

Paul, Marie; Dörr, Annabelle; Fitzenberger, Bernd; Kruppe, Thomas; Strittmatter, Anthony

Conference Paper

The Award of a Training Voucher and Labor Market Outcomes

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2013: Wettbewerbspolitik und Regulierung in einer globalen Wirtschaftsordnung - Session: Training and Apprenticeship, No. D07-V3

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Paul, Marie; Dörr, Annabelle; Fitzenberger, Bernd; Kruppe, Thomas; Strittmatter, Anthony (2013) : The Award of a Training Voucher and Labor Market Outcomes, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2013: Wettbewerbspolitik und Regulierung in einer globalen Wirtschaftsordnung - Session: Training and Apprenticeship, No. D07-V3, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at:

<https://hdl.handle.net/10419/79918>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

The Award of a Training Voucher and Labor Market Outcomes

Annabelle Doerr
University of Freiburg
IAB, Nuremberg

Bernd Fitzenberger
University of Freiburg
IFS, IZA, ZEW

Thomas Kruppe
IAB, Nuremberg

Marie Paul
University of Duisburg-Essen
RGS Econ

Anthony Strittmatter
University of St. Gallen
University of Freiburg

May 2, 2013

Preliminary!

Abstract

This paper estimates the labor market effects of being awarded with a training voucher. In Germany all public sponsored further training programs are allocated through vouchers and the system, we study here, thus represents a major case of the use of vouchers in the context of active labor market policies. Our study is based on process generated data in which we observe all training vouchers awarded in 2003 and 2004 as well as realized training participation. Results suggest that on average voucher recipients suffer from strong lock-in effects and only experience at best small positive employment effects and no earning gains four years after the award of the voucher. Subgroups of treated individuals, like individuals not holding a vocational degree and those participating in a degree program, benefit more.

JEL-Classification: J68, H43, C21

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

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

Contents

1	Introduction	1
2	Institutional Background	6
3	Data Description	8
4	Identification and Estimation	11
5	Results	15
5.1	Average Treatment Effects	15
5.2	Heterogenous Effects by Skill Level	18
5.3	Heterogeneous Effects by Type of Training	22
5.4	The Effect of Unredeemed Vouchers	24
6	Conclusions	25
A	Instrumental Variable Approach	31
B	Averaging across Starting Dates	33
C	Matching Quality	34

1 Introduction

The provision of further training as part of a countries active labor market policies (ALMP) is widespread.¹ Usually these programs are assigned by a caseworker, but the use of vouchers is an alternative to allocate further training programs. The most obvious advantage of vouchers is their potential to implement market mechanisms into the provision of public sponsored services through consumer choice and self responsibility. If these mechanisms unfold when using vouchers to allocate further training for the unemployed and if training vouchers lead to an improvement of labor market outcomes of the recipients is an open question. In this paper we estimate the causal effect of being awarded with a training voucher on future labor market outcomes.

The largest scale programs using vouchers-like systems to assign further training as part of a countries ALMP are probably the Adult and Dislocated Worker Program under the Workforce Investment Act (WIA) in the US and the German Training Vouchers - the latter is the program we study here. In Germany, since January 2003 all public sponsored further training programs provided by the Public Employment Service are allocated through vouchers. This represents a major case of voucher use, in 2003 for example, the Federal Employment Agency and the German Federation spent more than 6.5 billion euros for long and intensive training programs provided through training vouchers.

A caseworker in a local employment office may award a voucher to an unemployed individual, if he or she judges training to be necessary to reintegrate the unemployed into the labor market and if he or she predicts the probability of successful reintegration to be at least 70%. The voucher recipient may then choose a course offered by an approved provider which fits the course target and duration denoted on the voucher.² These rules are comparable to other voucher-like systems. Most customers in the American WIA program receiving training are allocated through a so called Individual Training Account (ITA). Similar to the German Training Vouchers, the ITA recipients may only choose from a list of

¹See Card and Weber (2010) for a meta-analysis of the effects of ALMP in many countries.

²For details on the German Training Vouchers see Schneider, Brenke, Jesske, Kaiser, Rinne, Schneider, Steinwede, and Uhlendorff (2007).

approved providers and redemption may be restricted to specific target occupations and cost and time limits may apply. Under the WIA, guidance by the local caseworkers is allowed and used to different degrees (Barnow, 2009, Heinrich, Mueser, Troske, Jeon, and Kahvecioglu, 2010, King and Barnow, 2011). In Germany direct guidance by the caseworker is not allowed with regard to the choice of the provider, but the content of the course is decided for by the caseworker as a result of a counselling interview and denoted on the voucher.

The present study is, to the best of our knowledge, the first econometric evaluation estimating the effect of *being awarded* with a training voucher as part of ALMP.³⁴ This is an interesting policy parameter, because under a system using vouchers the caseworker decides if a voucher is awarded but cannot directly influence training participation. The paper closest to ours is Rinne, Uhlendorff, and Zhao (2008). They study the effect of the 2003 reform in Germany, as part of which on the one hand the training vouchers have been introduced and on the other hand the selection rule which asks the caseworkers only to award a training voucher if they predict the future employment probability of the unemployed to be at least 70% have been implemented. Using a matching approach, Rinne, Uhlendorff, and Zhao (2008) find positive effects of participating in a training program after the reform on employment and earnings 1.5 years after program start. The effect of the introduction of vouchers is found to be slightly positive, while the introduction of the selection rule has no effect. Rinne, Uhlendorff, and Zhao (2008) do not observe the award of the voucher itself and thus define the treatment as participating in a training program which has been allocated through

³Education vouchers are not only used in the context of ALMP, but also to enhance training of employees, see Görlitz (2010) for a recent evaluation of such training vouchers in Germany. Traditionally education vouchers are mostly used in the schooling system. The literature on school vouchers is large and dates back to Friedman (1962, 1963). See Ladd (2002) for a review of the literature on school vouchers.

⁴There is a large number of studies analyzing the effect of public sponsored training for the unemployment. For papers focussing on Germany, see for example: Bernhard and Kruppe (2012), Biewen, Fitzenberger, Osikominu, and Paul (2012), Fitzenberger, Osikominu, and Völter (2008), Fitzenberger and Völter (2007), Fitzenberger, Osikominu, and Paul (2010), Hujer, Thomsen, and Zeiss (2006), Lechner, Miquel, and Wunsch (2009, 2007), Lechner and Wunsch (2006), Rinne, Schneider, and Uhlendorff (2011), Stephan and Pahnke (2011), and Wunsch and Lechner (2008). Most of these papers focus on the time period before vouchers have been introduced. With regard to employment and earnings effects of further training the evidence is mixed.

a voucher. In contrast to directly estimating the effect of being awarded with a voucher, in this approach individuals with unredeemed vouchers are in the control group and not in the treatment group. Defining the start of a training program and not the voucher as the treatment furthermore requires different assumptions to identify a causal effect. First, the researcher must account for the selection into receiving a voucher and into starting a training program. Second, using the start of the training program and not the time at which the voucher is awarded as the time in which treatment occurs may challenge no-anticipation assumption usually required in a dynamic setting.

Heinrich, Mueser, Troske, Jeon, and Kahvecioglu (2010) and Heinrich, Mueser, Troske, Jeon, and Kahvecioglu (2011) present a large scale econometric evaluation of the services provided by the Adult and Dislocated Worker Program under the WIA in the US. They find positive earnings effects for further training programs allocated through the voucher-like ITA's. Similar to Rinne, Uhlendorff, and Zhao (2008) they define treatment as receiving training. Finally, there are several studies evaluating the use of training vouchers as part of ALMP using experiments to look at particular questions or relying on descriptive evidence. The evidence from these studies is mixed, see Barnow (2009) for an overview.

