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

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Abstract

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. The change in the provision of public sponsored further training resulted from the first part of Germany's largest labor market reforms since World War II. 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 use decomposition methods in order to asses the influences of the assignment mechanisms and selection criteria on the overall return to training. We find no influences of the assignment mechanisms and negative influences of the selection criteria on the effectiveness of further training with respect to employment and earnings 48 months after the intended treatment time. However, the results vary strongly with respect to the time dimension, different labor market characteristics, and types of training.

JEL-Classification: J68, H43, C21

Keywords: Active Labor Market Policies, Treatment Effects Evaluation, Administrative Data, Voucher

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Contents

1 Introduction

The provision of public sponsored further training is a major part of active labor market politics (ALMP) in Germany. Between 2000 and 2002, the expenditures exceeded 20 billion Euros. Although the monetary value of further training was very high during this time period, its reputation among federal institutions and policy makers was poor. 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 and selection criteria.

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 vouchers-like systems to assign public sponsored 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), which 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 participate in further training courses in 2001 or 2002 as well as information on *all* individuals who received a training voucher in 2003 or 2004. We observe precise award and redemption dates for each voucher in the post-reform period. 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 structural and time dependence assumptions. The estimation is based on *Auxiliary-to-Study Tilting* (AST), a novel estimator proposed by Graham, De Xavier Pinto, and Egel (2011). Build 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 that the assignment of training through the voucher-like system has instantaneous positive effects on employment and earnings. These might reflect activation effects. In the medium term we find negative voucher effects, which might be explained by the share of programs with longer durations after the reform. After 48 months, we do not find any significant influences of assignment mechanisms on the return to further training. We find effect heterogeneity with respect to vocational education levels, types of training, and redemption decisions. The stricter selection criteria result in moderate negative effects after 48 months. In order to reveal the driving force behind the selection effects, we apply a non-parametric Blinder-Oaxaca decomposition (Blinder, 1973, Oaxaca, 1973). We find that the reduction in the returns to further training can be mainly explained because caseworkers select individuals with better labor market histories, i.e. longer employment spells and higher earnings before the begin of unemployment. Vocational education and other personnel characteristics have only minor influences on the selection effects. We find no effect heterogeneity with respect to redemption decisions.

Rinne, Uhlendorff, and Zhao (2013) is the most related study. However, there are several differences despite the fact they evaluate the same reform.[1] Most significantly, they use another data set in which they cannot observe voucher awards. Instead the treatment definition after the reform is based on participation in training programs which have been allocated through vouchers. Individuals with unredeemed vouchers are in the control group. In contrast, we observe the award and redemption of vouchers and use different treatment definitions.[2] In their main results, they report insignificant voucher as well as selection effects.[3]

Doerr et al. (2013) estimate the effectiveness of German Training Vouchers after the reform. Their findings suggest a slightly positive effect on employment

[1] Rinne, Uhlendorff, and Zhao (2013) consider further training programs with durations up to 12 months and follow individuals for 18 months after program start. In contrast, we consider further training programs with a program duration up to 36 months and follow each individual over a post treatment period of 48 months. In particular, we include retraining courses with the aim to obtain an occupational degree. Moreover, we focus on different types of effect heterogeneity.

[2] In our sample, 19% of all vouchers are not redeemed.

[3] In general, they find positive voucher and negative selection effects, however, they are in most samples insignificant.

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

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. The parameters of interest, identification, and estimation is presented in Section 3. A detailed data description can be found in Section 4. The results are presented in Section 5. The final section concludes.

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 changed individual condi-

[4] The effectiveness of further training under the conventional assignment mechanisms before the reform was extensively evaluated in a large 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 (2006), Rinne, Schneider, and Uhlendorff (2011), Stephan and Pahnke (2011), and Wunsch and Lechner (2008) beyond 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.

tions (due to health problems for example). The obtained certificates or vocational degrees serve as important signaling device for potential employers, especially in Germany with its specific apprenticeship system. In general, further training mainly comprises two types of programs: long-term training and degree courses. Long-term training courses vary in their planned duration and typically last between three months to one year and are usually conducted as full-time programs. Teaching takes place in class rooms or on-the-job in practice firms. Typical examples of further training schemes are courses on IT based accounting or on customer orientation and sales approach. Degree courses or retraining have a long duration up to three 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. That means unemployed were directly assigned to courses by caseworkers based on subjective measures. As a consequence there was a close cooperation and tight relationships between the employment offices and training providers. This was heavily criticized by federal institutions and various media coverage. Following the discussion in Rinne, Uhlendorff, and Zhao (2013), it seems that 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.[5] It is unclear to which extend 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 refuse to participate in ALMP. Practically,

[5]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.

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.[6]

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 duration of the course. It is to be redeemed within a one-day commuting zone. The validity of training vouchers is at most three months. Under the new regime, unemployed are faced with a high degree of freedom of choice regarding training providers.[7] No sanctions are imposed when a voucher is not redeemed. However, unemployed have to give reasonable explanations for not redeeming vouchers.[8]

Simultaneously with the voucher system, additional 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 profiling or targeting (Eberts, O'Leary, and Wandner, 2002).[9] These rules are applied to decide about awards of vouchers

[6] 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).

[7] 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.

[8] 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 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.

[9] 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

and about objectives, contents, and 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 counseling interviews and potential 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. 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. Simultaneously 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) find 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,

a certain unemployment duration).

Rosholm and Svarer, 2008).[10] 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).

On the supply side, the voucher-like system implements market mechanisms, following the principal ideas of Friedman (1962, 1963). This is likely to intensify the competition between training providers.[11] However, markets work not necessarily appropriate. 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.

Likewise, the influence of the new selection criteria on the overall effectiveness of further training is *a priori* not clear. Potentially caseworkers have accumulated expertise and knowledge about training providers and offered courses, such that they can allocate training programs more effectively than an allocation through statistical treatment rules. Dehejia (2005) demonstrates the potential of assignment decisions to increase individual returns to training. However, recent empirical studies find that caseworkers do not allocate training programs efficiently (Bell and Orr, 2002, Frölich, 2008, Lechner and Smith, 2007, Mitnik, 2009). There are three potential reasons for these findings. First, caseworkers could 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.

[10] 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.

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

Of course, the performance of statistical treatment rules depends critically on the details of the implemented system. In the example of German Training Vouchers, the statistical treatment rules apply only with respect to the award decisions, the objective, content, and duration of potential training courses. Unemployed have the challenge to find the most suitable training providers and courses by themselves.[12] 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 featured 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 increase 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. In 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. [13] 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).

[12] The impacts of the new freedom of choice for unemployed on the return to training is reflected in the voucher effects.

[13] Similar findings are made by Schwerdt, Messer, Woessmann, and Wolter (2012), who exploit voucher-like assignment mechanisms for adult training in a Swiss field experiment.

Obviously the performance of statistical treatment rules could be blurred when 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.[14] For the German Voucher system, the 70%-rule was abolished in 2005 again. The reason was that caseworkers had problems to match this rule. The general intention of an outcome oriented allocation of training vouchers maintained, however.

3 Data Description

We use unique data provided by the Federal Employment Agency of Germany which contain information on *all* individuals in Germany who participate in a training program in 2001 and 2002 or receive 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).[15] 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 Pool Database (*Bewerberangebot*), and the Participants-in-Measures Data (*Maßnahme-Teilnehmer-Gesamtdatenbank*). 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.[16] Thus, we are

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

[15] The IEB is a rich administrative data base and source of the subsamples of data used in all recent years studies evaluating German ALMP.

[16] A more detailed description of the IEB in English can be found on the website of the Research Data Center of the Federal Employment Agency (http://fdz.iab.de/en.aspx). The version of the IEB we use in this project has been supplemented with some personal and regional information

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.[17]

3.1 Treatment Definition

The treatment of interest is the first (intended) participation in further training courses. Under the assignment regime, treatment is defined as the participation in a training course of at least 31 days.[18] Under the voucher regime, we observe the award of training vouchers as well as the participation in training courses thereafter. In light of a non-redemption rate of 19% in our sample, the ignorance of individuals who do not redeem their voucher seems not convincing to us. The increased self-responsibility and freedom of choice could affect the outcomes of program participants, even if they do not redeem their vouchers. Policy makers can only influence the award but not the redemption of vouchers. Therefore, we count every voucher award as treatment irrespective of the redemption decision. In order to make the treatments as much comparable as possible under the two regimes, we account for the fact that the award dates of training vouchers differ in almost all cases from the start dates of training courses. We consider the start dates of training courses in the pre- and post-reform period. For individuals who do not redeem their vouchers we define intended treatment times. They are simulated based on a random draw of course start dates from those individuals who redeem their vouchers. Individuals who found already an employment at their intended treatment time end up in the control group. This is likely to

not available in the standard version.

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

[18] Paul (2009) showed that the occurrence of a dropout is highest within the first weeks of a training course.

happen also in the pre-reform period. This rule affects 386 observations or 0.01% of all individuals awarded with an voucher.

