Assignment Mechanisms, Selection Criteria, and the Effectiveness of Training Programs

Annabelle Doerr
Albert-Ludwigs-University Freiburg
IAB Nuremberg

Anthony Strittmatter
Albert-Ludwigs-University Freiburg
University of St. Gallen

Preliminary and Incomplete
Comments are very welcome!

July 24, 2013

Abstract

We analyze the effectiveness of further training for unemployed under two different regulatory regimes, which are featured by different assignment mechanisms and selection criteria. In the pre-reform period, unemployed are directly assigned to specific training providers and courses. Under the new regime a voucher-like system is implemented. Further, new selection criteria should increase the share of participants with high employment probabilities after training. We find no influences of the assignment mechanisms and selection criteria on the effectiveness of further training with respect to employment and earnings 48 months after treatment start. However, our results show changing compositions of program types and durations under the voucher regime, which lead to a higher effectiveness of training in the short run. In the medium run, the effectiveness of training decreases under the voucher regime.

JEL-Classification: J68, H43, C21
Keywords: Active Labor Market Policies, Treatment Effects Evaluation, Administrative Data, Voucher

This study is part of the project "Regional Allocation Intensities, Effectiveness and Reform Effects of Training Vouchers in Active Labor Market Policies", IAB project number 1155. This is a joint project of the Institute for Employment Research (IAB) and the University of Freiburg. We gratefully acknowledge financial and material support by the IAB. The usual disclaimer applies.

Contents

1 Introduction

The provision of public sponsored further training is a major part of active labor market policies (ALMP) in Germany.[1] Between 2000 and 2002, the expenditures exceeded 20 billion Euros. Although the monetary value of further training was very high, its reputation among federal institutions and policy makers was poor during this time period. The main criticism was focused on the assignment rules into further training courses and the close cooperation between employment offices and training providers. The latter resulted in low competition, lacking transparency, and high susceptibility for corruption. Reinforced by judgments of the Federal Court of Justice, the provision of further training was reorganized in January 2003.

The direct assignment of unemployed to specific training providers and courses by caseworkers was replaced by a voucher-like allocation system. Beside an increase in the freedom of choice and self-responsibility of program participants, training vouchers are supposed to intensify the competition among training providers and to overcome existing market failures. At the same time, new selection criteria for program participants were implemented. Unemployed receive a training voucher if caseworkers in local employment offices judge the participation in a further training course as an effective instrument to reintegrate this person into the labor market. According to the new criteria, caseworkers have to select voucher recipients such that the quota of successful reintegration into employment within six months after the end of training is at least 70%. In this study, we focus on the effectiveness of further training under the two different regulatory regimes. We separate effects which result from different assignment mechanisms (in the following: *institutional effects*) and selection criteria (in the following: *selection effects*).

The assignment rules in the German Training Voucher system are comparable to voucher-like systems in other countries. The German Training Vouchers and the Adult and Dislocated Worker Program under the Workforce Investment Act (WIA) in the United States are the largest programs using voucher-like systems to assign public sponsored

[1] Further training programs provide occupational specific skills to participants. Please find a detailed description in Section 2.1.

further training. German Training Voucher recipients may only choose approved training courses and providers. The redemption of the voucher is restricted to the definition of the course target, cost and time limits. This is similar for customers in the WIA program who receive training through Individual Training Accounts (ITA) that operate like vouchers. In contrast to the WIA, direct guidance regarding the choice of training providers by caseworkers is not allowed in the German Training Voucher system.

Our analysis is based on unique process generated data provided by the Federal Employment Agency of Germany. The data contain information on *all* individuals who participated in further training courses in 2001 or 2002 and information on *all* individuals who received a training voucher in 2003 or 2004. To enrich the voucher data with individual-specific information, we merge data records of the Integrated Employment Biographies (IEB). This data set contains information on employment outcomes and a rich set of control variables, e.g. the complete employment and welfare histories, various socioeconomic characteristics, and information on health and disabilities. We rely on an identification strategy which combines selection on observables assumptions (Rosenbaum and Rubin, 1983) with time dependence and structural assumptions. The estimation is based on *Auxiliary-to-Study Tilting* (AST), a novel estimator proposed by Graham, Campos De Xavier Pinto, and Egel (2011). Built on the idea of *Inverse Probability Weighting* (IPW, Horvitz and Thompson, 1952), this estimator imposes additional restrictions to ensure that the first moments of all control variables are exactly balanced in all treatment samples and equal to the efficient first moment estimates.

Our findings suggest instantaneous positive institutional effects on employment and earnings. In the medium term, we find negative institutional effects. These ambiguous findings partly reflect changing compositions of program types and durations after the reform. After 48 months, we do not find any significant influences of the assignment mechanism on the returns to further training. Institutional effects are more negative for training participants with a high vocational education level. The stricter selection criteria show on average no influence on employment and earnings. For this reason, we decompose the selection effects into their potentially opposing forces. Increasing shares of individuals with better labor market histories can be associated with negative selection effects. However, these effects are compensated by changes in the spatial and temporal allocation of training.

The introduction of German Training Vouchers is also evaluated in Rinne, Uhlendorff, and Zhao (2013). They report insignificant institutional and selection effects in the short term.[2] Since we use a much larger and richer data set, we estimate the effects of interest with higher precision and partly revise their policy conclusions.[3] Rinne, Uhlendorff, and Zhao (2013) consider further training programs with durations up to 12 months and follow individuals over 18 months after the courses start. In comparison, we consider *all* further training programs and follow each individual over a post-treatment period of 48 months. In particular, we consider retraining courses that provide participants the opportunity to obtain a vocational degree. The share of retraining courses is higher than 20%. This reflects the importance of retraining, especially in Germany where vocational education is organized within a dual apprenticeship system.

Doerr et al. (2013) estimate the effectiveness of German Training Vouchers after the reform. Their findings suggest slightly positive effects on employment and no earning gains four years after treatment.[4] Heinrich et al. (2010) present a large scale econometric evaluation of the services provided by the Adult and Dislocated Worker Program under the WIA. They find positive earning effects of further training programs allocated through

[2]Rinne, Uhlendorff, and Zhao (2013) find positive institutional and negative selection effects in the short run. However, these results are in most samples (including the main specifications) insignificant. We can qualitatively support these results for the short run. For institutional effects we reject the null of no significant effects.

[3]We observe 31,473 (63,628) treated individuals after (before) the reform. In contrast, Rinne, Uhlendorff, and Zhao (2013) include 1,319 (25,223) treated individuals after (before) the reform in their main specification. They apply single nearest-neighbor matching with bootstrapped standard errors. As they mention, such procedures are deemed to have low efficiencies (see Abadie and Imbens, 2008).

[4]The effectiveness of further training under the conventional assignment mechanisms before the reform was extensively evaluated in a number of studies. For Germany, see Biewen, Fitzenberger, Osikominu, and Paul (2013), Fitzenberger, Osikominu, and Völter (2008), Fitzenberger and Völter (2007), Fitzenberger, Osikominu, and Paul (2010), Hujer, Thomsen, and Zeiss (2006), Lechner, Miquel, and Wunsch (2011, 2007), Lechner and Wunsch (2009a), Rinne, Schneider, and Uhlendorff (2011), Stephan and Pahnke (2011), and Wunsch and Lechner (2008) among others. The evidence is mixed with regard to effects on employment probability and earnings. See Card, Kluve, and Weber (2010) for a recent review of the program evaluation literature.

the voucher-like ITA. The survey of Barnow (2009) gives an overview regarding the effectiveness of different ALMP using voucher-like assignment mechanisms in the United States. His conclusions depend critically on the details of the implemented system, in particular with regard to the counselling of voucher recipients.[5]

The remainder of the paper is structured as follows. The next section gives an overview of the institutional background and describes the expected results with regard to the existing literature. A detailed data description can be found in Section 3. The parameter of interest, identification, and estimation are presented in Section 4. We discuss the results in Section 5. In Section 6 we conclude. Additional information which are not content of the main paper are provided in Appendices A-E.

2 Background

2.1 Institutions

The main objective of further training for unemployed is the adjustment of skills to changing requirements of the labor market and/or to changed individual conditions (due to health problems for example).[6] The obtained certificates or vocational degrees serve as important signaling device for potential employers. Further training mainly comprises three types of programs: practice firm training, classical further training, and retraining. Classical further training courses are categorized by their planned durations. We distinguish between short training (maximum duration 6 months) and long training (minimum duration 6 months).[7] Teaching takes place in class rooms or on-the-job. Typical examples of further training schemes are courses on IT based accounting or on customer orientation and sales approach. Degree courses or retrainings have a long duration of up to three

[5] Training vouchers are not only implemented for unemployed individuals, but also to enhance training of employees. Recent evaluations of such vouchers include Gerards, De Grip, and Witlox (2012), Görlitz (2010), and Schwerdt, Messer, Woessmann, and Wolter (2012).

[6] Accordingly, further training includes only programs that provide occupational specific skills. This excludes for example application and integration courses.

[7] We follow the classification of program types as proposed by Lechner, Miquel, and Wunsch (2011). Due to small sample sizes for programs that focus on career improvement, we do not include this program types in our analysis.

years. They lead to a complete (new) vocational degree within the German apprenticeship system. Thus, they cover for example the full curriculum of vocational training for an elderly care nurse or an office clerk.

Before 2003, the assignment process into further training was characterized by strong authority and control of caseworkers regarding the choice of training providers and courses. Unemployed were directly assigned to courses by caseworkers based on subjective measures. As a consequence, close cooperations and tight relationships between the employment offices and training providers were well-established. This was heavily criticized by federal institutions and various media coverage. As argued in Rinne, Uhlendorff, and Zhao (2013), the pre-reform assignment process was not focused on the best match between the needs of unemployed and the content of training courses. Instead it was determined by the supply of courses and sociopolitical reasons, which lead to a low transparency and market failures.[8] It is unclear to which extent unemployed were involved in the decision to participate in further training programs and what happened if they did not correspond to the caseworkers decisions. In principle, caseworkers had the possibility to cut unemployment benefits completely for a duration of twelve weeks if unemployed refused to participate in ALMP. Practically, sanction possibilities were only casually implemented. Hofmann (2012) reports about 10,000 imposed sanctions per year for refusing participation in ALMP in 2001 and 2002.[9]

In January 2003, a voucher-like system was introduced with the intention to increase the self-responsibility of training participants and to overcome existing market failures. Potential training participants are awarded with a training voucher and have free choice in selecting the most suitable course subject to the following restrictions: the voucher specifies the objective, content, and maximum duration of the course. It is to be redeemed within a one-day commuting zone. The validity of training vouchers is maximum three months. Under the new regime, unemployed have the freedom to choose training

[8] For the United States, Mitnik (2009) finds that welfare agencies do not maximize returns when they assign individuals to Welfare-to-Work programs. Rather political decisions play an important role.

[9] This corresponds to a sanction rate of about 0.4% (# of ALMP refusion sanction/stock registered unemployed). The sanction policy of regional employment offices varied strongly, in particular with respect to regional labor market situations (Müller and Steiner, 2008).

providers and courses.[10] No sanctions are imposed if a voucher is not redeemed. However, unemployed have to give reasonable explanations for not redeeming vouchers.[11]

Simultaneously with the voucher system, stricter selection criteria were implemented. The post-reform paradigm of the Federal Employment Agency focuses on direct and fast 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 after training. Accordingly, the award of German Training Vouchers is based on statistical treatment rules, often labeled as profiling or targeting (Eberts, O'Leary, and Wandner, 2002).[12] These rules are applied to decide about the award of vouchers and about objectives, contents, and maximum durations of potential courses. Caseworkers consider the regional labor market conditions and individual characteristics to form their predictions. In addition, they have the opportunity to use information from mandatory counselling interviews and test results from medical or psychological services.

2.2 Expected results

There are various channels through which the change in the assignment regime may affect the overall impact of further training on employment and earnings. The increase in the freedom of choice and self-responsibility might change the attitudes towards training in a positive way. Receiving a training voucher may change the opinion towards services by the employment offices perceiving it more like an offer and less like an assignment.

[10] While market behavior under the direct assignment regime was mainly supply-side oriented, there is strict focus on demand orientation under the voucher system. To assure that training providers offer courses that are in line with the demand of the employment offices, the latter have to plan and publish their regional and sector-specific demand in a yearly time interval.

[11] Beside the individual choice not to start a program, there are several more reasons for non-participation. For example, there could be problems of reaching the provider because of a lack of public transport infrastructure or if the provider rejects the contract. The last could be due to the necessity of the provider to proof his performance, i.e. training providers could reject clients when they predict low employment probabilities after training.

[12] Such treatment rules are also applied in the WIA. Alternative allocation schemes could be random assignment (e.g. used in the Canadian Self-Sufficiency Project experiment) or deterministic assignment (e.g. in Germany all unemployed are entitled to a placement voucher after a certain unemployment duration).

Unemployed may value that a costly service is offered to them and participate in courses with higher motivation or increase their search effort. Arni, Lalive, and Van den Berg (2012) find positive earnings effects of policies which are likely to be perceived positively by participants, even before the imposition of programs. Moreover, they find positive pre- and post-treatment effects of policies which are likely to be perceived negatively by participants with negative interactions between the two types of policies. Van der Klaauw and Van Ours (2013) find positive financial incentives to be less effective than negative incentives. Behncke, Frölich, and Lechner (2010) report that close cooperations and harmonic relations between caseworkers and their clients harm the effectiveness of training with respect to employment. The direct assignment of unemployed to onerous training courses before the reform could have resulted in threat effects, which are found to have positive impacts on employment outcomes (Black, Smith, Berger, and Noel, 2003, Graversen and Van Ours, 2008, Rosholm and Svarer, 2008).[13] The limited possibility of caseworkers to impose sanctions after the reform might reduce the effectiveness of programs (Abbring, Van den Berg, and Van Ours, 2005, Arni, Lalive, and Van Ours, 2013, Lalive, Van Ours, and Zweimüller, 2005, Van den Berg, Van der Klaauw, and Van Ours, 2004).[14]

On the supply side, the voucher system implements market mechanisms following the principal ideas of Friedman (1962, 1955). This is likely to intensify the competition between training providers.[15] However, markets do not necessarily work appropriately. Competition could generate market outcomes which do not improve the quality of training, especially under information asymmetry (see discussion in Prasch and Sheth, 2000). In Germany, regulations aim to avoid market failures from wrong incentives. Further training providers and courses have to be certified by independent institutions.

[13]For the evaluation of German Training Vouchers, threat effects might not be important, because of other ALMP which are allocated based on the pre-reform system and could still impose threats for potential participants. Anyway, Arni, Lalive, and Van den Berg (2012) argue that further training programs are more likely to been perceived positively rather than negatively by unemployed.

[14]As mentioned above, the implementation of sanctions for refusing participation in ALMP was also not strict before the reform.

[15]For education vouchers, the review of Levine and Belfield (2002) reports the effect of competition to be positive but modest in size.

Likewise, the influence of the new selection criteria on the overall effectiveness of further training is *a priori* not clear. Dehejia (2005) demonstrates the potential of assignment decisions to increase individual returns to training. However, caseworkers have potentially accumulated expertise and knowledge about training providers and offered courses, such that they allocate training programs more effectively compared to an allocation by statistical treatment rules. Recent empirical studies reject that caseworkers allocate training programs efficiently (Bell and Orr, 2002, Frölich, 2008, Mitnik, 2009). Lechner and Smith (2007) suggest three potential reasons for these findings. First, caseworkers might not have the competence to allocate training programs efficiently. Second, caseworkers may have other goals than an efficient allocation of training programs. Third, federal institutions could impose restrictions which prevent caseworkers from an efficient allocation of training programs.

Of course, the performance of statistical treatment rules depends critically on the details of the implemented system. In the German Training Voucher system, the rules apply only with respect to the award decisions, the objective, content, and maximum duration of potential courses. Unemployed have the challenge to find the most suitable training providers and courses by themselves. Furthermore, the new selection rules are based on predicted employment outcomes under participation in training programs. Unemployed with high predicted employment outcomes under treatment are more likely to be awarded with vouchers. These unemployed are characterized by higher education levels and better employment histories. As discussed in Berger, Black, and Smith (2000), allocation of ALMP based on predicted outcomes rather than impacts does not serve efficiency goals, unless assumptions about correlations between outcomes and impacts are made. Heckman (2000) argues that the trainability of individuals increases with the education level. However, empirical findings suggest that cream-skimming is not very important or has even negative impacts on the return to training. Rinne, Schneider, and Uhlendorff (2011) find no significant interactions between vocational education and the return to public provided training in Germany. Biewen, Fitzenberger, Osikominu, and Waller (2007) and

Doerr et al. (2013) report evidence for negative influences of vocational education on the effectiveness of public sponsored training in Germany. On the same line, Wunsch and Lechner (2008) find that training participants with good labor market characteristics are generally worse-off, especially because of deep negative lock-in periods. For the United States, there exists strong evidence that short term outcome measures are only weakly correlated with long term impacts of training on employment and earnings (Heckman, Smith, and Taber, 1996, Heckman, Heinrich, and Smith, 2002, 2011).

Obviously, the performance of statistical treatment rules could be blurred if caseworkers do not comply to these rules. For Switzerland, Behncke, Frölich, and Lechner (2009) report that caseworkers do not respond to the implementation of a statistical support system, potentially because of missing incentives.[16] For the German Voucher system, the 70%-rule was abolished in 2005, because caseworkers had problems to match this rule.[17] The general intention of an outcome oriented allocation of training vouchers remained.

3 Data description

We use unique data provided by the Federal Employment Agency of Germany which contain information on *all* individuals in Germany who participated in a training program in 2001 and 2002 or received a training voucher in 2003 or 2004. We observe precise start and end dates for further training courses as well as precise award and redemption dates for each voucher in the post-reform period. Individual data records are collected from the Integrated Employment Biographies (IEB).[18] The IEB is a merged data file containing individual data records collected in 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

[16] Similar experiences are made with regard to the Service and Outcome Measurement System in Canada (Colpitts, 2002).

[17] We consider only treatments between January 2001 and December 2004 in this study.

[18] The IEB is a rich administrative data base and source of the subsamples of data used in all recent studies evaluating German ALMP (e.g Biewen, Fitzenberger, Osikominu, and Paul, 2013, Lechner, Miquel, and Wunsch, 2011, Lechner and Wunsch, 2013, Rinne, Uhlendorff, and Zhao, 2013).