For our study we use unique process generated data provided by the Federal Employment Agency of Germany. These data contain information on *all* individuals in Germany who receive a training voucher in 2003 or 2004, thus providing a large sample of treated individuals. We can follow the individuals for about four years after voucher receipt. The data include precise award and redemption dates for each voucher, information which has not been previously available for evaluation studies.⁵ It is the availability of this information which allows us to define the treatment as the award of a voucher. For each voucher recipient we merge the voucher data to the individual's data record in the Integrated Employment Biographies (IEB) which contains information on employment outcomes and a rich set of control variables. The control group originates also from the IEB data base and is constructed as a three percent random sample of individuals

⁵Comparable data are used by Kruppe (2009) to investigate redemption probabilities of different subgroups of unemployed individuals.

experiencing unemployment in the respective time period. The data involves a large set of control variables, e.g. the complete employment and welfare history, various socioeconomic characteristics, information on health and disabilities, and variables describing the regional labor market.

We identify the effects of interest using a matching strategy, thus we account for selection based on observable characteristics. For estimation we use inverse probability weighting (IPW) as well as ordinary least squares (OLS) regressions. To avoid the bias that is inevitable if a static evaluation approach is used in a dynamic setting (Frederiksson and Johansson (2008)), we follow Sianesi (2004) and estimate the effect of treatment versus no treatment in each month of elapsed unemployment duration. We also implement an IV approach exploiting variation in the regional conditional allocation intensity, which we argue represent different policy styles of the employment offices.

There are various channels through which the effect of receiving a training voucher may influence future labor market perspectives (see for example Barnow (2000), Barnow (2009), and Hipp and Warner (2008) for a discussion of the potential advantages and disadvantages of using vouchers for the allocation of further training programs). First of all, receiving a training voucher allows the unemployed to take part in an intensive further training program without having to pay for the direct training costs. Participating in such a professional training program may improve after a negative lock-in effect his or her employment chances and earnings if the program enhances his or her productivity. Additionally, there are programs which offer a vocational degree or some other certificate to successful participants which may represent an important signaling device on the labor market - especially in Germany with its apprenticeship system. In comparison to direct assignment of programs by the caseworker (as done before 2003), training may be more effective, because the voucher recipients have the freedom to choose the course provider and the particular program and they may know better than the caseworker which course suits their needs best or in which school, with which schedule or teaching practice they will study most successfully. In contrast, the opposite may also happen because an experienced caseworker may know better

which training provider offers the best program and which course is most suitable for the particular unemployed. Furthermore, the voucher system may unfold positive effects on the providers' side of the market, as one would expect that competition for the potential clients will lead to a positive selection of providers remaining on the market and efficient behavior of the remaining providers.

Apart from the effect through participation in a further training program, vouchers may have additional effects on the employment prospects of those being awarded. Receiving a voucher may have an effect on the aptitude towards services by the employment office. The unemployed may value that a costly service is offered to him or her and increase search effort or participate in a training program with a positive attitude. This effect may be enhanced by the fact that a voucher recipient is not strictly required to redeem a voucher, thus his freedom of choice also involves the decision if he or she starts a program or not. Nevertheless, he or she has to give a reasonable explanation to the caseworker if the voucher is not redeemed before expiration.⁶ On the negative side, some individuals may feel incapable or incompetent to find a suitable course, a feeling which may have a negative effect on motivation. The receipt of a voucher may furthermore act as a threat to the unemployed and enhance job search intensity, as the unemployed may anticipate that the caseworker could, if he or she neither redeems the voucher nor takes up a job, assign him or her to a different - mandatory - program, like a job creation scheme.

Our results suggest that the award of a voucher leads to a strong and very long lock-in effect. Only four years after the award of the voucher we find very small positive employment effects. There are no positive effects on earnings during the observation period. OLS and IPW lead to almost the same results. A comparison to raw employment differences shows that with regard to observables voucher recipients represent a strong positive selection with respect to both outcomes. This may be one explanation for the strong lock-in effect we find. The IV results

⁶Beside 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. if the provider does not feel positive in advance of a client's employment prospects after training he might decide not teach him or her.

unfortunately lead to imprecise estimates. If at all, they suggest that there may also be some positive selection based on unobservables which influences results in particular during the lock-in period.

An investigation of effect heterogeneity by skill group and by type of training shows a more positive picture for subgroups: individuals without a vocational degree are more successful in finding a job after training than higher skilled individuals and the voucher leads to considerable positive long-run effects. Programs leading to a vocational degree are, despite involving very long lock-in effects, more promising than those which do not. The strongest positive effects are found for individuals without a vocational degree participating in degree courses. Finally, descriptive evidence on the redemption decision suggests, that those who do not redeem the voucher do better than comparable individuals who are not awarded with a voucher in the short run, but worse in the long run.

The remainder of the paper is structured as follows: The next section gives a brief overview of the institutional background, followed by the data description. Section 4 discusses identification and estimations. We present our results on the average voucher effect and using alternative methods as well as effect heterogeneity in section 5. The final section concludes.

2 Institutional Background

Since January 2003 all public sponsored further training programs in Germany are allocated through a voucher system. While further training programs have already been an important and expensive part of ALMP in Germany for many years, unemployed individuals were directly assigned to a specific training provider and course by the caseworker before 2003. Due to tight relationships between the employment offices and training providers, the pre-reform assignment process was heavily criticized by federal institutions and various media coverage. As part of the major German labor market reforms, the First Modern Services on the Labor Market Act often called *Hartz I* Reform introduced the voucher system to foster

market mechanisms and transparency of the training market.⁷

Potential training participants are awarded with a training voucher and have free choice in choosing 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 varies, but is at most three months. Further training (*Berufliche Weiterbildung*) mainly comprises two types of programs: long-term training and degree courses. Further training programs are used to adjust the skills of the unemployed to changing requirements of the labor market and possibly to changed individual conditions of employability (due to health problems for example). They aim at improving the human capital and productivity of the participant. The main goal of ALMP in Germany is to reintegrate unemployed individuals into employment. Long-term training courses typically last several months to one year (on average five months in our sample) and are usually conducted as full-time programs. Teaching takes place in class rooms or on the job in training firms. The course curriculum may also include internships. Typical examples of further training schemes are courses on IT based accounting or on customer orientation and sales approach. Degree courses (formally called retraining) last, with a typical duration of two to three years, much longer and 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.

Training vouchers are expected to improve the self-responsibility of training participants and should introduce market mechanisms into the provision of public sponsored training. The first main difference to the old system is that the unemployed have a choice with regard to the course and the provider. This is expected to also change the behavior of the course providers and the selection of those providers which act on the market. 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

⁷For more details on the reform see for example Schneider, Brenke, Jesske, Kaiser, Rinne, Schneider, Steinwede, and Uhlendorff (2007).

time interval. A second difference to the old system is that there are no sanctions imposed when a voucher is not redeemed and he or she gives a reasonable explanation for that, though after redemption training participation is more or less mandatory. This is another potentially important change in the system, as before the reform unemployed who were assigned to a further training program had to participate and in principle risked to be sanctioned if they did not participate. In practice, though, there have been very few sanctions due to not participating in a further training program and it was possible to drop out of the course without being sanctioned by for example indicating that the course was too demanding. The share of dropout was about 20% of those who started participating in a further training program, see Paul (2009). Under the new system, with the alternative not to redeem the voucher, it is to be expected that those unemployed who from the beginning on do not want to participate in further training rather do not redeem the voucher than to start a program and to drop out. This would lead to less dropouts and a longer lock-in effect. Furthermore, the freedom not to redeem the voucher may change the attitude of the unemployed towards this service perceiving it more like an offer and less like an assignment. Further more, due to a special regulation, any participation in training prolongs the period covered by unemployment benefits. Simultaneously with the voucher system a new, additional, selection criteria was implemented. The employment offices are supposed to award vouchers such that, according to their prediction, at least 70% of the voucher recipients will find a job within six months after training.