The presented treatment definition is often used in applied work. However, existing concerns are related to the announcement of an intended training assignment or voucher award, which could have an influence on the outcomes *per se*. We exploit our rich data availability and experiment with different treatment definitions at least in the post-reform period. 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 intended treatment. This concern found already a lot attention in the literature.[19] Frederiksson and Johansson (2008) argue that in countries like Germany basically all unemployed would receive ALMP if their unemployment would be 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 after the start 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, without controlling for the elapsed unemployment duration at the treatment time, we would possibly observe individuals with better unobserved labor market characteristics in the control than in the treatment group. This implies to control for the elapsed unemployment duration and opens the question of how to measure this variable in the non-treated subpopulation. We simulate the elapsed unemployment duration for the control group using the distribution of elapsed unemployment durations from the treatment group (similar to e.g. Lechner and Smith, 2007, Lechner and Wunsch, 2013). To guarantee that the treatment definition is equal in the control and treatment sample we consider

[19] As an example, Lechner (2009) discusses sequential causal models and Heckman and Navarro (2007) dynamic discrete choice models in the context of program evaluation studies.

only individuals which are unemployed at their (pseudo) intended treatment time. Applying this procedure allows us to control for the dynamic treatment assignment and avoids the bias that is inevitable if a static evaluation approach would be used (see related discussions in Lechner, Miquel, and Wunsch, 2011, Sianesi, 2004).[20]

3.2 Definition of Evaluation Sample

The evaluation sample is constructed as inflow sample into unemployment. 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.[21] Entering unemployment is defined as the transition from (non-subsidized, non-marginal) employment to non-employment of at least one month plus subsequently (not necessarily immediately) some contact with the employment agency, either through benefit receipt, program participation, or a job search spell.[22] 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.

A graphical illustration of the Sample A is presented in Figure 1. The abscissa indicates the time dimension and the ordinate indicates the elapsed unemploy-

[20] 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.

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

[22] 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.

ment duration. We consider only individuals that became unemployed in 2001 or 2003, respectively. We follow each individual for a maximum of 12 months until the (pseudo) intended treatment takes place. After the (pseudo) intended treatment we follow all individuals for 48 months (we have information up to December 2008).

3.3 Descriptive Statistics

The baseline Sample A includes 240,476 unweighted or 1,232,373 weighted observations. Thereof, 80,047 individuals are directly assigned to a training course and 43,135 are awarded with a voucher during their first 12 twelve months of unemployment. 34,712 individuals redeem their vouchers and we observe 8,423 unredeemed vouchers.[23] We have 62,540 unweighted or 539,428 weighted observations in the control group in the pre-reform period. After the reform we have 54,754 unweighted or 569,763 weighted observations in the control group.

In Table 1 we report sample first moments for observed characteristics. Information on individual characteristics refer to the time of inflow into unemployment, with the exception of the elapsed unemployment duration which refers to the (pseudo) intended 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 for the treated and non-treated subsamples under the assignment

[23] These individuals are in the control group in the sample design of Rinne, Uhlendorff, and Zhao (2013).

regime. In the last three columns we report the standardized differences between the different subsamples and the treatment group under the voucher regime.[24]

Treated individuals are on average younger, healthier, more often single and females 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 voucher, selection, and business cycle effects. Consider a multiple treatment framework as proposed in Imbens (2000) and Lechner (2001). A direct assignment to a training course is indicated by $D_i = at_0$ in the pre-reform period and by $D_i = at_1$ in the post-reform period (a = direct assignment, t = time period 0 or 1). We never observe a direct assignment to a training course in the post-reform period, i.e. we never observe the treatment a in the post-reform period t_1. The intended redemption of a training voucher is indicated by $D_i = vt_0$ in the pre-reform period and by $D_i = vt_1$ in the post-reform period (v = intended

[24] Please find a description of how we measure standardized differences in Appendix B.

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 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 is awarded with 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 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 a training course in the pre-reform period. 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 a training course in this time period. 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 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 subpopulation which is awarded with a voucher would not be awarded in the post-reform period. The parameter γ^{post} is the average effect of being awarded with a training voucher in the post-reform period, for individuals receiving a training voucher. 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 being awarded with vouchers 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 subpopulations of interest, the time periods of treatment, and the assignment mechanisms. These differences correspond to selection, business cycle, and voucher effects, respectively.

As discussed earlier, individuals which are awarded with vouchers after the reform differ in observed characteristics from individuals being directly assigned to training courses before the reform, 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 are awarded with 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 are 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 awarded with training vouchers 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 voucher effect is defined as,

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

where we fix the subpopulation of interest and the time period, but change the type of treatment. The voucher effect is the average post-reform effect of being awarded with a training voucher in contrast to being directly assigned to a training course, for individuals who are awarded with a voucher in the post-reform period.

4.2 Identification Strategy

We apply an identification strategy with multiple stages. Firstly, 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}. Secondly, we make assumptions about the business cycle effects. The assumption we rely on has an analogy to difference-in-difference identification strategies. It allows us to identify γ^{bc1}. Thirdly, we make structural model assumptions in order to identify the voucher effect γ^{v}. Further, we implicitly rule out general equilibrium effects with regard to all parameters.

Assumption 1 *(Conditional Time 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 with a joint influence on the 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 3.3). Biewen, Fitzenberger, Osikominu, and Paul (2013) and Lechner and Wunsch (2013) assess the plausibility of identifying assumptions for the evaluation of German ALMP before the reform. Their findings support the plausibility of Assumption 1 in the context of this study. The reason is that we are able to control for all variables which appear to be important confounders in these type of studies. After the reform, Doerr et al. (2013) estimate the effectiveness of further training under selection on observables and unobservables assumptions. Using the equal suitable and rich data set as we do, they find that selection on unobserved characteristics is not important in the post reform period. Assumption 1 includes also the time dimension. Conditional on X_i, we assumes that individuals who become unemployed in t_0 would have the same expected potential outcome as individuals who become unemployed in t_1 and have the same treatment status, if they would have become unemployed in t_1. This implies that we assume to be able to control for all variables which have joint influences on the expected potential outcomes and the probability to have a specific treatment status in t_0 or t_1. It corresponds to a stronger version of the dynamic conditional independence assumption (Sianesi, 2004). In Section 5.3, we use samples with different calender time periods to assess the plausibility of this assumption.

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 $S_{at_0}^{at_0} \subseteq S_{nt_0}^{at_0}$.

This assumptions 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.[25]

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 $k \in \{d, g\}$. A formal proof of (1) can be found in Appendix C. In the case with $d = g$, the

[25] 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 incidence for support problems.

identification is much simpler. The parameter,

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

can be identified without any assumptions (see discussion in Smith and Todd, 2005).

Accordingly, the pre-reform ATT is identified by,

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

and the post-reform ATT by,

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

from observed data under Assumptions 1 and 2. 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,

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

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^{v} we impose additional assumptions.

Assumption 3 *(Common Trend Assumption).*

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

This assumption requires that the 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 the different types of treatment. We carefully asses the plausibility of this assumption in Section 5.3, using different evaluation samples and additional detailed information on regional labor market characteristics. Under Assumptions 1, 2, and 3, the parameter γ^{bc1} is identified.

Assumption 4 *(Additive Separability).* The reform effect can be separated into selection, business cycle, and voucher effects, such that,

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

is uniquely identified.

Assumption 4 excludes interactions between selection, business cycle and voucher 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 voucher effect. Under Assumptions 1, 2, 3, and 4, the voucher effect, $\gamma^{v} = \gamma^{ba} - \gamma^{s}$, is identified, calculating the difference between the before-after and selection effect.

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

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

This is an *Inverse Probability Weighting* (IPW) estimator. Hirano, Imbens, and Ridder (2003) show that consistency and efficiency of IPW depends 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 (see discussion in Graham, De Xavier Pinto, and Egel, 2011). The efficient estimator of the covariat 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 aim to recover $\hat{F}_g^{eff}$ instead of $\hat{F}_g$ are potentially more efficient. Recently Graham, De Xavier Pinto, and Egel (2011) propose a double robust and locally efficient semiparametric version of IPW, named *Auxiliary-to-Study Tilting* (AST).[26] This estimator balances the efficient first moments of all control variables in each treatment sample exactly.[27] The large sample properties are subject to assumptions about the specification of the propensity score.[28] We employ this estimator in our study.

For AST the propensity score $\hat{p}_g(x)$ is estimated using the probit model, $D_i(g) = \Phi\left(X_i'\hat{\beta} + \varepsilon_i\right)$, where $\Phi(\cdot)$ denotes the normal cumulative distribution function. The vector $\hat{\beta}$ is of dimension $1 \times \dim(X_i)$ and the error term ε_i has

[26] 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 where suggested by Abadie (2005), Hirano and Imbens (2001), and Qin and Zhang (2008).