Pool Database (*Bewerberangebot*), and the Participants-in-Measures Data (*Maßnahme-Teilnehmer-Gesamtdatenbank*).[19] The data contain detailed daily information on employment subject to social security contributions, receipt of transfer payments during unemployment, job search, and participation in different active labor market programs as well as rich individual information.[20] Thus, we are able to work with a large set of personal characteristics and long labor market histories for all individuals in the evaluation sample. The sample of control persons originate from the same data base and is constructed as a three percent random sample of those individuals who experience at least one switch from employment to non-employment (of at least one month) between 1999 and 2005.[21]

3.1 Treatment and sample definition

The treatment of interest is the first participation in a further training course of at least 31 days. We use the same treatment definition before and after the reform. Under the voucher regime, we observe the award of training vouchers as well as the participation in training courses thereafter. Individuals who do not redeem the voucher are in the control group after the reform.[22] It is likely that individuals who refuse to participate in further training before the reform also end up in the control group. Of course, an increase in the self-responsibility and freedom of choice could potentially affect the outcomes of individuals awarded with vouchers, even if they do not redeem it. We exploit our rich data availability and experiment with different treatment definitions in the post-reform period. We find very small effects of the award of a training voucher by itself. Please find an extensive discussion in Appendix A.

A second concern regarding the treatment definition is the timing with respect to the elapsed unemployment duration at the beginning of the treatment. This concern found already a lot of attention in the literature.[23] Frederiksson and Johansson (2008) argue

[19]IAB is the abbreviation for the research department of the German Federal Employment Agency.
[20]The version of the IEB we use in this project, has been supplemented with personal and regional information not available in the standard version.
[21]We account for the fact that we have different sampling probabilities in all calculations whenever necessary.
[22]In our sample the non-redemption rate is 19%.
[23]As an example, Lechner (2009) discusses sequential causal models and Heckman and Navarro (2007)

that in countries like Germany basically all unemployed would receive ALMP if their unemployment spell were long enough. Therefore, we restrict our treatment definition to a specific time interval of the elapsed unemployment duration. We consider only treatments within the first year of unemployment. Yet, the definition of the non-treated subpopulation is still problematic. Individuals who find jobs quickly have lower probabilities to receive training, because the treatment definition is restricted to unemployment periods. Accordingly, the ignorance of the elapsed unemployment duration at treatment start, would possibly lead to a higher share of individuals with better unobserved labor market characteristics in the control, than in the treatment group. This opens the question of how to measure this variable in the non-treated subpopulation. We randomly assign (pseudo) treatment start dates to each individual in the control group. Thereby, we recover the distribution of the elapsed unemployment duration at (pseudo) treatment start from the treatment group (similar to e.g. Lechner and Smith, 2007, Lechner and Wunsch, 2013). To make the treatment definitions between the treatment and control samples comparable, we consider only individuals who are unemployed at their (pseudo) treatment start.[24]

The evaluation sample is constructed as inflow sample into unemployment.[25] The baseline sample (Sample A) consists of individuals who become unemployed in 2001 under the assignment regime or in 2003 under the voucher regime, after having been continuously employed for at least three months.[26] We follow each individual over a maximum duration of 12 months until the (pseudo) treatment takes place. After the (pseudo) treatment we follow all individuals over 48 months (we have information up to December 2008). Entering unemployment is defined as the transition from (non-subsidized, non-marginal, non-seasonal) employment to non-employment of at least one month plus subsequently (not necessarily immediately) some contact with the employment agency, either through

dynamic discrete choice models in the context of program evaluation studies.

[24] Doerr et al. (2013) estimate the effect of being awarded with a training voucher in the post-reform period and match on the elapsed unemployment duration exactly. They define the treatment as being awarded with a voucher today versus waiting for at least one month. Their treatment effects are qualitatively and quantitatively similar to our results, even though we have a different treatment definition.

[25] In comparison, Rinne, Uhlendorff, and Zhao (2013) draw random samples from the stock of participants and non-participants in 2002 and 2003.

[26] In robustness checks we experiment also with different sample definitions. A description of these samples will follow in Section 5.3.

benefit receipt, program participation, or a job search spell.[27] 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 for further training participants. In order to exclude individuals eligible for specific labor market programs targeted to 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 192,780 unweighted or 959,833 weighted observations. Thereof, 63,628 individuals are directly assigned to a training course and 31,473 redeem a voucher during their first twelve months of unemployment. We use 45,271 unweighted or 374,235 weighted observations as control persons in the pre-reform period. After the reform, we observe 52,408 unweighted or 490,497 weighted control persons.

In Table 1, we report sample first moments of the observed characteristics. Information on individual characteristics refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and the monthly regional labor market characteristics which refer to the (pseudo) treatment time. The choice of the control variables is motivated by the study of Lechner and Wunsch (2013). We consider all variables which appear to be important confounders in this study, i.e. baseline characteristics, timing of program starts, region dummies, benefit and unemployment insurance claims, pre-program outcomes, and labor market histories. On top of this, we use proxy information about physical or mental health problems, motivation lacks, and reported sanctions. In the first two columns of Table 1, we show the sample moments for the treated and non-treated sub-samples under the voucher regime. In the third and fourth columns, we show the respective sample moments under the assignment regime. In the last three columns we report the standardized differences between the different subsamples and the treatment group under the voucher regime.

[27] Subsidized 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 1: Sample first moments of observed characteristics.

	Voucher Regime		Assignment Regime		Standardized 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	0.465	0.446	0.470	0.407	3.748	11.718	1.180
Age	38.590	41.335	38.631	41.379	31.708	32.001	0.545
Older than 50 years	0.010	0.112	0.018	0.122	43.610	46.345	7.098
No German citizenship	0.067	0.089	0.068	0.087	8.203	7.195	0.078
Children under 3 years	0.044	0.034	0.041	0.032	4.946	6.342	1.575
Single	0.296	0.264	0.245	0.223	7.133	16.696	11.478
Health problems	0.081	0.125	0.09	0.145	14.333	20.290	3.231
Sanction	0.007	0.007	0.01	0.008	0.103	0.911	3.171
Incapacity (e.g. illness, pregnancy)	0.102	0.189	0.095	0.190	24.809	25.245	2.219
Lack of Motivation	0.092	0.088	0.089	0.083	1.191	2.929	0.843
Education, Occupation and Sector							
No schooling degree	0.037	0.069	0.038	0.060	14.553	11.063	0.896
Schooling degree without Abitur	0.352	0.277	0.352	0.268	16.324	18.372	0.181
University entry degree (Abitur)	0.238	0.169	0.199	0.139	16.998	25.303	9.317
No vocational degree	0.206	0.226	0.225	0.224	4.741	4.303	4.728
Academic degree	0.117	0.094	0.082	0.062	7.329	19.452	11.517
White-collar	0.383	0.478	0.451	0.542	19.375	32.363	13.746
Elementary occupation	0.065	0.098	0.083	0.104	12.408	14.254	6.882
Skilled agriculture and fishery workers	0.009	0.016	0.012	0.020	5.943	9.038	2.483
Craft, machine operators and related	0.281	0.332	0.322	0.392	11.119	23.603	8.931
Clerks	0.256	0.166	0.217	0.140	22.247	29.532	9.279
Technicians and associate professionals	0.159	0.127	0.132	0.107	9.158	15.384	7.576
Professionals and managers	0.124	0.107	0.107	0.089	5.261	11.089	5.089
Employment and Welfare History							
Half months employed in the last 24 months	45.548	44.822	44.384	43.574	10.723	27.280	16.719
Half months unemployed in the last 24 months	0.381	0.356	0.584	0.591	1.516	11.465	10.946
Time since last unemployment in the last 24 months (half-months)	46.748	46.130	45.522	44.233	11.977	36.872	21.030
No unemployment in last 24 months	0.913	0.922	0.875	0.875	3.212	12.551	12.548
Unemployed 24 months before	0.034	0.041	0.047	0.053	3.597	9.315	6.443
# unemployment spells in the last 24 months	0.112	0.100	0.169	0.169	3.040	12.405	12.454
Any program in last 24 months	0.046	0.044	0.062	0.052	1.181	2.747	7.017
Time of last out of labor force in last 24 months	45.756	44.551	44.778	43.081	16.159	31.524	13.783
Remaining unemployment insurance claim	25.447	19.879	23.357	21.359	39.617	30.519	16.433
Eligibility unemployment benefits	13.398	14.729	13.066	14.580	22.81	19.614	6.460
Cumulative employment (last 4 years before Unemployment)	80.953	78.963	78.430	78.300	8.794	11.665	10.838
Cumulative earnings (last 4 years before Unemployment)	91,057	83,470	79,997	79,992	15.621	23.264	23.500
Cumulative benefits (last 4 years before Unemployment)	2.894	3.398	3.578	3.876	6.088	11.194	8.101
Start unemployment spell in January	0.060	0.103	0.109	0.083	15.712	9.014	17.719
Start unemployment spell in February	0.068	0.087	0.104	0.086	6.831	6.731	12.607
Start unemployment spell in March	0.096	0.084	0.100	0.079	4.107	6.044	1.551
Start unemployment spell in April	0.102	0.087	0.119	0.086	5.313	5.571	5.282
Start unemployment spell in June	0.059	0.077	0.057	0.074	7.509	6.400	0.542
Start unemployment spell in July	0.053	0.087	0.054	0.081	13.265	11.365	0.707
Start unemployment spell in August	0.081	0.080	0.083	0.076	0.409	1.840	0.772
Start unemployment spell in September	0.154	0.074	0.104	0.078	25.266	23.858	15.008
Start unemployment spell in October	0.127	0.081	0.090	0.089	15.334	12.467	11.931
Start unemployment spell in November	0.085	0.079	0.048	0.092	2.262	2.381	14.755
Start unemployment spell in December	0.045	0.081	0.041	0.095	14.859	19.907	1.783
Elapsed unemployment duration	5.051	3.597	4.599	3.451	43.771	48.328	13.167
State of Residence							
Baden-Württemberg	0.046	0.042	0.044	0.036	1.684	4.746	0.650
Bavaria	0.089	0.113	0.096	0.092	8.142	1.063	2.660
Berlin, Brandenburg	0.064	0.061	0.062	0.064	1.262	0.009	0.874
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.068	0.077	0.097	0.088	3.612	7.650	10.844
Hesse	0.236	0.207	0.179	0.199	7.126	8.943	14.009
Northrhine-Westphalia	0.010	0.008	0.008	0.008	2.248	2.539	2.359
Rhineland Palatinate, Saarland	0.219	0.206	0.176	0.177	3.279	10.554	10.908
Saxony-Anhalt, Saxony, Thuringia	0.107	0.134	0.170	0.179	8.533	20.810	18.479
Regional Characteristics							
Share of employed in the production industry	0.250	0.246	0.245	0.242	4.974	8.595	4.999
Share of employed in the construction industry	0.064	0.065	0.076	0.077	4.483	55.062	52.930
Share of employed in the trade industry	0.150	0.150	0.150	0.151	0.180	3.256	0.803
Share of male unemployed	0.564	0.563	0.543	0.541	3.574	54.195	49.653
Share of non-German unemployed	0.141	0.141	0.128´	0.129	0.660	12.740	14.407
Share of vacant fulltime jobs	0.794	0.794	0.800	0.799	0.333	7.490	8.646
Population per km^2	921.128	887.314	850.247	874.950	2.027	2.743	4.231
Unemployment rate (in %)	12.137	12.303	12.080	11.877	3.191	4.898	1.074

Note: In columns (1)-(4) we report the sample first moments of observed characteristics for the treated and non-treated subsamples. Information on individual characteristics refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and the monthly regional labor market characteristics which refer to the (pseudo) treatment time. In columns (5)-(7) we report the standardized differences between the different subsamples and the treatment group under the voucher regime. Please find a description of how we measure standardized differences in Appendix B.

Treated individuals are on average younger, healthier, more often single and female compared to individuals in the control groups. This pattern is revealed under both regimes, with more pronounced differences between the treatment and control groups under the assignment regime. Treated individuals hold on average higher schooling degrees than non-treated individuals under both regimes. However, treated individuals under the voucher system are better educated than under the assignment regime. Furthermore, they tend to have more successful employment histories in the past 4 years, in particular they had higher cumulative earnings and received less benefits. The information about potential placement handicaps of the unemployed, e.g. received sanctions or past incapacities due to illness, pregnancy or child care show that treated persons are less likely to have such problems under both regimes.

4 Empirical approach

4.1 Parameters of interest

The purpose of this study is to decompose the overall before-after effect of the reform into institutional, selection, and business cycle effects.[28] Consider a multiple treatment framework as proposed in Imbens (2000) and Lechner (2001). Direct assignment to training courses are 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 = time period 0 or 1). We never observe direct assignments to training courses in the post-reform period, i.e. we never observe the 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). Since the implementation of the voucher system was part of the reform, we never observe the treatment v in the pre-reform period t_0. In the pre-reform period, $D_i = nt_0$ indicates the absence of a treatment and $D_i = nt_1$ indicates no treatment in the post-reform period (n = non-treatment). Following the framework of

[28]We are mainly interested in the institutional and selection effects, but report also business cycle effects because they are crucial for our identification strategy.

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 which would be observed if individual i is directly assigned to a training course in the pre- or post-reform period. $Y_i(vt_0)$ and $Y_i(vt_1)$ are the potential outcomes which would be observed if individual i redeems a training voucher in the pre- or post-reform period. $Y_i(nt_0)$ and $Y_i(nt_1)$ are the potential outcomes when individual i would not be treated in the respective time period before or after the reform. For each individual we can only observe one potential outcome. The observed outcome equals,

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

with $D_i(g) = 1\{D_i = g\}$ for $g \in \{at_0, at_1, vt_0, vt_1, nt_0, nt_1\}$ and $1\{\cdot\}$ being 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 average treatment effects on the treated (ATT). The pre-reform ATT can be indicated by,

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

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, because $Y_i(nt_0)$ 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 who are assigned to training courses. The post-reform ATT can be indicated by,

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

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. $E[Y_i(nt_1)|D_i = vt_1]$ is a counterfactual expected potential outcome. It refers to the expected outcome which would be observed, if the training participants under the voucher system would not be 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 before-after effect of the reform can be indicated by,

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

The parameter γ^{ba} is the difference in the ATT of participating in training under the voucher system after the reform and the ATT of being directly assigned to training courses before the reform. The parameters γ^{pre} and γ^{post} differ with respect to the subpopulation of interest, the time period of treatment, and the assignment mechanism. These differences correspond to selection, business cycle, and institutional effects, respectively.

As discussed earlier, treated individuals before and after the reform differ in observed characteristics, due to a change in the selection criteria. The selection effect can be formalized by,

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

where the subpopulation of interest is changed, but the type of treatment and the time period are maintained. The selection effect can be interpreted as the difference of the average pre-reform treatment effect of being assigned to a training course, between individuals who redeem training vouchers in the post-reform period and individuals who are directly assigned to courses in the pre-reform period.

Further, the treatment effects could be different before and after the reform, even after the type of treatment and the subpopulation of interest have been fixed. We refer to the expected difference as the business cycle effect. We distinguish between two different business cycle effects,

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

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, because 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 fix the subpopulation of interest and the time period, but change the type of treatment. The institutional effect is the difference between the post-reform effect of training under a voucher and direct assignment regime, for individuals who are treated in the post-reform period.

4.2 Identification strategy

We apply an identification strategy with multiple stages. First, we control for a large set of confounding pre-treatment variables X_i ruling out selection based on observed characteristics. This allows us to identify γ^{pre}, γ^{post}, γ^{ba}, γ^{s}, and γ^{bc0}. Second, we rely on the common trend assumption to identify γ^{bc1}. Third, structural model assumptions are necessary to identify the institutional effect γ^{in}. The last two assumptions are often applied for difference-in-difference identification strategies.[29]

[29] For completeness, assume that X is not influenced by the treatment (for a discussion see Lechner, 2013) and that all moments required for the following analysis are available.

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

$$E[Y_i(d)|D_i = g, X_i = x] = E[Y_i(d)|D_i = d, X_i = x].$$

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 the treatment status have to be involved 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 we use (see discussion in Section 3.2). Biewen, Fitzenberger, Osikominu, and Paul (2013) and Lechner and Wunsch (2013) assess the plausibility of conditional independence assumptions for the evaluation of German ALMP before the reform. Their findings support the plausibility of Assumption 1 in the context of this study.[30] Assumption 1 includes also the time dimension. Conditional on X_i, we assume that individuals who are under treatment status vt_1 would have the same expected potential outcomes as individuals who are under treatment status nt_0, if they would be under non-treatment in t_0. Similarly, we assume that individuals who are under the treatment status vt_1 would have the same expected potential outcomes as individuals under the treatment status at_0, if they would be directly assigned to a training course in t_0 (conditional on X_i). This implies that the treatment groups in t_0 and t_1 do not differ systematically in unobserved characteristics which have an influence on the potential outcomes.[31] Yet, individuals which are similar in all relevant characteristics at treatment start might eventually have different potential outcomes. As an example, the post-treatment labor market situation is likely to be unrelated to the treatment probabilities (especially after long periods), but may affects the potential outcomes. In our main specifications, we control for monthly regional labor market characteristics at treatment start to address this issue. Moreover, we use samples

[30] Further, Doerr et al. (2013) analyze the effectiveness of further training after the reform relying on selection on observables and unobservables assumptions. They find that selection on unobserved characteristics is not important in the post reform period at least in the long-run.

[31] This corresponds to a stronger version of the dynamic conditional independence assumption, because the time period is longer (e.g. Sianesi, 2004).

with different calender time periods as robustness check (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 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 between the different subsamples (see discussion in Lechner, 2008). Given our exceptionally large data set, we are not concerned about a failure of this assumption.[32]

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 $k \in \{d, g\}$ (comp. Hirano, Imbens, and Ridder, 2003, Rosenbaum and Rubin, 1983). A formal proof of (1) can be found in Appendix C. In the case with $d = g$, the parameter,

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

is even simpler to identify.

Accordingly, the pre-reform ATT is identified by,

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

[32] In unreported calculations, we perform simple support tests in the fashion of Dehejia and Wahba (1999) and Lechner and Strittmatter (2013). We do not find any incidence for support problems.

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. Further, we can identify the before-after effect of the reform γ^{ba} taking the difference between γ^{post} and γ^{pre}.

The selection effect equals,

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

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

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

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 that business cycle effects are independent of the types of treatment. This is a strong assumption, because it requires that the difference between the potential outcomes in the time periods t_0 and t_1 are equal under different types of treatment. We carefully assess the plausibility of Assumption 3 in Section 5.3, using different evaluation samples and detailed information on monthly regional labor market characteristics. Under Assumptions 1, 2, and 3, the parameter γ^{bc1} is identified.

Assumption 4 *(Additive Separability).* The before-after effect 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 between selection, business cycle and institutional effects. Even though this assumption is strong, analogue assumptions are often made in evaluation studies using difference-in-difference identification strategies. This assumption has to be kept in mind when interpreting the institutional effects. Under Assumptions 1, 2, 3, and 4, the institutional effects, $\gamma^{in} = \gamma^{ba} - \gamma^{s}$, is identified, calculating the difference between the before-after and selection effects.