3 Data Description

This study is based on unique data provided by the Federal Employment Agency of Germany. These data contain information on *all* individuals in Germany who receive a training voucher in 2003 or 2004. The data set is generated from internal process generated data and includes precise award and redemption dates for each voucher - information which has not been previously available for evaluation studies.

For each voucher recipient, we merge the information on training vouchers to the individual's data record in the Integrated Employment Biographies (IEB). The IEB is a rich administrative data base which is the source of the subsamples of data used in all recent years studies evaluating German ALMP. It 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.⁸ Thus, we are able to enrich the information from the voucher data with a large set of personal characteristics and a long labor market history for all voucher recipients.

Our control persons originate from the same data base: A three percent random sample (based on twelve days of birth of the year) of those individuals in Germany who experience at least one switch from employment to non-employment (of at least one month) between 1999 and 2005 has been drawn. When constructing our sample of analysis, we apply the same selection rules for voucher recipients and control persons. We account for the fact that we use a 100% sample of voucher recipients and a 3% sample of non-recipients by using weights in all tables and estimations.

We consider an inflow sample into unemployment consisting of individuals who became unemployed in 2003 or 2004, after having been continuously employed for at least three months. We do not consider individuals who enter unemployment after December 2004, because a next step of the labor market reforms also affecting training was implemented in January 2005. Entering unemployment is defined as the transition from (non-subsidized, non-marginal) employment to

⁸A 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 not available in the standard version.

non-employment of at least one month plus a subsequent (not necessarily immediate) contact with the employment agency, either through benefit receipt, program participation, or a job search spell.⁹ We only consider individuals who are, at the time of inflow into unemployment, eligible for unemployment benefits. This sample choice reflects the main target group for the training vouchers. 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.

We aggregate the spell information in the original data into calendar months. We follow a person in the sample from the month of his or her first inflow into unemployment until the end of 2004 with regard to voucher award and until the end of 2008 with regard to the employment outcome. Information from prior periods is exploited when constructing the covariates referring to the labor market history. The focus is on the first voucher awarded. We distinguish the two outcome states non-subsidized, non-marginal employment (henceforth denoted as employment) and non-employment as alternative states. As an alternative outcome variable we use monthly earnings. The panel data set for the analysis is completed by adding personal, occupational, and regional information. Covariates on individual characteristics refer to the time of inflow into unemployment whereas covariates on regional characteristics are updated each month.

The final sample includes 126,059 unweighted observations, whereof 46,030 individuals are awarded with a voucher during their first twelve months of unemployment and 80,029 observations are in the control group. 38,405 individuals in our sample redeem their vouchers. This results into a redemption rate of 83%. We observe 7,625 vouchers that are awarded but not redeemed.¹⁰

In Tables 1-4 we report the mean values for the most important socioeconomic and labor market characteristics of the individuals in the evaluation sample. In

⁹Subsidized employment refers to employment in the context of an ALMP. Marginal employment refers to employment of a few hours per week only, this is due to specific social security regulations in Germany.

¹⁰These individuals would be in the control group if we used the sample design of Rinne, Uhlen-dorff, and Zhao (2008).

the first two columns of each table we show the mean value of the respective control variable in the treatment and in the control subsample. In column six and seven we distinguish between those who redeem the voucher and those who do not. Individuals receiving a training voucher are on average more often middle-aged, single or single-parent and females than individuals in the control group. They have less problems with their health at the beginning of the unemployment period and also in the years before unemployment. Individuals who redeem the training voucher and thus participate in a training course are on average slightly older and more healthy than individuals who do not redeem their voucher. Additionally, the fraction of individuals with children living in the same household is somewhat higher and the children are on average older than the children of individuals letting an awarded voucher expire.

Individuals being awarded with a training voucher hold on average a higher schooling degree. Furthermore, they tend to have more successful employment histories in the past 7 years, in particular they had higher earnings. The share of individuals with stable employment and no participation in an active labor market program in the past is remarkably higher in the treatment group, already suggesting a strong positive selection of the treated. We have also information about potential placement handicaps of the unemployed, e.g. indirect information about past psycho-social or drug problems, lack of motivation, received sanction from the caseworker or past incapacities due to illness, pregnancy or child care. Persons receiving a training voucher are less likely to have such problems. The fraction of people with motivation deficits or past incapacities is even lower for individuals who redeem the voucher.

4 Identification and Estimation

We consider vouchers that are awarded during the first twelve months of unemployment in the first unemployment spell between January 2003 and December 2004.¹¹ This ensures that we can follow each unemployed for at least 48

¹¹After an unemployment duration of twelve months only very few individuals are awarded with a voucher.

months. We denote the indicator for a voucher award as an intention to treat by $D_{im} \in \{0, 1\}$ (with individuals $i = 1, \dots, N$ and $m = 1, \dots, 12$ indicating the elapsed unemployment duration at the time when the voucher is awarded in months). The outcome variable is denoted by Y_{imt} (where $t = 1, \dots, 48$ indicates the months since the voucher is awarded). We consider employment and monthly earnings as outcome variables, and we estimate the effect of the voucher award (not the actual training participation). To avoid the bias that is inevitable if a static evaluation approach is used in a dynamic setting (Frederiksson and Johansson (2008)), we follow Sianesi (2004) and estimate the effect of treatment versus no treatment in each month of elapsed unemployment duration. In the results section, we report a weighted average of the twelve dynamic treatment effects (see Appendix B for details).

Following the framework of Rubin (1974), the potential outcomes are indicated by $Y_{imt}(d)$, where $d = 1$ under treatment and zero otherwise. For each individual only the realized outcome,

$$Y_{imt} = Y_{imt}(1) \cdot D_{im} + Y_{imt}(0) \cdot (1 - D_{im}),$$

is observed, implying that either $Y_{imt}(0)$ or $Y_{imt}(1)$ is counterfactual.

Our main interest lies in the expected difference between the outcomes Y_{imt}^0 and Y_{imt}^1 for individuals who are awarded with a voucher,

$$\gamma_{mt} = E[Y_{imt}^1 | D_{im} = 1] - E[Y_{imt}^0 | D_{im} = 1].$$

$E[Y_{imt}^1 | D_{im} = 1]$ can be simply observed in the data (Smith and Todd, 2005),

$$E[Y_{imt}^1 | D_{im} = 1] = E \left[\frac{D_{im}}{Pr(D_{im} = 1)} \cdot Y_{imt} \right]. \quad (1)$$

In contrast, $E[Y_{imt}^0 | D_{im} = 1]$ is the counterfactual non-treatment outcome for treated individuals, i.e. the expected labor market outcome for voucher recipients if they would have not been awarded with a voucher. In order to identify this parameter we need to make further assumptions. Under selection on observables

assumptions, it is possible to control for all confounding variables that jointly influence the probability to be awarded with a voucher and the potential outcome, summarized in the vector of pre-treatment variables X_{im} .¹²

Assumption 1 (*Strong Ignorability*).

i) Dynamic mean independence assumption:

$$E[Y_{imt}^0 | D_{im} = 1, X_{im}] = E[Y_{imt}^0 | D_{im} = 0, X_{im}],$$

ii) Common support:

$$p(X_{im}) < 1,$$

holds jointly for all $m = 1, \dots, 12$ and $t = 1, \dots, 48$, where $p(X_{im}) = Pr(D_{im} = 1 | X_{im})$.

