implemented simultaneously with the voucher system.

The post-reform paradigm of the Federal Employment Agency focuses on direct and rapid placement of unemployed individuals, high reintegration rates and low dropout rates. Caseworkers award vouchers such that at least 70% of all voucher recipients are expected to find jobs within six months of completing training. Accordingly, the award of training vouchers is based on statistical treatment rules, often labelled profiling or targeting (Eberts, O'Leary, and Wandner, 2002). Caseworkers consider regional labour market conditions and individual characteristics to form their predictions.

2.2 Potential reform effects

The change in the assignment mechanism may affect the overall effectiveness of vocational training through various channels. The increase in the freedom of choice and responsibility might have positive effects on attitudes towards training. The unemployed may experience higher motivation when participating in courses. However, it is unclear whether these factors increase participants' re-employment. If participants feel well accommodated, they might be more patient in finding a new job. This could have negative effects on search intensity and positive effects on reservation wages during training (DellaVigna and Paserman, 2005).

The introduction of a voucher assignment system affects caseworkers' counselling style. A voucher assignment system induces a greater degree of cooperation between caseworkers and potential training participants. Behncke, Frölich, and Lechner (2010) and Huber, Lechner, and Mellace (2014) report that less cooperative caseworkers are more successful in reintegrating the unemployed into employment. This might be due to threat effects (assignment to onerous programs, Black, Smith, Berger, and Noel, 2003, Rosholm and Svarer, 2008) or sanctions (Lalive, Van Ours, and Zweimüller, 2005, Van den Berg, Van der Klaauw, and Van Ours, 2004). Neither instrument is available under the voucher assignment system.

On the supply side, a voucher system implements market mechanisms following the principals of Friedman (1962). This is likely to intensify competition among training providers. However, markets do not necessarily function appropriately, and competition could generate market outcomes that do not improve the quality of training, especially under information asymmetry (see the discussion in Prasch and Sheth, 2000).

Similarly, the influence of the new selection criteria on the overall effectiveness of vocational training is not clear *a priori*. Dehejia (2005) demonstrates the potential of selection rules to increase the returns to training. Caseworkers might have accumulated expertise on training providers and offered courses. This knowledge can help them to make the allocation of training programmes more efficient relative to allocation using statistical treatment rules. However, recent empirical studies reject the notion that caseworkers allocate training programmes efficiently (Bell and Orr, 2002, Frölich, 2008, Lechner and Smith, 2007).

Clearly, the performance of statistical treatment rules critically depends on the details of the implemented system. In the German case, the rules only apply with respect to the award decisions, objective, content, and maximum duration of the courses. The unemployed have to find the most suitable training providers and courses by themselves. Furthermore, the new selection rule is based on predicted employment outcomes conditional on participation in training programmes. Unemployed individuals with higher predicted employment outcomes after participation are more likely to be awarded a voucher. Berger, Black, and Smith (2000) argue that the allocation of ALMP programmes based on predicted outcomes rather than on impacts does not serve efficiency goals. This is supported by Biewen, Fitzenberger, Osikominu, and Waller (2007), Doerr et al. (2014), and Wunsch and Lechner (2008) who report that participants with good education records are worse-off in terms of employment probabilities and earnings.

3 Data description

We use administrative data provided by the Federal Employment Agency of Germany. The data set contains information on *all* individuals in Germany who participated in a training programme between 2001 and 2004. We observe the precise start and end dates of vocational training courses and the precise award and redemption dates for each voucher in the post-reform period. Individual records are collected from the Integrated Employment Biographies (IEB) sample.[2] The data contain detailed daily information on employment subject to social security contributions, receipt of transfer payments during unemployment, job search, and participation in various active labour market programmes as well as rich individual information. Thus, we are able to consider a large set of personal characteristics and long labour market histories for all individuals in the evaluation sample. The sample used as the comparison group originates from the same database. It is constructed as a three percent random sample of individuals who experience at least one transition from employment to non-employment (of at least one month).[3]

[2]The IEB is a rich administrative database and the source of the sub-samples of data used in all recent studies that evaluate German ALMP programmes (e.g., Biewen, Fitzenberger, Osikominu, and Paul, 2014, Lechner, Miquel, and Wunsch, 2011, Lechner and Wunsch, 2013, Rinne, Uhlendorff, and Zhao, 2013). The IEB is a merged data file containing individual records collected from four different administrative processes: the IAB Employment History (*Beschäftigten-Historik*), the IAB Benefit Recipient History (*Leistungsempfänger-Historik*), the Data on Job Search originating from the Applicants Pool Database (*Bewerberangebot*), and the Participants-in-Measures Data (*Maßnahme-Teilnehmer-Gesamtdatenbank).* IAB (*Institut für Arbeitsmarkt- und Berufsforschung*) is the abbreviation for the research department of the German Federal Employment Agency.

[3]We account for the fact that we have different sampling probabilities in all calculations whenever necessary.

3.1 Treatment and sample definition

The treatment of interest is the first participation in a vocational training course. Participation begins during the first year of the unemployment period. One concern regarding the treatment definition is the timing with respect to the elapsed unemployment duration at the beginning of training participation. Frederiksson and Johansson (2008) argue that in countries such as Germany, nearly all unemployed persons would receive ALMP programmes if their unemployment spells were sufficiently long. Individuals who find jobs quickly are less likely to receive training, as the treatment definition is restricted to unemployment periods. Accordingly, ignorance of the elapsed unemployment duration at treatment start could lead to a higher share of individuals with better labour market characteristics in the control group than in the treatment group. To address this problem, we randomly assign pseudo treatment start dates to each individual in the comparison group. Thereby, we recover the distribution of the elapsed unemployment duration at treatment start from the treatment group (similar to, e.g., Lechner and Smith, 2007). To make the treatment definitions comparable between the treatment and control samples, we only consider individuals who are unemployed at their (pseudo) treatment start.[4]

The evaluation sample is constructed as an inflow sample into unemployment. The baseline sample (Sample A) consists of individuals who became unemployed in 2001 under the assignment regime or in 2003 under the voucher regime after having been continuously employed for at least three months. Entering unemployment is defined as the transition from (non-subsidised, non-marginal, non-seasonal) employment to non-employment of at least one month.[5] We focus on individuals who are eligible for unemployment benefits at the time of inflow into unemployment. This sample choice reflects the main target group. To exclude individuals who are eligible for specific labour market programmes targeting youths and individuals eligible for early retirement schemes, we only consider persons aged between 25 and 54 years at the beginning of their unemployment spell.

3.2 Descriptive statistics

The baseline Sample A includes 207,739 unweighted or 1,013,885 reweighted observations. We observe 30,982 unemployed individuals who redeem vouchers and 74,180 participants who are directly assigned to a training course. This is the full sample of vocational training participants in Germany who satisfy our sample selection criteria during the study period.

[4] Doerr et al. (2014) estimate the effect of being awarded a training voucher in the post-reform period and precisely match on the elapsed unemployment duration. They define the treatment as being awarded a voucher today versus waiting for at least one month. Their findings for the post-reform period are qualitatively similar to ours, although we use a different treatment definition.

[5] Subsidised employment refers to employment in the context of an ALMP. Marginal employment refers to employment of a few hours per week. This is due to specific social security regulations in Germany.

Table 2: Sample first moments of observed characteristics with large standardised differences.

	Voucher Regime		Assignment Regime		Absolute Standardised Differences between		
	Treatment-group	Control-group	Treatment-group	Control group	(1) and (2)	(1) and (3)	(1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Personal Characteristics							
Age	38.8	41.3	38.7	41.5	28.5	0.9	31.4
Older than 50 years	.010	.111	.019	.125	43.3	7.1	47.0
Incapacity (e.g., illness, pregnancy)	.022	.050	.032	.062	15.4	6.2	20.2
Health	.083	.128	.093	.146	14.5	3.4	20.0
Education and Occupation							
University entry degree (Abitur)	.229	.170	.197	.142	14.7	7.9	22.5
White-collar	.382	.476	.440	.527	19.2	12.0	29.5
Manufacturing	.069	.101	.101	.147	11.7	11.4	25.3
Employment and Welfare History							
Half months empl. (last 2 years)	45.6	44.9	44.5	43.7	10.1	15.4	25.7
Half months since last unempl. in last 2 years	46.8	46.2	45.6	44.4	11.6	19.7	35.0
Half months since last OLF (last 2 years)	45.8	44.6	44.9	43.3	15.5	12.5	29.9
Eligibility unempl. benefits	13.5	14.7	13.2	14.8	21.1	5.9	20.7
Remaining unempl. insurance claim	25.6	22.3	23.4	21.4	25.0	18.0	31.7
Cumulative earnings (last 4 years)	91,204	83,632	80,913	81,156	15.6	21.8	21.0
Timing of Unemployment and Programme Start							
Start unempl. in September	.151	.079	.099	.075	22.9	15.7	24.2
Elapsed unempl. duration	5.06	3.55	4.53	3.45	46.0	15.7	49.0
Characteristics of Local Employment Agency Districts							
Share of empl. in construction industry	.064	.065	.077	.077	2.3	54.3	55.5
Share of male unempl.	.564	.563	.541	.541	1.1	50.8	53.5

Note: See Table A.1 in Appendix A for sample first moments of observed characteristics with small standardised differences. In columns (1)-(4), we report the sample first moments of observed characteristics for the treated and non-treated subsamples. Information on individual characteristics refers to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics, which refer to the (pseudo) treatment time. In columns (5)-(7), we report the standardised differences between the different sub-samples and the treatment group under the voucher regime. A description of how we measure absolute standardised differences is available in Appendix C. Rosenbaum and Rubin (1985) classify absolute standardised difference of more than 20 as "large". OLF is the acronym for "out of labour force".

The sample includes 419,560 reweighted control persons before and 489,163 reweighted control persons after the reform.

In Table 2, we report the sample first moments of the observed characteristics with a large standardised difference above 20. Additionally, we present descriptive statistics for observed characteristics with small standardised differences in Table A.1 in Appendix A. Information on individual characteristics refers to the time of inflow into unemployment. Only the elapsed unemployment duration and the characteristics of local employment agency districts refer to the (pseudo) treatment time.

In the first two columns of Table 2, we report the sample first moments of our control variables for participants and non-participants under the voucher regime. The respective sample moments under the assignment regime can be found in the third and fourth columns. The last three columns display the standardised differences between the different sub-samples and the treatment group under the voucher regime. Training participants are, on average, younger, have fewer instances of incapacity and are better educated. They have more successful employment and welfare histories than unemployed individuals in the comparison group. These patterns are observed under both regimes. The primary differences between the two regimes are in the employment histories of participants and the regional characteristics. Training participants under the voucher regime have been employed longer and have higher cumulative earnings than participants under the assignment regime. Furthermore, participants under the voucher regime are more likely to reside in local employment agency districts with low employment in the construction sector and a high share of male unemployment. Overall, differences in observed characteristics of participants under the voucher and the assignment regimes are surprisingly small. In the following, we describe the empirical strategy for specifying the causal channels through which the reform operates.

4 Empirical strategy

4.1 Parameters of interest

The identification strategy is based on a multiple treatment framework as proposed in Imbens (2000) and Lechner (2001). Direct assignment to training courses is indicated by $D_i = at_0$ in the pre-reform period and by $D_i = at_1$ in the post-reform period (a = direct assignment, t = period 0 or 1). We never observe direct assignments to training courses in the post-reform period, i.e., we never observe treatment a in the post-reform period t_1. Training participation under the voucher regime is indicated by $D_i = vt_0$ in the pre-reform period and by $D_i = vt_1$ in the post-reform period (v = voucher redemption). As the implementation of the voucher system was part of the reform, we never observe treatment v in the pre-reform period t_0. In the pre-reform period, $D_i = nt_0$ indicates the absence of treatment and $D_i = nt_1$ indicates no treatment in the post-reform period (n = non-treatment).

Following the framework of Rubin (1974), the potential outcomes are indicated by $Y_i(d)$. They can be stratified into six groups: $Y_i(at_0)$ and $Y_i(at_1)$ indicate the potential outcomes that would be observed if individual i is directly assigned to a training course in the pre- or post-reform period, respectively. $Y_i(vt_0)$ and $Y_i(vt_1)$ are the potential outcomes that would be observed if individual i redeems a training voucher in the pre- or post-reform period, respectively. $Y_i(nt_0)$ and $Y_i(nt_1)$ are the potential outcomes when individual i is not treated in period before or after the reform, respectively. For each individual, we can only observe one potential outcome. The observed outcome equals

$$Y_i = D_i(at_0)Y_i(at_0) + D_i(vt_1)Y_i(vt_1) + D_i(nt_0)Y_i(nt_0) + D_i(nt_1)Y_i(nt_1),$$

where $D_i(g) = 1\{D_i = g\}$ for $g \in \{at_0, at_1, vt_0, vt_1, nt_0, nt_1\}$ and $1\{\cdot\}$ is the indicator function. The categories $D_i(at_1) = 0$ and $D_i(vt_0) = 0$ are omitted because they are never observed.

We focus on the estimation of the average treatment effects on the treated (ATT). The pre-reform ATT can be indicated

$$\gamma^{pre} = E[Y_i(at_0)|D_i = at_0] - E[Y_i(nt_0)|D_i = at_0],$$

where the treated subpopulation with $D_i = at_0$ is of prime interest. The expected potential outcome $E[Y_i(at_0)|D_i = at_0]$ is directly observed. $E[Y_i(nt_0)|D_i = at_0]$ is a counterfactual expected potential outcome, as $Y_i(nt_0)$ is never observed for the subpopulation with $D_i = at_0$. It is the expected non-treatment outcome for the subpopulation of individuals directly assigned to training courses. Accordingly, γ^{pre} is the average effect of being assigned to a training course in the pre-reform period for unemployed persons who are assigned to training courses. The post-reform ATT can be indicated

$$\gamma^{post} = E[Y_i(vt_1)|D_i = vt_1] - E[Y_i(nt_1)|D_i = vt_1],$$

where the treated subpopulation with $D_i = vt_1$ is of prime interest. The expected potential outcome $E[Y_i(vt_1)|D_i = vt_1]$ is directly observed. $E[Y_i(nt_1)|D_i = vt_1]$ refers to the expected outcome that would be observed, were the training participants under the voucher system not treated in the post-reform period. The parameter γ^{post} is the average effect of being treated in the post-reform period for treated individuals under the voucher regime. The difference in effects before and after the reform can be indicated

$$\gamma^{ba} = \gamma^{post} - \gamma^{pre}.$$

The parameters γ^{pre} and γ^{post} differ with respect to the subpopulation of interest, the period of the treatment, and the assignment mechanism.