4.3 Estimation strategy

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

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

with

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

This is an *Inverse Probability Weighting* (IPW) estimator. Hirano, Imbens, and Ridder (2003) show that consistency and efficiency of IPW depend critically on the estimated propensity scores. Naive specifications of the propensity score do not necessarily lead to efficient estimates. One reason is that (2) aims 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, $\hat{F}_g$ could be more efficiently estimated using information from the entire population rather than only from the random sample g. 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 which recover $\hat{F}_g^{eff}$ instead of $\hat{F}_g$ are potentially more efficient. Recently Graham, Campos De Xavier Pinto, and Egel (2011) propose a double robust and locally efficient semiparametric version of IPW, named *Auxiliary-to-Study Tilting* (AST).[33] This estimator balances the efficient first moments of all control variables in each treatment sample exactly.[34] We employ this estimator in our study.

For 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. However, the propensity score $\hat{p}_d(x)$ is replaced by $\tilde{p}_d(x)$. It is estimated under the following moment conditions,

$$\frac{1}{N} \sum_{i=1}^{N} \left(\begin{array}{c} \dfrac{D_i(d)}{\dfrac{1}{N} \sum\limits_{j=1}^{N} \hat{p}_g(X_j)} \cdot \dfrac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)} \\ \\ \dfrac{D_i(d)}{\dfrac{1}{N} \sum\limits_{j=1}^{N} \hat{p}_g(X_j)} \cdot \dfrac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)} \cdot X_i \end{array} \right) = \left(\begin{array}{c} 1 \\ \\ \dfrac{1}{N} \sum\limits_{i=1}^{N} \dfrac{\hat{p}_g(X_i)}{\dfrac{1}{N} \sum\limits_{j=1}^{N} \hat{p}_g(X_j)} \cdot X_i \end{array} \right), \tag{3}$$

where $\tilde{p}_d(X_i) = \Phi(X_i'\tilde{\beta})$ is specified such that the left and right side of (3) are numerically equivalent for all elements in X_i. The right parenthesis includes the efficient first moments estimates of a constant and all other control variables. Since 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.[35] The expected

[33] An analogue estimation concept is applied in Graham, De Xavier Pinto, and Egel (2012) to average treatment effects for the entire population. Other parametric approaches are suggested by Abadie (2005), Hirano and Imbens (2001), and Qin and Zhang (2008).

[34] Exact balancing is not guaranteed for the sample moments using conventional IPW estimators. The large sample properties are subject to assumptions about the specification of the propensity score. These assumptions imply that the propensity score is correctly specified, strictly increasing in its arguments, differentiable, and is well located within the unit interval.

[35] The constant guarantees that the weights sum up to one.

Table 2: Efficient first moments of observed characteristics.

	Treatmentgroup Voucher Regime (1)	Treatmentgroup Assignment Regime (2)	SD between (1) and (2)
Personal Characteristics			
Female	0.464	0.469	0.892
Age	38.540	38.603	0.670
Older than 50 years	0.010	0.019	5.433
No German citizenship	0.068	0.068	0.063
Children under 3 years	0.044	0.041	1.369
Single	0.296	0.245	9.335
Health problems	0.081	0.090	2.734
Sanction	0.007	0.010	2.483
Incapacity (e.g. illness, pregnancy)	0.101	0.095	1.608
Lack of Motivation	0.093	0.091	0.714
Education, Occupation and Sector			
No schooling degree	0.037	0.038	0.733
Schooling degree without Abitur	0.352	0.351	0.227
University entry degree (Abitur)	0.238	0.199	7.752
No vocational degree	0.205	0.224	3.735
Academic degree	0.118	0.082	10.040
White-collar	0.383	0.451	11.208
Elementary occupation	0.065	0.083	5.440
Skilled agriculture and fishery workers	0.009	0.012	2.109
Craft, machine operators and related	0.281	0.322	7.334
Clerks	0.253	0.216	7.189
Technicians and associate professionals	0.159	0.132	6.340
Professionals and managers	0.125	0.107	4.649
Employment and Welfare History			
Half months employed in the last 24 months	45.512	44.358	12.784
Half months unemployed in the last 24 months	0.385	0.591	8.836
Time since last unemployment in the last 24 months (half months)	46.734	45.494	16.963
No unemployment in last 24 months	0.913	0.873	10.183
Unemployed 24 months before	0.034	0.047	5.226
# unemployment spells in the last 24 months	0.113	0.171	9.897
Any program in last 24 months	0.046	0.063	5.893
Time of last out of labor force in last 24 months	45.714	44.763	10.449
Remaining unemployment insurance claim	25.416	23.409	12.645
Eligibility unemployment benefits	13.366	13.049	5.028
Cumulative employment (last 4 years before Unemployment)	80.781	78.375	8.141
Cumulative earnings (last 4 years before Unemployment)	90,911	80,089	18.762
Cumulative benefits (last 4 years before Unemployment)	2.925	3.613	6.433
Start unemployment spell in January	0.059	0.108	13.870
Start unemployment spell in February	0.068	0.103	9.896
Start unemployment spell in March	0.095	0.099	1.080
Start unemployment spell in April	0.103	0.118	3.877
Start unemployment spell in June	0.059	0.057	0.486
Start unemployment spell in July	0.053	0.055	0.837
Start unemployment spell in August	0.081	0.083	0.526
Start unemployment spell in September	0.155	0.104	12.776
Start unemployment spell in October	0.127	0.091	9.535
Start unemployment spell in November	0.086	0.050	12.145
Start unemployment spell in December	0.045	0.042	1.035
Elapsed unemployment duration	5.071	4.614	11.357
State of Residence			
Baden-Württemberg	0.045	0.044	0.786
Bavaria	0.088	0.095	1.945
Berlin, Brandenburg	0.063	0.061	0.766
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.067	0.096	8.432
Hesse	0.236	0.179	11.732
Northrhine-Westphalia	0.010	0.008	1.892
Rhineland Palatinate, Saarland	0.220	0.180	8.338
Saxony-Anhalt, Saxony, Thuringia	0.109	0.172	14.370
Regional Characteristics			
Share of employed in the production industry	0.250	0.246	4.578
Share of employed in the construction industry	0.064	0.076	39.520
Share of employed in the trade industry	0.150	0.150	0.916
Share of male unemployed	0.564	0.542	35.221
Share of non-German unemployed	0.140	0.128	10.593
Share of vacant fulltime jobs	0.794	0.800	7.233
Population per km^2	909.876	836.200	3.492
Unemployment rate	12.158	12.042	1.741

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

potential outcomes are estimated using,

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

with

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

We report the efficient first moments for all control variables and both treatment groups in Table 2. The corresponding sample first moments can be found in columns (1) and (3) of Table 1.

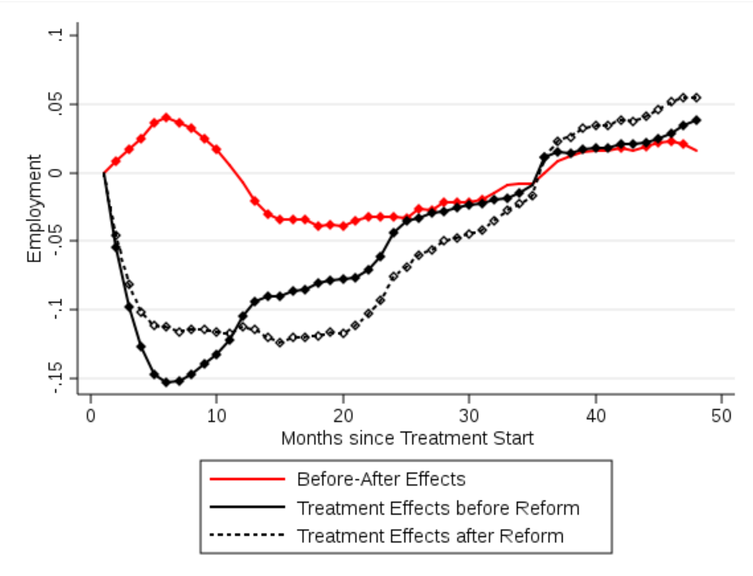
5 Results

5.1 Treatment effects before and after the reform

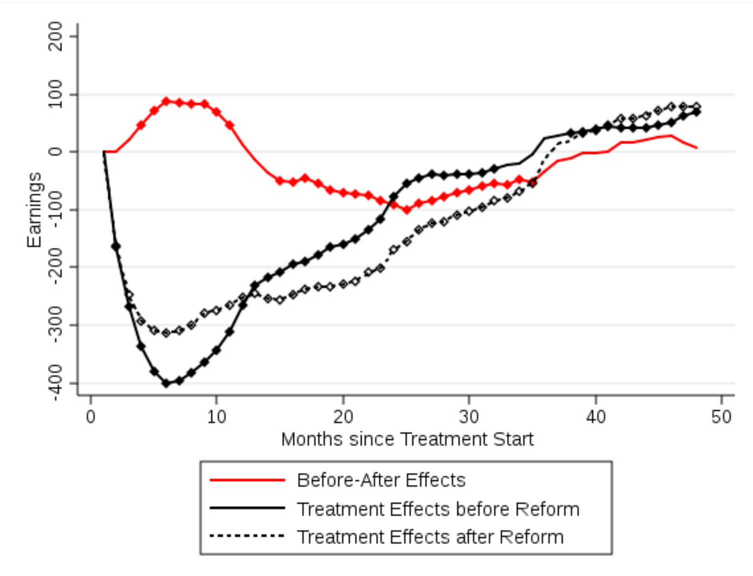
The treatment effects of a participation in further training courses under the direct assignment (γ^{pre}) and the voucher regime (γ^{post}) on employment and earnings are presented in Figure 1. We report separate effects for each of the 48 months following the treatment start dates. Under both regimes, treated individuals suffer from lock-in effects in comparison to their non-treated counterparts. Pre-reform lock-in effects are steeper in the first year after treatment. After the reform, the lock-in effects have longer durations. Participation in further training leads to long-term positive effects on the employment probability and monthly earnings under both regimes.[36] The difference between the pre- and post-reform treatment effects identify the overall before-after effects (γ^{ba}), which incorporate the impacts of a stricter selection of participants, changing assignment mechanism, as well as effects related to changing economic conditions. In the short-term, the before-after effects on employment and monthly earnings are positive and evolve to negative effects in the 2nd and 3rd year after treatment. In the long-term, before-after effects on employment

[36]The results for the post-reform period are comparable to those found by Doerr et al. (2013), even though they use a different treatment definition, a different dynamic evaluation framework, and apply different estimators. Therefore, we argue that our findings are not subject to the specific evaluation framework and estimator we apply.

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 Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

are significantly positive whereas the effects on earnings appear to be insignificant and fairly zero. In the following, we aim to identify the driving forces behind these ambiguous results.

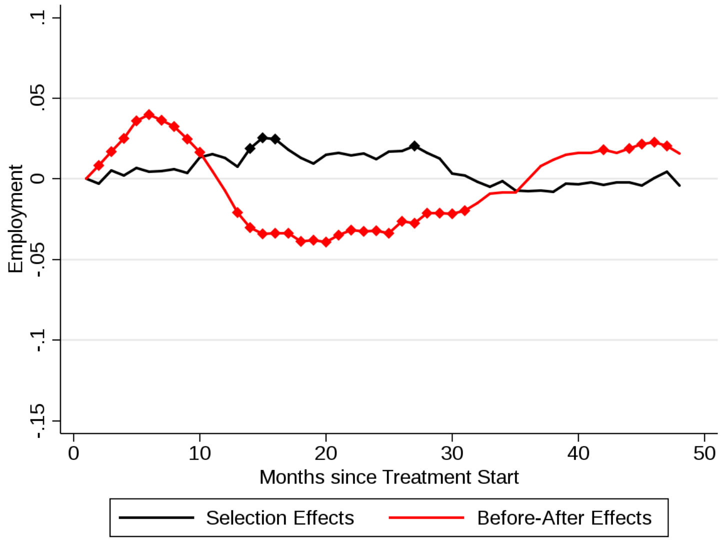
5.2 Selection effects

The implemented selection criteria change the composition of training participants with respect to their characteristics. In Table 2, we report the efficient first moments of all confounding control variables for the treatment groups before and after the reform. The share of post-reform treated with an academic degree is higher than before the reform. Less white-collar workers are treated after the reform. Treated before and after the reform differ with respect to their employment histories, such that treated individuals under the voucher regime have on average more successful employment and earnings profiles. After the reform, training starts on average after longer elapsed unemployment durations. The stricter selection rule results in a changing regional allocation of further training programs.

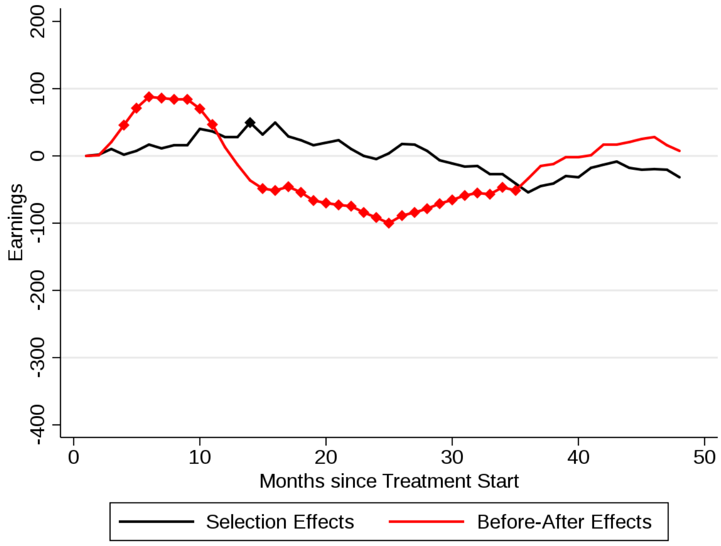
In Figure 2, we report the selection effects (γ^s). Additionally, we include the before-after effects in this figure, to get a feeling about the size of the selection effects. The selection effects have only minor influences on the effectiveness of training. If at all, we find significant positive selection effects on employment after approximately 1.5 and 2.5 years. After 48 months, the selection effect is fairly zero. For earnings, we find only significant positive selection effects after approximately 1.5 years. These findings are surprising, given the difference in the observed control variables (see Table 2). In order to reveal potentially opposing forces, we apply a non-parametric Blinder-Oaxaca decomposition to the selection effects. This decomposition method allows us to change one block of control variables between the pre- and post-reform period, holding all other characteristics constant on the pre-reform level. Please find a detailed description of the applied decomposition method in Appendix D.

The results of the decomposition are reported in Figure 3. In this figure, we report the overall selection effects and the selection effects decomposed by different blocks of control variables. The first block involves personal characteristics, education, occupation, and sector. The second block includes employment and welfare histories. The third block incorporates timing of unemployment and treatment start, state of residence, and

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



(a) Effects on employment



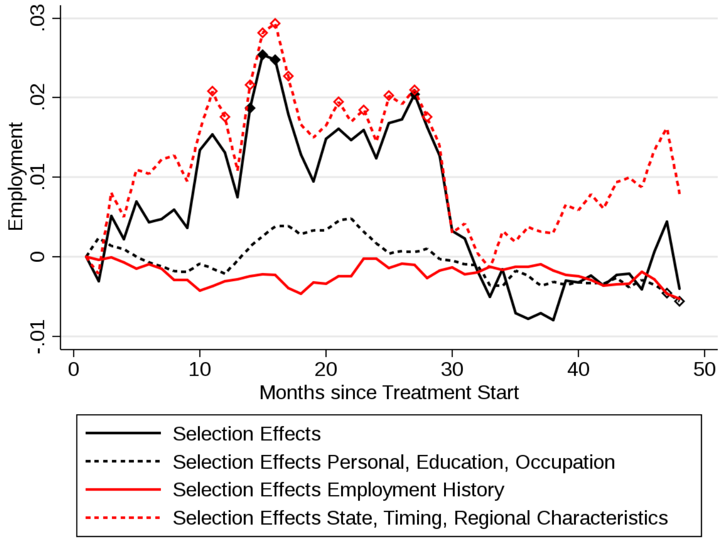
(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

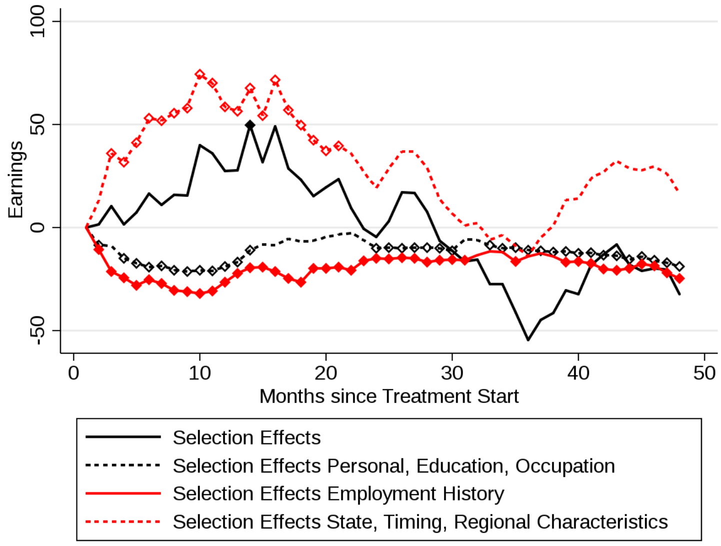
monthly regional labor market characteristics.[37] It turns out, that treatment under direct assignment is less effective if individuals are positively selected with respect to their personal, educational and occupational characteristics. In particular these negative effects

[37]Please find a description of all variables that belong to the different blocks in Table 2. In unreported results, we apply the decomposition to a finer set of blocks. The additional insights are rather limited and do not justify an increase in the complexity of Figure 3.

Figure 3: Decomposition of selection effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

are significant for earnings. For employment, these effects are mostly insignificant and show mixed patterns. We find negative influences of selection based on the employment and welfare histories on monthly earnings. These results are in line with Biewen, Fitzenberger, Osikominu, and Waller (2007), Doerr et al. (2013), and Wunsch and Lechner (2008), who find that individuals with better labor market characteristics profit less from further training.[38] However, these negative findings are compensated by changes in the timing of unemployment and treatment start, state of residence, and monthly regional labor market characteristics. For the third block, we report positive selection effects, in particular in the short and medium term.