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

[28] These assumptions imply that the propensity score is correctly specified, strictly increasing in its arguments, differentiable, and is well located within the unit interval.

expectation zero. The conditional treatment probability equals the fitted values $\hat{p}_g(X_i) = \Phi(X_i'\hat{\beta})$. The propensity score $\hat{p}_d(x)$ is replace by $\tilde{p}_d(x)$. The parameter $\tilde{p}_d(x)$ is a method of moments estimate. It is estimated under the following moment conditions,

$$\frac{1}{N}\sum_{i=1}^{N}\begin{pmatrix} \dfrac{D_i(d)}{\dfrac{1}{N}\displaystyle\sum_{j=1}^{N}\hat{p}_g(X_j)} \cdot \dfrac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)} \\ \\ \dfrac{D_i(d)}{\dfrac{1}{N}\displaystyle\sum_{j=1}^{N}\hat{p}_g(X_j)} \cdot \dfrac{\hat{p}_g(X_i)}{\tilde{p}_d(X_i)} \cdot X_i \end{pmatrix} = \begin{pmatrix} 1 \\ \\ \dfrac{1}{N}\displaystyle\sum_{i=1}^{N}\dfrac{\hat{p}_g(X_i)}{\dfrac{1}{N}\displaystyle\sum_{j=1}^{N}\hat{p}_g(X_j)} \cdot X_i \end{pmatrix}, \quad (3)$$

where $\tilde{p}_d(x)$ is specified such that the left and right side of (3) are numerically equivalent. The right parenthesis include 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.[29] We specify $\tilde{p}_d(x) = \Phi(X_i'\tilde{\beta})$ using the similar probit model as before (with $\tilde{\beta}$ being of dimension $1 \times \dim(X_i)$), where the restriction,

$$\frac{1}{N}\sum_{i=1}^{N}\left(\frac{D_i(d)}{\Phi\left(X_i'\tilde{\beta}\right)} - 1\right) \cdot \frac{\hat{p}_g(X_i)}{\frac{1}{N}\sum_{j=1}^{N}\hat{p}_g(X_j)} \cdot X_i = 0,$$

has to hold for all elements in X_i, including a constant term. The expected 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

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

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

The reweighted first moments of all control variables are presented in Table 3. They are exactly balanced between all treatment groups and have standardized differences of zero. The sample first moments for voucher recipients in the post-reform period can be found in column (1) of Table 1, while we report the efficient first moments in columns (1)-(4) of Table 3.

5 Results

5.1 Treatment Effects Before and After the Reform

In Figure 2 we present the effects of further training participation under direct assignment (pre-reform) γ^{pre} and the effects under a voucher regime (post-reform) γ^{post}. Further, we report the before-after reform effects γ^{ba}. The results for the outcome employment can be found in the left panel of Figure 2, in the right panel we present the results for deflated monthly earnings (in Euro). The outcome variables are displayed on the ordinate. On the abscissa we report the time since the intended start of training courses. We report results for each of the 48 months after the intended start dates. Triangles report significant point estimates at the 5%-level. In case we report lines without triangles, the point estimates are not significantly different from zero.

We find negative lock-in effects under both regimes. The lock-in period before the reform is steeper, but has a shorter duration. For both periods we find jumps in the slopes of the treatment effects after 12, 24, and 36 months, which corresponds to typical durations of further training programs. After 48 months the treatment effects on employment and monthly earnings are positive.[30] In both reform periods, we find an increase of 5 ppoints in employment and 100 Euro monthly earnings after 48 months.

[30] 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 employ different estimators.

For the before-after reform effects γ^{ba} we draw ambiguous results on employment and monthly earnings. The effects are positive in the first 10 months after treatment. Thereafter, results evolve to negative effects in the medium run. However, in the long run they appear to be insignificant and fairly zero. In the following, we decompose the before-after reform effects in order to identify the driving forces behind these ambiguous results.

5.2 Selection Effects

One explanation behind the before-after reform effects are the new selection criteria. Potentially γ^{ba} partly results from a different selection of program participants with respect to observed characteristics. In Table 1, we report the sample first moments of all confounding control variables. The treated sample after the reform can be found in column (1) and the treated sample before the reform can be found in column (3). The standardized difference between these two samples can be found in column (6). The largest differences in observed characteristics can be found with respect to the employment history. Treated individuals under the voucher regime have more successful employment histories. The share of post-reform treated with an academic degree and being a white-collar worker is higher. Craft and machine operators have a 14 ppoints lower probability to be treated after the reform. We also find that rich states like Hesse increase their shares of program participants in the post-reform period.

The selection effects γ^{s} are reported in Figure 3. For employment and monthly earnings, these effects are after 10 months negative and decrease even further over time. They are significant in most time periods. The impact of the selection effects suggests 2 ppoints reduction in employment and a reduction of 60 Euros monthly earnings after 48 months. The selection effects alone cannot explain the ambiguous findings for the before-after reform effects.

Before we dig deeper into the driving force behind the before-after reform effects, we investigate the important factors of the selection effects. Therefore, we apply a non-parametric Blinder-Oaxaca decomposition. This decomposition method allows us to change one block of control variables between to pre- and post-reform period. The other control variables remain at the pre-reform level. That means we can simulate the selection effects which would have occurred if the new selection rule would only focus on separate blocks of observed characteristics. This allows us to get an deeper understanding of what type of characteristics explain the selection effects. Please find a detailed description of the applied decomposition method in Appendix D. We apply this method with respect to the block personal characteristics, education, and occupation, the block employment histories, and the block states of residence. The findings are reported in Figure 4.[31] We find that personal characteristics, education, and occupation have no strong influence on the selection effects with regard to employment and monthly earnings. This might be explained by small differences in these control variables or weak influences on the treatment effects. The results suggest that selection with regard to characteristics describing the employment histories are more important. We find strong negative influences of the employment history which evolve over time. The size of the effects account for half of the overall selection effects on employment and for one-third of the overall selection effects on monthly earnings. Finally, we focus on the influences of the state of residence on the selection effects. Results show instantaneous negative effects accounting for one-third of the overall selection effects on monthly earnings. For employment, the selection effects with regard to the state of residence have no clear pattern. The remaining unexplained parts of the selection effects can be related to the timing of inflow into unemployment and the elapsed unemployment duration at program start. These findings are in line with Biewen, Fitzenberger, Osikominu, and Waller (2007), Doerr et al. (2013), and Wunsch and Lechner (2008), who find

[31] In unreported results, we applied the decomposition also to a finer set of blocks. The additional insights are rather limited and do not justify an increase in the complexity of Figure 4.

that individuals with better labor market characteristics profit less from further training.[32]

Finally, we investigate effect heterogeneity of selection effects with respect to redemption decisions. If we find more negative selection effects for unredeemed than for redeemed vouchers, then we would expect that unemployed are more effective than caseworkers in selecting themselves into training courses. It would imply that unemployed with low expected returns to training would have low probabilities to redeem their vouchers. Empirical results are reported in Figure 5. Neither for employment nor for earnings, we find effect heterogeneity with respect to the redemption decision. This is not surprising, because the difference in observed characteristics of individuals redeeming and not redeeming their vouchers is rather small. The results suggest that unemployed and caseworkers achieve an equally (in)efficient selection into training courses.

5.3 Business Cycle Effects

Before we focus on the voucher effects as a possible explanation for the before-after reform effects, we have to asses the plausibility of the common trend assumption (Assumption 3). This assumption is of such a big importance for the identification of the voucher effects, that we require an institutional setting where this assumption is very likely to be valid. 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 reweighted samples in Figure 6. 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.[33] Given these parallel trends, it is likely that these would also hold after 2001 or 2003 in the absence of a

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

[33] The same findings are obtained without reweighting.

treatment, respectively.

Second, we use different sample definitions. An alternative evaluation sample design (Sample B) is presented in Figure 7. Before the reform, we consider individuals which enter their first unemployment in 2002 and are treated within the following twelve months but not later than December 2002. The post-reform evaluation sample is not altered in Samples A and B, in order to make a comparison of results regarding the different samples straightforward. Using this sample 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. On the other hand, Sample A is balanced and Sample B is not. As a consequence not all of the individuals in Sample B have the opportunity to be treated within the first twelve months of their unemployment period.

Third, we use additional information about regional labor market characteristics to show that our findings are not sensitive to these factors. In Table 4 we report the sample first moments and standardized differences of these additional variables for baseline Sample A. We do not find large differences between the first moments of the regional labor market characteristics between the treatment and control groups in the pre- and post-reform period, respectively. However, the differences between the pre- and post-reform period are large. We find more unemployed males and unemployed without German nationality in the post-reform period.

In Table 8 we report the business cycle effects γ^{bc0} for Samples A and B. We report separate results for estimations with and without the additional regional labor market characteristics. For employment, the business cycle effects are insignificant until month 12 after the pseudo intended treatment. Afterwards the business cycle effect is increasing. We find that individuals in the control group have 5 ppoints higher employment probabilities in the post- than in the pre-reform period, between the second and fourth year after the pseudo intended treatment. For monthly earnings, the business cycle effects are insignificant until month 20 after the pseudo intended treatment. After 48 months individuals in the control group earn on average 120 Euro more in the post- than in the pre-reform period. All results are not sensitive to the different sample designs and number of control variables.

These findings suggest that the different sample designs and the additional regional labor market characteristics do not alter the business cycle effects γ^{bc0} strongly. However, Germany was undertaking major reforms on the labor market, in particular in 2005. An improvement in the labor market situation can be found in the business cycle effects γ^{bc0}, in particular in the long run. This could rise concerns about the plausibility of the common trend assumption, even in light of the robustness of our findings. Recently Lechner and Wunsch (2009) show that training programs work more effective when the unemployment rate is high. 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 (Table 4). Nevertheless, if the business cycle effects γ^{bc1} are larger than γ^{bc0}, then the voucher effects γ^{v} would most probably be negatively biased.