As discussed above, individuals treated before and after the reform differ in their observed characteristics due to changes in the selection criteria. The selection effect can be formalised

$$\begin{aligned}\gamma^{s} =& [E[Y_i(at_0)|D_i = vt_1] - E[Y_i(nt_0)|D_i = vt_1]] \\ & - [E[Y_i(at_0)|D_i = at_0] - E[Y_i(nt_0)|D_i = at_0]]\,,\end{aligned}$$

where the subpopulation of interest changes but the type of treatment and period are held constant. The selection effect can be interpreted as the differences in the characteristics of the participants selected under the voucher system compared to those selected under the direct assignment system.

Furthermore, the treatment effects could differ before and after the reform, even after controlling for the type of treatment and the subpopulation of interest. We distinguish between two different business cycle (or time) effects

$$\begin{aligned}\gamma^{bc0} =& E[Y_i(nt_1)|D_i = vt_1] - E[Y_i(nt_0)|D_i = vt_1], \text{ and} \\ \gamma^{bc1} =& E[Y_i(at_1)|D_i = vt_1] - E[Y_i(at_0)|D_i = vt_1],\end{aligned}$$

which are both defined for individuals who are treated in the post-reform period. The business cycle effect under non-treatment is γ^{bc0}, and the business cycle effect under direct course assignment is γ^{bc1}. It should be emphasised that $E[Y_i(at_1)|D_i = vt_1]$ differs from the other counterfactual expected potential outcomes, as we never observe $Y_i(at_1)$ in the data.

Finally, the institutional effect is defined as

$$\gamma^{in} = E[Y_i(vt_1)|D_i = vt_1] - E[Y_i(at_1)|D_i = vt_1],$$

where we hold the subpopulation of interest and period constant but change the type of treatment. The institutional effect is the difference between training effectiveness under the voucher and direct assignment regimes, holding individual characteristics and time constant.

4.2 Identification strategy

We apply an identification strategy with three stages. First, we control for a large set of K confounding pre-treatment variables $X \in \mathbb{X} \subseteq \mathbb{R}^K$ to exclude the possibility of selection based on observed characteristics. This allows us to identify γ^{pre}, γ^{post}, γ^{ba}, γ^{s}, and γ^{bc0} from the joint distribution of random variables (Y, D, X). Second, we rely on the common trend assumption to identify γ^{bc1}. Third, additive separability assumptions are necessary to identify the institutional effect γ^{in}.

Assumption 1 *(Conditional Mean Independence).* For all $d, g \in \{at_0, vt_1, nt_0, nt_1\}$,

$$E[Y_i(d)|D_i = g, X_i = x] = E[Y_i(d)|D_i = d, X_i = x] \text{ for } \forall x \in \mathbb{X},$$

and all necessary moments exist.

This assumption implies that the expected potential outcomes are independent of the type of treatment D_i after controlling for the pre-treatment control variables X_i. All confounding variables, which jointly influence the expected potential outcomes and treatment status, must be included in the vector X_i. This is a strong assumption, but we are confident that it is satisfied in this study given the exceptionally rich data set. Biewen, Fitzenberger, Osikominu, and Paul (2014) and Lechner and Wunsch (2013) assess the plausibility of conditional independence assumptions in the evaluation of German ALMPs. Our choice of control variables is motivated by these studies. In particular, we use baseline personal characteristics, the timing of programme starts, regions, benefit and unemployment insurance claims, pre-programme outcomes, and labour market histories (see Table 2 and Table A.1 in Appendix A). In addition to the standard variables, we control for proxy information concerning physical or mental health problems, lack of motivation, and reported sanctions. Furthermore, we control for regional characteristics at the level of local employment agency districts, which are often not available with such precision.

Assumption 1 also includes a time dimension. For example, we assume that individuals with treatment status vt_1 would have the same expected potential outcomes as individuals with treatment status at_0 if they were directly assigned to a training course at t_0 (conditional on X_i). This implies that the treatment groups at t_0 and t_1 do not differ systematically in unobserved characteristics that influence the potential outcomes. However, individuals who are similar in all relevant characteristics at treatment start might have different potential outcomes. For instance, the post-treatment labour market situation is likely unrelated to the treatment probabilities (especially after long periods) but may have an effect on outcomes. In our main specifications, we control for characteristics of local employment agency districts at treatment start as a sensitivity test for this assumption. Moreover, we use samples with different calendar periods as robustness checks (see Section 5.3).

Assumption 2 *(Support).*
Let $S_g^{vt_1} = \{p_{vt_1}(x) : f(p_{vt_1}(x)|D_i = g) > 0\}$ and $S_g^{at_0} = \{p_{at_0}(x) : f(p_{at_0}(x)|D_i = g) > 0\}$ for $g \in \{at_0, vt_1, nt_0, nt_1\}$, where $f(p_d(x)|D_i = g)$ is the density of the conditional treatment probability (propensity score) $p_d(x) = Pr(D_i(d) = 1|X_i = x)$ for the subpopulation with $D_i = g$. Then, $S_{vt_1}^{vt_1} \subseteq S_{nt_1}^{vt_1}$, $S_{vt_1}^{vt_1} \subseteq S_{at_0}^{vt_1} \subseteq S_{nt_0}^{vt_1}$, and $S_{at_0}^{at_0} \subseteq S_{nt_0}^{at_0}$.

Assumption 2 requires overlap in the propensity score distributions of the different subsamples (see the discussion in Lechner and Strittmatter, 2014). In unreported calculations, we perform simple support tests and do not observe any incidence of support problems. Given this result and our exceptionally large data set, we are not concerned that this assumption fails to hold.

Under Assumptions 1 and 2, for all $d, g \in \{at_0, vt_1, nt_0, nt_1\}$,

$$E[Y_i(d)|D_i = g] = E\left[\frac{p_g(x)}{p_g p_d(x)} D_i(d) Y_i\right], \tag{1}$$

is identified from observed data on the joint distribution of $(Y, D(d), D(g), X)$, with $p_k(x) = Pr(D_i(k) = 1|X_i = x)$ and $p_k = Pr(D_i(k) = 1)$ for $k \in \{d, g\}$ (cf. Hirano, Imbens, and Ridder, 2003, Rosenbaum and Rubin, 1983).

Accordingly, the pre-reform ATT is identified by

$$\gamma^{pre} = E\left[\frac{1}{p_{at_0}} D_i(at_0) Y_i\right] - E\left[\frac{p_{at_0}(x)}{p_{at_0} p_{nt_0}(x)} D_i(nt_0) Y_i\right],$$

and the post-reform ATT by

$$\gamma^{post} = E\left[\frac{1}{p_{vt_1}} D_i(vt_1) Y_i\right] - E\left[\frac{p_{vt_1}(x)}{p_{vt_1} p_{nt_1}(x)} D_i(nt_1) Y_i\right],$$

from observed data under Assumptions 1 and 2. Thus, we can identify the difference in effects before and after the reform γ^{ba} as the difference between γ^{post} and γ^{pre}.

The selection effect equals

$$\begin{aligned} \gamma^s = &\left[E\left[\frac{p_{vt_1}(x)}{p_{vt_1} p_{at_0}(x)} D_i(at_0) Y_i\right] - E\left[\frac{p_{vt_1}(x)}{p_{vt_1} p_{nt_0}(x)} D_i(nt_0) Y_i\right]\right] \\ &- \left[E\left[\frac{1}{p_{at_0}} D_i(at_0) Y_i\right] - E\left[\frac{p_{at_0}(x)}{p_{at_0} p_{nt_0}(x)} D_i(nt_0) Y_i\right]\right]. \end{aligned}$$

Furthermore, we can identify the business cycle effect γ^{bc0} as

$$\gamma^{bc0} = E\left[\frac{p_{vt_1}(x)}{p_{vt_1} p_{nt_1}(x)} D_i(nt_1) Y_i\right] - E\left[\frac{p_{vt_1}(x)}{p_{vt_1} p_{nt_0}(x)} D_i(nt_0) Y_i\right],$$

under Assumptions 1 and 2. For the identification of γ^{bc1} and γ^{in}, we impose additional assumptions.

Assumption 3 *(Common Trend Assumption).*

$$\gamma^{bc0} = \gamma^{bc1}.$$

This assumption requires the business cycle effects to be independent of treatment status. Potential outcomes would evolve parallel to one another in the absence of reform of the provision of vocational training programmes. We carefully assess the plausibility of Assumption 3 in Section 5.3 using different evaluation samples and detailed information on monthly regional labour market characteristics.

Assumption 4 *(Additive Separability).* The difference in effects before and after the reform can be separated into selection, business cycle, and institutional effects, such that

$$\gamma^{ba} = \gamma^{s} + (\gamma^{bc0} - \gamma^{bc1}) + \gamma^{in},$$

is uniquely identified.

Assumption 4 excludes interactions among selection, business cycle and institutional effects. This assumption is crucial for the interpretation of the institutional effect. We discuss the plausibility of this assumption in Section 5.4.

We apply a semi-parametric reweighting estimator, *Auxiliary-to-Study Tilting* (Graham, De Xavier Pinto, and Egel, 2011). This estimator is well suited to our empirical design because it balances the efficient sample first moments exactly. Furthermore, it is $\sqrt{N}$-consistent and asymptotically normal. This estimator is described in Appendix B.

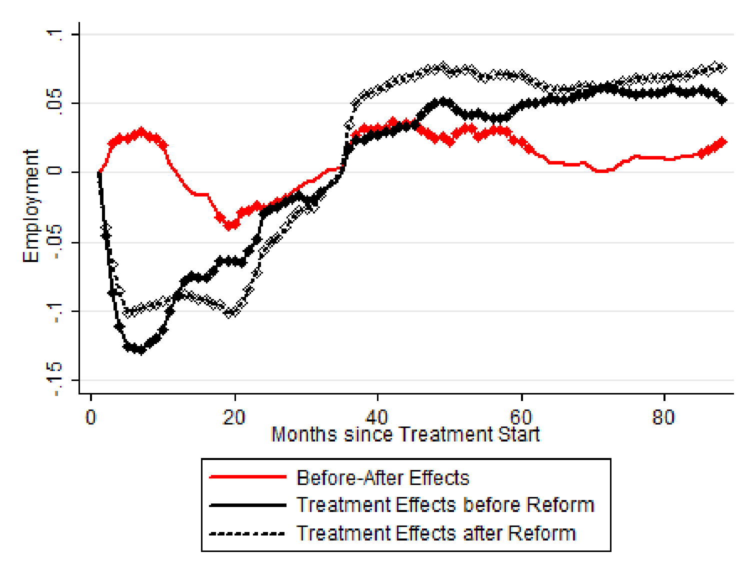
5 Results

5.1 The effectiveness of training before and after the reform

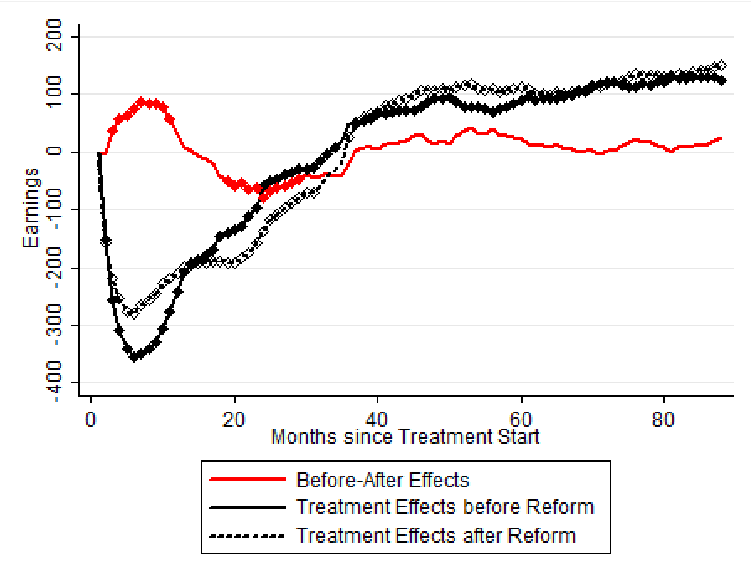
We begin with a discussion of the estimation results regarding the effectiveness of training *per se*. Figure 1 presents the average treatment effects for participants in vocational training courses under the direct assignment regime (γ^{pre}) before the reform and the voucher regime (γ^{post}) after the reform. The outcomes of interest are the employment probabilities and monthly earnings. We report separate effects for each of the seven years following the course start. The solid lines are point estimates and the diamonds indicate significant effects at the 5% level.

Training participants suffer from negative lock-in effects under both regimes. Lock-in effects may occur because training participants reduce their search intensity during course participation. The lock-in effects are steeper in the pre-reform period but have longer durations after the reform. Under both regimes, the long-term effects of participation in vocational training courses on employment probability and monthly earnings are positive. Training participation increases long-term employment probability (seven years after the start of training) by 5 percentage points before the reform and by 7.5

Figure 1: Overall reform, post-reform, and pre-reform treatment effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euros)

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A).

percentage points after the reform. Monthly earnings increase over the long term by approximately 120 Euros (150 Euros) per month before (after) the reform. These results support the existing consensus in the literature. Vocational training only leads to positive labour market effects, if any, after long negative lock-in periods (for Germany see Biewen, Fitzenberger, Osikominu, and Paul, 2014, Hujer, Thomsen, and Zeiss, 2006, Lechner, Miquel, and Wunsch, 2007, 2011, among others).

The raw difference between the post- and pre-reform effectiveness of training identifies the overall difference in effects before and after the reform (γ^{ba}). As seen from the red solid line in Figure 1, the differences in the duration and magnitude of the lock-in effects lead to a positive difference in effects before and after the reform over the short term and negative effects in the second and third years after the course start. Over the long term (seven years after the course start), the difference between the post- and pre-reform effectiveness of training is significant and positive with respect to employment probability. For monthly earnings, the difference appears to be insignificant and essentially zero. This overall difference is the starting point of our analysis and will be decomposed into the individual effects of stricter participant selection and the change in the assignment mechanism.