In a next step, we investigate heterogenous selection effects by program types (comp. Figure 4). Further training programs can be separated by practice firm training, short training, long training, and retraining.[39] Our findings suggest that selection effects for participants in short training and retraining are significantly positive in the short and medium run. Especially short training would be more effective in the pre-reform period, if participants were selected according to implemented post-reform selection criteria. In Table 7 in Appendix E, we report the efficient first moments for participants in different program types before and after the reform. We observe selection of treated individuals in short training programs with regard to education levels, vocational status, employment histories and monthly regional labor market characteristics.[40] After the reform, unemployed participating in short training programs have on average better labor market characteristics. This suggests a higher effectiveness of short training programs for unemployed with better labor market characteristics (comp. Figure 4).[41] One possible explanation for this type of selection into short training is strategic behavior of caseworkers. The selection rule focus exclusively on the share of participants who find a job after training participation.[42] This might lead to a selection of unemployed with good labor market opportunities (even in the absence of training) in short programs to get early payoffs.

Large differences between participants in retraining before and after the reform can be found for monthly regional labor market characteristics. In particular, these types of

[38]Note that these studies investigate effect heterogeneity and do not account for correlations between different characteristics, e.g. vocational education and employment histories.

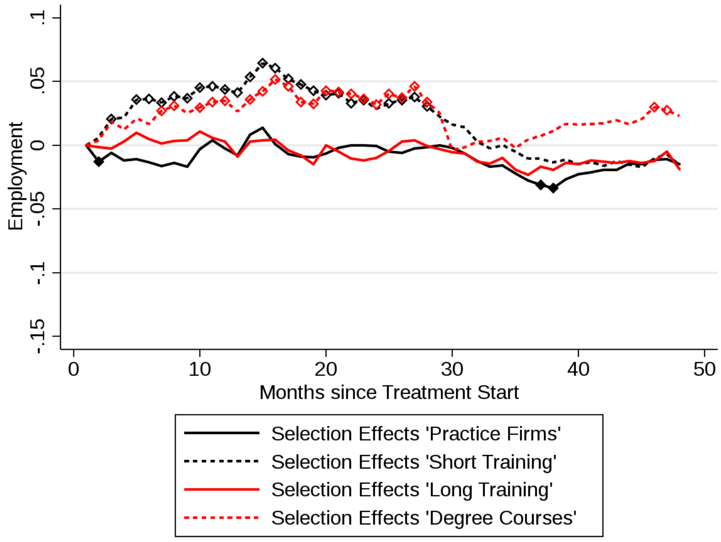
[39]See description in Section 2.1.

[40]For practice firm training, we find similar types of selection, however, the difference between the pre- and post-reform period is not as large as for short training.

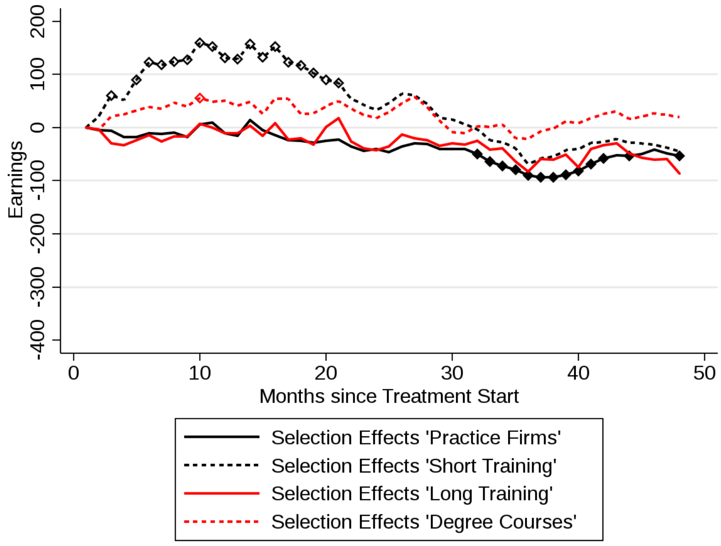
[41]This finding might be explained by an increase of the trainability of high skilled individuals for short training (Heckman, 2000).

[42]The share of re-employed participants should be on average 70% in a period of 6 months after training ends (see description in Section 2.1).

Figure 4: Selection effects on employment and earnings by program type.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

programs are more frequently allocated when the monthly regional unemployment rate is high. We expect that these positive short and medium term effects can be explained by the composition of the control group. Nevertheless, skill upgrading during periods with bad labor market situations can be economically efficient. Recently, Lechner and Wunsch (2009b) show that training programs work more effective during periods of high unemployment.

5.3 Business cycle effects

Before we focus on the changing assignment mechanism, we assess the plausibility of the common trend assumption (Assumption 3).[43] We follow three strategies to convince the reader of the plausibility of this assumption. First, we report long-term trends in the outcome variables for different samples in Figure 5. We report these time trends for years between 1990 and 2008. Prior to treatment start dates in 2001 and 2003, the treated and non-treated samples evolve parallel to each other. Given these parallel trends, it is likely that we would observe the same pattern after 2001 or 2003 in the absence of a treatment, respectively.

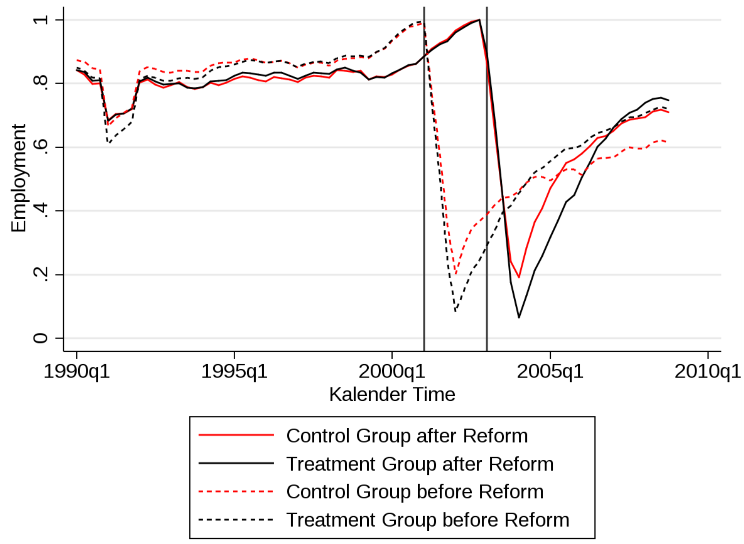
Second, we experiment with additional information about monthly regional labor market characteristics. We assess the sensitivity of our findings with respect to an inclusion or exclusion of these factors. We expect that our results are not sensitive to these variables when the common trend assumption holds.

Third, we use an alternative sample definition (Sample B). In the pre-reform period, we consider individuals who enter their first unemployment in 2002 and are treated within the following twelve months but not later than December 2002. As a consequence not all individuals in Sample B could be treated within the first twelve months of their unemployment period. The post-reform evaluation sample is not altered in Sample B in order to make a comparison of results regarding the different samples straightforward. Using Sample B, we approximate the timing of the reform implementation with regard to the inflow into unemployment. We argue that the common trend assumption is more likely to hold when the time difference between the pre- and post reform period is smaller.

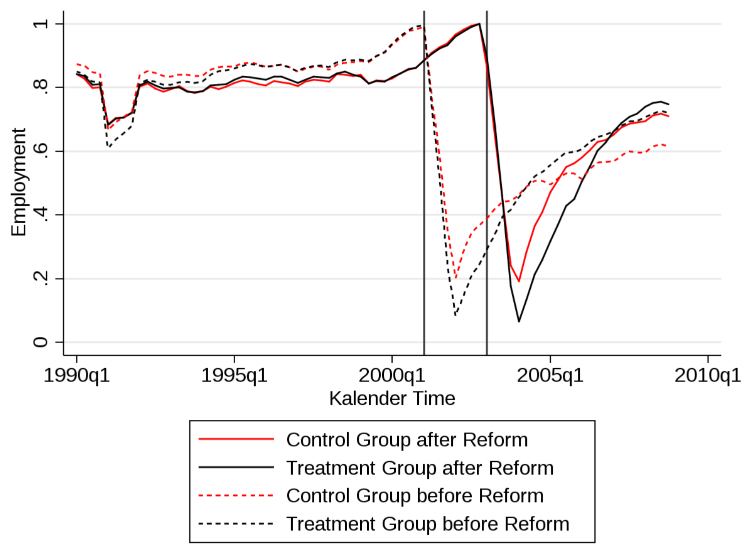
The business cycle effects under non-treatment (γ^{bc0}) for Sample A and B with and without monthly regional labor market characteristics are presented in Figure 6. During the first year after treatment, most effects are close to zero for both outcome variables.

[43] Under Assumption 3, the difference in the pre- and post reform outcomes of treated and non-treated are equal (if the assignment mechanism and subpopulation of interest do not change).

Figure 5: Time trends of employment and earnings for different subgroups of individuals for a time period from 1991-2008.



(a) Time trends of employment

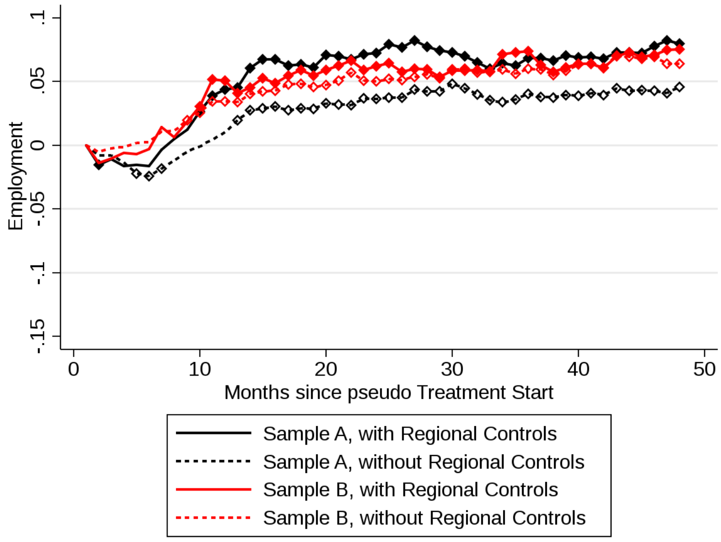


(b) Time trends of monthly earnings (in Euro)

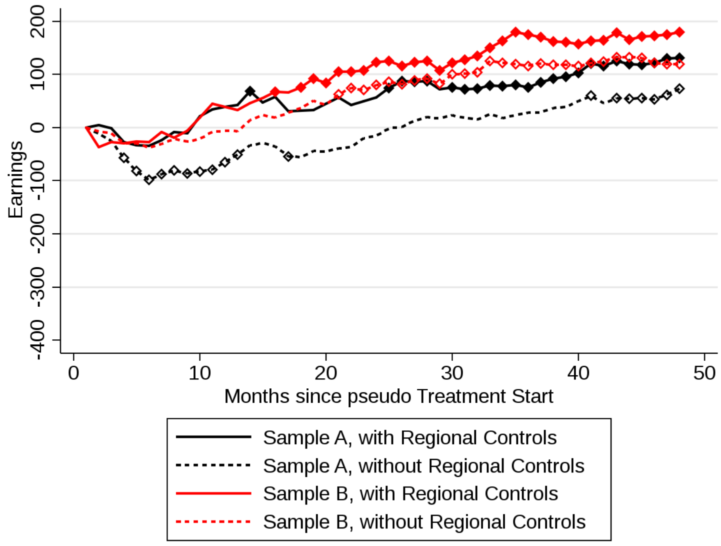
Note: We report time trends for years between 1990 and 2008. The outcome variables are reweighted as described in Section 4.3. Similar findings are obtained without reweighting.

Afterwards, the business cycle effects on employment increase sharply and remain subsequent on a stable level. After 48 months, we find 4-8 percentage points higher employment probabilities of non-treated individuals in the post-, compared to the pre-reform period. The business cycle effects on earnings evolve smoothly over the observation period. After 48 months, individuals in the control group earn on average between 80-180 Euro more

Figure 6: Business cycle effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero.

in the post-, compared to the pre-reform period.[44]

The general patterns of the business cycle effects are not sensitive to the sample designs and the inclusion of regional labor market characteristics. This supports the plausibility

[44]These findings do support the plausibility of Assumption 1. The potentially outcomes under non-treatment differ between period t_0 and t_1 only in the long run (excluding Sample A without regional labor market characteristics). This suggest there are no systematic difference in the treatment groups nt_0 and nt_1 at treatment start and briefly thereafter.

of the common trend assumption. However, the German labor market was intensively reformed during our observation period, particulary in 2005.[45] An improvement of the labor market situation can be observed in the long run. This could raise concerns about the plausibility of the common trend assumption, even in light of the robustness of our findings. Lechner and Wunsch (2009b) suggest that training programs work less efficiently in economic boom periods.[46] If the common trend assumption is invalid and the business cycle effects under treatment (γ^{bc1}) are larger than under non-treatment (γ^{bc0}), then the institutional effects (γ^{in}) might be positively biased.

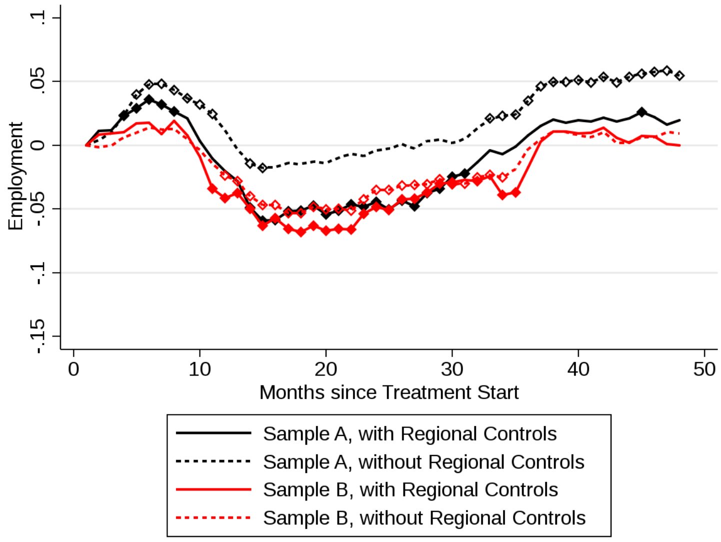
5.4 Institutional effects

After having discussed the plausibility of the common trend assumption, we consider the effects of the change in the assignment mechanism in this section. The institutional effects (γ^{in}) are presented in Figure 7. We show results for Sample A and B, with and without controlling for monthly regional labor market characteristics. In the short-term, institutional effects are positive, implying a higher effectiveness of training under the voucher regime. In the best case, training participants under the voucher regime have on average between 2-5 percentage points higher employment probabilities, and 70-150 Euros higher earnings per month than would they be, directly assigned to training after the reform. In the medium-term, the institutional effects turn negative. In the worst case, the employment probability decreases by 5 percentage points and earnings by 100 Euro per month. Not before 3 years after training start, we observe an increase to slightly positive but mostly insignificant institutional effects. Using the baseline sample (Sample A) without monthly regional labor market characteristics, we observe positive and significant institutional effects on employment and earnings in the longer run. Given the discussion in Section 5.3, these results could be positively biased. We interpret the results in a conservative way and rely on the insignificant results after 48 months.

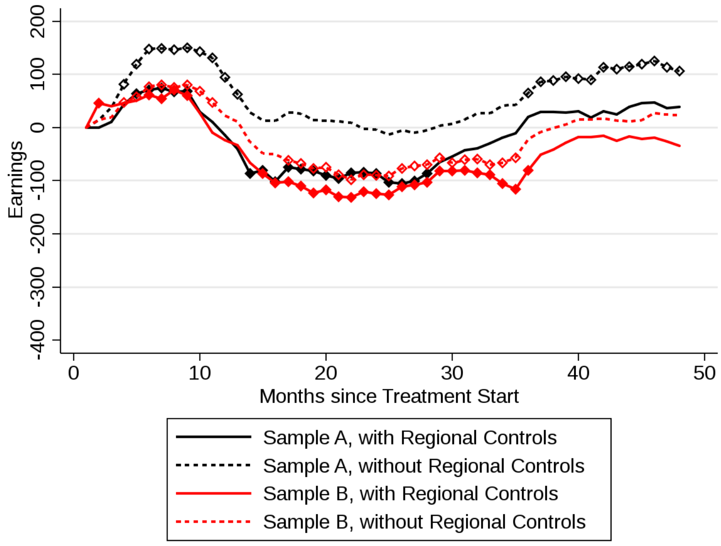
[45] We consider only treatment before 2005.

[46] Their findings are related to the unemployment rate at program start dates. At these times, we find that the unemployment rates are equally large in the pre- and post-reform period (comp. Table 1).

Figure 7: Institutional effects on employment and earnings.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero.

Changing compositions of program durations, changing motivation of program participants, changing responsibilities for the selection of courses, and a changing competition among training providers might be possible explanations for the ambiguous institutional effects (see discussion in Section 2.2). In the following, we investigate the changing composition of program types and durations after the reform. In Table 3, we report descriptive

Table 3: Average program durations by training types.

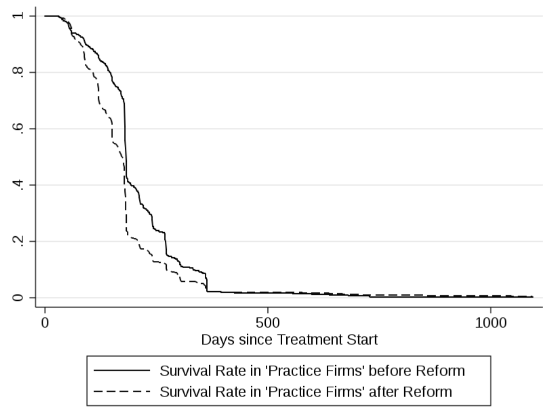
	# Obs	Percent	Average Planned Duration	Average Actual Duration	Difference
			Pre-Reform		
Practice Firms	10,770	17%	184 days	179 days	5 days
Short Training	17,255	27%	117 days	101 days	16 days
Long Training	21,343	34%	303 days	308 days	-5 days
Retraining	12,888	20%	754 days	712 days	42 days
Others	1,372	2%	346 days	364 days	-18 days
			Post-Reform		
Practice Firms	4,012	13%	158 days	146 days	12 days
Short Training	13,369	42%	130 days	113 days	17 days
Long Training	5,594	18%	278 days	278 days	0 days
Retraining	7,857	25%	799 days	760 days	39 days
Others	641	2%	456 days	407 days	49 days

Note: We use the baseline sample (Sample A). The category 'others' contains different types of training programs with only very few participants, e.g. programs which focus on career improvements.