5.4 Voucher Effects

The voucher effects γ^{v} is reported in Figure 9. We report results for Samples A and B with and without additional regional control variables. We find significant positive lock-in effects of about 4 ppoints higher employment and 100 Euro higher earnings. Between 12 and 36 months after the intended training start, we find significant negative voucher effects. They exhibit 2-5 ppoints lower employment probability and up-to 70 Euro lower earnings. After 48 months the voucher effects have no clear pattern. They depend critically on the sample definition.

Depending on the sampling design we find zero or slightly positive significant effects. The specifications where we control more carefully for time dependence tend to suggest zero voucher effects. Given the discussion in Section 5.3, it could be that these results are negatively biased. We are conservative and rely on the insignificant results after 48 months.

The short-term voucher effects could reflect an activation effect. If this assertion is true, then we expect to find this activation effect also for unredeemed vouchers. In Figure 10 we report effect heterogeneity for the voucher effects with respect to the redemption decision. We find positive lock-in effects for the redeemed and unredeemed vouchers. However, the lock-in effect is much more pronounced for unredeemed vouchers. This finding supports our prior that the positive lock-in effects result form an activation of unemployed. Individuals awarded with a training voucher increase their job search intensity in the short-run stronger than individuals directly assigned to a training course. In the medium and long run we find negative voucher effects for unredeemed vouchers. This might reflect that individuals who do not redeem their vouchers find faster an job, but these jobs are less persistent. This could potentially result in another unemployment spell. An alternative explanation would be that individuals who do not redeem their voucher receive a second voucher at a later point in time.

The negative medium run effects may result from longer training programs and longer lock-in periods under the voucher regime. In Table 5, we report the frequency of four different types of training before and after the reform. For the definition of the different types of training we follow Lechner, Miquel, and Wunsch (2011). We distinguish between firm practice, short training, long training, and retraining. Firm practice are on-the-job training programs. Short and long training are class room training programs. Short training has a duration of less than 6 months. Retraining might be on-the-job and/or class room training. It has the aim to obtain a complete new occupational degree within the German apprenticeship system. In Figure 11 we report effect heterogeneity of the voucher effects with respect to the type of training. Notice that we only estimate the effects for redeemed vouchers. We find that the negative medium term effects on employment are driven by firm practice, long training, and retraining. On employment, we only find negative medium term effects for firm practice and long training. In the long run, we find that retraining has the strongest positive impact on employment. For the employment outcome, the long run effects are of equal size.

Now we aim to compare our results to the study of Rinne, Uhlendorff, and Zhao (2013). As mentioned in the introduction, they do not observe the award of vouchers and define the treatment in the post-reform period as participation in a training course which has be allocated through the voucher system. Further they look only at programs with a maximum duration of 12 months. We replicate their in Figure 12 by using their treatment definition. We find insignificant voucher effects on employment and monthly earnings until 2 years after the start of training. Accordingly, these results are comparable to Rinne, Uhlendorff, and Zhao (2013). After 48 months, we find an increase in the employment probability by 3 ppoints and 100 Euro higher monthly earnings using their definitions.

Finally, we investigate effect heterogeneity of the voucher effect with respect to the vocational education level. Results are reported in Figure 13. We find that individuals with an academic degree exhibit positive voucher effects after 48 months. For this group we find 8 ppoints higher employment and 200 Euro higher monthly earnings which can be associated with the assignment mechanisms. Doerr et al. (2013) show that low educated individuals profit most from being awarded with a voucher and training in the post-reform period. In contrast, the gain from the new assignment mechanisms is higher for individuals with a better education. One possible explanation is that higher educated individuals match themselves to more appropriate training programs than caseworkers would do. In light of the discussion in Lechner and Smith (2007) and Mitnik (2009), these findings suggest at least high educated unemployed make for themselves a more efficient allocation of training programs than caseworkers.

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. The change in the provision of public sponsored further training resulted from the first part of Germany's largest labor market reform since World War II. In the pre-reform period, unemployed where directly assigned to specific training providers and courses. Under the new regime a voucher system is implemented. Further, new selection criteria should guarantee that only individuals with high employment probabilities participate in further training.

Our results suggest that voucher effects can be classified in three periods. The first period is the activation period. Here we find that the award of an voucher increases the job search intensity more than a direct assignment to a training course. The second period can be defined as the training period. Individuals awarded with a voucher opt to take part in longer training programs which result in longer lock-in periods than shorter courses. We find negative voucher effects in the medium time period after treatment. In the last period voucher effects are in most samples insignificant, even though we find a tendency for positive voucher effects. We find that high educated individuals profit most from the new assignment mechanisms.

For the new selection criteria we find negative effects. The driving force behind the selection effects are the better employment histories of program participants in the post-reform period. Accordingly, program participants after the reform have better employment opportunities even in the absence of training.

The effectiveness of training is lower for this group.

A Alternative Treatment Definitions

As mentioned in Section 3.1, existing concerns about the treatment definition are related with the announcement of an intended assignment to a training course or voucher award. The announcement could have an instantaneous effect 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, including training. 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 how 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 6 supports these findings, 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 are 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 the treatment start dates, enabling a sensitivity analysis 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 Section 3.1. We use the intended redemption dates as treatment times. For the second treatment definition (Treatment 2) we use the dates of voucher awards as treatment time. Results for employment and monthly earnings can be found in Figure 14. There are two distinct features between the results for the two treatment definitions. First, we find steeper lock-in effects. This can be explained by the shorter elapsed unemployment duration at program start using Treatment 2.[34] Individuals with shorter unemployment in the control group have on average better labor market opportunities than individuals in the treatment group. Second, the treatment effects for Treatment 2 are lacking behind the effects for Treatment 1. This can be associated with the fact that Treatment 1 takes place after Treatment 2. We find that the difference between Treatment 2 and 1 is positive and significant. This suggests that studies which use the announcement and not the start of training courses as treatment time, would possibly draw more positive conclusions. However, the difference appears to be not very strong and the general pattern and quality of the conclusions are not affected. For our results it appears important to have the same treatment definition before and after the reform. Otherwise, the before-after reform effect γ^{ba} could be altered by this factor.

[34]Notice that all other control variables are defined at the start of the unemployment spell. Accordingly, they are not affected by the treatment definitions.

B Matching Quality

We assess the matching quality by showing the means of the matched control group for different control variables. The standardized differences are defined by,

$$SD = \frac{\bar{X}_d - \bar{X}_g}{\sqrt{0.5(\sigma^2_{X_d} + \sigma^2_{X_g})}} \cdot 100,$$

where $\bar{X}_k$ is the mean and $\sigma^2_{X_k}$ the variance in the respective treatment group $k \in \{at_0, vt_1, nt_0, nt_1\}$. The before matching standardized differences in the sample first moments are reported in Table 1. The after matching standardized differences in the efficient first moments are exactly zero due to the properties of AST. Therefore, we do not even report the standardized difference in Table 3. This indicates a very good matching quality with regard to the first moments. As discussed in Section 4.3, matching requires a balance between the treatment samples in the entire covariate distributions $\hat{F}_g^{eff}$ and not only in the first moments. Therefore, we additionally apply a balancing test suggested in Smith and Todd (2005).

We also apply a second balancing test following an approach of Smith and Todd (2005). Therefore, we run the regression

$$x_k = \hat{\beta}_0 + \hat{\beta}_1 D_{im} + \hat{\beta}_2 \hat{p}(X_{im}) + \hat{\beta}_3 D_{im} \hat{p}(X_{im}) + \hat{\varepsilon}_{im},$$

where x_k indicates the specific control variable. We perform a joint F-test for the null hypothesis that $\hat{\beta}_1$ and $\hat{\beta}_3$ equal zero. In Table 3 we report the summarized results of the test for each of the twelve treatment times. Overall we run 1,368 regressions whereof the test indicates a rejection of the null hypothesis in only 48 cases. We take the results of the assessment as an indication that the propensity score is well balanced and acceptable for the performance of IPW estimations. Since we control directly for X_{im} in the OLS and IV regressions, it is not necessary to assume that the propensity score is balanced for these estimators.

C Proof of 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. We make a backward application of the law of iterative expectations in equality seven. Finally, we replace the integral by an expectation in equality eight.

□

D Blinder-Oaxaca Decomposition

For the selection effects we apply a non-parametric Blinder-Oaxaca Decomposition. See Fortin, Lemieux, and Firpo (2010) for a recent review of decomposition methods. Our aim is to change one block of variables in the selection effect and remain all other variables at the initial level. Let $X_i = (X_{1i}, X_{2i})$ be a vector of control variables with dimension $1 \times \dim(X)$. Using the notation of Section 4.1, the selection effects can be formalized by,

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

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

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

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 in γ^{ds} in an appealing way., One can impose additional constraints in (3). We specify the conditions,

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

with $d \in \{at_0, nt_0\}$. The second (double) integral in γ^{ds} can be estimated in the conventional way, as described in Section 4.3.