5.2 Selection effects

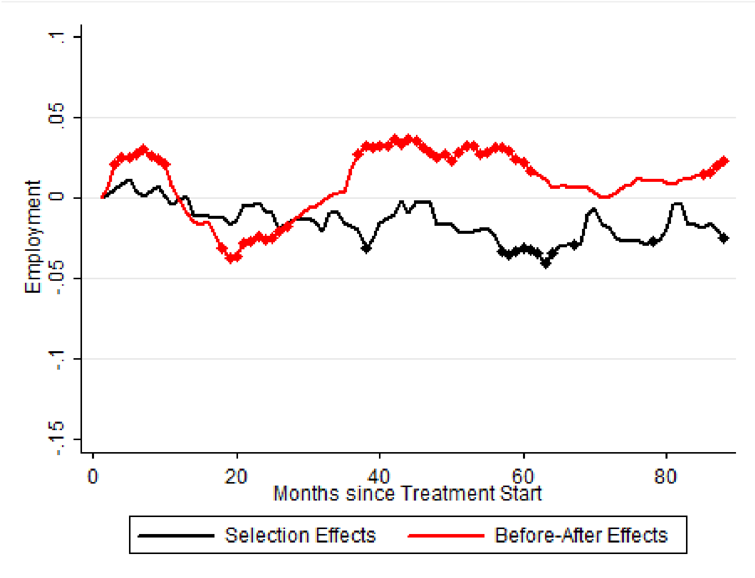
5.2.1 Main results

The imposition of stricter selection criteria changes the composition of training participants with respect to their labour market characteristics. As caseworkers are instructed to assign training to unemployed individuals with high re-employment probabilities, we expect to observe training participants with better labour market characteristics after the reform. In Table A.2 in Appendix A, we report the efficient first moments of all confounding control variables for training participants before and after the reform. The largest differences between the two groups can be found for the employment and welfare histories and the characteristics of local employment agency districts (similar to the discussion of the sample moments in Section 4.2). Unemployed persons who participate in the voucher regime (i.e., after the reform) have, on average, more successful employment and earnings profiles than those who participated under the assignment regime. Nevertheless, the overall differences in observed characteristics are surprisingly small.

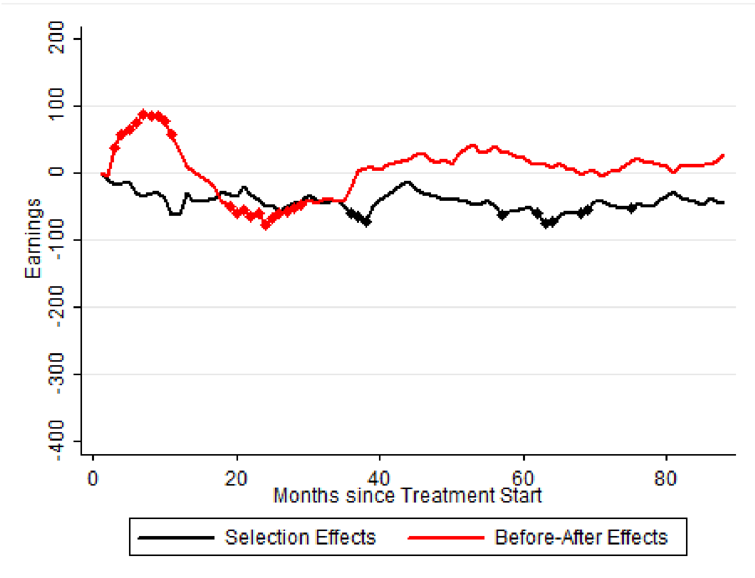
The impact of stricter participant selection criteria on the effectiveness of training can be captured by the selection effects (γ^{s}), which are reported in Figure 2. The interpretation of the selection effects can be clarified by the following thought experiment: Assign unemployed individuals with the same characteristics as participants in the post-reform period to training in the pre-reform period. Then, compare them to actually observed participants in the pre-reform period. The results suggest that stricter participant selection criteria only have a minor influence on the effectiveness of training. If anything, we find negative selection effects on employment and monthly earnings over the long run. Given the small differences in most observed characteristics, such small and mostly insignificant selection effects are plausible.

To reveal potentially opposing forces underlying the selection effects, we apply a non-parametric Blinder-Oaxaca decomposition in unreported calculations. This decomposi-

Figure 2: Selection and overall reform effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euros)

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A).

tion method allows us to change one block of observed characteristics between the pre- and post-reform periods, holding all other characteristics constant at the pre-reform level. We distinguish among three blocks of observed characteristics. The first block includes personal characteristics and information on education, occupation, and sector. The second block includes information on participants' employment and welfare histories. The third block includes information on the timing of unemployment and treatment start, the state of residence, and characteristics of local employment agency districts. However, we find weakly significant negative selection effects for all blocks.[6]

5.2.2 Effect heterogeneity by programme type

In a next step, we investigate heterogeneous selection effects by programme type (see Figure D.1 in Appendix D). We distinguish among practice firm training, short training, long training, and retraining programmes (see the description in Section 2.1).

The post-reform selection of participants leads to significantly lower effectiveness of practice firm and short training. In Table D.1 in Appendix D, we present the efficient first moments of the observed characteristics by programme type before and after the reform. The comparison of characteristics between training participants before and after the reform for the different course types reveals a strong positive selection of participants into shorter courses. For short training (practice firm training), the share of participants with a university entry level degree increased by approximately 9 percentage points (5 percentage points) after the reform. The share of participants with an academic degree increased by 6 percentage points (2 percentage points). Furthermore, the selection with respect to employment and welfare history is positive for these shorter programmes. One possible explanation for the selection of highly educated unemployed individuals into shorter programmes is strategic behaviour on the part of caseworkers. The selection rule exclusively focuses on the share of participants who find a job after participating in a training programme. The share of re-employed participants should average 70% in the six-month period after training. This incentivises caseworkers to steer unemployed individuals with good labour market prospects (even in the absence of training) into shorter programmes to obtain early payoffs.

For long training, the results of the selection effects are mostly negative but insignificant. For retraining, the selection effects are essentially zero. As seen from Table D.1 in Appendix D, there are only very small differences between retraining participants before and after the reform with respect to observed characteristics. The exception is that the allocation intensity of retraining courses increased in local employment agency districts with high unemployment rates and few vacant full-time jobs after the reform (see the bottom section of Table D.1 in Appendix A). Skill upgrading during periods with poor labour market conditions can be economically efficient. Lechner and Wunsch (2009) demonstrate that training programmes are more effective during periods of high unemployment.

[6] The results are available upon request.

5.3 Business cycle effects

Before we focus on the institutional effects, we assess the plausibility of the common trend assumption, which implies that the potential outcomes of participants and non-participants would follow the same trend in the absence of the reform. We advance three arguments to convince the reader of the plausibility of this assumption. First, Figure 3 reports the long-term trends in the outcome variables for different samples for the years between 1990 and 2012. Prior to the treatment start dates in 2001 and 2003, the outcomes of the participants and non-participants samples evolve in parallel over many years. Given these parallel trends, it is likely that we would observe the same respective patterns after 2001 or 2003 in the absence of a treatment.

Second, we experiment with additional information on local employment agency districts (i.e., regional control variables). We assess the sensitivity of our findings to these factors. We expect that our results are not sensitive to these variables if the common trend assumption holds.

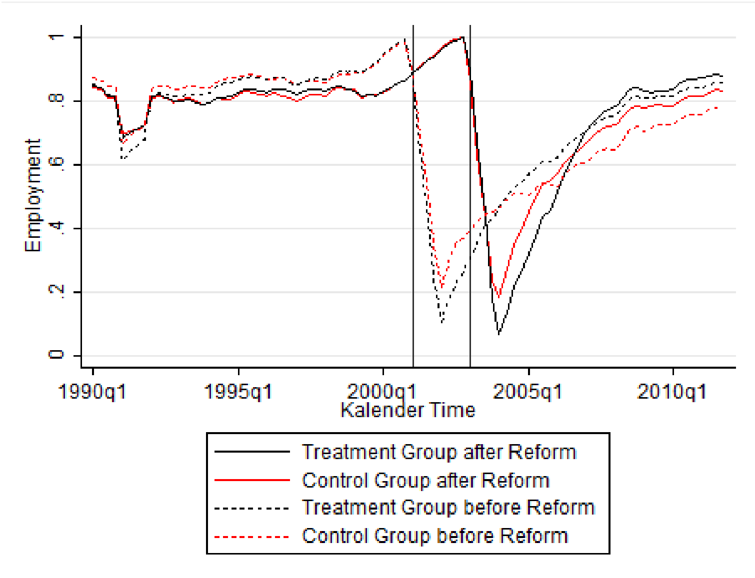
Third, we use an alternative sample definition (Sample B) for which we alter the pre-reform sample restrictions. We consider individuals who enter unemployment in 2002 and start training within the following twelve months but no later than December 2002. Consequently, not all individuals in Sample B can participate during the first twelve months of their unemployment period (e.g., an individual who enters unemployment in October can only receive treatment under the assignment regime in the following 3 months). The post-reform evaluation sample is not altered in Sample B to ensure that the comparison of results for the different samples is straightforward.[7] Using Sample B, we approximate the timing of the reform implementation with respect to the inflow into unemployment. We argue that the common trend assumption is more likely to hold if the time difference between the pre- and post-reform periods is smaller. However, in contrast to the baseline sample (Sample A), Sample B is not balanced in the pre- and post-reform periods.

Figure 4 presents the business cycle effects under non-participation (γ^{bc0}) for Samples A and B with and without additional regional control variables. The business cycle effects show an immediate, sharp increase in employment probabilities, which peaks after three years. Thereafter, the effects fade to a 3-5 percentage points higher employment probability in the post-reform period compared to that in the pre-reform period. The business cycle effects for monthly earnings evolve more smoothly over the observation period. After seven years, the difference in monthly earnings between the post- and pre-reform periods amounts to 50-100 Euros.[8]

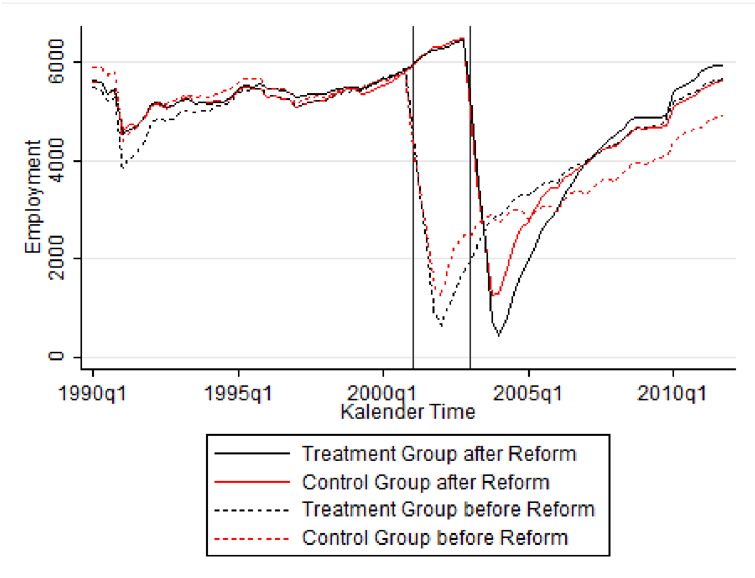
[7] In Sample B, 48,440 unemployed individuals, who are assigned to a training programme, are observed. It is representative of 395,574 control persons in the pre-reform period.

[8] These findings also support the plausibility of the conditional mean independence assumption. The potential outcomes under non-treatment are significantly different between period t_0 and t_1 only after more

Figure 3: Time trends of employment and earnings for different subgroups of individuals for the 1991-2008 period.



(a) Time trends of employment



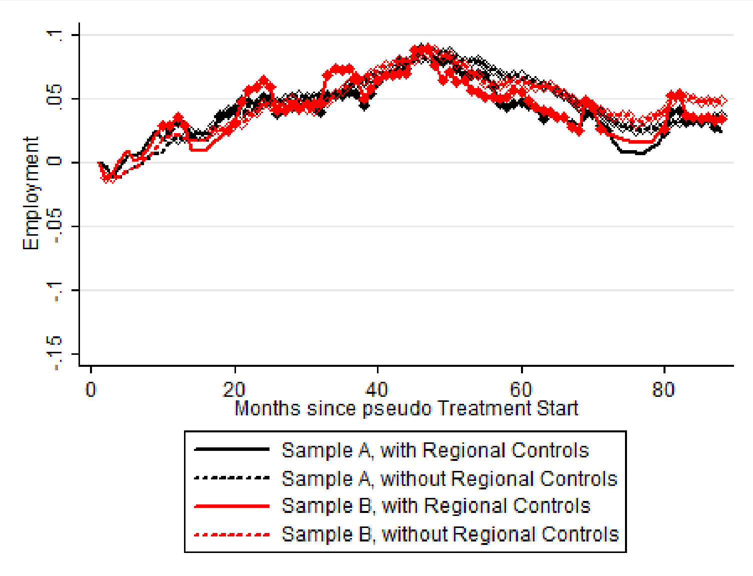
(b) Time trends of monthly earnings (in Euros)

Note: We report time trends for years between 1990 and 2008. The outcome variables are reweighted as described in Appendix B. Similar findings are obtained without reweighting.

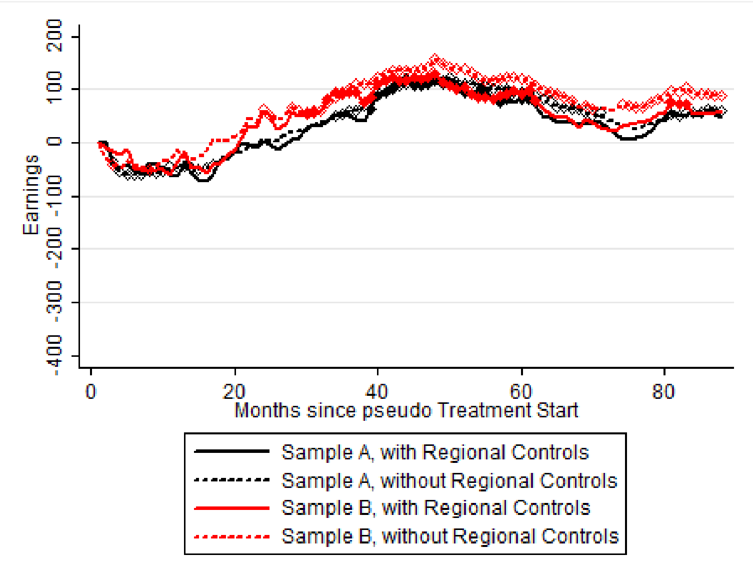
The general pattern of the business cycle effects is not sensitive to the sample or to the inclusion of additional regional labour market characteristics. This supports the

than one year (excluding specifications without regional labour market characteristics). This suggests that there are no systematic differences in the treatment groups nt_0 and nt_1 at (pseudo) treatment start or shortly thereafter. Long-run differences in the economic conditions do not violate the conditional mean independence assumption, as these differences have no influence on the treatment probability (if they could not be anticipated).