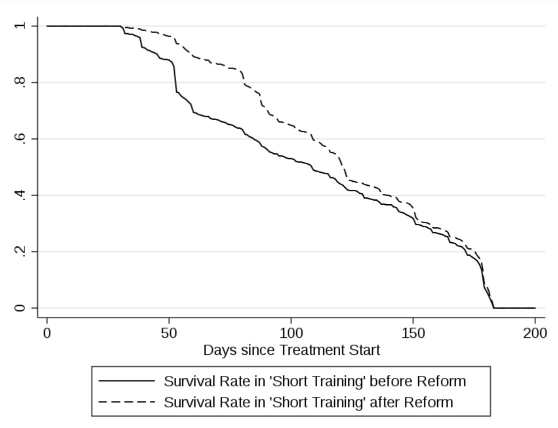
statistics for different types of training programs before and after the reform. The share of short training programs increases from 27% to 42%. The shares as well as the average planned and actual durations of practice firm and long training decrease after the reform. The share of retraining participants increases from 20% to 25%. Retraining programs are remarkably longer after the reform, the planned and actual durations are on average extended by nearly 50 days. In Figure 8, we plot the actual survival rates in training programs for participants before and after the reform. The survival rates in practice firm training after the reform are lower over the entire time horizon whereas the survival rates in short training programs increase. For long training, we find a spread in the actual survival rates implying that we observe more very short and very long training durations. After the reform, the actual survival rates in retraining programs increase. The dropout rates from retraining programs under the voucher regime is remarkably lower briefly after course start (comp. Figure 8(d)).

The changing composition of program durations might reflect an increased freedom of choice under the voucher regime. Training vouchers are determined with respect to their maximum program durations. Unemployed are free to choose between different

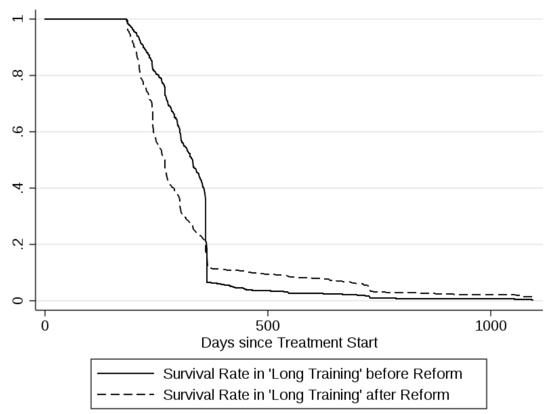
Figure 8: Survival rates in training programs for different program types before and after the reform.



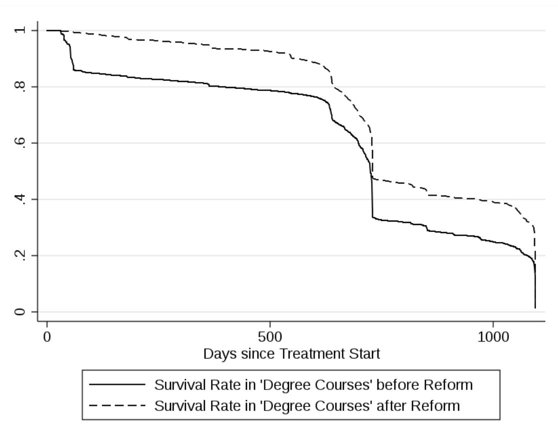
(a) Practice Firm Programs



(b) Short Training Programs



(c) Long Training Programs



(d) Retraining Programs

We report the share of participants who actually survive in training. We use the baseline Sample A.

training providers and courses, which potentially differ in their durations. However, after the reform, caseworkers could have an incentive to assign maximum program durations in a strategic way to comply to the stricter selection rule.[47] This constitutes a problem for the interpretation of the institutional effects. If caseworkers change the maximum program duration systematically in order to respond to the selection rule, the resulting effects correspond to changes in the selection criteria and not to changes in the assignment mechanisms.[48] In Table 3, we report the planned and actual duration of training programs. The planned program durations are under the control of the caseworkers.[49] We

[47] See discussion in Section 5.2.

[48] This might invalidate Assumption 4.

[49] Nevertheless, caseworkers and unemployed interact about potential training durations in obligatory counseling interviews.

find large changes in both duration measures between the pre- and post-reform period but the difference between the planned and actual duration does not change strongly.[50] This suggests that the change in observed program durations are heavily influenced by the caseworkers. We react to this potential drawback in two different ways. First, we investigate effect heterogeneity with respect to the program types. Second, we manipulate program durations in the post-reform treatment group and report changes of the counterfactual outcomes.

In Figure 9, we report heterogenous institutional effects by different types of training. The results suggest that the overall institutional effects cannot solely be explained by changes in the composition of program types. We find institutional effects for the different training types that might be explained by changing training durations, even after controlling for the type of training (comp. Table 3 and Figure 8). Short-term positive effects are found for practice firm training. Participants in practice firm training suffer less from lock-in effects, because program durations are shorter under the voucher regime. Between the 2nd and 3rd year after treatment start, institutional effects are negative for all program types. We only find significant effects on the employment probability. For retraining programs, the negative medium-term effects could be driven by longer program durations after the reform. The negative medium term effects for the other program types cannot be explained by longer lock-in periods, since most programs end within the first 12 months after treatment.[51] In the long run, we find zero or slightly positive but insignificant institutional effects for all program types. We conclude that the changing composition of program types is, indeed, an important factor of the overall institutional effects. The general pattern is very similar for all program types, even though some training durations are extended and others reduced after the reform.

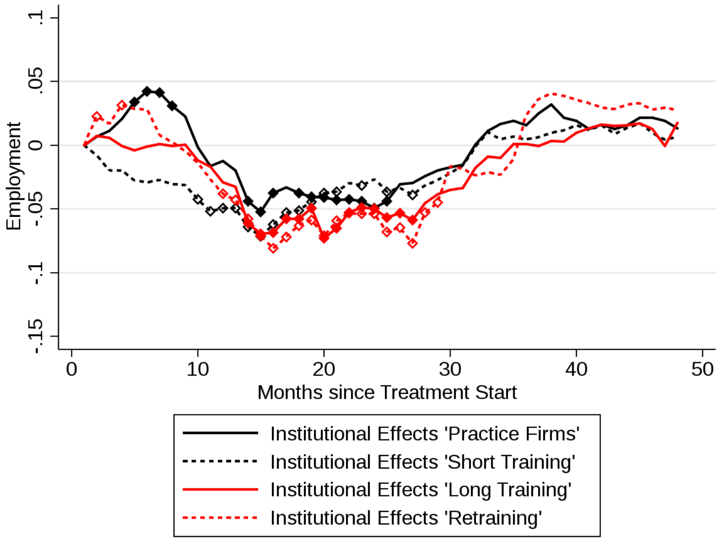
Next, we manipulate the program types and program durations in the treatment group after the reform, simultaneously.[52] Therefore, we estimate the expected outcomes under

[50] Excluding the category others.

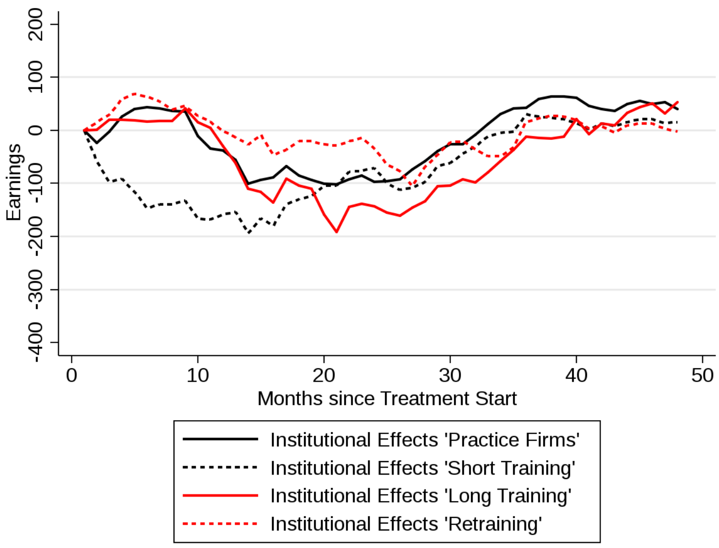
[51] Excluding some long training programs.

[52] It is not straightforward to control for the program duration in our main effects. These variables are part of the treatment and we do not observe program types and program durations in the control group. A possibility to overcome this problem is to apply a continuous treatment framework, as it was considered

Figure 9: Institutional effects on employment and earnings by program type.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

treatment in the post-reform period. Afterwards, we re-weight the post-reform treated observations in a way that the program types and durations of treated in the pre-reform period are revealed.[53] At the same time, we maintain the distribution of observed char-

for example in Flores, Flores-Lagunes, Gonzalez, and Neuman (2012) and Kluve, Schneider, Uhlendorff, and Zhao (2012).

[53] We control for the planned training duration, dummies for the program types, as well as for interactions

acteristics from treated persons after the reform. For this reason, we apply an analogue decomposition method as for the selection effects (see Section 5.2 and Appendix D). Accordingly, we are able to compare outcomes of two treated samples, which are similar in the observed individual and regional characteristics, but differ in the program types and durations. In Figure 10, we report the influences of program types and durations on the outcomes of the post-reform treatment group and the institutional effects.[54] In the short run, the influences of program types and durations are positive, and quantitatively comparable to the institutional effects. In the medium term, we report negative influences of the compositions of program types and durations, but these are by far not as steep as the institutional effects during this time interval.[55] In the long run, the composition of program types and durations influence both outcomes but the influence is lower than the overall institutional effects. Taken together, we find that changes in the composition of program types and durations may explain parts of the institutional effects, especially positive effects in the short run. There remains a large part unrelated to these factors in the medium-term after treatment. The arguments of Arni, Lalive, and Van den Berg (2012) and Van der Klaauw and Van Ours (2013) could explain the negative institutional effects in the medium term. They suggest that labor market policies which aim to improve the motivation of unemployed are less efficient than negative incentives (see Section 2.2 for a discussion).

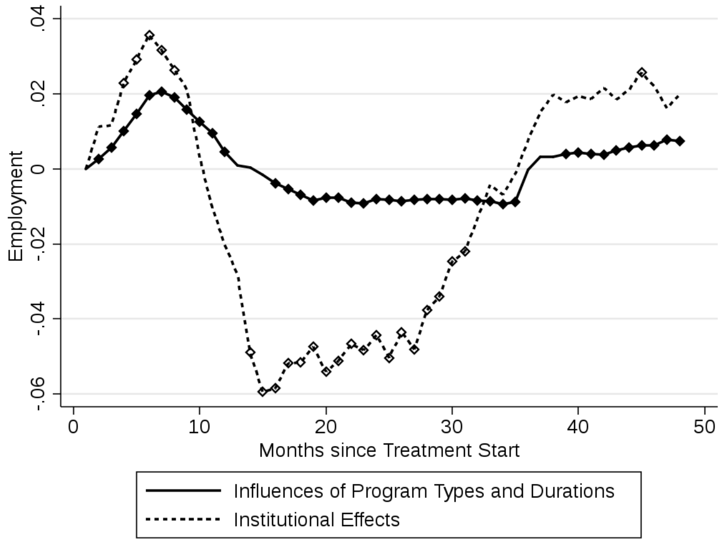
As discussed in Section 5.2, caseworkers may be motivated to assign individuals with good labor market perspectives to shorter programs to comply with the stricter selection rule. In Table 4, we report shares and average durations of training programs for individuals with different vocational skill levels. The program durations for treated persons changed strongly by vocational levels between the two time periods. We find shorter planned and actual program durations for high-educated training participants and longer for low-educated ones. The planned duration of training for individuals with an aca-

between the program types and the planned duration.

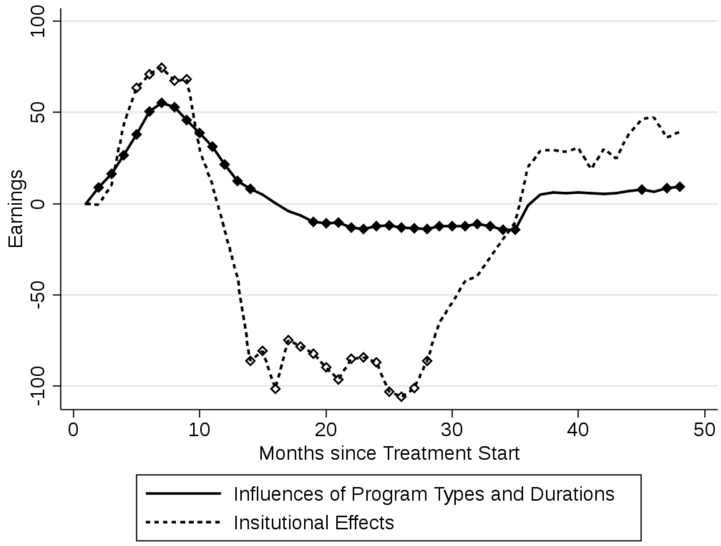
[54] However, it is difficult to compare these two parameters in a causal way, because they are estimated based on different propensity scores.

[55] For these time periods, Kluve, Rinne, Uhlendorff, and Zhao (2013) report similar effects of program durations on the effectiveness of training.

Figure 10: Influences of program types and durations.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

demic degree is on average reduced by 48 days. However, the actual duration is reduced even more by 74 days. This indicates that high educated individuals utilize the increased freedom of choice under the voucher regime and choose shorter courses.[56]

[56] An alternative explanation could be more program drop outs under the voucher regime, but we do not find any incidence for this explanation.

Table 4: Average program duration by vocational education level.

	# Obs	Percent	Average Planned Duration	Average Actual Duration	Difference
			Pre-Reform		
No Vocational Degree	14,44	23%	401 days	379 days	22 days
Vocational Degree	42,996	68%	301 days	292 days	9 days
Academic Degree	5,242	8%	307 days	300 days	7 days
Others	1,046	2%	368 days	350 days	18 days
			Post-Reform		
No Vocational Degree	6,484	21%	459 days	429 days	30 days
Vocational Degree	20,871	66%	307 days	300 days	7 days
Academic Degree	3,677	12%	259 days	226 days	33 days
Others	441	1%	379 days	330 days	49 days

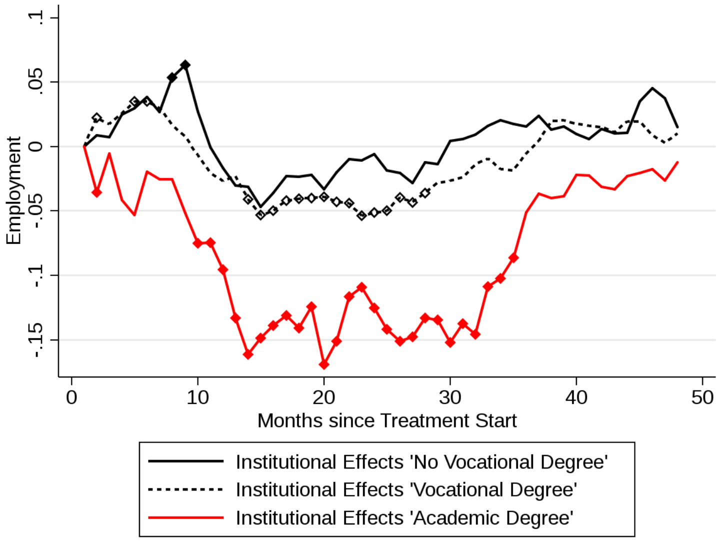
Note: We use the baseline Sample A. The category 'others' contains missings.

The institutional effects by skill levels are presented in Figure 11. The effects for individuals with no vocational degree are close to zero and almost never significant. The institutional effects for individuals with a vocational degree are positive in the short and negative in the medium run. In Figure 12(b) we plot the actual survival rates in training of these individuals before and after the reform. The actual survival rates after the reform are lower in the short run and turn out to be higher 1.5 years after the treatment. This suggests a spread in the program durations for middle-skilled individuals. Changing lock-in effects might (partly) explain the pattern of the institutional effects for this subpopulation. The institutional effects for academics are negative over the whole observation period, although the average program duration for high skilled individuals is remarkably shorter after the reform (comp. Table 4 and Figure 12(c)). The effects are significant for employment in the short- and medium-term. High-skilled academics suffer from the reform in this time interval. In the long term, there seem to be only small institutional effects for academics.[57]

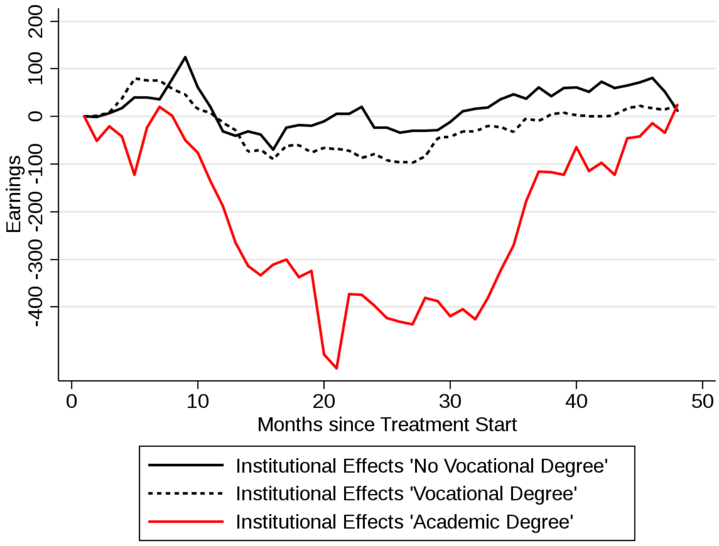
The positive institutional effects in the short-run are potentially driven by an increase of short training courses and lower program durations of firm practice training and long

[57] In unreported results, we estimate effect heterogeneity for interactions between program types and the skill level of participants. We find similar results for all program types. The negative medium term institutional effects are driven by the high-skilled participants.

Figure 11: Institutional effects on employment and earnings by vocational degree.