Assumption i) implies that after controlling for X_{im} there are no other variables that jointly influence the expected value of Y_{imt}^0 and D_{im} . In this application, there are many factors that simultaneously affect the probability of being awarded with a training voucher and future labor market outcomes, e.g. age, health, family status and employment history. Due to our rich and reliable administrative data set that includes detailed information on the personal and socioeconomic characteristics, information about past employment and welfare histories as well as regional information, we argue, that we observe all relevant confounding factors. There are many matching studies which use the same or very similar administrative data. In particular the studies of Biewen, Fitzenberger, Osikominu, and Paul (2012) and Lechner and Wunsch (2011), which investigate the plausibility of the conditional independence assumption, suggest that our data set is rich enough or contains even more information than necessary to satisfy this assumption. As a robustness check, when we estimate the treatment effect for the whole sample, we also implement an IV approach (see Appendix A for the implementation details).

The common support assumption ii) requires that it is possible to identify for each treated observation at least one non-treated comparison observation that must be comparable in all important characteristics X_{im} .

¹²This assumption is well known as strong ignorability, conditional unconfoundedness, selection on observables or conditional independence (see e.g. Imbens, 2004).

Given Assumption 1,

$$E[Y_{imt}^0 | D_{im} = 1] = E \left[\frac{(1 - D_{im}) \cdot p(X_{im})}{Pr(D_{im} = 1) \cdot (1 - p(X_{im}))} \cdot Y_{imt} \right],$$

is identified from observed data on $(Y_{imt}, D_{im}, X_{im})$ (see e.g. Hirano, Imbens, and Ridder, 2003).

As estimators for the effect of being awarded with a voucher on employment and earnings, we use inverse probability weighting (IPW, Horvitz and Thompson, 1952, Hirano, Imbens, and Ridder, 2003) and ordinary least squares (OLS). In both approaches we perform exact matching on the elapsed unemployment duration and the duration since the voucher is awarded. Thus, we allow even in the parametric estimation for a sustainable amount of flexibility.

Asymptotic results suggest that IPW has some efficiency advantage in comparison to classical matching estimators in large samples (Heckman, Ichimura, and Todd, 1997, Hirano, Imbens, and Ridder, 2003). Moreover, recent simulation studies support this result (Busso, DiNardo, and McCrary, 2009). Concerning the reweighting technique we follow the suggestions of Busso, DiNardo, and McCrary (2009) and use weights that make a small sample correction, even though this study uses a large sample. The average effect for the treated is estimated by

$$\hat{\gamma}_{mt} = \sum_{i=1}^N \frac{D_{im}}{\sum_{i=1}^N D_{im}} \cdot Y_{imt} - \sum_{i=1}^N \frac{(1 - D_{im}) \cdot \frac{\hat{p}(X_{imt})}{1 - \hat{p}(X_{imt})}}{\sum_{i=1}^N \frac{(1 - D_{im}) \cdot \hat{p}(X_{imt})}{1 - \hat{p}(X_{imt})}} \cdot Y_{imt},$$

where $t = 1, \dots, 48$ indicates the time after treatment and $m = 1, \dots, 12$ the elapsed unemployment duration until treatment. The denominator in the second term ensures that all weights sum up to one. The propensity score $p(X_{im})$ is specified in a non-linear probit model. We perform different balancing tests (see Appendix C for details).

IPW received recently also critique. In particular because IPW exhibits fat tails when the treatment probability gets close to one. This is not the case in our approach. However, to show that our results are not driven by specific properties

of the IPW estimator, we use OLS as a second very basic estimation approach. Linear regressions make demanding smoothing assumptions. Even though these assumption do not reflect the true effect heterogeneity, they might be good approximations at least in the average. Angrist and Pischke (2009) suggest that OLS results do not differ significantly from results obtained by more demanding non-parametric or semi-parametric estimators in most applied work. In particular, they argue that OLS finds exactly the conditional expectation function in fully saturated models. Since nearly all of the control variables in this study are binary (excluding the earnings history and regional characteristics), our model is almost saturated. We show that OLS leads to qualitatively and quantitatively similar results as IPW. The linear equation is given by

$$\hat{Y}_{imt} = \hat{\gamma}_{mt} \cdot D_{im} + X'_{im} \cdot \hat{\beta}_{mt},$$

where $\hat{\beta}_{mt}$ is a vector of estimated coefficients for the control variables including a constant. The estimated coefficient of interest is indicated by $\hat{\gamma}_{mt}$.

5 Results

5.1 Average Treatment Effects

In this section we present average effects of being awarded with a voucher on employment and earnings. In the following sections we investigate effect heterogeneity with regard to qualification, different types of training and the redemption decision. The evaluation results are shown in graphs. We report descriptive average differences between the treatment and control group and average treatment effects for treated subpopulations. As explained in more detail in Section 4 and Appendix B, we estimate separately the effect of treatment versus waiting for each of the first twelve months of elapsed unemployment durations. Due to reasons of clarity we only report an average over those effects. Separate results by different elapsed unemployment durations are available upon request. On the time axis in our graphs we depict the months since voucher receipt and on the ordinate

the outcome variable. Triangles and diamonds indicate a significant effect for the according month. In each figure, the results for the labor market outcome employment probability are placed on the left-hand side and those for the labor market outcome monthly pre-tax earnings are placed on the right-hand side.

In Figure 1 we present the average treatment effect obtained by different estimators. First, we show the expected unconditional difference between the treatment and control group (the highest lines). Second, we present the matching results and treatment effects on the treated using ordinary least squares regressions (OLS) (the lines in the middle). The results suggest a very long and pronounced lock-in effect. With a duration of about 40 months for the outcome employment probability and an even longer duration for the outcome earnings, the lock-in effect is much longer than what is typically found in studies evaluating German further training programs, see for example Biewen, Fitzenberger, Osikominu, and Paul (2012) for the pre-reform period and Rinne, Uhlendorff, and Zhao (2008) for the post-reform period. But these studies restrict their sample to participants in long-term training and do not consider the much longer degree courses. Only at the end of our observation period of four years after the voucher was awarded OLS results suggest a very small positive and significant treatment effect (about 1-2 pp) on the employment probability. The effect on earnings remains negative over the whole observation period. The results obtained from using IPW are not depicted in Figure 1, because they are almost the same as those obtained using OLS. This can be seen in Figure 2 which shows the differences between the results of the different estimators. This suggest that the way how we implement the OLS regression is sufficiently flexible to account for effect heterogeneity.

Figure 1 evidently shows that there are jumps in the slopes of the treatment effect after about 12 to 14, 24 to 26, and 36 to 38 months. This can be explained because many programs have a duration of 12, 24 or 36 months and most treated individuals enter training within the first two months after receiving the voucher (see Figure 3). In Figure 4 we show the average employment probability and average earnings for treated individuals under treatment and under non-treatment (using the weights of the IPW estimation). The comparison shows that the

employment probability under non-treatment is higher than under treatment for the first three years after treatment. Not before 40 months after treatment this pattern changes and the outcome for the treated under treatment exceeds the outcome for the treated under non-treatment. We find that the slope of the treated under non-treatment is smooth, while the slope of the treated under treatment shows again the before mentioned jumps.