Figure 4: Business cycle effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euros)

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A).

plausibility of the common trend assumption.[9]

[9]However, the German labour market was intensively reformed during our period of observation, particularly in 2005. An improvement in labour market conditions can be observed over the long run. This does not affect the plausibility of the common trend assumption.

5.4 Institutional effects

5.4.1 Main results

In this section, we discuss the results for the institutional effects (γ^{in}). Figure 5 displays the institutional effects for samples A and B, with and without additional regional control variables. The following thought experiment clarifies the interpretation of the institutional effects: Compare the employment outcomes of training participants who receive a training voucher with the employment outcomes that they would obtain if they were directly assigned to a (potentially different) vocational training course. Individual labour market characteristics and the period are fixed.

The pattern of the institutional effects varies in the different periods after course start. In the short term, the institutional effects are positive, implying that training is more effective under the voucher regime. In the best case, training participants who receive a voucher have employment probabilities that are approximately 2-3 percentage points higher and monthly earnings that are 100-120 Euros higher compared to participants in the direct assignment regime. Over the medium term, the institutional effects are negative. The specifications using Sample B present a slightly more negative picture. In the worst-case scenario, the employment probability decreases by 5 percentage points, and earnings are reduced by 80 Euros per month. Three years after the training start date, we observe an increase to slightly positive but mostly insignificant institutional effects. After seven years, the effects are positive for all specifications. However, only for Sample A are the effects significant, with a 4-5 percentage point increase in employment probabilities and 50 Euros increase in monthly earnings.

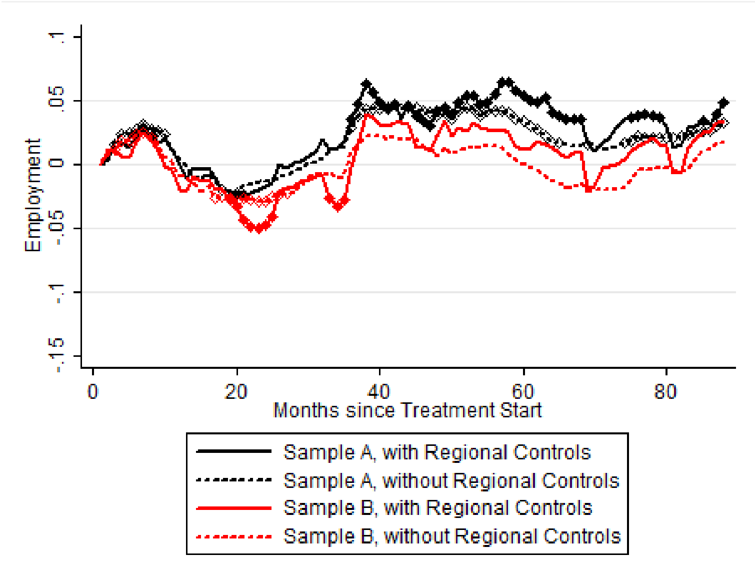
5.4.2 Channels of the institutional effects

To interpret the institutional effects, it is necessary to investigate the channels that may clarify these ambiguous patterns. First, we investigate the influences of programme types and durations. Table 3 reports descriptive statistics for different types and durations of training programmes before and after the reform. The share of short training programmes increases from 21% to 42% after the reform. Moreover, the share of long training programmes decreases from 41% to 19%. The average planned and actual durations of long programmes (practice firm) decrease nearly three (two) months after the reform. The share of participants in retraining courses increases from 19% to 25%. The average planned duration is extended by more than one month.[10]

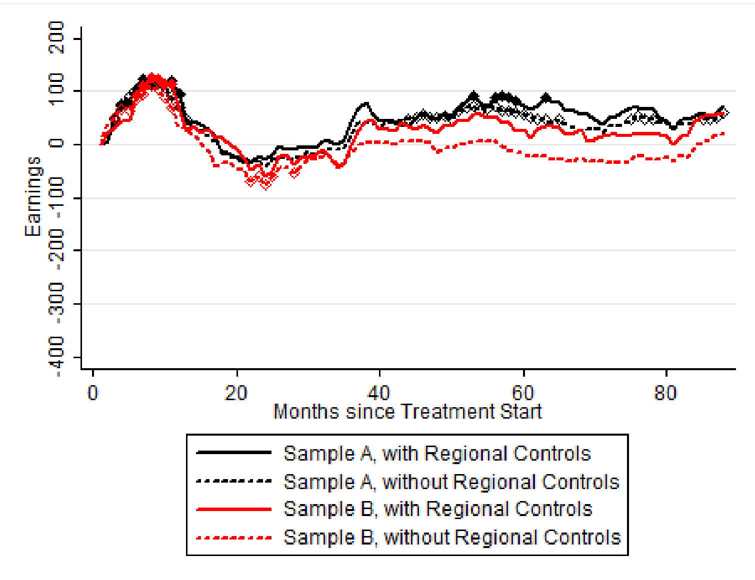
Accordingly, the composition of programme types and durations changed substantially after the reform. We observe higher shares of participants in programmes with durations

[10] In 2003, there was also a reduction in the total number of vocational training programmes for political reasons.

Figure 5: Institutional effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euros)

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A).

of less than six months and higher shares of participants in very long programmes with durations of more than two years. The first development might reflect increased freedom of choice under the voucher regime. Training vouchers are determined with respect to the maximum programme duration. The unemployed individuals are free to choose a training provider and may self-select into shorter courses. However, after the reform, caseworkers may have an incentive to strategically assign maximum programme durations to comply

Table 3: Average programme durations by training type.

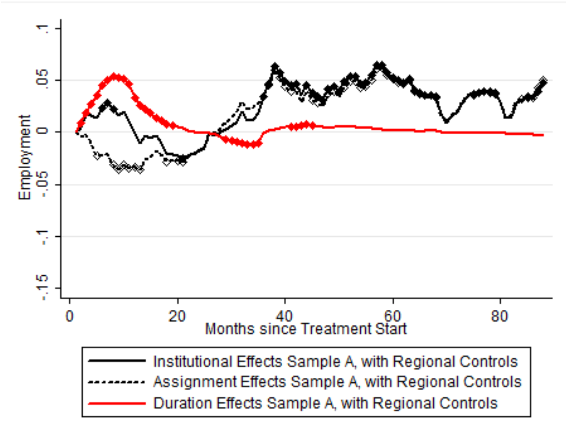
	# Obs	Per cent	Average Planned Duration	Average Actual Duration
			Pre-Reform	
Practice Firms	11,231	16%	201 days	191 days
Short Training	14,564	21%	114 days	114 days
Long Training	28,348	41%	352 days	336 days
Retraining	13,340	19%	762 days	719 days
Others	1,065	2%	403 days	383 days
			Post-Reform	
Practice Firms	3,409	13%	156 days	152 days
Short Training	10,864	42%	116 days	115 days
Long Training	4,985	19%	272 days	279 days
Retraining	6,487	25%	799 days	774 days
Others	590	1%	467 days	434 days

Note: We use the baseline sample (Sample A). The category "Others" contains different types of training programmes with very few participants, e.g., programmes that focus on career improvements.

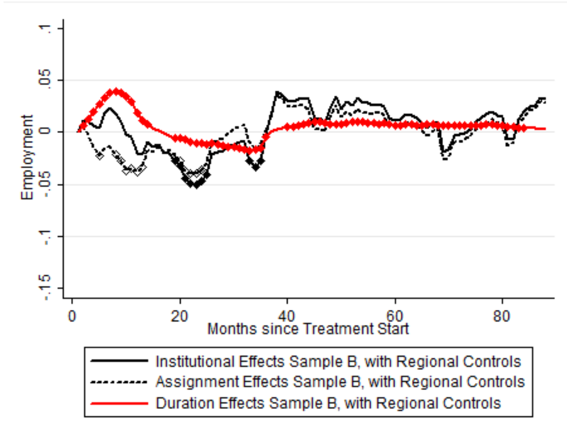
with the stricter selection rule (see the discussion in Section 5.2). This constitutes a problem for the interpretation of the institutional effects. If caseworkers systematically change the maximum programme duration in response to the selection rule, the resulting effects correspond to changes in the selection criteria and not to the changes in the assignment mechanism. This would invalidate the additive separability assumption (Assumption 4).

To overcome this problem we apply a mediation framework (see, for instance, the seminal paper by Baron and Kenny, 1986). We consider the type and duration of training as so-called mediators, i.e., intermediate outcomes on the causal path of the assignment mechanism to the individual labour market outcomes. We are particularly interested in the so-called controlled direct effect, i.e., the effect of the voucher assignment mechanism for a fixed type and duration of training (see, for instance, Pearl, 2001). Causal mechanisms, however, are not easily identified. Even if the institutional effects were identified, this would not imply identification of the mediator effects. Addressing the endogeneity of the programme type and duration requires that they are independent of the potential outcomes conditional on the assignment system and the covariates. In our application, this assumption implies that training participants with "better" unobserved labour market opportunities do not systematically select into programmes of a specific type or duration. This sequential conditional independence assumption is related to those invoked in the non-parametric mediation literature for identifying controlled direct effects (see, for instance, Petersen, Sinisi, and van der Laan, 2006, Van der Weele, 2009) and in the multiple treatment effect framework (see Imbens, 2000, Lechner, 2001). The latter framework is

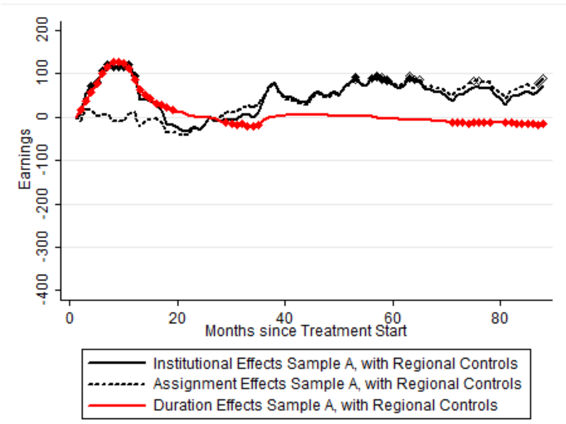
Figure 6: Institutional, programme duration, and assignment effects on employment and earnings.



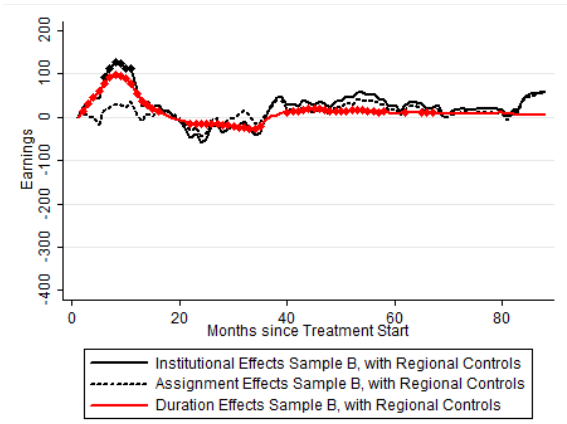
(a) Effects on employment, Sample A



(b) Effects on employment, Sample B



(c) Effects on monthly earnings (in Euros), Sample A



(d) Effects on monthly earnings (in Euros), Sample B

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A). In the duration effects, we account for the planned course durations and interactions using fixed duration dummies.

incorporated into our empirical design. In practice, we manipulate the programme durations such that they are similar in the two treatment samples (using additional moment conditions for those two subpopulations).[11] The programme duration effects reveal the isolated impacts of the changed composition of programme types and durations after the reform. The assignment effects reflect the impact of a change in assignment mechanism if the programme durations were held constant for both periods.

Figure 6 presents the programme duration, the assignment and the institutional effects.

[11]We generate dummies for the planned programme durations (less than 6 months, between 6 and 12 months, between 12 and 24 months, and more than 24 months). These durations correspond to different programme types. Furthermore, we account for interactions between these dummies and the planned programme duration to allow for linear trends within each period.

We report results for Samples A and B with regional control variables.[12] The patterns of the duration effect are similar for the employment and earnings outcomes. We find positive short-term duration effects that can be explained by the larger share of shorter programmes after the reform. After 2-3 years, we find a decrease in the duration effect. This can be explained by a larger share of very long retraining programmes under the voucher regime. Over the long term, the durations effects again become slightly positive but remain fairly close to zero. In line with Flores et al. (2012) and Kluve et al. (2012), we conclude that changing programme compositions and durations are important factors in the overall effectiveness of training.

The assignment effects become negative immediately after the start of training. After one and two years, the voucher assignment system leads to a 3-5 percentage points decline in the employment probability. The assignment effects remain negative until three years after the start of training. These results support the findings in the literature that positive incentives are less effective than negative ones (e.g., Behncke, Frölich, and Lechner, 2010, Huber, Lechner, and Mellace, 2014, Van der Klaauw and Van Ours, 2013). The assignment of training vouchers encourages caseworkers to implement a counselling style that accommodates potential participants. Assignment to onerous vocational training programmes and sanction possibilities are limited under the voucher assignment system. However, participants might change their attitudes towards training in a positive way and participate with higher motivation. For example, unemployed individuals might perceive less pressure to find a job under the voucher assignment system, as they feel more accommodated and have more positive attitudes towards the vocational training programme than under the direct assignment system. This increased patience negatively affects job search intensity and may increase the reservation wages. Job search intensity and reservation wages have opposite effects on realised earnings. Empirically, we find no significant effects on monthly earnings over the short run.

Three years after the start of training, the assignment effects become positive. Using Sample A, the change in the assignment mechanism amounts to a 5 percentage point increase in employment probabilities and higher monthly earnings of 90 Euros. These findings are stable until seven years after programme start date. In the more conservative specification (Sample B), we do not observe any significant long-term impacts. However, the patterns tend to be positive. This suggests that participants invest in more human capital under the voucher regime, and the payoffs of these investments need time to unfold.

[12] The results without additional regional control variables are available upon request.

5.4.3 Effect heterogeneity by programme type

Finally, we consider effect heterogeneity by programme type. In Figure D.2 in Appendix D, we report heterogeneous institutional, programme duration, and assignment effects.[13] Overall, the re-employment probabilities and monthly earnings are lower for participants in longer programmes (long training and retraining) than for those in shorter programmes (practice firm training and short training). It appears that the voucher assignment system is more effective for shorter vocational training programmes than for longer vocational training programmes in the time period under investigation.

6 Discussion and conclusions

This study exploits a large reform to the provision of vocational training programmes for the unemployed to analyse the effectiveness of training courses under two different assignment regimes. The pre-reform regime granted caseworkers substantial authority, directly assigning unemployed individuals to training courses. The post-reform regime introduced a voucher system and stricter selection criteria. Caseworkers are instructed to select unemployed persons to participate in training if they have high reintegration probabilities after the programme.