(a) Effects on employment

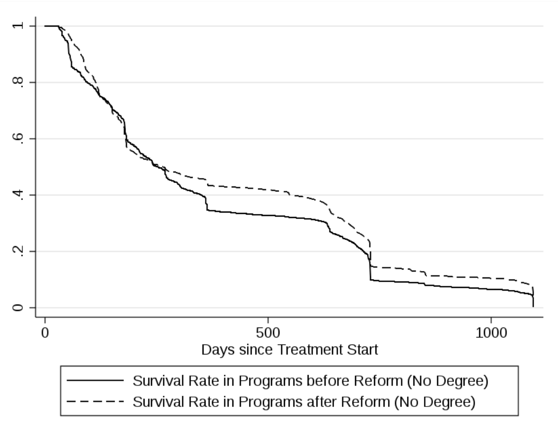


(b) Effects on monthly earnings (in Euro)

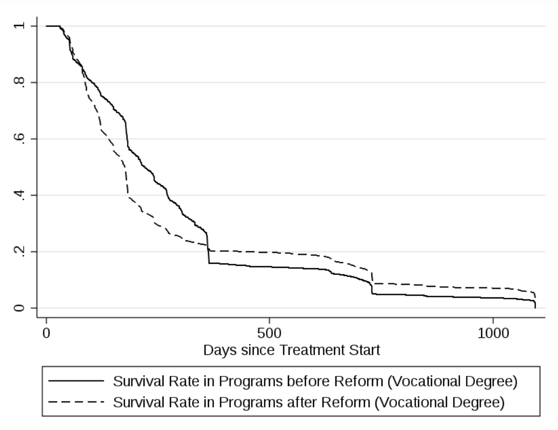
Note: We estimate separate effects for each of the first 48 months following the treatment. Diamonds report significant point estimates at the 5%-level. Standard errors are bootstrapped with 250 replications. In case we report lines without diamonds, the point estimates are not significantly different from zero. We use baseline Sample A and control for monthly regional labor market characteristics.

training programs under the voucher regime. The explanation of the lower effectiveness in the medium run is twofold: First, the share of retraining courses and their average duration increased after the reform. Participants in retraining suffer from longer lock-in periods. Second, the institutional setting after the reform selects high-skilled unemployed into shorter courses, which seems to be less effective (at least in the medium run). The

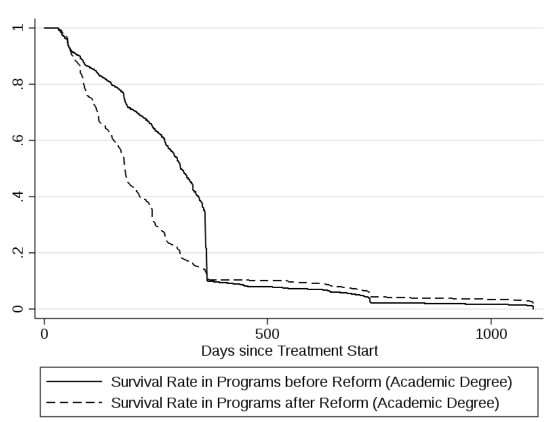
Figure 12: Survival rates in training programs by vocational degree of participants before and after the reform.



(a) Without Vocational Degree



(b) Vocational Degree



(c) Academic Degree

We report the share of participants who actually survive in training. We use the baseline Sample A.

increased freedom of choice under the voucher regime and the implementation of stricter selection rules are possible reasons for the observed (self-)selection of high-skilled into shorter courses.

6 Conclusions

This study analyzes the effectiveness of further training for unemployed under two different regulatory regimes, which are featured by different assignment mechanisms and selection criteria. In the pre-reform period, unemployed are directly assigned to specific training providers and courses. Under the new regime a voucher-like system is implemented. Further, stricter selection criteria should guarantee that only individuals with high employment probabilities participate in further training.

Our results suggest that effects resulting from the new assignment mechanisms (institutional effects) are driven by a changing composition of programs and program durations. The effects can be classified in three periods. We find positive institutional effects in the short run, which can be associated with changes in the composition of program types and durations. We find negative institutional effects in the medium time period after treatment. The increased shares and durations of long retraining programs can partly explain these negative findings. Additionally, training is less effective for high-skilled academics under the voucher regime. In the long-term (4 years after treatment start), the institutional effects are close to zero and almost never significant. The stricter selection criteria show on average no effects on the outcomes of interest. Using decomposition methods we find that changes in the spatial and temporal allocation of training lead to positive selection effects. The selection of participants with better labor market histories can be associated with negative effects.

As always in this type of evaluation studies, with a focus on the empirical identification of reform effects, the analysis relies on strong identifying assumptions. Especially the additive separability assumption is very critical in this study. Unobserved variables could potentially confound the effects of interest. The results of the sensitivity analysis and the use of different evaluation samples as well as the remarkably large and manifold data set we use, make us confident that the results are robust. Future research may focus on the long-term reform effects (beyond the 4 years time horizon), especially for high-skilled participants in training.

References

ABADIE, A. (2005): "Semiparametric Difference-in-Difference," *Review of Economic Studies*, 72(1), 1–19.

Abadie, A., and G. W. Imbens (2008): "On the Failure of the Bootstrap for Matching Estimators," *Econometrica*, 76(6), 1537–1557.

Abbring, J., G. Van den Berg, and J. Van Ours (2005): "The Effects of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment," *The Economic Journal*, 115(505), 602–630.

Arni, P., R. Lalive, and G. Van den Berg (2012): "Carrots & Sticks - Do Public Employment Service Policy Mixes Matter for Job Seekers' Post-Unemployment Earnings?," *Working Paper*.

Arni, P., R. Lalive, and J. Van Ours (2013): "How Effective Are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit," *Journal of Applied Econometrics*, forthcoming.

Barnow, B. (2009): "Vouchers in US Vocational Training Programs: An Overview of What We have Learned," *Journal of Labor Market Research*, 42(1), 71–84.

Behncke, S., M. Frölich, and M. Lechner (2009): "Targeting Labour Market Programmes: Results from a Randomized Experiment," *Swiss Journal of Economics and Statistics*, 145(3), 221–268.

Behncke, S., M. Frölich, and M. Lechner (2010): "Unemployed and their Case Workers: Should they be friends or foes?," *The Journal of the Royal Statistical Society - Series A*, 173(1), 67–92.

Bell, S., and L. Orr (2002): "Screening (and Creaming?) Applicants to Job Training Programs: The AFDC Homemaker Home Health Aide Demonstration," *Labour Economics*, 9(2), 279–302.

Berger, M., D. Black, and J. Smith (2000): "Evaluating Profiling as a Means of Allocating Government Services," in *Econometric Evaluation of Labour Market Policies*, ed. by M. Lechner, and F. Pfeiffer, pp. 59–84. Physica, Heidelberg.

Biewen, M., B. Fitzenberger, A. Osikominu, and M. Paul (2013): "The Effectiveness of Public Sponsored Training Revisited: The Importance of Data and Methodological Choices," *Journal of Labor Economics*, forthcoming.

Biewen, M., B. Fitzenberger, A. Osikominu, and M. Waller (2007): "Which Program for Whom? Evidence on the Comparative Effectiveness of Public Sponsored Training Programs in Germany," *IZA Discussion Paper 2885*.

Black, D. A., J. A. Smith, M. Berger, and B. J. Noel (2003): "Is the Threat of Reemployment Services more Effective than the Services themselves? Evidence from Random Assignment in the UI System," *American Economic Review*, 93(4), 1313–1327.

Card, D., J. Kluve, and A. Weber (2010): "Active Labour Market Policy Evaluations: A Meta-Analysis," *The Economic Journal*, 120(548), 452–477.

Colpitts, T. (2002): "Targeting Reemployment Services in Canada: The Service and Outcome Measurement System (SOMS) Experience," in *Targeting Employment Services*, ed. by R. Eberts, C. O'Leary, and S. Wandner, pp. 283–301. Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Dehejia, R. (2005): "Program Evaluation as a Decision Problem," *Journal of Econometrics*, 125(1-2), 141–173.

Dehejia, R. H., and S. Wahba (1999): "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs," *Journal of the American Statistical Association*, 94(448), 1053–1062.

DiNardo, J. E., N. M. Fortin, and T. Lemieux (1996): "Labor Markets Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach," *Econometrica*, 64(5), 1001–1044.

Doerr, A., B. Fitzenberger, t. Kruppe, M. Paul, and A. Strittmatter (2013): "The Award of Training Vouchers and Labor Market Outcomes," *Working Paper*.

Eberts, R., C. O'Leary, and S. Wandner (2002): *Targeting Employment Services.* Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Fitzenberger, B., A. Osikominu, and M. Paul (2010): "The Heterogeneous Effects of Training Incidence and Duration on Labor Market Transitions," *IZA Discussion Paper*, 5269.

Fitzenberger, B., A. Osikominu, and R. Völter (2008): "Get Training or Wait? Long Run Employment Effects of Training Programs for the Unemployed in West Germany," *Annales d'Economie et de Statistiquer*, 91-92, 321–355.

Fitzenberger, B., and R. Völter (2007): "Long-run Effects of Training Programs for the Unemployed in East Germany," *Labour Economics*, 14(4), 370–755.

Flores, C. A., A. Flores-Lagunes, A. Gonzalez, and T. C. Neuman (2012): "Estimating the Effects of Length of Exposure to Instruction in a Training Program: The Case of Job Corps," *Review of Economics and Statistics*, 94(1), 153–171.

Fortin, N. M., T. Lemieux, and S. Firpo (2010): "Decomposition Methods in Economics," in *Handbook of Labor Economics, Vol. 4 Part A*, ed. by O. Ashenfelter, and D. Card, pp. 1–102. North Holland.

Frederiksson, P., and P. Johansson (2008): "Dynamic Treatment Assignment - The Consequences for Evaluations Using Observational Studies," *Journal of Business Economics and Statistics*, 26(4), 435–445.

Friedman, M. (1955): "The Role of Government in Education," in *Economics and the Public Interest*, ed. by R. A. Solo. Rutgers University Press, New Brunswick.

——— (1962): *Capitalism and Freedom.* University of Chicago Press, Chicago.

Frölich, M. (2008): "Statistical Treatment Choice: An Application to Active Labour Market Programmes," *Journal of the American Statistical Association*, 103(482), 547–558.

Gerards, R., A. De Grip, and M. Witlox (2012): ""Employability-Mile" and Worker Employability Awareness," *ROA Research Memorandum*, ROA-RM-2012/10.

Görlitz, K. (2010): "The Effect of Subsidizing Continuous Training Investments - Evidence from German Establishment Data," *Labour Economics*, 17(5), 789–798.

Graham, B., C. De Xavier Pinto, and D. Egel (2012): "Inverse Probability Tilting for Moment Condition Models with Missing Data," *Review of Economic Studies*, 79(3), 1053–1079.

Graham, B. S., C. Campos De Xavier Pinto, and D. Egel (2011): "Efficient Estimation of Data Combination Models by the Method of Auxiliary-to-Study Tilting," *NBER Working Paper*, 16928.

Graversen, B. K., and J. C. Van Ours (2008): "How to Help Unemployed find Jobs Quickly: Experimental Evidence from a Mandatory Activation Program," *Journal of Public Economics*, 92(10-11), 2020–2035.

Heckman, J. (2000): "Policies to Foster Human Capital," *Research in Economics*, 54(1), 3–56.

Heckman, J., C. J. Heinrich, and J. Smith (2002): "The Performance of Performance Standards," *Journal of Human Resources*, 37(4), 778–811.

——— (2011): "Lessons for Advancing Future Performance Standards Systems," in *The Performance of Performance Standards*, ed. by J. Heckman, C. J. Heinrich, P. Courty, G. Marschke, and J. Smith, pp. 305–309. W.E. Upjohn Institute for Employment Research, Michigan.

Heckman, J., and S. Navarro (2007): "Dynamic Discrete Choice and Dynamic Treatment Effects," *Journal of Econometrics*, 136(2), 341–396.

Heckman, J. J., J. A. Smith, and C. Taber (1996): "What Do Bureaucrats Do? The Effects of Performance Standards and Bureaucratic Preferences on Acceptance into the JTPA Program," in *Advances in the Study of Entrepreneurship, Innovation and Growth, Vol. 7*, ed. by G. Libecap, pp. 191–217. JAI Press.

Heinrich, C., P. Mueser, K. Troske, K. Jeon, and D. Kahvecioglu (2010): "New Estimates of Public Employment and Training Program Net Impacts: A Nonexperimental Evaluation of the Workforce Investment Act Program," *Working Paper No. 1003, Department of Economics, University of Missourir.*

Hirano, K., and G. W. Imbens (2001): "Estimation of Causal Effects using Propensity Score Weighting: An Application to Data on Right Heart Catheterization," *Health Services and Outcomes Research*, 2(3-4), 259–278.

Hirano, K., G. W. Imbens, and G. Ridder (2003): "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score," *Econometrica*, 71(4), 1161–1189.

Hofmann, B. (2012): "Short- and Long-Term Ex-Post Effects of Unemployment Insurance Sanctions," *Journal of Economics and Statistics*, 232(1), 31–60.

Horvitz, D. G., and D. J. Thompson (1952): "A Generalization of Sampling Without Replacement from a Finite Universe," *Journal of the American Statistical Association*, 47(260), 663–685.

Hujer, R., S. Thomsen, and C. Zeiss (2006): "The Effects of Vocational Training Programmes on the Duration of Unemployment in Eastern Germany," *Allgemeines Statistisches Archiv*, 90(2), 299–322.

Imbens, G. (2000): "The Role of the Propensity Score in Estimating Dose-Response Functions," *Biometrika*, 87(3), 706–710.

Kluve, J., U. Rinne, A. Uhlendorff, and Z. Zhao (2013): "The Impact of Training Duration on Employment Outcomes: Evidence from LATE Estimates," *Economic Letters*, 120(3), 487–490.

Kluve, J., H. Schneider, A. Uhlendorff, and Z. Zhao (2012): "Evaluating Continous Training Measures using the Generalized Propensity Score," *Journal of the Royal Stastical Society, Series A*, 175(2), 587–617.

Lalive, R., J. Van Ours, and J. Zweimüller (2005): "The Effect of Benefit Sanctions on the Duration of Unemployment," *Journal of European Economic Association*, 3(6), 1386–1407.

Lechner, M. (2001): "Identifcation and Estimation of Causal Effects of Multiple Treatments under the Conditional Independence Assumption," in *Econometric Evaluation of Labour Market Policies*, ed. by M. Lechner, and F. Pfeiffer, pp. 43–58. ZEW Economic Studies 13. New York: Springer-Verlag.

Lechner, M. (2008): "A Note on the Common Support Problem in Applied Evaluation Studies," *Annales d'Economie et de Statistique*, 91-92, 217–234.

Lechner, M. (2009): "Sequential Causal Models for the Evaluation of Labor Market Programs," *Journal of Business and Economic Statistics*, 27, 71–83.

Lechner, M. (2013): "Treatment Effects and Panel Data," *Economics Working Paper Series 2013-14, University of St. Gallen, School of Economics and Political Science.*

Lechner, M., R. Miquel, and C. Wunsch (2007): "The Curse and Blessing of Training the Unemployed in a Changing Economy: The Case of East Germany after Unification," *German Economic Review*, 8(4), 465–509.

——— (2011): "Long-run Effects of Public Sector Sponsored Training," *The Journal of the European Economic Association*, 9(4), 742–784.

Lechner, M., and J. Smith (2007): "What is the Value Added by Caseworkers?," *Labour Economics*, 14(2), 135–151.

Lechner, M., and A. Strittmatter (2013): "Practical Procedures to Circumvent Support Problems in Program Evaluation Studies," *Working Paper.*

Lechner, M., and C. Wunsch (2009a): "Active Labour Market Policy in East Germany: Waiting for the Economy to Take Off," *Economics of Transition*, 17(4), 661–702.

——— (2009b): "Are Training Programs More Effective When Unemployment is High?," *Journal of Labor Economics*, 27(4), 653–692.

——— (2013): "Sensitivity of Matching-Based Program Evaluations to the Availability of Control Vriables," *Labour Economics*, 21(C), 111–121.

Levine, H. M., and C. Belfield (2002): "The Effects of Competition on Educational Outcomes: A Review of U.S. Evidence," *Review of Educational Research*, 72(2), 279–341.

Mitnik, O. A. (2009): "How Do Training Programs Assign Participants to Training? Characterizing the Assignment Rules of Government Agencies for Welfare-to-Work Programs in California," *IZA Discussion Paper*, 4024.

Müller, K. U., and V. Steiner (2008): "Imposed Benefit Sanctions and the Unemployment-to-Employment Transition: The German Experience," *DIW Discussion Paper*, 792.

Prasch, R. E., and F. A. Sheth (2000): "What is wrong with Education Vouchers?," *Journal of Economic Issues*, 34(2), 509–515.

Qin, J., and B. Zhang (2008): "Empirical-Likelihood-Based Difference-in-Difference Estimators," *Journal of the Royal Statistical Association B*, 70(2), 329–349.

Rinne, U., M. Schneider, and A. Uhlendorff (2011): "To Bad to Benefit? Effect Heterogeneity of Public Training Programs," *Applied Economics*, 43(25), 3465–3494.

Rinne, U., A. Uhlendorff, and Z. Zhao (2013): "Vouchers and Caseworkers in Training Programs for the Unemployed," *Empirical Economics*, forthcoming.

Rosenbaum, P. R., and D. B. Rubin (1983): "The Central Role of Propensity Score in Observational Studies for Causal Effects," *Biometrika*, 70(1), 41–55.

ROSHOLM, M., AND M. SVARER (2008): "The Threat Effect of Active Labor Market Programmes," *Scandinaavian Journal of Economics*, 11(2), 385–401.

RUBIN, D. B. (1974): "Estimating the Causal Effect of Treatments in Randomized and Non-Randomized Studies," *Journal of Educational Psychology*, 66(5), 688–701.

SCHWERDT, G., D. MESSER, L. WOESSMANN, AND S. C. WOLTER (2012): "Effects of Adult Education Vouchers on the Labor Market: Evidence from a Randomized Field Experiment," *Journal of Public Economics*, 96 (7-8), 569–583.

SIANESI, B. (2004): "An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s," *The Review of Economics and Statistics*, 86(1), 133–155.

STEPHAN, G., AND A. PAHNKE (2011): "The Relative Effectiveness of Selected Active Labor Market Programs: an Empirical Investigation for Germany," *The Manchester School*, 79(6), 1262–1293.

VAN DEN BERG, G. J., A. BERGEMANN, AND M. CALIENDO (2009): "The Effect of Active Labor Market Programs on Not-Yet Treated Unemployed Individuals," *Journal of the European Economic Association*, 7(2-3), 606–616.

VAN DEN BERG, G. J., B. VAN DER KLAAUW, AND J. VAN OURS (2004): "Punitive Sanctions and the Transition Rate from Welfare to Work," *Journal of Labor Economics*, 22(1), 211–241.

VAN DER KLAAUW, B., AND J. VAN OURS (2013): "Carrot and Stick: How Reemployment Bonuses and Benefit Sanctions Affect Exit Rates from Welfare," *Journal of Applied Econometrics*, 28(2), 275–296.

WUNSCH, C., AND M. LECHNER (2008): "What Did All the Money Do? On the General Ineffectiveness of Recent West German Labour Market Programmes," *Kyklos*, 61(1), 134–174.

A Alternative treatment definitions

As mentioned in Section 3.1, existing concerns about the treatment definition are related to the announcement of an intended assignment to a training course or voucher award. The announcement could have instantaneous effects on the job search intensity. Van den Berg, Bergemann, and Caliendo (2009) argue that the pure existence of training programs has already effects on job search behaviors and reservation wages. Arni, Lalive, and Van den Berg (2012) report positive ex-ante earnings effects of different labor market policies. Arni, Lalive, and Van Ours (2013) and Lalive, Van Ours, and Zweimüller (2005) suggest that the announcement of sanctions *per se* have negative effects on unemployment.