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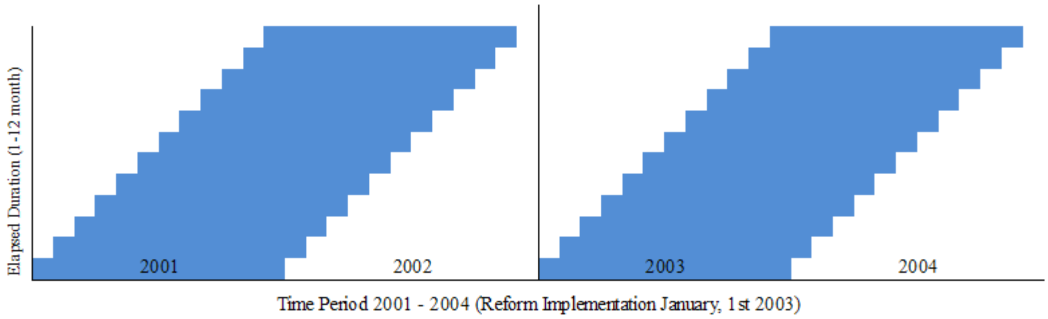
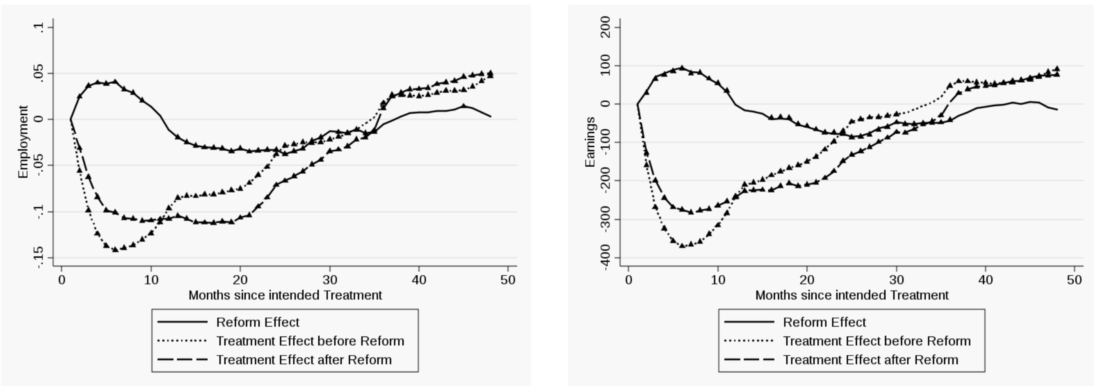
Figure 1: Definition of Evaluation Sample A (Baseline Definition)



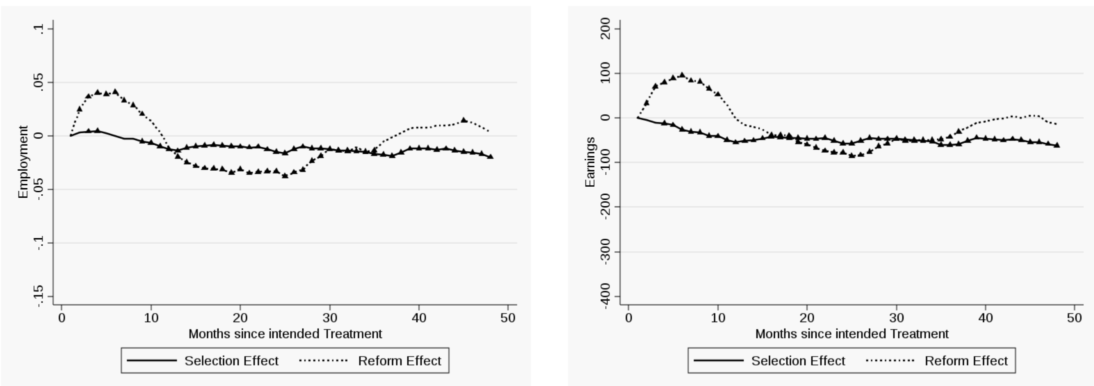
Note:

Figure 2: Overall Reform Effect, Post-Reform and Pre-Reform Treatment Effects on Employment and Earnings.



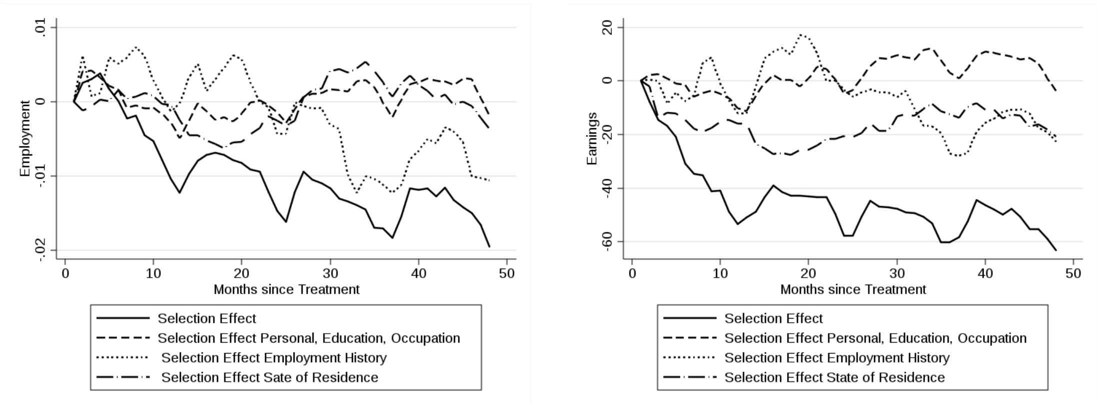
Note: Triangles indicate significance at the 5%-level.

Figure 3: Selection Effect and Overall Reform Effect on Employment and Earnings.



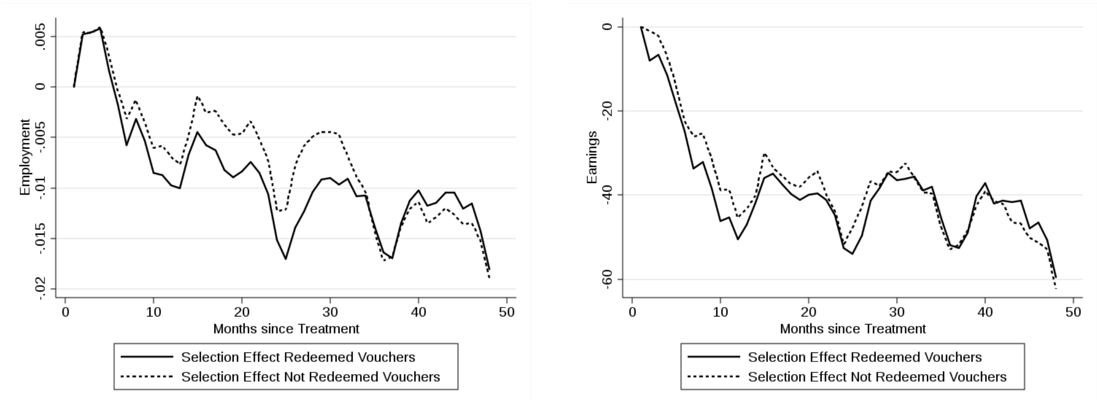
Note: Triangles indicate significance at the 5%-level.

Figure 4: Decomposition of Selection Effects for Employment and Earnings.



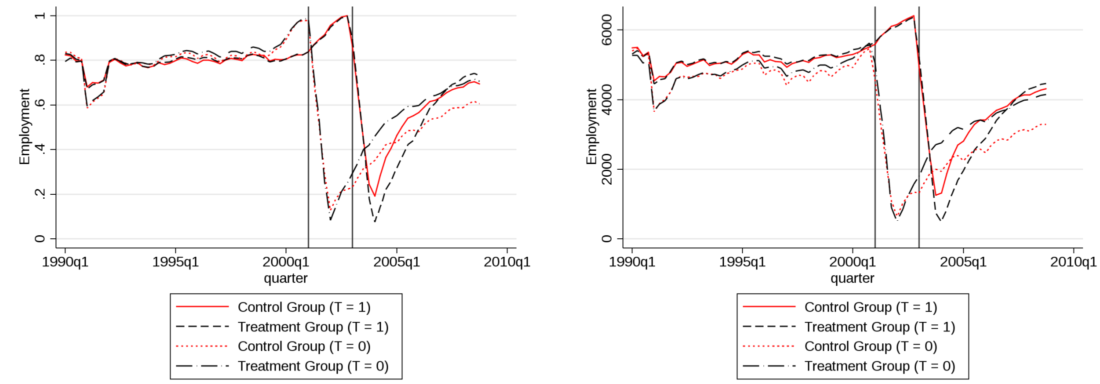
Note: Triangles indicate significance at the 5%-level.

Figure 5: Selection Effect by Redemption Decision on Employment and Earnings.



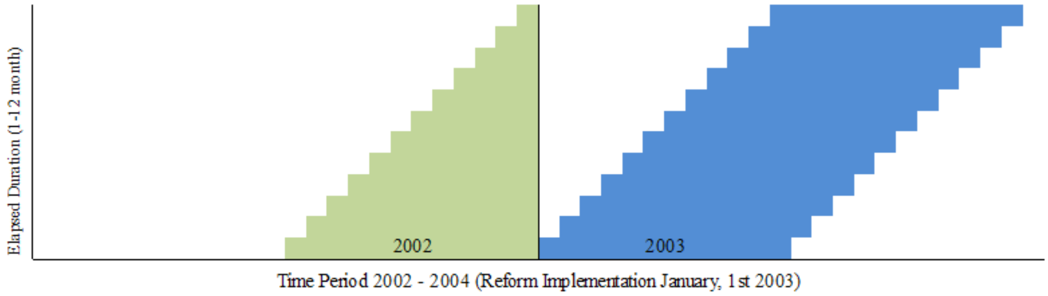
Note: Triangles indicate significance at the 5%-level.