We find that the direct assignment of courses leads to higher employment probabilities than the voucher assignment system between the first and second years after the start of training. One potential explanation for these results is reduced search intensity among participants under the voucher assignment system, as they can select the course they prefer and are thus more motivated to participate. Another potential explanation is the limited ability to impose sanctions and assign onerous programmes under the voucher assignment system. Seven years after the start of training, we find evidence of a positive impact of the voucher assignment system on employment and earnings. Participants appear to accumulate more human capital under the voucher assignment system, which has long-term payoffs. The voucher assignment system seems to work somewhat better for shorter vocational training programmes compared to longer vocational training programmes.

Caseworkers respond to the new selection rules by allocating unemployed individuals with good labour market prospects to shorter programmes. This behaviour helps caseworkers to achieve the required 70% reintegration rate but reduces the effectiveness of these training programmes. However, retraining courses are assigned more often during periods and in regions with poor labour market conditions, which does not negatively impact the effectiveness of retraining courses. Accordingly, more appropriate selection rules

[13] We only report results for Sample A with additional regional control variables. The results without regional control variables are available upon request.

could improve the effectiveness of vocational training programmes without increasing training costs. These rules should select more participants with high returns to training.

As is always the case in this type of evaluation study, that is, studies that focus on the empirical identification of reform effects, this analysis relies on strong identifying assumptions. The additive separability assumption is particularly critical in this study. Unobserved variables could confound the effects of interest. However, the results of the sensitivity analysis, the use of different evaluation samples and the remarkably large and manifold data we employ make us confident that the results are robust.

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A Descriptive statistics

Table A.1: Sample first moments of observed characteristics with small standardised differences.

	Voucher Regime		Assignment Regime		Standardised Differences between		
	Treatment-group	Control-group	Treatment-group	Control-group	(1) and (2)	(1) and (3)	(1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Personal Characteristics							
Female	.472	.447	.477	.411	5.0	.9	12.4
No German citizenship	.054	.080	.052	.071	10.5	1.0	7.2
Children under 3 years	.042	.035	.040	.031	3.7	1.2	6.1
Single	.300	.285	.270	.251	3.4	6.7	11.1
Sanction	.007	.007	.009	.008	.2	2.0	.5
Lack of motivation	.007	.007	.009	.008	.2	2.0	.5
Education and Occupation							
No schooling degree	.036	.068	.036	.056	14.3	.4	9.3
Schooling degree without Abitur	.720	.731	.750	.770	2.4	6.8	11.4
Missing	.014	.031	.017	.032	10.9	2.4	11.9
No vocational degree	.203	.227	.218	.219	5.9	3.6	3.8
Academic degree	.112	.096	.081	.063	5.4	10.6	17.3
Agriculture, Fishery	.012	.020	.015	.023	6.7	3.2	8.7
Construction	.054	.032	.027	.022	10.9	13.3	16.6
Trade and Retail	.127	.169	.148	.175	11.8	6.2	13.5
Communication and Information Service	.108	.137	.122	.128	8.6	4.2	6.1
Employment and Welfare History							
Half months unempl. in last 2 years	.398	.370	.578	.581	1.6	9.5	9.7
No unempl. in last 2 years	.914	.921	.877	.878	2.7	11.8	11.6
Unemployed in last 2 years	.034	.040	.046	.052	3.1	6.2	9.1
# unemployment spells in last 2 years	.113	.102	.166	.165	2.8	11.6	11.4
Cumulative empl. in last 4 years	81.1	79.1	79.1	78.8	9.2	8.9	10.5
Cumulative benefits in last 4 years	3.00	3.52	3.70	4.02	6.3	8.2	11.5
Any programme in the last 2 years	.047	.042	.056	.049	2.6	4.3	1.0
Timing of Unemployment and Programme Start							
Start unempl. in January	.060	.101	.117	.105	15.0	19.8	16.1
Start unempl. in February	.070	.089	.108	.089	7.2	13.4	7.0
Start unempl. in March	.096	.083	.105	.085	4.5	3.0	3.7
Start unempl. in April	.102	.088	.120	.086	4.8	5.7	5.8
Start unempl. in June	.059	.078	.058	.072	7.6	.6	5.3
Start unempl. in July	.052	.080	.053	.078	11.1	.3	10.4
Start unempl. in August	.081	.078	.080	.078	1.0	.3	.9
Start unempl. in October	.127	.078	.085	.082	16.4	13.8	14.9
Start unempl. in November	.086	.079	.045	.082	2.6	16.6	1.7
Start unempl. in December	.045	.082	.040	.089	15.0	2.8	17.6
State of Residence							
Baden-Württemberg	.087	.113	.095	.090	8.6	2.9	1.2
Bavaria	.159	.138	.111	.115	6.1	14.1	12.8
Berlin, Brandenburg	.093	.093	.107	.111	.1	4.7	6.0
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	.076	.088	.098	.092	4.3	7.9	5.6
Hesse	.064	.068	.063	.058	1.7	.1	2.3
Northrhine-Westphalia	.232	.206	.182	.197	6.2	12.4	8.6
Rhineland Palatinate, Saarland	.056	.054	.055	.049	.9	.6	3.4
Saxony-Anhalt, Saxony, Thuringia	.123	.142	.189	.190	5.5	18.4	18.5
Characteristics of Local Employment Agency Districts							
Population per km^2	910	889	789	895	1.3	7.5	.9
Unemployment rate (in %)	12.2	12.3	12.1	12.0	1.9	1.4	3.8
Share of empl. in production industry	.250	.246	.246	.241	5.1	4.7	9.9
Share of empl. in trade industry	.150	.150	.150	.150	1.8	2.7	2.8
Share of non-German unempl.	.139	.141	.126	.128	2.5	14.3	12.1
Share of vacant fulltime jobs	.794	.794	.800	.799	0	8.4	7.6

Note: See Table 2 for sample first moments of observed characteristics with large standardised differences. In columns (1)-(4), we report the sample first moments of observed characteristics for the treated and non-treated sub-samples. Information on individual characteristics refers to the time of inflow to unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics, which refer to the (pseudo) treatment time. In columns (5)-(7), we report the standardised differences between the different sub-samples and the treatment group under the voucher regime. Please find a description of how we measure standardised differences in Appendix C. OLF is the acronym for "out of labour force".

Table A.2: Efficient first moments of observed characteristics.

	Voucher Regime (1)	Assignment Regime (2)	SD between (1) and (2)
Personal Characteristics			
Female	.472	.476	.8
Age	38.754	38.697	.8
Older than 50 years	.011	.019	7.1
No German citizenship	.054	.052	1.1
Children under 3 years	.042	.040	1.2
Single	.300	.270	6.6
Health problems	.083	.093	3.7
Sanction	.007	.009	2.1
Incapacity (e.g., illness, pregnancy)	.022	.032	6.3
Lack of motivation	.007	.009	2.1
Education and Occupation			
No schooling degree	.036	.035	.5
Schooling degree without Abitur	.719	.762	9.8
University entry degree (Abitur)	.230	.185	11.2
No vocational degree	.204	.217	3.3
Academic Degree	.114	.081	11.3
White-collar	.383	.440	11.7
Agriculture, Fishery	.012	.015	3.2
Manufacturing	.069	.101	11.6
Construction	.053	.027	13.1
Trade and Retail	.127	.148	6.1
Communication and Information Service	.109	.122	4.1
Employment and Welfare History			
Half months empl. in last 2 years	45.5	44.5	15
Half months unempl. in last 2 years	.401	.587	10
Half months since last unempl. in last 2 years	46.7	45.6	20.7
No unempl. in last 2 years	.913	.876	12.1
Unempl. in last 2 years	.034	.047	6.3
# unemployment spells in last 2 years	.114	.168	12
Any program in last 2 years	.047	.057	4.5
Half months since of last OLF in last 2 years	45.7	44.9	12.1
Remaining unempl. insurance claim	25.6	23.4	17.6
Eligibility unempl. benefits	13.5	13.2	5.9
Cumulative empl. in last 4 years	81	79	8.3
Cumulative earnings in last 4 years	91,018	80,928	21.1
Cumulative benefits in last 4 years	3.02	3.74	8.4
Timing of Unemployment and Programme Start			
Start unempl. in January	.059	.116	19.9
Start unempl. in February	.070	.108	13.4
Start unempl. in March	.095	.104	2.9
Start unempl. in April	.102	.120	5.7
Start unempl. in June	.059	.058	.4
Start unempl. in July	.052	.053	.7
Start unempl. in August	.082	0.08	.6
Start unempl. in September	.152	.099	16.2
Start unempl. in October	.127	.085	13.5
Start unempl. in November	.087	.046	16.6
Start unempl. in December	.045	.040	2.6
Elapsed unempl. duration	5.08	4.54	16.2
State of Residence			
Baden-Württemberg	.085	.093	2.9
Bavaria	.159	.113	13.4
Berlin, Brandenburg	.090	.103	4.3
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	.077	.099	8
Hesse	.064	.064	0
Northrhine-Westphalia	.231	.180	12.7
Rhineland Palatinate, Saarland	.056	.055	.7
Saxony-Anhalt, Saxony, Thuringia	.125	.191	18
Characteristics of Local Employment Agency Districts			
Share of empl. in production industry	.250	.246	5.1
Share of empl. in construction industry	.064	.077	52.4
Share of empl. in trade industry	.150	.150	3.1
Share of male unempl.	.564	.541	46.9
Share of non-German unempl.	.138	.126	13.3
Share of vacant fulltime jobs	.793	.800	8.9
Population per km^2	902	778	7.4
Unemployment rate (in %)	12.2	12.1	2.5

Note: In columns (1)-(2), we report the efficient first moments of observed characteristics for the treated sub-samples. They are exactly equal in the other re-weighted sub-samples, which are not reported. Information on individual characteristics refers to the time of inflow to unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics which refer to the (pseudo) treatment time. In column (3), we report the standardised differences (SD) between the two treatment groups. Please find a description of how we measure standardised differences in Appendix C. OLF is the acronym for "out of labour force".

B Estimation strategy

A straightforward estimation strategy is based on the sample analogue of (1)

$$\hat{E}[Y_i(d)|D_i = g] = \frac{1}{N}\sum_{i=1}^{N}\hat{\omega}_i Y_i,$$

with

$$\hat{\omega}_i = \frac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_g(X_j)} \cdot \frac{\hat{p}_g(X_i)}{\hat{p}_d(X_i)}, \tag{2}$$

where $\hat{p}_g(X_i)$ and $\hat{p}_d(X_i)$ indicate the estimated conditional treatment probabilities (henceforth, *propensity scores*). This is an *Inverse Probability Weighting* (IPW) estimator. Hirano, Imbens, and Ridder (2003) demonstrate that the consistency and efficiency of an IPW critically depend on the estimated propensity scores. Parametric specifications of the propensity score do not necessarily lead to efficient estimates. One reason is that (2) seeks to balance the sample covariate distributions, which equal

$$\hat{F}_g = \frac{1}{\sum_{i=1}^{N}\hat{p}_g(X_i)}\sum_{i=1}^{N} D_i(g)1\{X_i \leq x\},$$

when $g = d$. However, $\hat{F}_g$ can be more efficiently estimated using information from the entire population rather than from the random sample g alone. The efficient estimators for the covariate distributions of subpopulation g equal

$$\hat{F}_g^{eff} = \frac{1}{\sum_{i=1}^{N}\hat{p}_g(X_i)}\sum_{i=1}^{N}\hat{p}_g(X_i)1\{X_i \leq x\}.$$

Accordingly, reweighting estimators that recover $\hat{F}_g^{eff}$ rather than of $\hat{F}_g$ may be more efficient. We report the efficient first moments for all control variables and both treatment groups in Table A.2 in Appendix A.

Graham, De Xavier Pinto, and Egel (2011) recently proposed a double robust and locally efficient semiparametric version of IPW, named *Auxiliary-to-Study Tilting* (AST). This estimator precisely balances the efficient first moments of all control variables in each treatment sample.[14] Using AST, the propensity score is estimated in a conventional parametric way. We use the probit model $\hat{p}_g(X_i) = \Phi(X_i'\hat{\beta})$, where $\Phi(\cdot)$ denotes the cumulative normal distribution function and $X_i'\hat{\beta}$ is the estimated linear index. The estimated propensity score $\hat{p}_d(x)$ is replaced by $\tilde{p}_d(x)$. It is estimated under the following

[14] Exact balancing is not guaranteed for the sample moments using conventional IPW estimators.

moment conditions:

$$\frac{1}{N}\sum_{i=1}^{N}\frac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_g(X_j)}\cdot\frac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)}\cdot X_i=\frac{1}{N}\sum_{i=1}^{N}\frac{\hat{p}_g(X_i)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_g(X_j)}\cdot X_i, \tag{3}$$

where $\tilde{p}_d(X_i)=\Phi(X_i'\tilde{\beta})$ is specified such that the left and right sides of (3) are numerically equivalent for all elements in X_i (including a constant term). The right side is the efficient first moment estimate. As the efficient first moment estimates are independent of subpopulation d, the first moments are exactly balanced in all treatment groups for $d\in\{at_0,vt_1,nt_0,nt_1\}$ using this procedure. The constant guarantees that the weights sum to one. The expected potential outcomes are estimated using

$$\tilde{E}[Y_i(d)|D_i=g]=\frac{1}{N}\sum_{i=1}^{N}\tilde{\omega}_iY_i,$$

with

$$\tilde{\omega}_i=\frac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_g(X_j)}\cdot\frac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)}.$$

It can be shown that this estimator is $\sqrt{N}$-consistent and asymptotically normal distributed.[15] Similar to Graham, De Xavier Pinto, and Egel (2011), we compute the significance levels (p-values) of our estimated parameters based on a non-parametric bootstrapping procedure (sampling individual observations with replacement).

[15]The large sample properties of AST are subject to assumptions regarding the specification of the propensity score. These assumptions imply that the propensity score is correctly specified, strictly increasing in its arguments, differentiable, and well located within the unit interval.