The descriptive effect in Figure 1 involves a shorter and less pronounced lock-in effect than the OLS results accounting for selection. This suggests that selection based on observables is positive and relatively strong both for the employment and the earnings outcome. As discussed in Section 3 those unemployed who receive a voucher are clearly a positive selection of the unemployed with regard to their labor market chances. Their labor market history is more favorable with less experience of unemployment and higher earnings in the past, they hold higher schooling degrees, suffer less from health problems and less events like sanctions or prior dropouts out of programs are denoted in the data. This positive selection corresponds to the requirement of awarding vouchers only to those unemployed individuals who are expected to have at least a 70% chance entering employment soon after the program. The control group for the descriptive effect has average characteristics and will thus have a lower employment rate than the matched control group (see column 4 in Tables 1 to 4 for the average characteristics of the matched control group). The shorter and less pronounced lock-in effect of the descriptive results also suggests that one reason for the long and deep lock-in effect we find in our main results is the strong positive selection into treatment. Because the treated are unemployment individuals with relatively good labor market chances, many of them would have found a job on the short or medium run in the counterfactual situation.

Figure 1 also shows that the results obtained by IV (see Appendix A for implementation details) are even more negative than the OLS results, though this difference is not significant for most months (see again Figure 2 depicting the differences by estimator). It is a drawback of our IV approach that it leads to imprecise estimation results. Keeping this in mind, still the results seem to

suggest that during the lock-in period there may be positive selection based on unobservables. Thus caseworkers might cream skim not only with regard to observables, but also with regard to those characteristics that are unobservable to the researcher, but observable to the caseworker who is in personal contact with the potential voucher recipients. But at the end of the lock-in period the IV results are close to the OLS results.

Summarizing, the results presented so far imply that a voucher award leads to a strong and very long lock-in effect. Only four years after the voucher award, we find very small positive employment effects. There are no positive effects on earnings within the observation period. Methods based on selection on observables assumptions seem to work equally well. Raw employment differences show that with regard to observables voucher recipients represent a strong positive selection with respect to both outcomes.

5.2 Heterogenous Effects by Skill Level

Given that the average effect over all voucher recipients draws a negative picture on the effectiveness of the vouchers, in this section we investigate if the voucher is more effective for subgroups of the treated population. In the following we present the OLS results for different subgroups of individuals in one graph to make the comparison between them easier. Additionally, we show the IPW results and the descriptive difference between the treatment and control group for all subgroups we are interested in.

We first investigate effect heterogeneity by vocational degrees.¹³ One could fear that low skilled individuals could be overstrained by the voucher system and may not find the best training provider or do not redeem the voucher. Even if they cope well with the course choice, they might be discouraged during participation. On the other hand individuals with low formal training could gain particularly strongly by a major investment into their human capital and also by obtaining a course certificate or even a vocational degree. 22% of the treated in our sample

¹³We have also looked into effect heterogeneity by gender. The effects of the voucher are quite similar for men and women. If at all, women face a little less deep lock-in effect and results are a little more positive at the end of the observation period.

hold no vocational degree (low skilled individuals). 11% of the treated are high skilled, holding an academic degree. The majority of the treated has a vocational degree as their highest degree (medium skilled). The highest line in Figure 5 depicts the effect of being awarded with a voucher for the group of those without a vocational degree. The lock-in effect is about three years long (thus one year shorter than for the whole sample) and four years after the award of the voucher we find a significant positive employment effect of almost 6 pp and a significant positive earnings effect of about €115. In contrast, the effect for the high skilled is strongly negative over the whole observation period and there is also no positive effect of the voucher for those holding a vocational degree (the earnings effect being always negative and the employment effect reaching zero at the end of the observation period).

Can we say more on why only low skilled individuals benefit from being awarded with a voucher? A potential explanation would be that low skilled have a shorter lock-in effect because they had a lower probability to redeem the voucher. In our sample this is not the case: 22.0% of those individuals who redeem the voucher hold no vocational degree and the share is about the same (22.3%) among those who let the voucher expire. Furthermore, the average time spend in a training program (conditional on redeeming the voucher) is 13 months for the low skilled and nine for the high skilled. Thus shorter courses or early dropout may not explain a shorter lock-in effect. Comparing the employment effects of the low skilled with those of the medium skilled in more detail, one can see that in between month eight and month 24 after the voucher award the lines depicting the employment effects run almost parallel, but on a different level. The lock-in effect for the medium skilled is stronger than the one for the low skilled. But from month 25 to month 36 after being awarded with a voucher the lines with the employment effects develop differently. The line for the low skilled increases at a much higher rate. This is the time in which participants of the longer courses complete their courses and should intensively search for jobs. Individuals without a vocational degree much more frequently participate in degree courses (44% as opposed to 22% among the medium skilled). Those voucher recipients who take

part in a degree course spend on average two years in their course. To obtain the degree one needs to attend two to three years, depending on the course. So two years after the award of the voucher those participants who have quickly redeemed their voucher will re-enter the labor market with their new degree and others will follow during the year to come. This is exactly the time period (month 25 to month 36 after voucher award) for which we see in Figure 5 the slope of the line representing the employment effect for the low skilled increasing much more strongly.

Thus, it seems to be the case that there is an important group of low skilled voucher recipients who use the voucher to obtain a vocational degree and then quickly find a job. Results thus suggest that the low skilled do much better in finding a job after completing the course than the medium skilled. This interpretation is supported when looking at Figure 6 which shows the employment rates of the treated and matched controls by skill level. After about 36 months after the voucher award, the treated without vocational degree experience almost the same employment rate as those treated with a vocational or academic degree. The matched controls without vocational degree, on the contrary, always have a much lower employment rate than those with degrees. So the low skilled in fact benefit from the voucher after the time they have completed a training program. Figures 7 to 9 show that the treatment effects by skill group using IPW are of the same size as the effects obtained by using OLS.

So while our results on the average effects suggest a negative picture, vouchers seems to be more of a success for low skilled individuals. This is in contrast to papers evaluating further training in the pre-reform period which find few effect heterogeneity by skill group. Rinne, Schneider, and Uhlendorff (2011) investigate effect heterogeneity for OLS estimates by the three skill groups (with and without a vocational training degree, with an academic degree) for long-term training in the pre-reform period. They find few evidence for heterogeneous effects. Biewen, Fitzenberger, Osikominu, and Paul (2012) use ex-post outcomes regressions after matching to study different forms of effect heterogeneity for the pre-reform period. Generally there is no evidence for effect heterogeneity. There is only one exception

with regard to skill groups: not holding a vocational degree goes along with a little more positive effects of training programs for those males who start their program in month 4 to 6 of their elapsed unemployment duration (see online appendix of Biewen, Fitzenberger, Osikominu, and Paul (2012)). With regard to degree programs, there exists relatively few prior evidence, because to look beyond the lock-in effect of these very long programs one needs an observation period of at least three or four years. A series of studies using data from the 1990th are an exception as they have an extraordinary long period to observe the labor market outcomes of up to eight years. These studies find positive employment effects for the very long retraining program which is similar to the degree courses existing today (see Fitzenberger and Völter (2007), Fitzenberger, Osikominu, and Völter (2008), Lechner, Miquel, and Wunsch (2007), and Lechner, Miquel, and Wunsch (2009)). Lechner, Miquel, and Wunsch (2009) estimate separate effects for those unemployed who hold a vocational degree and those who do not and by gender and find the highest effects for women without a vocational degree. This is in line with our finding. We will have a closer look at the combinations of the skill level and the type of training in the next subsection.

Another reason for the low skilled benefiting more from being awarded with a voucher may be that if someone without a vocational degree is chosen to receive a voucher, he or she may be a well selected member of this skill group. Because of the objective that 70% of the voucher recipients should be employed after their course, caseworkers are demanded to award those with a voucher who will have a good chance to find a job and this requirement is more demanding among the low skilled. But Figures 7 to 9, which also show the descriptive differences of treated and controls by skill group, suggest that selection on observables is only a bit stronger for the low skilled than for those individuals holding degrees. Selection is strong among the medium skilled and the high skilled and only a little stronger for the low skilled.