Figure 6: Timetrends of Employment and Earnings for different subgroups of individuals for a time period from 1991-2008.



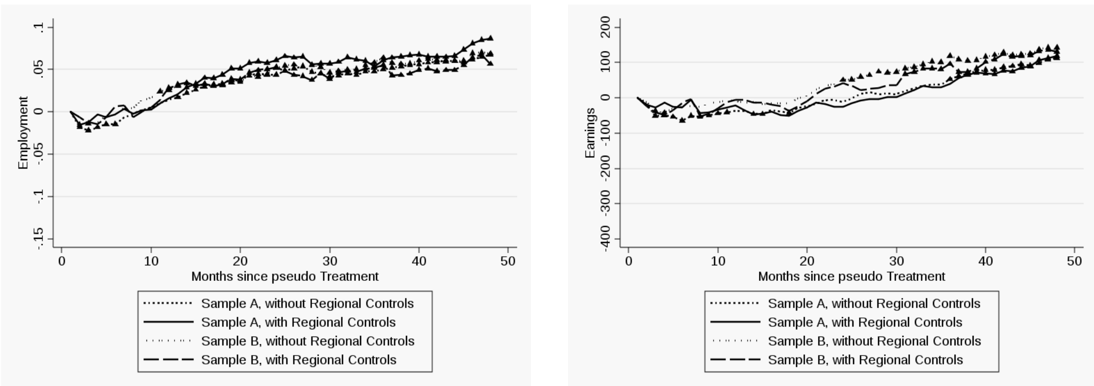
Note:

Figure 7: Definition of Evaluation Sample B (Alternative Definition)



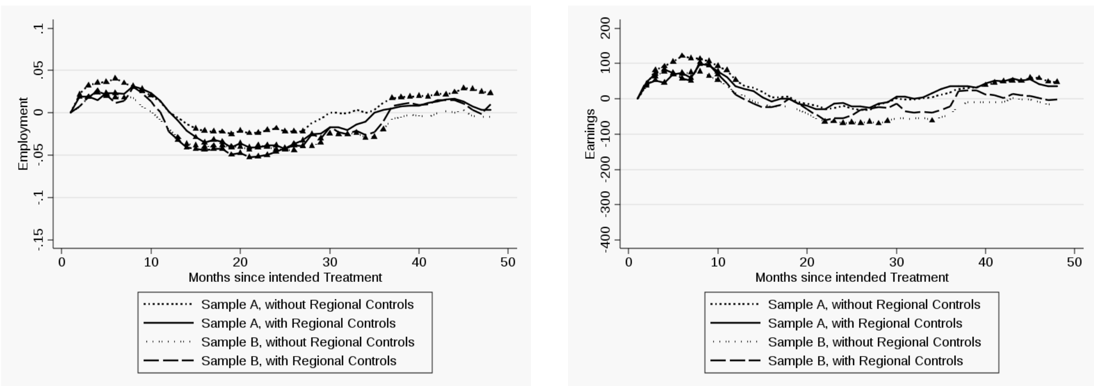
Note:

Figure 8: Business Cycle Effects on Employment and Earnings.



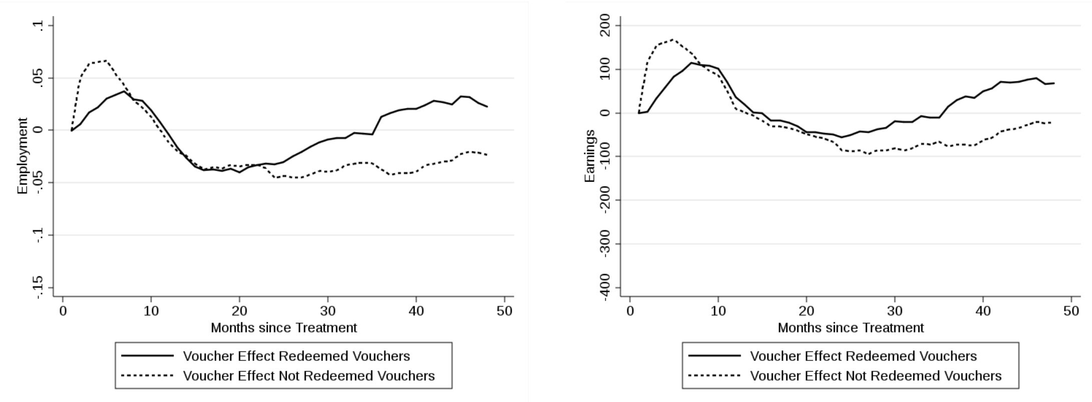
Note: Triangles indicate significance at the 5%-level.

Figure 9: Voucher Effect on Employment and Earnings.



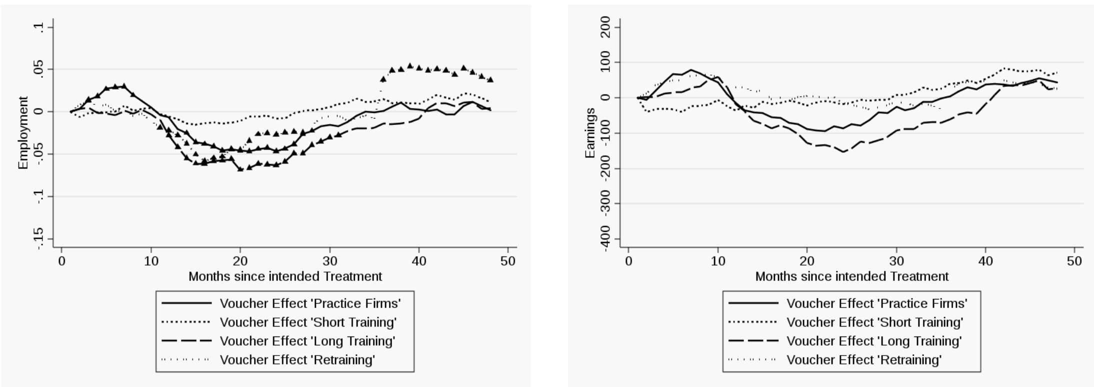
Note: Triangles indicate significance at the 5%-level.

Figure 10: Voucher Effect by Redemption Decision on Employment and Earnings.



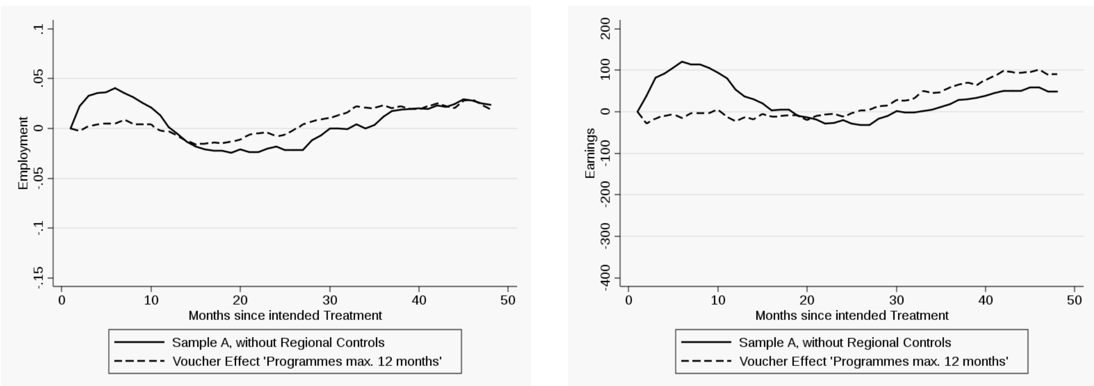
Note: Triangles indicate significance at the 5%-level.

Figure 11: Voucher Effect by Program Type on Employment and Earnings.



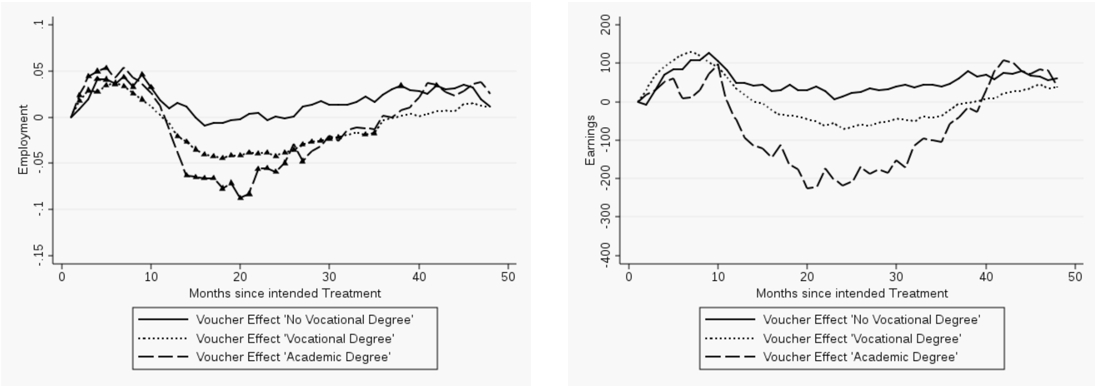
Note: Triangles indicate significance at the 5%-level.