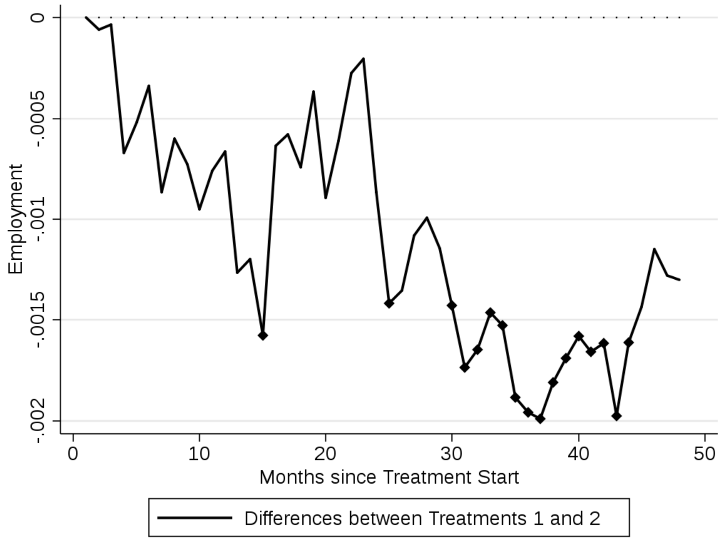
There are not many ways to deal with this concern in the pre-reform period. The announcement of a planned assignment to a training course is usually not observed. Therefore, most evaluation studies in the pre-reform period define the treatment time at the start of training courses. Lechner, Miquel, and Wunsch (2011) show descriptive results which suggest that anticipation effects are unlikely the be an important determinant for the effectiveness of further training under the direct assignment regime. Figure 5 supports their arguments, because the slopes of the treatment and control groups are equal after 2001 (and 2003). This suggests that the behavior of participants and non-participants is equal in the first time of unemployment.

In contrast to the pre-reform period, we observe the award and redemption of vouchers in the post-reform period. It is almost impossible that the announcement of a planned assignment to a training course and the start of the course happen on the same day. Yet, caseworkers can announce and award vouchers on the same day. Therefore, even though the award of vouchers is not a perfect measure for announcements, it might be a good approximation. At least, it allows for an interesting variation in treatment start dates, enabling sensitivity analyses with respect to this factor.

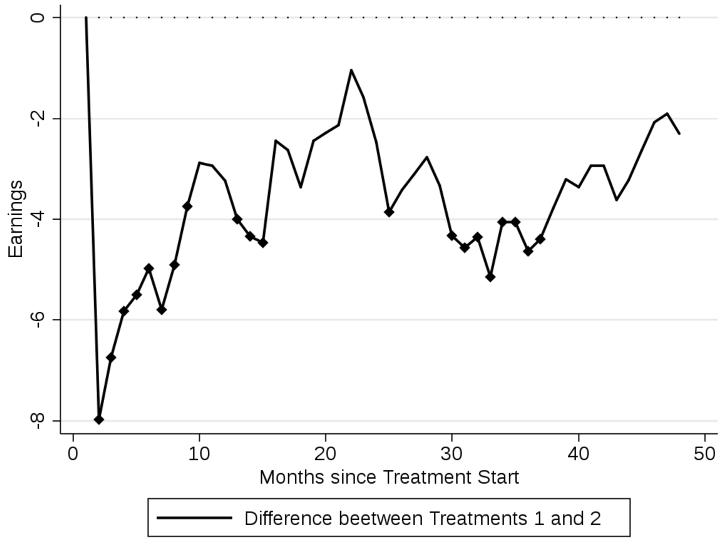
In the following we define two treatments for the post-reform period. The first treatment (Treatment 1) is equal to the treatment definition in this study (see Section 3.1). We use the program start dates as treatment times. Individuals with expired vouchers are in the control group. For the second treatment definition (Treatment 2), we use the dates of voucher awards as treatment time. Individuals with expired vouchers are in the treatment group. The difference in the post-reform treatment effects between Treatment 1 and 2 on employment and monthly earnings are presented in Figure 13. Over the entire observation period, we find that the effects of Treatment 2 are higher than the effects of Treatment 1. One explanation could be, that the effects of Treatment 2 for individuals who do not redeem the training voucher are positive.[58] Studies that rely on the announcement rather than the participation as treatment, possibly draw more positive conclusions. However, the differences appear to be very small. In the worst case the effect on the employment probability decrease by 0.2 percentage points and the effect on monthly earnings is 8 Euros lower. For our identification strategy, it is very important to use the same treatment definition before and after the reform. Otherwise, the effects of interest could be altered by this factor.

[58]Individuals with expired vouchers have higher past earning profiles, but more often health problems or incapacities than individuals with redeem vouchers. See Doerr et al. (2013) for a description of individuals with redeemed and expired vouchers.

Figure 13: Comparison of different treatment definitions.



(a) Effects on employment



(b) Effects on monthly earnings (in Euro)

Note: We use the baseline sample (Sample A) and control for monthly regional labor market characteristics. Diamonds report significant point estimates at the 5%-level. In case we report lines without diamonds, the point estimates are not significantly different from zero.

B Matching quality

We assess the matching quality by reporting the moments (mean, variance, skewness, kurtosis) and standardized differences for the control variables in all four sample. The standardized 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 $\sigma^2_{\mu_k}$ the variance of the moment in the respective treatment group $k \in \{at_0, vt_1, nt_0, nt_1\}$. The before matching standardized differences between the sample first moments are reported in Table 1. The after matching standardized differences between the efficient first moments are exactly zero, because the first moments are exactly balanced (see discussion in Section 4.3). Therefore, we do not even report the standardized difference of the matched samples in Table 2. We only report the standardized differences between the efficient first moments matched to the treatment groups under the voucher and assignment regimes.

In the optimal case, matching estimators balance the entire distributions of all control variables and not 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. Thereof, 43 are binary variables. 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 5. Further, we show the higher moments for the different samples matched to the treatment group under the assignment regime in Table 6. For most moments we report small standardized difference. However, especially for the monthly regional labor market characteristics when the samples are matched to the treatment group under the assignment regime we find large differences in the higher moments.

Table 5: Higher moments of observed characteristics matched to the treatment group under the voucher regime.

	Voucher Regime		Assignment Regime		Standardized 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	55.58	63.54	57.31	62.78	11.81	2.73	10.94
Half months employed in the last 24 months	42.06	40.95	38.71	37.58	1.00	3.09	4.19
Half months unemployed in the last 24 months	2.77	2.91	2.93	2.79	0.69	0.78	0.09
Time since last unemployment in the last 24 months (half-months)	20.38	20.72	21.06	19.97	0.37	0.69	0.42
# unemployment spells in the last 24 months	0.17	0.17	0.17	0.17	0.35	0.02	0.69
Time of last out of labor force in last 24 months	42.44	42.49	40.79	40.47	0.03	1.18	1.43
Remaining unemployment insurance claim	179.68	209.81	163.82	175.22	10.66	6.17	1.72
Eligibility unemployment benefits	24.79	24.89	25.05	24.07	0.23	0.61	1.69
Cumulative employment (last 4 years before Unemployment)	516.21	472.42	493.26	445.63	5.63	2.94	9.20
Cumulative earnings (last 4 years before Unemployment)	$2.42{\cdot}10^9$	$2.49{\cdot}10^9$	$2.39{\cdot}10^9$	$2.38{\cdot}10^9$	2.02	0.77	1.05
Cumulative benefits (last 4 years before Unemployment)	60.78	59.90	66.86	59.26	0.29	1.94	0.51
Elapsed unemployment duration	11.37	12.35	13.13	12.12	9.05	16.03	6.98
Share of employed in the production industry	0.00787	0.00803	0.00864	0.00887	1.66	7.50	9.71
Share of employed in the construction industry	0.00040	0.00042	0.00036	0.00037	3.08	5.90	4.77
Share of employed in the trade industry	0.00032	0.00035	0.00030	0.00030	5.42	3.83	3.99
Share of male unemployed	0.00173	0.00180	0.00152	0.00157	2.75	8.69	6.83
Share of non-German unemployed	0.00732	0.00747	0.00604	0.00594	1.76	15.96	16.91
Share of vacant fulltime jobs	0.00584	0.00589	0.00404	0.00388	0.42	19.35	21.68
Population per km^2	2791649	2817132	2236202	2301452	0.27	6.21	5.43
Unemployment rate (in %)	25.24	26.04	20.15	20.14	2.30	15.46	15.31
Skewness							
Age	54	116	90	133	4.96	3.22	6.59
Half months employed in the last 24 months	-700	-676	-615	-584	0.98	3.65	5.08
Half months unemployed in the last 24 months	27	30	32	29	1.04	1.51	0.59
Time since last unemployment in the last 24 months (half-months)	-388	-408	-457	-424	0.91	2.60	1.39
# unemployment spells in the last 24 months	0.31	0.32	0.30	0.34	0.54	0.14	1.06
Time of last out of labor force in last 24 months	-858	-872	-792	-771	0.32	1.58	2.13
Remaining unemployment insurance claim	-61	276	-226	-159	3.32	1.90	1.11
Eligibility unemployment benefits	143	155	152	151	2.08	1.63	1.23
Cumulative employment (last 4 years before Unemployment)	-15701	-13720	-14268	-12866	4.43	3.20	6.47
Cumulative earnings (last 4 years before Unemployment)	$69.2{\cdot}10^{12}$	$89.4{\cdot}10^{12}$	$74.3{\cdot}10^{12}$	$84.6{\cdot}10^{12}$	4.21	1.10	3.34
Cumulative benefits (last 4 years before Unemployment)	1964.56	1977.19	2303.21	1886.83	0.07	1.84	0.44
Elapsed unemployment duration	4.33	4.13	5.11	3.77	0.25	0.93	0.70
Share of employed in the production industry	0.0002343	0.0002966	0.0004729	0.0004752	2.92	9.99	10.19
Share of employed in the construction industry	0.0000064	0.0000070	0.0000085	0.0000089	2.18	6.62	7.50
Share of employed in the trade industry	0.0000024	0.0000026	-0.0000002	-0.0000003	0.82	12.90	13.18
Share of male unemployed	-0.0000381	-0.0000420	-0.0000029	-0.0000098	1.29	12.85	10.27
Share of non-German unemployed	0.0001427	0.0001496	0.0000594	0.0000912	0.40	5.19	3.15
Share of vacant fulltime jobs	-0.0003946	-0.0004476	-0.0001745	-0.0001411	1.64	8.85	10.50
Population per km^2	$14.9{\cdot}10^9$	$15.1{\cdot}10^9$	$11.3{\cdot}10^9$	$11.7{\cdot}10^9$	0.33	6.13	5.32
Unemployment rate (in %)	112	125	88	88	2.88	5.90	5.66
Kurtosis							
Age	7017	9209	7381	8691	12.11	2.51	10.03
Half months employed in the last 24 months	14408	14094	12463	11721	0.57	3.80	5.42
Half months unemployed in the last 24 months	343	420	484	397	1.12	1.85	0.85
Time since last unemployment in the last 24 months (half-months)	8580	9362	11876	10957	1.40	4.28	3.22
# unemployment spells in the last 24 months	0.80	0.86	0.75	0.92	0.37	0.44	0.79
Time of last out of labor force in last 24 months	22428	23188	20023	19040	0.51	1.72	2.52
Remaining unemployment insurance claim	103407	132590	87898	95158	9.33	6.08	3.21
Eligibility unemployment benefits	2335	2599	2489	2531	3.35	2.15	2.41
Cumulative employment (last 4 years before Unemployment)	912069	787352	814195	730438	5.24	4.12	7.81
Cumulative earnings (last 4 years before Unemployment)	$17.6{\cdot}10^{18}$	$19.9{\cdot}10^{18}$	$18{\cdot}10^{18}$	$18.5{\cdot}10^{18}$	3.77	0.69	1.57
Cumulative benefits (last 4 years before Unemployment)	92385	97592	113125	91505	0.41	1.67	0.07
Elapsed unemployment duration	243	272	299	263	8.18	15.55	5.74
Share of employed in the production industry	0.0001453	0.0001589	0.0002030	0.0002052	3.31	12.12	12.99
Share of employed in the construction industry	0.0000005	0.0000005	0.0000005	0.0000006	3.22	3.11	4.98
Share of employed in the trade industry	0.0000003	0.0000003	0.0000003	0.0000003	4.16	5.26	3.13
Share of male unemployed	0.0000094	0.0000107	0.0000075	0.0000077	3.88	6.76	6.02
Share of non-German unemployed	0.0001246	0.0001280	0.0000952	0.0000980	1.24	11.28	9.87
Share of vacant fulltime jobs	0.0001574	0.0001715	0.0000657	0.0000554	1.45	12.61	14.36
Population per km^2	$98.7{\cdot}10^{12}$	$100{\cdot}10^{12}$	$74.1{\cdot}10^{12}$	$77.3{\cdot}10^{12}$	0.38	6.28	5.42
Unemployment rate (in %)	1736.15	1978	1471	1529	4.40	5.51	4.17

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 refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and the monthly regional labor market characteristics which refer to the (pseudo) treatment time. In columns (5)-(7) we report the standardized differences between the different subsamples and the treatment group under the voucher regime. All control variables which are not reported in this table are binary distributed. The higher moments of these variables are exactly balanced in the matched samples.

Table 6: Higher moments of observed characteristics matched to the treatment group under the assignment regime.

	Voucher Regime		Assignment Regime		Standardized 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	60.92	65.97	60.18	65.74	1.09	7.92	7.75
Half months employed in the last 24 months	60.48	60.61	55.80	54.48	3.94	3.90	1.15
Half months unemployed in the last 24 months	4.01	4.58	4.22	3.98	1.01	1.55	1.15
Time since last unemployment in the last 24 months (half-months)	43.29	45.74	48.26	46.42	3.42	1.67	1.15
# unemployment spells in the last 24 months	0.26	0.29	0.26	0.26	0.25	2.51	0.19
Time of last out of labor force in last 24 months	61.60	63.69	59.26	59.78	1.38	2.55	0.32
Remaining unemployment insurance claim	162.11	180.26	144.77	162.79	7.44	14.95	7.83
Eligibility unemployment benefits	27.17	26.97	27.80	27.09	1.36	1.73	1.45
Cumulative employment (last 4 years before Unemployment)	615.87	554.15	574.60	529.18	5.10	2.65	5.94
Cumulative earnings (last 4 years before Unemployment)	$2.12{\cdot}10^9$	$2.19{\cdot}10^9$	$2.03{\cdot}10^9$	$2.1{\cdot}10^9$	2.76	4.78	1.94
Cumulative benefits (last 4 years before Unemployment)	83.95	89.33	83.60	79.88	0.09	1.46	1.03
Elapsed unemployment duration	10.51	12.32	12.27	12.24	16.09	0.46	0.29
Share of employed in the production industry	0.00553	0.00554	0.00868	0.00832	36.15	35.25	3.54
Share of employed in the construction industry	0.00078	0.00079	0.00068	0.00073	13.47	13.99	5.50
Share of employed in the trade industry	0.00042	0.00041	0.00038	0.00038	7.51	5.40	1.00
Share of male unemployed	0.00303	0.00282	0.00204	0.00214	32.23	26.24	3.69
Share of non-German unemployed	0.00999	0.00961	0.00837	0.00825	16.14	12.65	1.30
Share of vacant fulltime jobs	0.00511	0.00538	0.00511	0.00481	0.00	2.76	3.18
Population per km^2	3055373	3120766	2688290	2717393	3.68	4.30	0.30
Unemployment rate (in %)	31.82	33.50	32.14	31.55	1.07	4.49	1.82
Skewness							
Age	81	129	113	163	2.53	1.18	3.61
Half months employed in the last 24 months	-903	-948	-806	-777	3.79	5.02	1.15
Half months unemployed in the last 24 months	34	46	41	35	2.11	1.29	1.87
Time since last unemployment in the last 24 months (half-months)	-774	-878	-1071	-997	7.38	4.54	1.55
# unemployment spells in the last 24 months	0.53	0.81	0.47	0.50	1.01	4.58	0.59
Time of last out of labor force in last 24 months	-1248	-1338	-1133	-1162	2.20	3.76	0.57
Remaining unemployment insurance claim	38	219	-130	95	2.17	4.34	3.00
Eligibility unemployment benefits	152	168	165	172	1.89	0.42	0.91
Cumulative employment (last 4 years before Unemployment)	-17825	-14815	-15636	-14138	4.66	1.85	3.44
Cumulative earnings (last 4 years before Unemployment)	$65{\cdot}10^{12}$	$82.3{\cdot}10^{12}$	$63.6{\cdot}10^{12}$	$80.6{\cdot}10^{12}$	0.36	4.12	3.84
Cumulative benefits (last 4 years before Unemployment)	3066	3446	2889	2683	0.79	2.39	0.96
Elapsed unemployment duration	9.14	9.05	11.19	9.99	2.47	2.46	1.38
Share of employed in the production industry	0.0001414	0.0001674	0.0004265	0.0004055	14.68	12.98	0.87
Share of employed in the construction industry	0.0000065	0.0000085	0.0000112	0.0000133	9.49	5.25	3.69
Share of employed in the trade industry	0.0000061	0.0000057	0.0000043	0.0000044	6.33	5.06	0.28
Share of male unemployed	-0.0001002	-0.0000847	-0.0000154	-0.0000241	22.29	19.32	2.70
Share of non-German unemployed	0.0005184	0.0004506	0.0002440	0.0002494	12.01	9.31	0.27
Share of vacant fulltime jobs	-0.0002995	-0.0003292	-0.0003834	-0.0003058	2.79	1.94	2.90
Population per km^2	$17.5{\cdot}10^9$	$17.9{\cdot}10^9$	$14.8{\cdot}10^9$	$15.1{\cdot}10^9$	4.07	4.62	0.40
Unemployment rate (in %)	100	101	103	98	0.73	0.51	1.11
Kurtosis							
Age	8325	10435	8232	10018	0.55	10.00	8.36
Half months employed in the last 24 months	18354	20611	16614	15937	3.10	5.73	1.12
Half months unemployed in the last 24 months	370	570	544	396	2.60	0.37	2.19
Time since last unemployment in the last 24 months (half-months)	16984	20602	29535	26761	10.32	6.91	1.82
# unemployment spells in the last 24 months	1.78	3.48	1.24	1.41	1.67	5.55	0.94
Time of last out of labor force in last 24 months	34575	37973	30010	31070	2.59	4.32	0.64
Remaining unemployment insurance claim	86770	97100	69034	84460	7.48	11.43	7.07
Eligibility unemployment benefits	2803	3042	3050	3240	2.59	0.08	1.67
Cumulative employment (last 4 years before Unemployment)	1083762	895325	937410	841010	6.13	1.87	4.39
Cumulative earnings (last 4 years before Unemployment)	$14.7{\cdot}10^{18}$	$17.4{\cdot}10^{18}$	$13.9{\cdot}10^{18}$	$16.3{\cdot}10^{18}$	1.47	5.78	4.20
Cumulative benefits (last 4 years before Unemployment)	157708	179622	144020	130948	0.92	2.31	0.93
Elapsed unemployment duration	223	275	278	273	14.02	0.94	1.24
Share of employed in the production industry	0.0000772	0.0000840	0.0001808	0.0001729	27.04	24.31	1.59
Share of employed in the construction industry	0.0000012	0.0000012	0.0000011	0.0000013	2.78	5.17	5.85
Share of employed in the trade industry	0.0000005	0.0000005	0.0000004	0.0000004	6.63	5.39	2.33
Share of male unemployed	0.0000212	0.0000187	0.0000109	0.0000125	27.59	22.46	5.16
Share of non-German unemployed	0.0002222	0.0002058	0.0001514	0.0001518	17.71	14.07	0.11
Share of vacant fulltime jobs	0.0001308	0.0001274	0.0001255	0.0001046	0.53	0.22	2.44
Population per km^2	$117{\cdot}10^{12}$	$120{\cdot}10^{12}$	$98.6{\cdot}10^{12}$	$100{\cdot}10^{12}$	4.22	4.82	0.42
Unemployment rate (in %)	1747	1888	2098	2059	9.24	5.39	0.91

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 refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and the monthly regional labor market characteristics which refer to the (pseudo) treatment time. In columns (5)-(7) we report the standardized differences between the different subsamples and the treatment group under the voucher regime. All control variables which are not reported in this table are binary distributed. The higher moments of these variables are exactly balanced in the matched samples.