C Matching quality

We assess the matching quality by reporting the moments (mean, variance, skewness, kurtosis) and standardised differences for the control variables in all four samples. The standardised differences are defined by

$$SD = \frac{|\mu_d - \mu_g|}{\sqrt{0.5(\sigma^2_{\mu_d} + \sigma^2_{\mu_g})}} \cdot 100\%,$$

where μ_k is the moment and $\sigma^2_{\mu_k}$ is the variance of the moment in the respective treatment group $k \in \{at_0, vt_1, nt_0, nt_1\}$. The pre-matching standardised differences between the sample first moments are reported in Table 2. The post-matching standardised differences between the efficient first moments are exactly zero, as the first moments are precisely balanced (see the discussion in Appendix B). Therefore, we do not report the standardised difference of the matched treatment and control samples in Table A.2 (only between the voucher and direct assignment regime).

In the optimal case, matching estimators balance the complete distributions of all control variables rather than only the first moments. For all binary variables, this requirement is satisfied because the first moments are balanced. In the main specifications, we control for 63 variables, 43 of which are binary. For the other variables, we report the variance, skewness, and kurtosis for the different samples matched to the treatment group under the voucher regime in Table C.1. Furthermore, we present the higher moments for the different samples matched to the treatment group under the assignment regime in Table C.2. For most moments, we report small standardised differences. However, particularly for the monthly regional labour market characteristics, we find large differences in the higher moments for the samples that are matched to the treatment group under the assignment regime.

Table C.1: Higher moments of observed characteristics matched to the treatment group under the voucher regime.

	Voucher Regime		Assignment Regime		Standardised Differences between		
	Treatment-group	Control-group	Treatment-group	Control-group	(1) and (2)	(1) and (3)	(1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variance							
Age	55.48	64.64	56.33	63.1	13.62	1.35	11.68
Half months empl. in the last 24 months	41.45	40.17	38.23	36.72	1.16	2.98	4.47
Half months unempl.in the last 24 months	2.98	3.12	3.12	3.14	.72	.67	.77
Time since last unemployment in the last 24 months (half-months)	20.07	20.45	20.73	20.5	.42	.67	.44
# unemployment spells in the last 24 months	.17	.17	.17	.17	.20	.07	.48
Time of last out of labour force in last 24 months	42.36	41.97	40.48	40.62	.26	1.34	1.24
Remaining unemployment insurance claim	174.91	193.14	163.49	185.54	6.64	4.47	4.00
Eligibility unemployment benefits	25.6	25.53	25.83	25.22	.17	.54	.89
Cumulative employment (last 4 years before unemployment)	513.34	468.74	491.27	442.71	5.73	2.83	9.28
Cumulative earnings (last 4 years before unemployment)	$2.42 \cdot 10^9$	$2.52 \cdot 10^9$	$2.38 \cdot 10^9$	$2.48 \cdot 10^9$	2.71	1.33	1.68
Cumulative benefits (last 4 years before unemployment)	62.67	61.98	66.42	66.77	.23	1.19	1.22
Elapsed unemployment duration	11.31	12.23	13.06	12.4	8.58	16.09	10.22
Share of empl.in production industry	.00783	.00816	.00913	.00918	3.54	12.84	13.40
Share of empl. in construction industry	.0004	.00043	.00038	.00039	4.26	3.82	1.98
Share of empl. in trade industry	.00032	.00035	.00032	.00030	6.03	1.03	3.65
Share of male unempl.	.00175	.00175	.00154	.00152	.15	8.55	9.64
Share of non-German unempl.	.00734	.00742	.00659	.00644	.97	9.16	10.92
Share of vacant fulltime jobs	.00584	.00583	.00383	.00391	.12	21.42	20.81
Population per km^2	2814047	2820432	2039321	2152881	.07	8.78	7.37
Unemployment rate (in %)	25.56	26.3	19.42	19.02	2.15	19.77	20.87
Skewness							
Age	46.23	110.42	81.03	115.84	5.20	3.18	5.87
Half months empl. in the last 24 months	-691.31	-663.08	-610.47	-566.93	1.18	3.50	5.52
Half months unempl. in the last 24 months	29.37	32.61	34.63	35.01	.97	1.40	1.52
Time since last unemployment in the last 24 months (half-months)	-381.07	-400.22	-449.84	-439.91	.89	2.61	2.30
# unemployment spells in the last 24 months	0.31	0.29	0.31	0.33	.59	.16	.63
Time of last out of labour force in last 24 months	-865.15	-866.25	-789.08	-797.59	.03	1.80	1.61
Remaining unemployment insurance claim	1.07	353.20	-210.13	48.83	3.62	2.44	.52
Eligibility unemployment benefits	145.11	157.14	154.61	152.87	2.10	1.72	1.33
Cumulative employment (last 4 years before unemployment)	$-1.57 \cdot 10^4$	$-1.37 \cdot 10^4$	$-1.42 \cdot 10^4$	$-1.25 \cdot 10^4$	4.64	3.41	7.38
Cumulative earnings (last 4 years before unemployment)	$7.04 \cdot 10^{13}$	$9.0 \cdot 10^{13}$	$7.02 \cdot 10^{13}$	$9.41 \cdot 10^{13}$	4.15	0.04	5.03
Cumulative benefits (last 4 years before unemployment)	2,014.09	2,016.49	2,268.74	2,441.50	.01	1.34	1.86
Elapsed unemployment duration	4.15	3.54	4.58	3.41	.75	.52	.91
Share of empl. in production industry	.00022	.00031	.00053	.00054	4.33	13.62	13.83
Share of empl. in construction industry	.00001	.00001	.00001	.00001	2.61	9.02	10.29
Share of empl. in trade industry	.00000	.00000	.00000	.00000	1.20	9.53	10.75
Share of male unempl.	-.00004	-.00004	.00000	-.00001	.32	13.85	10.23
Share of non-German unempl.	.00016	.00016	.00010	.00012	.36	3.13	2.28
Share of vacant fulltime jobs	-.00040	-.00043	-.00019	-.00017	.92	8.39	9.28
Population per km^2	$1.52 \cdot 10^{10}$	$1.53 \cdot 10^{10}$	$1.03 \cdot 10^{10}$	$1.11 \cdot 10^{10}$	0.17	8.30	6.82
Unemployment rate (in %)	112.15	124.58	71.48	69.47	2.82	10.81	11.08
Kurtosis							
Age	6984	9302	7132	8593	13.28	1.05	9.75
Half months empl. in the last 24 months	14214	13745	12377	11302	.88	3.64	5.95
Half months unempl. in the last 24 months	375	440	521	520	.96	1.82	1.84
Time since last unemployment in the last 24 months (half-months)	8409	9053	11762	11308	1.22	4.30	3.95
# unemployment spells in the last 24 months	.84	.69	.75	.89	.81	.47	.27
Time of last out of labour force in last 24 months	22815	23205	20015	20243	.26	1.98	1.85
Remaining unemployment insurance claim	100847	117898	87180	105122	5.70	5.40	1.60
Eligibility unemployment benefits	2374	2615	2533	2621	3.11	2.23	3.06
Cumulative employment (last 4 years before unemployment)	912559	781159	807997	705710	5.50	4.38	9.00
Cumulative earnings (last 4 years before unemployment)	$1.78 \cdot 10^{19}$	$2.04 \cdot 10^{19}$	$1.74 \cdot 10^{19}$	$1.96 \cdot 10^{19}$	4.15	0.88	3.08
Cumulative benefits (last 4 years before Unemployment)	94493	98694	113972	139252	.33	1.48	2.36
Elapsed unemployment duration	241	266	295	269	7.26	15.21	8.26
Share of empl. in production industry	.0001413	.0001612	.0002073	.0002076	4.96	14.45	14.70
Share of empl. in construction industry	.0000005	.0000005	.0000006	.0000006	4.02	5.82	8.46
Share of empl. in trade industry	.0000003	.0000004	.0000003	.0000003	4.92	3.75	4.30
Share of male unempl.	.0000095	.0000098	.0000078	.0000074	1.03	5.88	7.43
Share of non-German unempl.	.0001247	.0001258	.000105	.0001041	.38	7.28	7.60
Share of vacant fulltime jobs	.0001584	.0001677	.0000669	.0000632	.94	12.29	13.13
Population per km^2	$1.0 \cdot 10^{14}$	$1.0 \cdot 10^{14}$	$6.8 \cdot 10^{14}$	$7.3 \cdot 10^{14}$	.22	8.46	6.91
Unemployment rate (in %)	1740	1941	1219	1239	3.82	12.50	11.49

Note: In columns (1)-(4), we report the variance, skewness, and kurtosis of observed characteristics for the treated and non-treated sub-samples. Information on individual characteristics refers to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics, which refer to the (pseudo) treatment time. In columns (5)-(7), we report the standardised differences between the different sub-samples and the treatment group under the voucher regime. All control variables that are not reported in this table have binary distributions. The higher moments of these variables are precisely balanced in the matched samples.

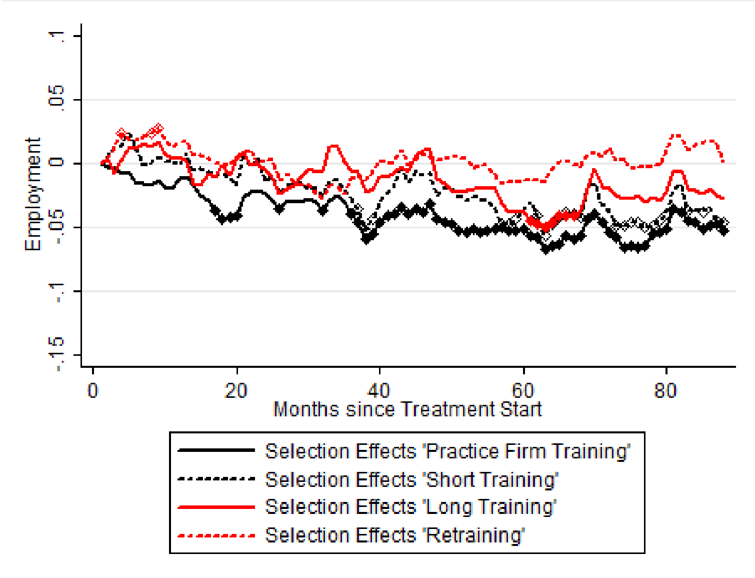
Table C.2: Higher moments of observed characteristics matched to the treatment group under the assignment regime.

	Voucher Regime		Assignment Regime		Standardised Differences between		
	Treatment-group	Control-group	Treatment-group	Control-group	(1) and (2)	(1) and (3)	(1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variance							
Age	59.75	60.3	72.45	66.49	.82	16.27	8.64
Half months empl. in the last 24 months	58.91	54.18	62.6	53.03	3.98	6.51	1
Half months unempl. in the last 24 months	3.93	4.32	4.4	4.2	1.83	.33	.51
Time since last unemployment in the last 24 months (half-months)	41.11	45.83	44.56	44.64	3.34	.84	.75
# unemployment spells in the last 24 months	.25	.26	.25	.26	.64	.55	.13
Time of last out of labour force in last 24 months	60.4	57.71	60.05	58.79	1.59	1.46	.66
Remaining unemployment insurance claim	156.52	145.13	178.65	169.72	4.91	14.17	10.4
Eligibility unemployment benefits	27.96	28.14	28.14	28.02	.39	.02	.24
Cumulative employment (last 4 years before unemployment)	606.44	559.64	553.45	518.33	5.73	.78	5.39
Cumulative earnings (last 4 years before unemployment)	$2.14\cdot10^{9}$	$2.04\cdot10^{9}$	$2.11\cdot10^{9}$	$2.15\cdot10^{9}$	3.152	2.242	3.228
Cumulative benefits (last 4 years before unemployment)	85.48	84.85	76.86	83.58	.17	2.28	.33
Elapsed unemployment duration	10.82	12.12	13.18	12.03	11.8	9.35	.81
Share of empl. in production industry	.00577	.0086	.00573	.00842	32.26	31.97	1.76
Share of empl. in construction industry	.00082	.0007	.00083	.00075	15.05	16.32	6.07
Share of empl. in trade industry	.00041	.00038	.00038	.00038	4.26	1.29	.29
Share of male unempl.	.00283	.00204	.00264	.00211	26.2	21.6	.43
Share of non-German unempl.	.00991	.0084	.00931	.00833	14.99	9.19	.83
Share of vacant fulltime jobs	.00502	.00524	.005	.00485	2.3	2.57	4.11
Population per km^2	2800928	2321975	2881181	2377444	5.02	5.81	.62
Unemployment rate (in %)	31.81	32.41	34.03	31.58	1.99	5.25	2.52
Skewness							
Age	85.74	110.07	153.47	163.78	2	2.96	3.9
Half months empl. in the last 24 months	-894.18	-795.24	-1055.27	-772.03	3.87	8.51	.9
Half months unempl. in the last 24 months	31.99	43.57	39.91	40.72	3.36	1.02	.69
Time since last unemployment in the last 24 months (half-months)	-732.4	-1014.77	-896.02	-969.59	7.23	2.76	.96
# unemployment spells in the last 24 months	.44	.48	.43	.5	1.09	1.44	.41
Time of last out of labour force in last 24 months	-1244.94	-1115.88	-1147.04	-1168.59	2.46	.66	1.05
Remaining unemployment insurance claim	126.4	-109.32	176.5	228.35	3.05	3.59	4.3
Eligibility unemployment benefits	152.86	166.34	184.87	175.92	2.08	2.49	1.31
Cumulative employment (last 4 years before unemployment)	-18075.06	-15691.71	-15693.52	-14064.85	5.02	.004	3.72
Cumulative earnings (last 4 years before unemployment)	$6.47\cdot10^{13}$	$6.33\cdot10^{13}$	$7.69\cdot10^{13}$	$8.62\cdot10^{13}$	.34	3.08	5.03
Cumulative benefits (last 4 years before unemployment)	3005.55	2948.6	2372.48	2995.96	.25	2.75	.18
Elapsed unemployment duration	9.71	12.41	11.45	11.55	3.23	1.07	1
Share of empl. in production industry	.0001537	.000418	.0001786	.0004147	13.62	11.99	.14
Share of empl. in construction industry	.0000071	.0000112	.0000112	.0000135	8.1	.06	4.02
Share of empl. in trade industry	.0000054	.0000044	.0000049	.0000045	3.59	1.7	.44
Share of male unempl.	-.0000996	-.0000117	-.0000718	-.0000223	23.68	18.35	3.3
Share of non-German unempl.	.0005108	.0002599	.0004424	.0002689	11	8.29	.44
Share of vacant fulltime jobs	-.0002477	-.0004009	-.0002327	-.0003027	5.25	6.01	3.66
Population per km^2	$1.63\cdot10^{10}$	$1.28\cdot10^{10}$	$1.68\cdot10^{10}$	$1.33\cdot10^{10}$	5.56	6.26	.84
Unemployment rate (in %)	104.8	104.26	115.28	101.21	.14	2.7	0.7
Kurtosis							
Age	7962.29	8231.21	11811	10104.53	1.62	15.71	8.85
Half months empl. in the last 24 months	18211.33	16415.04	23904.57	15972.42	3.16	9.7	.7
Half months unempl. in the last 24 months	331.3	602.69	439.63	540.75	3.88	2.29	.72
Time since last unemployment in the last 24 months (half-months)	15975.52	27816.77	22022.23	26122.82	10.04	4.4	1.13
# unemployment spells in the last 24 months	1.11	1.28	1	1.4	.86	2.17	.69
Time of last out of labour force in last 24 months	34620.56	29581.17	28946.72	31456.16	2.86	.41	1.12
Remaining unemployment insurance claim	83697.48	69367.07	95652.58	92332.71	6.03	11.01	9.91
Eligibility unemployment benefits	2813.09	3052.54	3399.18	3331.8	2.61	2.98	2.49
Cumulative employment (last 4 years before unemployment)	1086431	929914.3	939481.9	827675.9	6.41	.41	4.58
Cumulative earnings (last 4 years before unemployment)	$1.48\cdot10^{19}$	$1.40\cdot10^{19}$	$1.57\cdot10^{19}$	$1.72\cdot10^{19}$	1.57	2.9	5.35
Cumulative benefits (last 4 years before unemployment)	149761.7	151238.6	108257.2	165057.4	.1	2.98	.66
Elapsed unemployment duration	232.13	274.49	302.67	268.76	10.38	6.53	1.36
Share of empl. in production industry	.00008	.00018	.00009	.00018	24.78	22.17	.09
Share of empl. in construction industry	.0000012	.0000011	.0000014	.0000014	4.01	9.89	6.4
Share of empl. in trade industry	.0000004	.0000004	.0000004	.0000005	1.97	1.76	2.03
Share of male unempl.	.00002	.00001	.00002	.00001	23.47	15.54	4.27
Share of non-German unempl.	.00022	.00015	.0002	.00015	16.55	11.75	.31
Share of vacant fulltime jobs	.00012	.00013	.00011	.0001	1.8	2.87	3.6
Population per km^2	$1.10\cdot10^{14}$	$8.52\cdot10^{13}$	$1.14\cdot10^{14}$	$8.88\cdot10^{13}$	5.78	6.51	.89
Unemployment rate (in %)	1761.13	2128.73	1985.96	2093.44	9.62	3.59	.8