Figure 12: Voucher Effect in Comparison to Effects by Rinne et. al on Employment and Earnings.



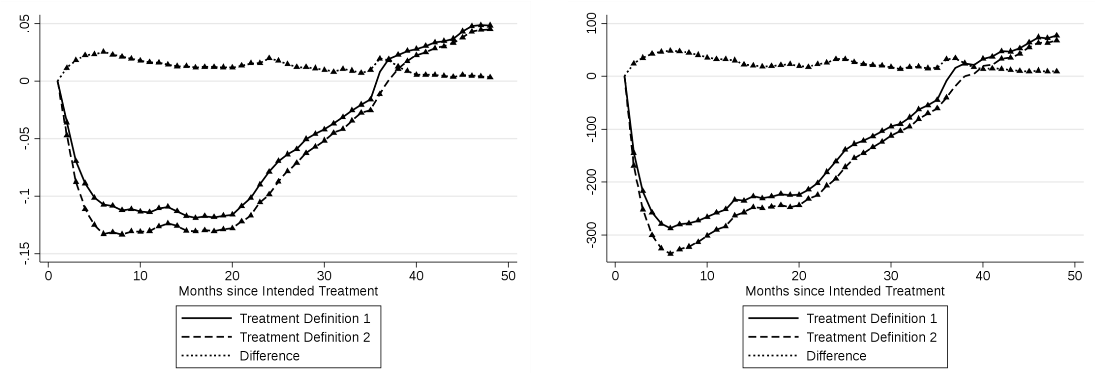
Note: Triangles indicate significance at the 5%-level.

Figure 13: Voucher Effect by Vocational Degree on Employment and Earnings.



Note: Triangles indicate significance at the 5%-level.

Figure 14: Illustration of different Treatment definitions.



Note: Triangles indicate significance at the 5%-level.

Table 1: Sample first moments and standardized differences (SD) for observed characteristics (Baseline Sample)

	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	0.460	0.445	0.448	0.376	2.491	1.947	14.064
Age	38.305	41.168	38.265	41.126	25.608	0.429	25.060
Older than 50 years	0.010	0.112	0.017	0.120	31.720	5.027	33.175
No German citizenship	0.071	0.094	0.069	0.085	6.806	0.505	4.334
Children under 3 years	0.044	0.036	0.042	0.035	3.452	0.791	3.870
Single	0.309	0.274	0.270	0.258	6.404	7.072	9.311
Health problems	0.090	0.144	0.100	0.161	13.204	2.654	16.922
Sanction	0.010	0.009	0.013	0.011	0.866	2.517	0.648
Lack of Motivation	0.102	0.097	0.104	0.099	1.401	0.487	13.972
Incapacity	0.123	0.207	0.119	0.227	18.018	0.926	21.677
Education, Occupation and Sector							
No schooling degree	0.038	0.076	0.043	0.069	12.655	1.999	10.621
Schooling degree without Abitur	0.351	0.275	0.356	0.275	13.647	0.727	13.600
University entry degree (Abitur)	0.232	0.163	0.189	0.120	14.500	8.765	25.363
No vocational degree	0.215	0.237	0.236	0.245	4.264	4.138	5.796
Academic degree	0.113	0.089	0.077	0.052	6.464	10.388	19.441
White-collar	0.395	0.495	0.486	0.597	16.529	15.030	33.728
Elementary occupation	0.070	0.106	0.089	0.114	10.003	5.619	12.081
Skilled agriculture and fishery workers	0.010	0.019	0.015	0.026	5.910	3.701	9.533
Craft, machine operators and related	0.281	0.335	0.341	0.420	9.496	10.416	23.621
Clerks	0.252	0.162	0.206	0.126	18.824	8.964	27.921
Technicans and associate professionals	0.156	0.121	0.123	0.093	8.311	7.890	16.319
Professionals and managers	0.122	0.103	0.101	0.078	4.964	5.559	12.248
Employment and Welfare History							
Half months employed in the last 24 months	44.721	43.551	42.847	41.392	12.523	19.789	34.035
# employment spells in the last 24 months	1.100	1.157	1.235	1.379	12.872	24.953	42.403
Half months unemployed in the last 24 months	0.513	0.562	1.033	1.277	2.018	18.353	25.551
Time since last unemployment in the last 24 months (half-months)	46.113	44.922	43.294	40.362	14.708	28.283	48.045
No unemployment in last 24 months	0.886	0.883	0.772	0.724	0.669	23.887	32.303
Unemployed 24 months before	0.059	0.083	0.092	0.122	7.406	9.810	17.053
# unemployment spells in the last 24 months	0.148	0.150	0.319	0.398	0.443	22.834	30.491
Any program in last 24 months	0.066	0.065	0.088	0.078	0.554	6.510	3.599
Time of last out of labor force in last 24 months	44.905	43.065	43.623	41.506	16.388	12.328	27.088
Remaining unemployment insurance claim	24.970	19.710	23.211	21.544	30.586	11.656	21.346
Eligibility unemployment benefits	12.982	14.026	12.181	12.987	13.871	12.473	0.070
Cumulative employment (last 4 years before Unemployment)	78.554	76.018	75.503	74.985	8.789	10.468	12.869
Cumulative earnings (last 4 years before Unemployment)	87749	78962	75496	74645	15.018	22.093	23.828
Cumulative benefits (last 4 years before Unemployment)	3.674	4.523	5.359	6.368	7.068	13.738	20.798
Elapsed unemployment duration	4.928	3.698	4.674	3.479	30.359	6.010	35.988
Start unemployment spell in January	0.070	0.101	0.110	0.098	8.784	11.192	8.018
Start unemployment spell in February	0.071	0.088	0.104	0.091	4.838	9.194	5.739
Start unemployment spell in March	0.093	0.088	0.100	0.081	1.267	2.052	3.347
Start unemployment spell in April	0.099	0.088	0.118	0.081	3.071	4.817	5.129
Start unemployment spell in June	0.063	0.080	0.058	0.077	5.078	1.757	4.208
Start unemployment spell in July	0.059	0.085	0.054	0.076	7.916	1.896	5.407
Start unemployment spell in August	0.082	0.078	0.083	0.074	0.981	0.250	2.304
Start unemployment spell in September	0.139	0.076	0.103	0.079	17.663	9.330	16.481
Start unemployment spell in October	0.117	0.076	0.089	0.079	11.846	7.756	10.963
Start unemployment spell in November	0.084	0.079	0.049	0.085	1.493	12.021	0.264
Start unemployment spell in December	0.050	0.077	0.041	0.101	8.766	3.432	15.169
State of Residence							
Baden-Württemberg	0.047	0.043	0.044	0.038	1.758	1.165	3.777
Bavaria	0.093	0.110	0.093	0.086	4.636	0.108	1.993
Berlin, Brandenburg	0.077	0.062	0.065	0.065	4.812	3.880	3.869
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.067	0.076	0.097	0.088	2.635	8.633	6.052
Hesse	0.229	0.210	0.175	0.189	3.833	11.129	8.274
Northrhine-Westphalia	0.010	0.008	0.008	0.007	2.036	1.740	3.155
Rhineland Palatinate, Saarland	0.214	0.203	0.173	0.187	2.250	8.632	5.675
Saxony-Anhalt, Saxony, Thuringia	0.104	0.138	0.176	0.184	8.468	16.504	18.158
# Obs	43,135	54,754	80,047	62,540			
# Wgt. Obs	43,135	569,763	80,047	539,428			

Note:

Table 2: Reweighted efficient first moments and standardized differences balanced to treatment group after reform