5.3 Heterogeneous Effects by Type of Training

We now turn to heterogeneous effects with respect to the type of training. Figures 10-13 distinguish between the two types of further training programs: long-term training and degree courses (mostly retraining), see section 2. Very special programs or unredeemed vouchers are not considered here. In the literature on multiple training programs (see for example Biewen, Fitzenberger, Osikominu, and Paul, 2012, and Wunsch and Lechner, 2008) separate effects are estimated for different types of training programs using control persons from the group of non-participants in any program. This works because the type of training is decided for at the same time as the training assignment itself, thus there is no endogenous decision on the type of program at a later point in time. The same argument basically holds also in our case, because the length of the course and the objective of the course are specified on the awarded voucher. Hence, with only a few exceptions, there is no possibility to change the type of course after the award of the voucher. However, beside the possibility that the training provider does not admit a person, individuals may decide not to redeem the voucher. If so, this decision may be endogenous and it may depend on the type of training for which the voucher is suitable. This should be kept in mind, when interpreting the results in Figures 10-13.

Table 1 to 3 show that participants in degree courses are younger, more likely to be female, have experienced more unemployment and lower wages in the recent past than participants in long-term training. Degree courses have typically a very long duration. It is thus not surprising that we find long and very deep lock-in effects of more than 3 years, reducing the employment probability by almost 36 pp and earnings by over 600 Euro per month. But after 48 months the employment effect is 8 pp and earning gains are relatively large with over €105 per month (Figure 10). So vouchers which allow to choose a degree course involve high costs if they are redeemed due to a very long and deep lock-in period, but after three to four years they considerably increase the labor market chances of their recipients. Considering long-term training programs we find a pronounced lock-in period of about 12 months. This lock-in period is comparable to Rinne, Uhlenhoff,

and Zhao (2008). But after this pronounced lock-in period, the estimated effects remain negative for the whole observation period even though the effect size is reduced over time. Our results suggest that vouchers designed for long-term training are not effective, at least for a period of 48 months. In contrast to our results, Rinne, Uhlendorff, and Zhao (2008) find a positive employment effect of about 7 pp at the end of their observation period of 1.5 years after program start. The main differences between their paper and our analysis relate to the evaluation framework, the estimation method, and the sample. Furthermore, in their paper, those unemployed with unredeemed vouchers are members of the control group and in case these persons are assigned high weights in the matching procedure this will lead to a more positive effect, as suggested by the results reported in the following.

Figure 11 and Figure 12 compare the effects for individuals participating in long-term courses and degree courses obtained by different estimators. IPW again suggests almost the same results as OLS. Interestingly, the difference to the descriptive effect is a little stronger for long-term courses than for degree courses suggesting that there is a little stronger cream-skimming (or self-selection when the recipient decides to redeem the voucher) for long-term training. Note that Figure 11 and Figure 12 have a different scaling. The difference in selection can be better seen on the right-hand side of Figure 13. Comparing the characteristics of the control group to the treatment group of the degree courses and to the treatment group of long-term training (last two columns in Table 1 to 3) suggest that the positive selection into voucher award is mainly due to those participants in long-term training.

When discussing the results on effect heterogeneity by skill group, we have suggested that the positive employment effects for the low skilled may result from those low skilled who participate in degree courses. In Table 2 we can see that a much higher share of participants in the degree courses has no vocational degree (36.5%) than in long-term training (15.9%). Now we have seen that degree generally lead to more positive long term effects than long-term courses. So, are the positive effects we see among all the negative effects due to degree courses

working well or due to the low skilled benefiting from vouchers? Figure 14 answers this questions by distinguishing by skill group and by course type. It shows that at least small positive employment effects are reached for all skill groups in degree courses and for the low skilled in long-term training. The highest effect occurs for the low skilled in degree courses. Positive earnings effects are found for the low skilled in both types of courses and for the medium skilled taking degree courses. Thus, the positive effects result from degree courses and from awarding vouchers to those without vocational degrees - and mostly from the combination of both. The main reason for degree programs to be effective in helping the low skilled to find a job is likely to be that especially for them a strong increase in professional skills and a degree may particularly improve their chances on the job market.

5.4 The Effect of Unredeemed Vouchers

As mentioned in the introduction, the award of the voucher may have an effect through allowing the individual to participate in a training program, but it may also have an effect on labor market outcomes itself. Figure 15 shows the labor market outcomes by the redemption decision. The effect is estimated in the usual way by OLS, but it does not allow for a causal interpretation, since the redemption decision itself is endogenous. The redemption decision may depend on the luck of receiving a job offer during the time between voucher award and potential redemption and furthermore individuals who do redeem their voucher may differ systematically from individuals who do not redeem it with regard to unobservables. But even the descriptive comparison allows for some interesting implications.

For individuals who redeem their vouchers we find overall the same pattern as for the unconditional effect. (The IPW estimator again leads to the same results, see Figures 16 and 17.) This is not surprising, because most individuals redeem their voucher (83%) and accordingly this subpopulation gets the largest weight. However, positive as well as negative effects are slightly more pronounced. Individuals who do not redeem their voucher are first better off than the control group of unemployed not being awarded with a voucher. This positive effect may

represent a threat effect, individuals may fear to be assigned to a mandatory active labor market program, like for example a job creation scheme, if they neither redeem the voucher nor leave unemployment. But the positive effect may also be due to those individuals who receive a job offer quickly after the award of the voucher therefore not redeeming it. After five months the effect turns negative. There may be at least three potential reasons for this: first, those who do not redeem the voucher may be participating in other programs, second, the threat effect may lead to negative consequences on the long run and third, those who do not succeed in finding a training course may suffer from a loss of motivation.

6 Conclusions

This paper estimates the effect of being awarded with a training voucher on the employment probability and earnings for the unemployed in Germany. We use process generated data in which we observe all training vouchers awarded in 2003 and 2004 and participation in training in case the voucher is redeemed. We estimate the effects of interest using a flexible specification and an OLS estimator and, alternatively, IPW. As a robustness check we use an instrumental variable strategy. As an instrument we exploit variation in the regional conditional allocation intensity which we argue represents different policy styles of the employment offices.

Our results suggest that the award of a voucher leads to a strong and very long lock-in effect. Only four years after the voucher was awarded, we find very small positive employment effects. There are no positive effects on earnings within the observation period. The two methods based on a selection on observables assumption (IPW and OLS) lead to almost the same results. A comparison to raw employment differences shows that with regard to observables voucher recipients represent a strong positive selection with respect to both outcomes. The IV results unfortunately lead to imprecise estimates. If at all, they suggest that there may also be some positive selection based on unobservables.

An investigation of effect heterogeneity by skill group and by type of training shows a more positive picture for subgroups: individuals without a vocational degree are more successful in finding a job after training than higher skilled individuals and the voucher leads to considerable positive long-run effects. Programs leading to a vocational degree are, despite involving very long lock-in effects, more promising than those which do not. The strongest positive effects are found for individuals without a vocational degree participating in degree courses. Finally, descriptive evidence on the redemption decision suggests, that those who do not redeem the voucher do better than comparable individuals who are not awarded with a voucher in the short run, but worse in the long run.

Overall, we conclude that at least in our period of study (the first two years after vouchers have been introduced) many of the vouchers being awarded have not improved the labor market perspectives of the voucher recipients. The disappointing results is, that even though most recipients use the voucher to participate in training, even in the long run they often do not better as if they had not been awarded with a voucher. But they suffer from a lock-in effect which seems to be particularly pronounced due to the strong positive selection of voucher recipients. An exception to this overall negative picture are voucher recipients who do not hold a vocational degree. They benefit from receiving a voucher as they do well in finding a job after training, in particular if they attend a degree program.