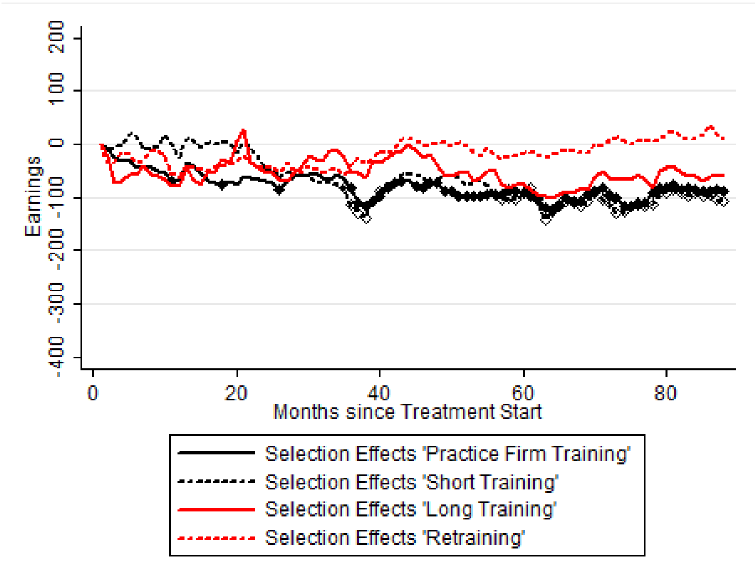
Note: In columns (1)-(4), we report the variance, skewness, and kurtosis of observed characteristics for the treated and non-treated sub-samples. Information on individual characteristics refers to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics, which refer to the (pseudo) treatment time. In columns (5)-(7), we report the standardised differences between the different sub-samples and the treatment group under the voucher regime. All control variables that are not reported in this table have binary distributions. The higher moments of these variables are precisely balanced in the matched samples.

D Heterogeneous results by programme type

Figure D.1: Selection effects on employment and earnings by programme type.



(a) Effects on employment



(b) Effects on monthly earnings (in Euros)

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency district characteristics and the full set of observed characteristics (see Table A.2 in Appendix A).

Table D.1: Efficient first moments of observed characteristics by programme type.

	Practice Firm Training			Short Training		
	Post-reform treatment-group	Pre-reform treatment-group	SD	Post-reform treatment-group	Pre-reform treatment-group	SD
Personal Characteristics						
Female	.505	.506	0.2	.438	.455	3.4
Age	40.379	40.742	4.6	39.838	39.821	0.2
Older than 50 years	.017	.029	8	.014	.027	9.1
No German citizenship	.049	.056	3.2	.053	.051	0.8
Children under 3 years	.032	.033	0.9	.042	.039	1.3
Single	.287	.253	7.7	.32	.271	10.8
Health problems	.109	.132	7.1	.085	.097	4
Sanction	.007	.009	2.9	.006	.005	0.9
Incapacity (e.g., illness, pregnancy)	.024	.04	9.4	.022	.032	6.4
Lack of motivation	.007	.009	2.9	.006	.005	0.9
Education and Occupation						
No schooling degree	.044	.056	5.6	.036	.041	2.4
Schooling degree without Abitur	.797	.826	7.3	.714	.792	18.3
University entry degree (Abitur)	.147	.101	13.9	.235	.15	21.7
No vocational degree	.175	.235	14.9	.152	.192	10.8
Academic degree	.058	.035	10.7	.122	.058	22.5
White-collar	.404	.494	18.2	.36	.473	23.1
Agriculture, Fishery	.009	.019	8.8	.009	.014	5
Manufacturing	.061	.102	15	.077	.121	14.9
Construction	.018	.011	6	.056	.019	20
Trade and Retail	.111	.154	12.8	.117	.154	10.8
Communication and Information Service	.108	.128	6.2	.106	.123	5.1
Employment and Welfare History						
Half months empl. in last 2 years	45.791	44.351	20.8	46.161	44.699	22.8
Half months unempl. in last 2 years	.342	.647	16.1	.299	.584	15.9
Half months since last unempl. in last 2 years	46.837	45.288	25.8	47.026	45.613	25.4
No unempl. in last 2 years	.923	.867	18.4	.926	.876	16.7
Unemployed in last 2 years	.03	.052	11.1	.027	.048	11.2
# unemployment spells in last 2 years	.102	.184	17.5	.092	.168	17.2
Any program in last 2 years	.044	.057	5.9	.042	.054	5.5
Half months since of last OLF in last 2 years	45.853	44.766	15.1	46.205	45.057	17
Remaining unempl. insurance claim	24.422	21.954	20.7	26.814	23.191	27.8
Eligibility unempl. benefits	14.304	14.025	4.8	14.246	13.83	7.5
Cumulative empl. in last 4 years	82.519	79.073	15.1	83.359	80.452	13.3
Cumulative earnings in last 4 years	87508.63	76438.78	25.7	99140.05	83323.52	33.1
Cumulative benefits in last 4 years	2.693	4.107	16.3	2.586	3.733	13.9
Timing of Unemployment and Program Start						
Start unempl. in January	.072	.122	17	.077	.155	24.6
Start unempl. in February	.072	.101	10.1	.08	.115	11.6
Start unempl. in March	.099	.093	1.9	.106	.103	0.8
Start unempl. in April	.079	.109	10.1	.103	.111	2.4
Start unempl. in June	.065	.062	1.4	.06	.049	4.7
Start unempl. in July	.056	.076	8.1	.047	.05	1.2
Start unempl. in August	.081	.075	2.2	.051	.064	5.8
Start unempl. in September	.123	.09	10.5	.133	.08	17.3
Start unempl. in October	.107	.075	11.1	.129	.084	14.7
Start unempl. in November	.11	.063	17	.099	.057	15.8
Start unempl. in December	.066	.054	5.2	.044	.041	1.1
Elapsed unempl. duration	5.313	5.089	6.8	5.603	4.528	32.7
State of Residence						
Baden-Württemberg	.109	.11	0.4	.103	.114	3.6
Bavaria	.197	.164	8.4	.19	.102	25.1
Berlin, Brandenburg	.009	.026	12.8	.104	.117	4.2
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	.061	.112	18.1	.075	.124	16.2
Hesse	.047	.067	8.7	.056	.064	3.3
Northrhine-Westphalia	.182	.115	19.1	.248	.172	18.9
Rhineland Palatinate, Saarland	.074	.077	1.2	.058	.056	1
Saxony-Anhalt, Saxony, Thuringia	0.1	.163	18.9	.09	.17	23.7
Characteristics of Local Employment Agency Districts						
Share of empl. in production industry	.28	.266	15	.253	.248	5.2
Share of empl. in construction industry	.065	.077	51.9	.062	.076	63
Share of empl. in trade industry	.15	.15	0.7	.15	.151	4.5
Share of male unempl.	.562	.539	55.4	.566	.543	53.3
Share of non-German unempl.	.137	.122	18.4	.148	.129	20.6
Share of vacant fulltime jobs	.794	.801	9.8	.799	.802	3.6
Population per km^2	438.754	431.641	0.9	992.365	808.334	11
Unemployment rate (in %)	10.861	11.165	6.1	11.687	11.866	3.4

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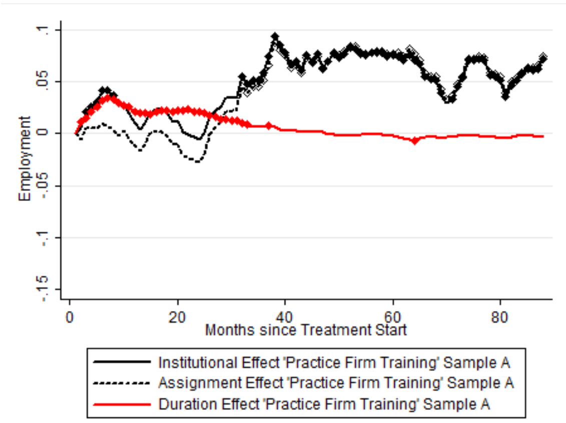
Table D.1: < continued >

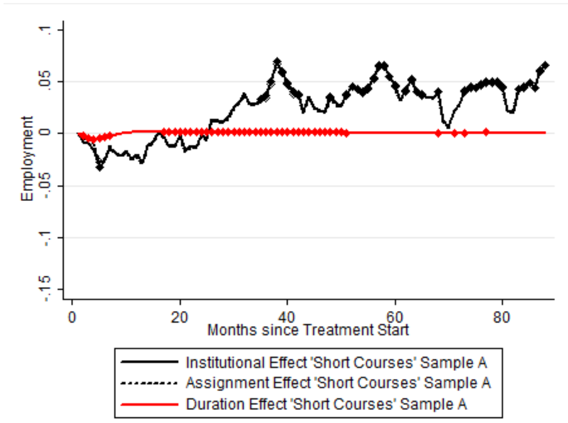
	Long Training			Retraining		
	Post-reform treatment-group	Pre-reform treatment-group	SD	Post-reform treatment-group	Pre-reform treatment-group	SD
Personal Characteristics						
Female	.437	.466	5.8	.541	.505	7.2
Age	39.424	39.139	3.9	35.84	35.082	12
Older than 50 years	.009	.018	8.1	.001	.001	1.3
No German citizenship	.037	.037	.1	.072	.08	3
Children under 3 years	.042	.038	2.3	.047	.049	.7
Single	.324	.279	9.8	.253	.259	1.4
Health problems	.085	.083	.7	.064	.073	3.7
Sanction	.005	.004	1	.011	.023	9
Incapacity (e.g., illness, pregnancy)	.018	.031	8.1	.023	.026	1.6
Lack of motivation	.005	.004	1	.011	.023	9
Education and Occupation						
No schooling degree	.024	.024	.1	.043	.039	1.7
Schooling degree without Abitur	.606	.687	17	.781	.775	1.4
University entry degree (Abitur)	.361	.274	18.7	.158	.161	.9
No vocational degree	.117	.135	5.5	.366	.408	8.6
Academic degree	.21	.133	20.6	.049	.033	8
White-collar	.262	.347	18.5	.498	.555	11.5
Agriculture, Fishery	.009	.013	4.4	.02	.017	2
Manufacturing	.073	.105	11.2	.056	.065	3.8
Construction	.112	.043	26	.023	.017	3.7
Trade and Retail	.078	.113	12	.188	.213	6.4
Communication and Information Service	.096	.124	8.9	.122	.115	2.2
Employment and Welfare History						
Half months empl. in last 2 years	45.57	44.696	12.9	44.512	44.011	6.5
Half months unempl. in last 2 years	.42	.522	5.6	.577	.637	2.7
Half months since last unempl. in last 2 years	46.717	45.728	17.4	46.296	45.706	10.2
No unempl. in last 2 years	.911	.884	8.7	.889	.873	5.1
Unemployed in last 2 years	.039	.043	2.4	.045	.046	.3
# unemployment spells in last 2 years	.112	.153	9.6	.155	.178	4.5
Any program in last 2 years	.047	.06	5.6	.057	.052	2.3
Half months since of last OLF in last 2 years	45.872	45.04	12.1	44.925	44.435	6.5
Remaining unempl. insurance claim	27.906	24.483	25.9	22.535	22.284	2.2
Eligibility unempl. benefits	13.588	13.358	4.4	11.756	11.414	9.7
Cumulative empl. in last 4 years	81.22	79.723	6.6	76.626	75.987	2.5
Cumulative earnings in last 4 years	100047.1	85702.63	28.1	72954.52	71362.17	4
Cumulative benefits in last 4 years	3.011	3.419	4.9	3.835	3.898	.7
Timing of Unemployment and Program Start						
Start unempl. in January	.063	.124	20.9	.027	.056	14.8
Start unempl. in February	.065	.114	17.2	.055	.098	16.2
Start unempl. in March	.103	.12	5.2	.073	.085	4.4
Start unempl. in April	.101	.135	10.4	.118	.111	2.1
Start unempl. in June	.067	.066	.3	.049	.045	2
Start unempl. in July	.048	.035	6.5	.062	.077	5.9
Start unempl. in August	.064	.06	1.6	.141	.14	.3
Start unempl. in September	.137	.087	15.6	.201	.149	13.8
Start unempl. in October	.113	.068	15.7	.146	.128	5.2
Start unempl. in November	.099	.04	23.5	.042	.029	6.9
Start unempl. in December	.059	.04	8.8	.027	.026	.1
Elapsed unempl. duration	.036	4.812	6.7	4.078	3.486	16.5
State of Residence						
Baden-Württemberg	.059	.064	2.1	.068	.127	20.1
Bavaria	.103	.074	10.2	.132	.158	7.6
Berlin, Brandenburg	.146	.159	3.5	.077	.054	9.4
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	.07	.083	4.8	.09	.083	2.4
Hesse	.063	.057	2.7	.087	.076	4
Northrhine-Westphalia	.251	.193	14	.219	.226	1.8
Rhineland Palatinate, Saarland	.04	.039	.8	.053	.069	6.7
Saxony-Anhalt, Saxony, Thuringia	.177	.256	19.4	.147	.095	16
Characteristics of Local Employment Agency Districts						
Share of empl. in production industry	.227	.225	2.5	.248	.271	25.5
Share of empl. in construction industry	.065	.08	56.8	.067	.071	22
Share of empl. in trade industry	.148	.149	3.9	.15	.153	16.9
Share of male unempl.	.563	.541	48.1	.562	.543	41.9
Share of non-German unempl.	.136	.118	19.4	.128	.145	20.2
Share of vacant fulltime jobs	.785	.792	9.1	.79	.814	32.8
Population per km^2	1237.779	975.571	13.4	776.062	680.728	7.1
Unemployment rate (in %)	13.657	13.534	2.2	12.593	10.091	50

Note: We report the efficient first moments of observed characteristics for the treated sub-samples by programme type. Information on individual characteristics refers to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and monthly regional labour market characteristics, which refer to the (pseudo) treatment time. Furthermore, we report the standardised differences (SD) between the two treatment groups for each programme type. A description of how we measure standardised differences (SD) is available in Appendix C. OLF is the acronym for "out of labour force".