	Voucher Regime		Assignment Regime		Standardized Differences between
	Treatment-group	Control-group	Treatment-group	Control group	group (1) and all other groups
	(1)	(2)	(3)	(4)	(5)
Personal Characteristics					
Female	0.461	0.461	0.461	0.461	0.000
Age	38.226	38.226	38.226	38.226	0.000
Older than 50 years	0.010	0.010	0.010	0.010	0.000
No German citizenship	0.071	0.071	0.071	0.071	0.000
Children under 3 years	0.045	0.045	0.045	0.045	0.000
Single	0.308	0.308	0.308	0.308	0.000
Health problems	0.089	0.089	0.089	0.089	0.000
Sanction	0.010	0.010	0.010	0.010	0.000
Lack of Motivation	0.104	0.104	0.104	0.104	0.000
Incapacity	0.122	0.122	0.122	0.122	0.000
Education, Occupation and Sector					
No schooling degree	0.038	0.038	0.038	0.038	0.000
Schooling degree without Abitur	0.351	0.351	0.351	0.351	0.000
University entry degree (Abitur)	0.234	0.234	0.234	0.234	0.000
No vocational degree	0.215	0.215	0.215	0.215	0.000
Academic degree	0.113	0.113	0.113	0.113	0.000
White-collar	0.393	0.393	0.393	0.393	0.000
Elementary occupation	0.070	0.070	0.070	0.070	0.000
Skilled agriculture and fishery workers	0.010	0.010	0.010	0.010	0.000
Craft, machine operators and related	0.280	0.280	0.280	0.280	0.000
Clerks	0.251	0.251	0.251	0.251	0.000
Technicans and associate professionals	0.156	0.156	0.156	0.156	0.000
Professionals and managers	0.124	0.124	0.124	0.124	0.000
Employment and Welfare History					
Half months employed in the last 24 months	44.664	44.664	44.664	44.664	0.000
# employment spells in the last 24 months	1.101	1.101	1.101	1.101	0.000
Half months unemployed in the last 24 months	0.514	0.514	0.514	0.514	0.000
Time since last unemployment in the last 24 months (half months)	46.100	46.100	46.100	46.100	0.000
No unemployment in last 24 months	0.886	0.886	0.886	0.886	0.000
Unemployed 24 months before	0.060	0.060	0.060	0.060	0.000
# unemployment spells in the last 24 months	0.147	0.147	0.147	0.147	0.000
Any program in last 24 months	0.067	0.067	0.067	0.067	0.000
Time of last out of labor force in last 24 months	44.828	44.828	44.828	44.828	0.000
Remaining unemployment insurance claim	24.948	24.948	24.948	24.948	0.000
Eligibility unemployment benefits	12.944	12.944	12.944	12.944	0.000
Cumulative employment (last 4 years before Unemployment)	78.323	78.323	78.323	78.323	0.000
Cumulative earnings (last 4 years before Unemployment)	87540.020	87540.020	87540.020	87540.020	0.000
Cumulative benefits (last 4 years before Unemployment)	3.697	3.697	3.697	3.697	0.000
Elapsed unemployment duration	4.929	4.929	4.929	4.929	0.000
Start unemployment spell in January	0.069	0.069	0.069	0.069	0.000
Start unemployment spell in February	0.071	0.071	0.071	0.071	0.000
Start unemployment spell in March	0.092	0.092	0.092	0.092	0.000
Start unemployment spell in April	0.099	0.099	0.099	0.099	0.000
Start unemployment spell in June	0.064	0.064	0.064	0.064	0.000
Start unemployment spell in July	0.060	0.060	0.060	0.060	0.000
Start unemployment spell in August	0.081	0.081	0.081	0.081	0.000
Start unemployment spell in September	0.140	0.140	0.140	0.140	0.000
Start unemployment spell in October	0.118	0.118	0.118	0.118	0.000
Start unemployment spell in November	0.083	0.083	0.083	0.083	0.000
Start unemployment spell in December	0.051	0.051	0.051	0.051	0.000
State of Residence					
Baden-Württemberg	0.047	0.047	0.047	0.047	0.000
Bavaria	0.092	0.092	0.092	0.092	0.000
Berlin, Brandenburg	0.077	0.077	0.077	0.077	0.000
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.068	0.068	0.068	0.068	0.000
Hesse	0.229	0.229	0.229	0.229	0.000
Northrhine-Westphalia	0.010	0.010	0.010	0.010	0.000
Rhineland Palatinate, Saarland	0.213	0.213	0.213	0.213	0.000
Saxony-Anhalt, Saxony, Thuringia	0.104	0.104	0.104	0.104	0.000

Table 3: Reweighted efficient first moments and standardized differences balanced to treatment group before reform

	Voucher Regime		Assignment Regime		Standardized Differences between
	Treatment-group	Control-group	Treatment-group	Control group	group (1) and all other groups
	(1)	(2)	(3)	(4)	(5)
Personal Characteristics					
Female	.447	.447	.447	.447	0.000
Age	38.215	38.215	38.215	38.215	0.000
Older than 50 years	.017	.017	.017	.017	0.000
No German citizenship	.069	.069	.069	.069	0.000
Children under 3 years	.043	.043	.043	.043	0.000
Single	.271	.271	.271	.271	0.000
Health problems	.099	.099	.099	.099	0.000
Sanction	.014	.014	.014	.014	0.000
Lack of Motivation	.106	.106	.106	.106	0.000
Incapacity	.118	.118	.118	.118	0.000
Education, Occupation and Sector					
No schooling degree	.043	.043	.043	.043	0.000
Schooling degree without Abitur	.355	.355	.355	.355	0.000
University entry degree (Abitur)	.191	.191	.191	.191	0.000
No vocational degree	.236	.236	.236	.236	0.000
Academic degree	.077	.077	.077	.077	0.000
White-collar	.484	.484	.484	.484	0.000
Elementary occupation	.088	.088	.088	.088	0.000
Skilled agriculture and fishery workers	.015	.015	.015	.015	0.000
Craft, machine operators and related	.340	.340	.340	.340	0.000
Clerks	.207	.207	.207	.207	0.000
Technicans and associate professionals	.123	.123	.123	.123	0.000
Professionals and managers	.102	.102	.102	.102	0.000
Employment and Welfare History					
Half months employed in the last 24 months	42.848	42.848	42.848	42.848	0.000
# employment spells in the last 24 months	1.234	1.234	1.234	1.234	0.000
Half months unemployed in the last 24 months	1.034	1.034	1.034	1.034	0.000
Time since last unemployment in the last 24 months (half months)	43.333	43.333	43.333	43.333	0.000
No unemployment in last 24 months	0.772	0.772	0.772	0.772	0.000
Unemployed 24 months before	0.093	0.093	0.093	0.093	0.000
# unemployment spells in the last 24 months	0.319	0.319	0.319	0.319	0.000
Any program in last 24 months	0.089	0.089	0.089	0.089	0.000
Time of last out of labor force in last 24 months	43.638	43.638	43.638	43.638	0.000
Remaining unemployment insurance claim	23.282	23.282	23.282	23.282	0.000
Eligibility unemployment benefits	12.166	12.166	12.166	12.166	0.000
Cumulative employment (last 4 years before Unemployment)	75.416	75.416	75.416	75.416	0.000
Cumulative earnings (last 4 years before Unemployment)	75608.920	75608.920	75608.920	75608.920	0.000
Cumulative benefits (last 4 years before Unemployment)	5.383	5.383	5.383	5.383	0.000
Elapsed unemployment duration	4.687	4.687	4.687	4.687	0.000
Start unemployment spell in January	0.110	0.110	0.110	0.110	0.000
Start unemployment spell in February	0.103	0.103	0.103	0.103	0.000
Start unemployment spell in March	0.100	0.100	0.100	0.100	0.000
Start unemployment spell in April	0.118	0.118	0.118	0.118	0.000
Start unemployment spell in June	0.058	0.058	0.058	0.058	0.000
Start unemployment spell in July	0.054	0.054	0.054	0.054	0.000
Start unemployment spell in August	0.083	0.083	0.083	0.083	0.000
Start unemployment spell in September	0.103	0.103	0.103	0.103	0.000
Start unemployment spell in October	0.090	0.090	0.090	0.090	0.000
Start unemployment spell in November	0.050	0.050	0.050	0.050	0.000
Start unemployment spell in December	0.042	0.042	0.042	0.042	0.000
State of Residence					
Baden-Württemberg	0.044	0.044	0.044	0.044	0.000
Bavaria	0.093	0.093	0.093	0.093	0.000
Berlin, Brandenburg	0.064	0.064	0.064	0.064	0.000
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.097	0.097	0.097	0.097	0.000
Hesse	0.176	0.176	0.176	0.176	0.000
Northrhine-Westphalia	0.008	0.008	0.008	0.008	0.000
Rhineland Palatinate, Saarland	0.174	0.174	0.174	0.174	0.000
Saxony-Anhalt, Saxony, Thuringia	0.175	0.175	0.175	0.175	0.000

Table 4: Sample first moments and standardized differences (SD) for regional characteristics (Baseline Sample)

	Voucher Regime		Assignment Regime				
	Treatment-group (1)	Control-group (2)	Treatment-group (3)	Control group (4)	Standardized Differences between (1) and (2)	(1) and (3)	(1) and (4)
Regional Characteristics							
Production	0.249	0.245	0.244	0.243	3.618	4.322	5.158
Construction	0.064	0.065	0.076	0.078	4.739	43.381	46.041
Trade	0.150	0.150	0.150	0.150	1.173	0.574	2.212
Share of male unemployed	0.565	0.563	0.543	0.540	3.780	41.455	45.748
Share of non-German unemployed	0.142	0.140	0.126	0.127	1.457	14.290	14.195
Share of vacant fulltime jobs	0.794	0.794	0.799	0.798	0.192	6.137	5.263
Population per km^2	975.523	895.964	867.042	855.324	3.820	5.129	5.744
Unemployment rate	12.151	12.363	12.209	11.965	3.288	0.863	2.795

Note:

Table 5: Types of Training

Type of Training	Post-Reform			Pre-Reform		
	# Obs	Percent	Average Duration	# Obs	Percent	Average Duration
Practice Firms	4,428	10%	146 days	13,788	17%	178 days
Short Training	14,635	34%	113 days	22,163	28%	101 days
Long Training	6,147	14%	278 days	26,234	33%	306 days
Retraining	8,810	20%	756 day	16,113	20%	700 days
Others	9,115	21%	-	1,749	2%	361 days

Note: The category "Others" includes also unredeemed vouchers in the post-reform period.