References

- ABADIE, A., J. ANGRIST, AND G. IMBENS (2002): “Instrumental Variables Estimates of the Effect of Subsidized Training on the Quantiles of Trainee Earnings,” *Econometrica*, 70(1), 91–117.
- ANGRIST, J., AND J. PISCHKE (2009): *Mostly Harmless Econometrics*. Princeton University Press.
- BARNOW, B. (2000): “Vouchers for Federal Training Programs,” in *Vouchers and the Provision of Public Services*, ed. by C. Steuerle, V. Doorn Ooms, G. Peterson, and R. Reischauer. Brookings Institution Press, Washington, D.C.

- BARNOW, B. (2009): “Vouchers in US Vocational Training Programs: An Overview of What We have Learned,” *Zeitschrift für ArbeitsmarktForschung*, 42, 71–84.
- BERNHARD, S., AND T. KRUPPE (2012): “Effectiveness of further vocational training in Germany- Empirical findings for persons receiving means-tested unemployment benefit,” *IAB Discussion Paper*, 10.
- BIEWEN, M., B. FITZENBERGER, A. OSIKOMINU, AND M. PAUL (2012): “The Effectiveness of Public Sponsored Training Revisited: The Importance of Data and Methodological Choices,” *University of Zurich, Department of Economics Working Paper No. 91, 2012*.
- BLACK, S. (1999): “Do Better Schools Matter? Parental Valuation of Elementary Education,” *Quarterly Journal of Economics*.
- BUSSO, M., J. DINARDO, AND J. MCCRARY (2009): “New Evidence on the Finite Sample Properties of Propensity Score Matching and Reweighting Estimators,” *IZA Discussion Paper*, 3998.
- CARD, D. (1995): “Using Geographic Variation in College Proximity to Estimate the Return to Schooling,” in *Aspects of Labor Market Behaviour: Essays in Honour of John Vanderkamp*, ed. by L. Christofides, E. Grant, and R. Swidinsky. University of Toronto Press, Toronto.
- CARD, D., K. J., AND A. WEBER (2010): “Active Labour Market Policy Evaluations: A Meta-Analysis,” *The Economics Journal*, 120, F452–F477.
- FITZENBERGER, B., A. OSIKOMINU, AND M. PAUL (2010): “The Heterogeneous Effects of Training Incidence and Duration on Labor Market Transitions,” *IZA Discussion Paper*, 5269.
- FITZENBERGER, B., A. OSIKOMINU, AND R. VÖLTER (2008): “Get Training or Wait? Long Run Employment Effects of Training Programs for the Unemployed in West Germany,” *Annales d’Economie et de Statistique*, 91-92, 321–355.

- FITZENBERGER, B., AND R. VÖLTER (2007): “Long-run Effects of Training Programs for the Unemployed in East Germany,” *Labour Economics*, 14, 730–755.
- FREDERIKSSON, P., AND P. JOHANSSON (2008): “Dynamic Treatment Assignment – The Consequences for Evaluations Using Observational Studies,” *Journal of Business Economics and Statistics*, 26, 435–445.
- FRIEDMAN, M. (1962): *Capitalism and Freedom*. University of Chicago Press, Chicago.
- (1963): “The Role of Government in Education,” in *Economics and the Public Interest*, ed. by R. Solo. Rutgers University Press, New Brunswick.
- FRIEDLICH, M., AND M. LECHNER (2010): “Exploiting Regional Treatment Intensity for the Evaluation of Labour Market Policies,” *Journal of the American Statistical Association*, 105, 1014–1029.
- GÖRLITZ, K. (2010): “The Effect of Subsidizing Continuous Training Investments – Evidence from German Establishment Data,” *Labour Economics*, 17 (5), 789–798.
- HECKMAN, J., H. ICHIMURA, AND P. TODD (1997): “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme,” *Review of Economic Studies*, 64(4), 605–654.
- HEINRICH, C., P. MUESER, K. TROSKE, K. JEON, AND D. KAHVECIOGLU (2010): “New estimates of public employment and training program net impacts: A nonexperimental evaluation of the Workforce Investment Act program,” *Working Paper No. 1003, Department of Economics, University of Missouri*.
- (2011): “The Workforce Investment Act: Implementation Experiences and Evaluation Findings,” in *The Workforce Investment Act: Implementation Experiences and Evaluation Findings*, ed. by D. J. Besharov, and P. H., pp. 371–404. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

- HIPP, L., AND M. E. WARNER (2008): “Market Forces for the Unemployed? Training Vouchers in Germany and the USA,” *Social Policy and Administration*, 42, 77–101.
- HIRANO, K., G. IMBENS, AND G. RIDDER (2003): “Efficient estimation of Average Treatment Effects Using the Estimated Propensity Score,” *Econometrica*, pp. 1161–1189.
- HORVITZ, D., AND D. THOMPSON (1952): “A Generalization of Sampling Without Replacement from a Finite Population,” *Journal of the American Statistical Association*, 47, 663–685.
- HUJER, R., S. THOMSEN, AND C. ZEISS (2006): “The Effects of Vocational Training Programmes on the Duration of Unemployment in Eastern Germany,” *Allgemeines Statistisches Archiv*, 90, 299–322.
- IMBENS, G. W. (2004): “Nonparametric Estimation of Average Treatment Effects under Exogeneity: A Review,” *Review of Economics and Statistics*, 86(1), 4–29.
- KING, C. T., AND B. S. BARNOW (2011): “The Use of Market Mechanisms,” in *The Workforce Investment Act: Implementation Experiences and Evaluation Findings*, ed. by D. J. Besharov, and P. H., pp. 81–111. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.
- KRUPPE, T. (2009): “Bildungsgutscheine in der aktiven Arbeitsmarktpolitik,” *Sozialer Fortschritt*, (58), 9–19.
- LECHNER, M., R. MIQUEL, AND C. WUNSCH (2007): “The Curse and the Blessing of Training the Unemployed in a Changing Economy: The Case of East Germany after Unification,” *German Economic Review*, 8, 468–509.
- (2009): “Long- run Effects of Public Sector Sponsored Training,” *The Journal of the European Economic Association*.
- LECHNER, M., AND C. WUNSCH (2006): “Active Labour Market Policy in East Germany: Waiting for the Economy to take off,” *Economics of Transition*, 17, 661–702.

- (2011): “Sensitivity of matching-based program evaluations to the availability of control variables,” *Unpublished Work*.
- PAUL, M. (2009): “Many Dropouts? Never Mind! - Employment Prospects of Dropouts from Training Programs,” *CDSE Discussion Paper*, 63.
- RINNE, U., M. SCHNEIDER, AND A. UHLENDORFF (2011): “Do the skilled and prime-aged unemployed benefit more from training? Effect heterogeneity of public training programmes in Germany,” *Applied Economics*, 43, 3465–3494.
- RINNE, U., A. UHLENDORFF, AND Z. ZHAO (2008): “Vouchers and Caseworkers in Public Training Programs: Evidence from the Hartz Reform in Germany,” *ZEW Discussion Paper*, 3910.
- RUBIN, D. (1974): “Estimating the Causal Effect of Treatments in Randomized and Non-Randomized Studies,” *Journal of Educational Psychology*, 66(5), 688–701.
- SCHNEIDER, H., K. BRENKE, B. JESSKE, L. KAISER, U. RINNE, M. SCHNEIDER, J. STEINWEDE, AND A. UHLENDORFF (2007): “Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hartz-Kommission, Bericht 2006,” *IZA Research Report*, 10.
- SIANESI, B. (2004): “An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s,” *The Review of Economics and Statistics*, 86, 133–155.
- SMITH, J., AND P. TODD (2005): “Does Matching Overcome LaLonde’s Critique of Nonexperimental Methods?,” *Journal of Econometrics*, 125(1-2), 305–353.
- STEPHAN, G., AND A. PAHNKE (2011): “The Relative Effectiveness of Selected Active Labor Market Programs: an Empirical Investigation for Germany,” *The Manchester School*, 79, 1262–1293.
- WUNSCH, C., AND M. LECHNER (2008): “What Did All the Money Do? On the General Ineffectiveness of Recent West German Labour Market Programmes,” *Kyklos*, 61, 134–174.