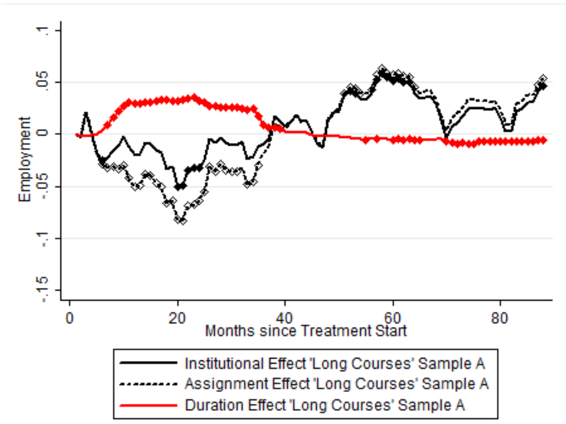
Figure D.2: Institutional, programme duration, and assignment effects by programme type.



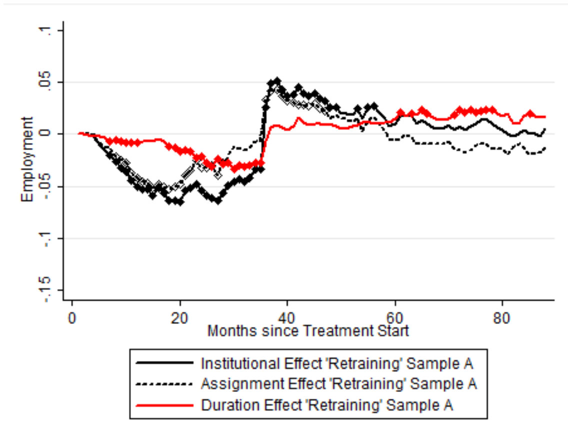
(a) Practice firm programmes



(b) Short training programmes



(c) Long training programmes



(d) Retraining programmes

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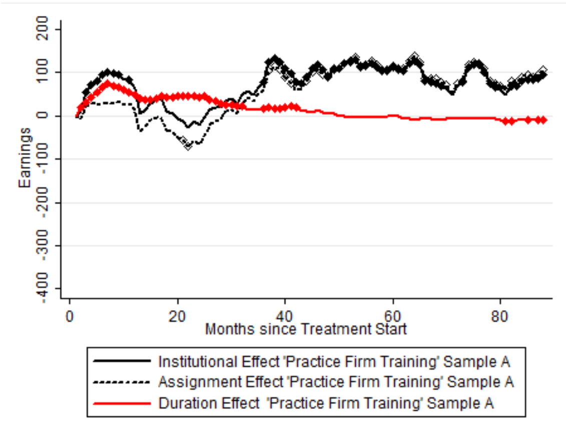
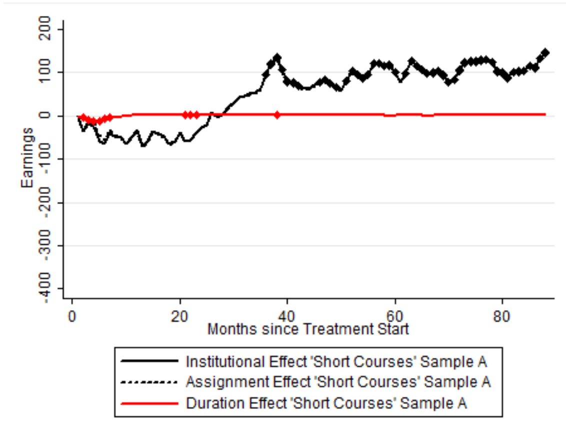
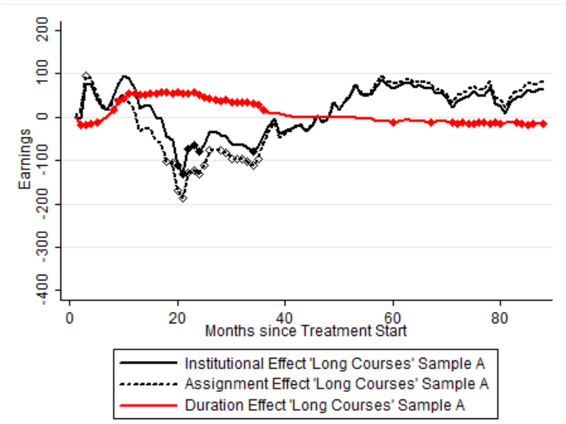
Figure D.2: < continued >



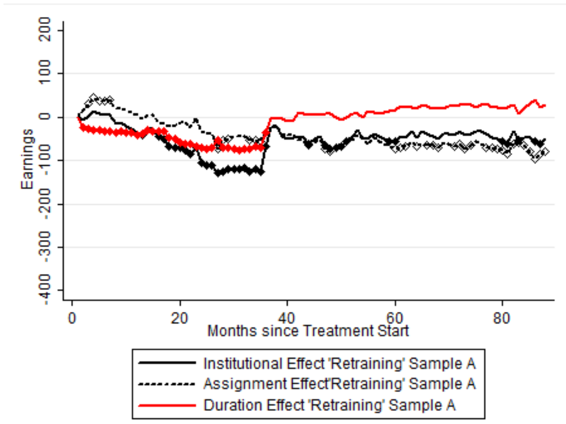
(a) Practice firm programmes



(b) Short training programmes



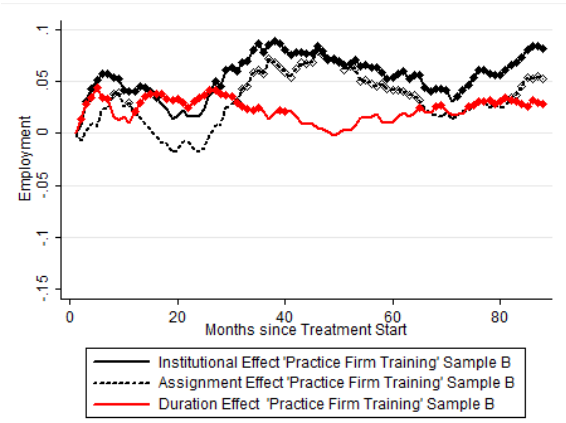
(c) Long training programmes



(d) Retraining programmes

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency districts characteristics and the full set of observed characteristics (see Table A.2 in Appendix A). In computing the duration effects, we account for the planned course durations.

Figure D.3: Institutional, programme duration, and assignment effects by programme type.



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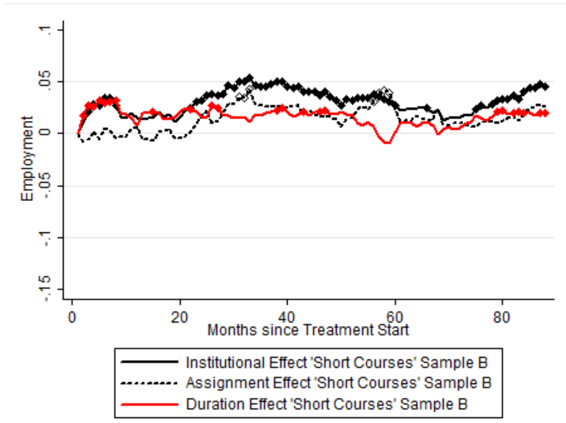
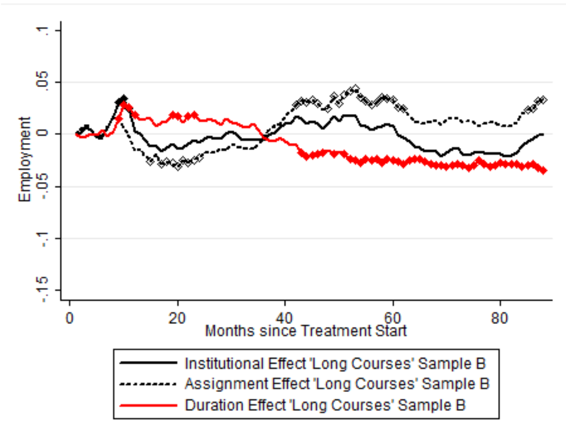
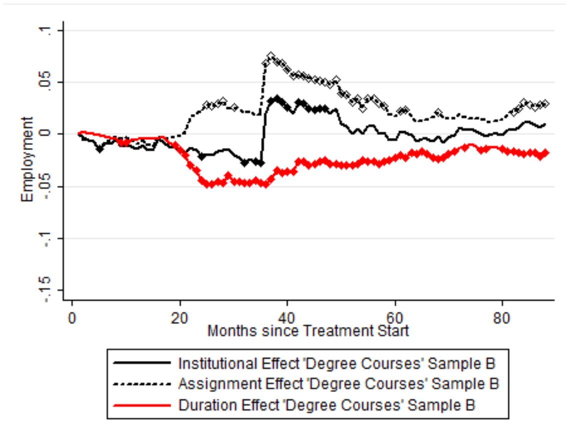


(b) Short training programmes



(c) Long training programmes



(d) Retraining programmes

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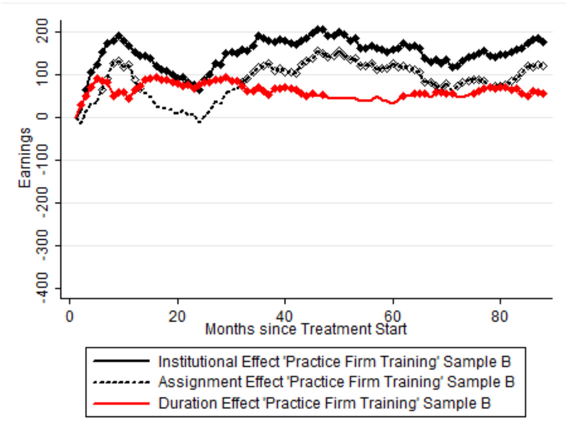
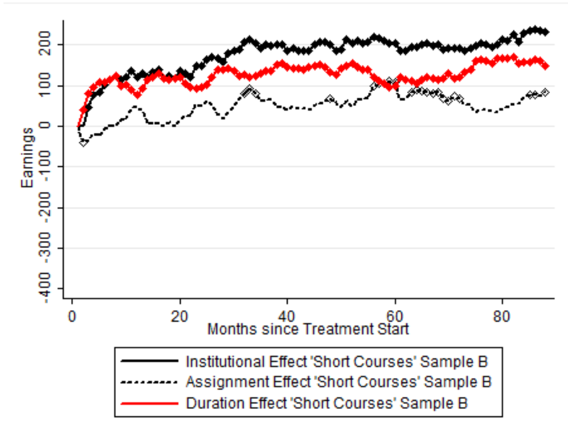
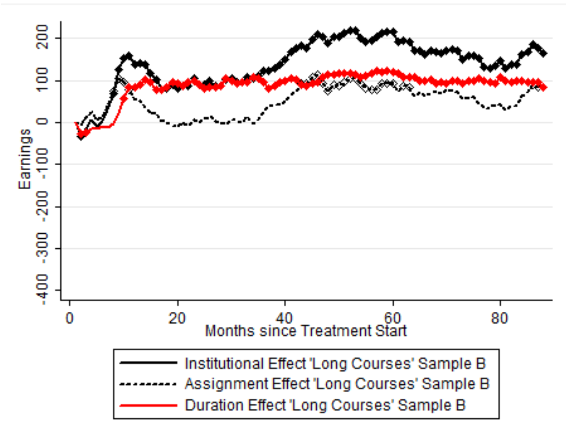
Figure D.3: < continued >



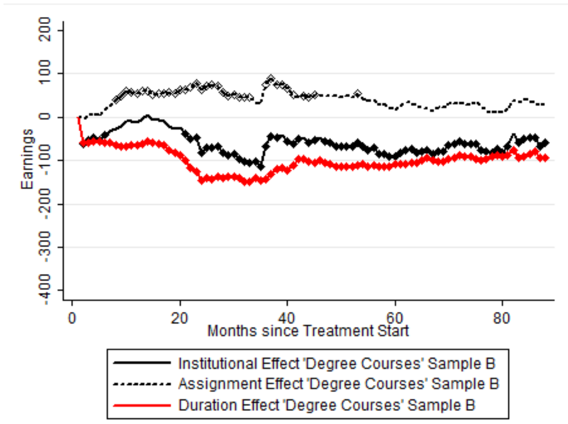
(a) Practice firm programmes



(b) Short training programmes



(c) Long training programmes



(d) Retraining programmes

Note: We estimate separate effects for each of the first seven years following the treatment. Diamonds indicate significant point estimates at the 5%-level. Significance levels are bootstrapped with 499 replications. Lines without diamonds indicate point estimates that are not significantly different from zero. We use baseline Sample A and control for local employment agency districts characteristics and the full set of observed characteristics (see Table A.2 in Appendix A). In computing the duration effects, we account for the planned course durations.