C Proof Equation (1)

We show that $E[Y_i(d)|D_i = g]$ can be identified from the joint distribution of random variables $(Y, D(d), D(g), X)$ under Assumptions 1 and 2 (comp. Hirano, Imbens, and Ridder, 2003, Rosenbaum and Rubin, 1983):

$$\begin{aligned}
E[Y_i(d)|D_i = g] = & \int E[Y_i(d)|D_i = g, X_i = x] f_X(x|D_i = g)dx, \\
= & \int E[Y_i(d)|D_i = d, X_i = x] f_X(x|D_i = g)dx, \\
= & \int E[Y_i|D_i = d, X_i = x] f_X(x|D_i = g)dx, \\
= & \int E[D_i(d)Y_i|D_i = d, X_i = x] f_X(x|D_i = g)dx, \\
= & \int \frac{1}{p_d(x)} E[D_i(d)Y_i|X_i = x] f_X(x|D_i = g)dx, \\
= & \int \frac{p_g(x)}{p_g \cdot p_d(x)} E[D_i(d)Y_i|X_i = x] f_X(x)dx, \\
= & \int \frac{p_g(x)}{p_g \cdot p_d(x)} D_i(d)Y_i f_X(x)dx, \\
= & E\left[\frac{p_g(x)}{p_g \cdot p_d(X)} D_i(d)Y_i\right].
\end{aligned}$$

In the first equation, we apply the law of iterative expectations. In the second equality we condition on $D_i = d$, which is possible because we assume that the expected potential outcomes are independent of the treatment after controlling for X_i (Assumption 1). In equality three, we replace the potential by the observed outcome. In equality four, we multiply the outcome Y_i with the treatment dummy $D_i(d)$. In equality five, we use the fact that $E[DY] = E[DY|D = 1]Pr(D = 1)$. In equality six, we apply Bayes' rule. A backward application of the law of iterative expectations is made in equality seven. Finally, we replace the integral by an expectation in equality eight. $\square$

D Blinder-Oaxaca decomposition

We apply a non-parametric Blinder-Oaxaca Decomposition on the selection effects. See Fortin, Lemieux, and Firpo (2010) for a recent review of decomposition methods. We have the intention to change one block of variables and remain all other variables at the initial level.[59] Let $X_i = (X_{1i}, X_{2i})$ be a vector of control variables. Using the notation of Section 4.1, the selection effects can be formalized by,

$$\begin{aligned}\gamma^s = &\int E[Y_i(at_0) - Y_i(nt_0)|X_i = x] f_{X_i}(x|D_i = vt_1)dx \\ &- \int E[Y_i(at_0) - Y_i(nt_0)|X_i = x] f_{X_i}(x|D_i = at_0)dx.\end{aligned}$$

This is the difference in the pre-reform treatment effects between individuals with observed characteristics like in the post-reform period and individuals with observed characteristics like in the pre-reform period. Next we only want to change one block of characteristics X_{1i}. The decomposed selection effects (γ^{ds}) can be indicated by,

$$\begin{aligned}\gamma^{ds} = &\int\int E[Y_i(at_0) - Y_i(nt_0)|X_{1i} = x_1, X_{2i} = x_2] \\ &\cdot f_{X_1}(x_1|D_i = vt_1, X_{2i} = x_2) f_{X_2}(x_2|D_i = at_0)dx_1dx_2 \\ &- \int E[Y_i(at_0) - Y_i(nt_0)|X_i = x] f_{X_i}(x|D_i = at_0)dx.\end{aligned}$$

where we change the variables in the vector X_{1i} between the pre- and post-reform period, but maintain the variables in the vector X_{2i} constant at the pre-reform level. Using AST, it is possible to estimate the first (double) integral of the decomposed selection effects in an

[59] Since we apply non-parametric decomposition methods, the single effects of the blocks do not necessarily need to sum up. Therefore, we follow Fortin, Lemieux, and Firpo (2010) and change the blocks one by one. This means we change all variables in one block and maintain the others. Afterwards, we return the variables in this block to their initial values and change another block.

appealing way. One can impose additional constraints in (3). We specify the conditions,

$$\frac{1}{N}\sum_{i=1}^{N}\begin{pmatrix} \dfrac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_{vt_1}(X_j)}\cdot\dfrac{\hat{p}_{vt_1}(X_i)}{\tilde{p}_d(X_i)} \\ \\ \dfrac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_{vt_1}(X_j)}\cdot\dfrac{\hat{p}_{vt_1}(X_i)}{\tilde{p}_d(X_i)}\cdot X_{1i} \\ \\ \dfrac{D_i(d)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_{vt_1}(X_j)}\cdot\dfrac{\hat{p}_{vt_1}(X_i)}{\tilde{p}_d(X_i)}\cdot X_{2i} \end{pmatrix} = \begin{pmatrix} 1 \\ \\ \frac{1}{N}\sum_{i=1}^{N}\dfrac{\hat{p}_{vt_1}(X_i)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_{vt_1}(X_j)}\cdot X_{1i} \\ \\ \frac{1}{N}\sum_{i=1}^{N}\dfrac{\hat{p}_{at_0}(X_i)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_{at_0}(X_j)}\cdot X_{2i} \end{pmatrix},$$

with $d \in \{at_0, nt_0\}$. Using these additional constraints, the decomposed selection effects can be estimated in a similar way as described in Section 4.3. It is possible to manipulate observed characteristics of any moment of the outcomes in analogue ways. DiNardo, Fortin, and Lemieux (1996) suggest similar approaches for conventional IPW estimators.

E Supplementary material

Table 7: Efficient first moments of observed characteristics by program 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	0.498	0.492	1.124	0.433	0.436	0.553
Age	40.177	40.611	5.518	39.686	39.085	7.728
Older than 50 years	0.016	0.029	8.247	0.014	0.022	6.243
No German citizenship	0.064	0.073	3.598	0.064	0.069	1.951
Children under 3 years	0.038	0.036	1.346	0.042	0.044	0.605
Single	0.275	0.227	11.232	0.314	0.245	15.459
Health problems	0.108	0.125	5.072	0.083	0.094	3.913
Sanction	0.007	0.01	3.135	0.006	0.006	0.019
Incapacity (e.g. illness, pregnancy)	0.113	0.117	1.093	0.113	0.101	3.921
Lack of Motivation	0.09	0.089	0.404	0.076	0.079	1.084
Education, Occupation and Sector						
No schooling degree	0.046	0.059	5.525	0.037	0.044	3.797
Schooling degree without Abitur	0.357	0.329	6.056	0.341	0.367	5.405
University entry degree (Abitur)	0.152	0.104	14.548	0.247	0.157	22.53
No vocational degree	0.18	0.241	15.002	0.148	0.21	16.027
Academic degree	0.06	0.038	10.158	0.132	0.064	23.109
White-collar	0.408	0.511	20.726	0.355	0.493	28.191
Elementary occupation	0.057	0.101	16.505	0.051	0.086	14.146
Skilled agriculture and fishery workers	0.008	0.017	8.116	0.007	0.011	4.166
Craft, machine operators and related	0.318	0.359	8.751	0.279	0.368	19.135
Clerks	0.314	0.235	17.816	0.29	0.216	16.903
Technicians and associate professionals	0.149	0.112	11.099	0.177	0.126	14.443
Professionals and managers	0.064	0.049	6.597	0.121	0.08	13.66
Employment and Welfare History						
Half months employed in the last 24 months	45.771	44.212	22.33	46.101	44.484	24.662
Half months unemployed in the last 24 months	0.316	0.655	18.297	0.288	0.605	17.92
Time since last unemployment in the last 24 months (half-months)	46.879	45.143	28.477	46.988	45.441	26.856
No unemployment in last 24 months	0.925	0.864	20.245	0.925	0.87	18.083
Unemployed 24 months before	0.03	0.053	11.589	0.027	0.049	11.406
# unemployment spells in the last 24 months	0.098	0.186	19.098	0.093	0.174	18.367
Any program in last 24 months	0.042	0.062	8.936	0.041	0.06	8.462
Time of last out of labor force in last 24 months	45.882	44.685	16.661	46.16	44.888	18.585
Remaining unemployment insurance claim	24.576	22.064	21.061	26.814	23.124	28.575
Eligibility unemployment benefits	14.214	13.871	5.999	14.145	13.385	14.041
Cumulative employment (last 4 years before Unemployment)	82.379	78.363	17.47	83.133	79.347	17.037
Cumulative earnings (last 4 years before Unemployment)	87,561	75,670	27.548	98,820	81,072	37.398
Cumulative benefits (last 4 years before Unemployment)	2.529	4.06	17.742	2.509	3.679	14.34
Start unemployment spell in January	0.074	0.12	15.633	0.077	0.145	21.847
Start unemployment spell in February	0.067	0.098	11.381	0.078	0.111	11.316
Start unemployment spell in March	0.097	0.09	2.471	0.106	0.103	0.99
Start unemployment spell in April	0.081	0.108	9.563	0.104	0.113	3.127
Start unemployment spell in June	0.067	0.062	1.991	0.059	0.053	2.374
Start unemployment spell in July	0.057	0.077	7.927	0.047	0.047	0.193
Start unemployment spell in August	0.082	0.076	2.405	0.052	0.068	6.799
Start unemployment spell in September	0.124	0.09	10.999	0.138	0.079	18.954
Start unemployment spell in October	0.106	0.077	10.232	0.128	0.083	14.901
Start unemployment spell in November	0.108	0.066	14.845	0.097	0.056	15.161
Start unemployment spell in December	0.065	0.055	3.944	0.044	0.044	0.177
Elapsed unemployment duration	5.324	5.153	5.191	5.553	4.69	26.139
State of Residence						
Baden-Württemberg	0.059	0.062	1.42	0.049	0.04	4.394
Bavaria	0.117	0.112	1.557	0.102	0.095	2.091
Berlin, Brandenburg	0.017	0.025	4.99	0.073	0.063	4.081
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.011	0.04	18.638	0.073	0.132	19.689
Hesse	0.187	0.118	19.296	0.254	0.184	16.913
Northrhine-Westphalia	0.015	0.01	4.326	0.004	0.006	3.621
Rhineland Palatinate, Saarland	0.241	0.227	3.081	0.234	0.151	21.215
Saxony-Anhalt, Saxony, Thuringia	0.098	0.182	24.408	0.086	0.174	26.449
Regional Characteristics						
Share of employed in the production industry	0.28	0.266	15.397	0.251	0.241	11.807
Share of employed in the construction industry	0.065	0.077	52.69	0.062	0.077	65.237
Share of employed in the trade industry	0.151	0.15	3.036	0.151	0.151	1.834
Share of male unemployed	0.562	0.539	54.838	0.566	0.543	51.952
Share of non-German unemployed	0.139	0.122	20.784	0.148	0.126	24.48
Share of vacant fulltime jobs	0.795	0.8	7.936	0.799	0.798	1.21
Population per km^2	443.786	445.303	0.194	1010.094	875.333	7.726
Unemployment rate	10.796	11.219	8.604	11.762	12.375	11.49

< table continues on next page >

Table 7: < 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	0.427	0.468	8.198	0.529	0.509	4.046
Age	39.19	39.548	4.875	35.646	35.033	9.643
Older than 50 years	0.009	0.021	9.287	0.001	0.001	0.21
No German citizenship	0.046	0.047	0.374	0.09	0.096	2.136
Children under 3 years	0.044	0.035	4.295	0.048	0.049	0.419
Single	0.321	0.261	13.14	0.256	0.233	5.259
Health problems	0.084	0.081	1.226	0.064	0.071	2.712
Sanction	0.004	0.004	0.102	0.011	0.025	10.611
Incapacity (e.g. illness, pregnancy)	0.099	0.1	0.229	0.08	0.062	6.971
Lack of Motivation	0.088	0.074	5.05	0.12	0.127	1.939
Education, Occupation and Sector						
No schooling degree	0.021	0.023	1.085	0.042	0.041	0.904
Schooling degree without Abitur	0.349	0.387	7.883	0.372	0.287	18.099
University entry degree (Abitur)	0.374	0.302	15.217	0.166	0.163	0.747
No vocational degree	0.119	0.118	0.473	0.373	0.413	8.186
Academic degree	0.217	0.149	17.594	0.049	0.033	7.991
White-collar	0.26	0.318	12.767	0.504	0.56	11.383
Elementary occupation	0.05	0.053	1.101	0.103	0.112	3.007
Skilled agriculture and fishery workers	0.006	0.008	2.224	0.016	0.015	1.346
Craft, machine operators and related	0.202	0.243	9.843	0.323	0.359	7.609
Clerks	0.294	0.263	6.925	0.147	0.133	3.902
Technicians and associate professionals	0.182	0.174	2.104	0.111	0.084	9.188
Professionals and managers	0.191	0.161	7.897	0.111	0.103	2.55
Employment and Welfare History						
Half months employed in the last 24 months	45.527	44.639	13.019	44.53	43.937	7.701
Half months unemployed in the last 24 months	0.422	0.509	4.842	0.54	0.621	3.878
Time since last unemployment in the last 24 months (half-months)	46.697	45.697	17.359	46.313	45.642	11.5
No unemployment in last 24 months	0.908	0.885	7.492	0.891	0.873	5.656
Unemployed 24 months before	0.04	0.042	1.111	0.044	0.046	0.915
# unemployment spells in the last 24 months	0.116	0.152	8.357	0.149	0.176	5.418
Any program in last 24 months	0.047	0.067	8.423	0.056	0.057	0.278
Time of last out of labor force in last 24 months	45.836	44.972	12.459	44.957	44.354	7.954
Remaining unemployment insurance claim	27.898	24.776	23.28	21.743	22.287	4.758
Eligibility unemployment benefits	13.454	13.462	0.158	11.728	11.372	10.189
Cumulative employment (last 4 years before Unemployment)	81.015	79.461	6.805	76.576	75.501	4.255
Cumulative earnings (last 4 years before Unemployment)	100,324	86,703	26.45	73,037	70,649	6.051
Cumulative benefits (last 4 years before Unemployment)	3.008	3.196	2.291	3.618	3.608	0.109
Start unemployment spell in January	0.059	0.113	19.387	0.026	0.045	10.456
Start unemployment spell in February	0.064	0.108	15.859	0.057	0.095	14.12
Start unemployment spell in March	0.103	0.116	4.09	0.074	0.083	3.237
Start unemployment spell in April	0.099	0.134	11.051	0.117	0.111	1.961
Start unemployment spell in June	0.066	0.067	0.179	0.05	0.044	2.768
Start unemployment spell in July	0.049	0.036	6.636	0.064	0.078	5.455
Start unemployment spell in August	0.062	0.058	1.735	0.138	0.144	1.538
Start unemployment spell in September	0.141	0.098	13.256	0.2	0.153	12.238
Start unemployment spell in October	0.118	0.076	14.097	0.143	0.133	2.854
Start unemployment spell in November	0.101	0.043	22.514	0.042	0.031	6.214
Start unemployment spell in December	0.058	0.041	7.758	0.027	0.027	0.062
Elapsed unemployment duration	5.052	5.012	1.218	4.093	3.378	19.938
State of Residence						
Baden-Württemberg	0.03	0.031	0.737	0.043	0.056	5.765
Bavaria	0.059	0.067	3.127	0.071	0.133	20.587
Berlin, Brandenburg	0.097	0.095	0.868	0.049	0.04	4.327
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.078	0.12	13.841	0.079	0.062	6.391
Hesse	0.257	0.181	18.575	0.221	0.224	0.68
Northrhine-Westphalia	0.012	0.007	4.669	0.017	0.008	7.815
Rhineland Palatinate, Saarland	0.169	0.135	9.568	0.219	0.238	4.712
Saxony-Anhalt, Saxony, Thuringia	0.14	0.21	18.6	0.121	0.081	13.532
Regional Characteristics						
Share of employed in the production industry	0.228	0.224	5.289	0.248	0.272	26.371
Share of employed in the construction industry	0.065	0.078	52.623	0.066	0.071	23.121
Share of employed in the trade industry	0.148	0.148	0.368	0.15	0.153	14.545
Share of male unemployed	0.564	0.543	45.697	0.563	0.544	44.124
Share of non-German unemployed	0.138	0.121	17.613	0.131	0.147	18.976
Share of vacant fulltime jobs	0.785	0.793	11.462	0.791	0.814	33.162
Population per km^2	1234.151	1128.671	5.113	791.508	702.821	6.44
Unemployment rate	13.589	13.53	1.08	12.452	9.971	50.471

Note: We report the efficient first moments of observed characteristics for the treated sub-samples by program type. Information on individual characteristics refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration and the monthly regional labor market characteristics which refer to the (pseudo) treatment time. Further, we report the standardized differences (SD) between the two treatment groups for each program type. Please find a description of how we measure standardized differences in Appendix B.