A Instrumental Variable Approach

The instrumental variable approach is an alternative identification concept, which can be used when it is not possible to control for all confounding variables. In our application we assume that the conditional independence assumption holds. However, we assess the plausibility of this assumption using the following instrumental variable approach. The plausibility of an IV identification strategy depends critically on the choice of the instrument. A number of reasonable instruments have been proposed in the education/training literature. As an example, Abadie, Angrist, and Imbens (2002) use the randomized offer to participate in a training program as instrument for the actual participation. Card (1995) uses college proximity as an instrument for educational attainment. Black (1999) uses regional variation in the policy of education as an instrument for rental prices. Frölich and Lechner (2010) use regional policy variation as an instrument for training.

In this paper, we exploit the variation in the conditional regional specific allocation intensity of training vouchers to construct an instrument for a voucher award. The variation in the conditional employment district specific allocation intensity, that we name conditional regional policy style, can be explained by preferences and sentiments regarding the use of training vouchers that differ by employment offices. This preference is assumed to be independent of the regional labor market characteristics, after we control for a large set of variables when generating regional dummies. Just living in a region with a high allocation intensity, without being awarded with an instrument has no influence on the potential outcomes.

The number of vouchers awarded per unemployed differs by employment office. The employment offices may themselves decide on how much of their budget they use for training vouchers and how much for alternative ALMP. There may be several reasons for these differences. They can be partly explained by the fact that managers and caseworkers in different employment offices have different attitudes towards the voucher system. This is in particular the case in the time period in focus, which is the two years directly after the reform. Especially during

this period, the introduction of vouchers was judged very differently by different teams. To understand this, one should keep in mind that the implementation of training vouchers resulted in large-scale changes of the assignment process into further training that was not only remarkable for training participants but also for the caseworkers in the employment offices. Prior to the reform the caseworkers were responsible to assign the unemployed to training courses. Consequently, the reform caused a loss of authority in the allocation of training courses that may lead to negative attitudes regarding the reform. The positive impact can be seen in the reduced work burden for the caseworkers. Furthermore, some managers supported the idea of freedom of choice for the unemployed while others have been sceptical. We assume that this variation in the instrument reflects an exogenous policy style.

Apart from the policy style, the allocation intensity is likely to depend on regional labor market characteristics and differences in the characteristics of the customer stock of the employment offices. We argue that we can account for the latter differences using precise information on the regional characteristics and the characteristics of the unemployed registered at a certain employment office, like for example the number of vacant full time jobs, the share of foreigners among the unemployed, and the industry structure in the region.

We estimate the conditional regional policy styles using an regression fully interacted with the regional dummies,

$$D_{im} = \sum_{r=1}^{181} d_{ir} \cdot X'_{im} \cdot \hat{\alpha}_{mr} + \hat{v}_{im}, \quad (2)$$

where X_{im} contains regional and individual characteristics and d_{ir} is a dummy variable for each employment office r (with $r = 1, \dots, 181$). The conditional regional policy styles are indicated by $p_{im} = \sum_{r=1}^{181} d_{ir} \cdot X'_{im} \cdot \hat{\alpha}_{mr}$. This parameter is used as instrument in a two squares least squares regression. The first stage is indicated by,

$$\tilde{D}_{im} = p_{im} \cdot \tilde{\gamma}_m + X'_{im} \cdot \tilde{\beta}_m, \quad (3)$$

and the second stage estimate equals,

$$\hat{Y}_{imt} = \hat{\gamma}_{mt} \cdot \tilde{D}_{im} + X'_{im} \cdot \hat{\beta}_{mt}.$$

B Averaging across Starting Dates

Following a dynamic treatment evaluation approach (Sianesi, 2004, Frederiksson and Johansson, 2008) we estimate the effect of a voucher award versus waiting for each of the first twelve months of the unemployment period m separately. In the first month the treatment group includes only individuals who are awarded with a training voucher during the first month. Individuals who either receive a voucher later or never are in the control group. In the second month we drop all individuals who have left the risk set in the first month, i.e. received a voucher or found employment in the first month. The treatment group in the second month consist of voucher recipients that are awarded with a voucher in their second month of the unemployment period. Everybody in the risk set who does not receive a voucher in the second month belongs to the control group. This procedure continues until month twelve. By using this dynamic approach we end up with twelve different treatment effects for each of the twelve different times of elapsed unemployment duration. In order to communicate our results, we reduce the dimension of the results by reporting a weighted average of the twelve dynamic treatment effects in the following. The weights are calculated as fraction of treated in the respective month of the total number of treated individuals

$$\hat{\gamma}_t = \frac{\sum_{m=1}^M \sum_{i=1}^N D_{im} \cdot \hat{\gamma}_{mt}}{\sum_{m=1}^M \sum_{i=1}^N D_{im}},$$

where $\hat{\gamma}_{mt}$ is a generic term for $\hat{\gamma}_{mt}^{LATE}$ and $\hat{\gamma}_{mt}^{TOT}$. Given that we observe the labor market outcomes of each individual for 48 months after treatment ($t = 1, \dots, 48$), we specify a separate model for each month after treatment. This induces flexibility in all parameters with respect to the duration since treatment.

C Matching Quality

We assess the matching quality by showing the means of the matched control group for different control variables in Tables 1-3. Further, we report the standardized differences before and after matching. The standardized differences are defined as

$$SD = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{0.5(\sigma_{X_1}^2 + \sigma_{X_2}^2)}} \cdot 100,$$

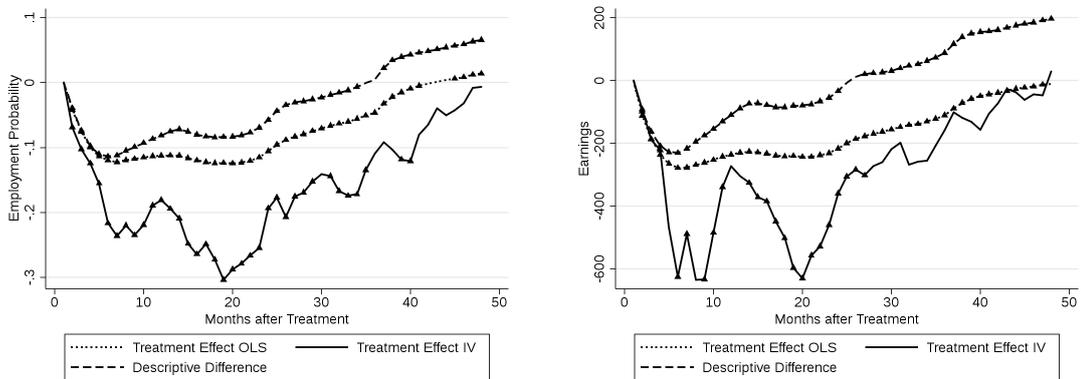
where \bar{X}_d is the mean and $\sigma_{X_d}^2$ the variance in the respective treatment group $d \in \{0, 1\}$. Before matching we observe standardized differences larger than 40. After matching the standardized differences are always below one, suggesting a very good matching quality.

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

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

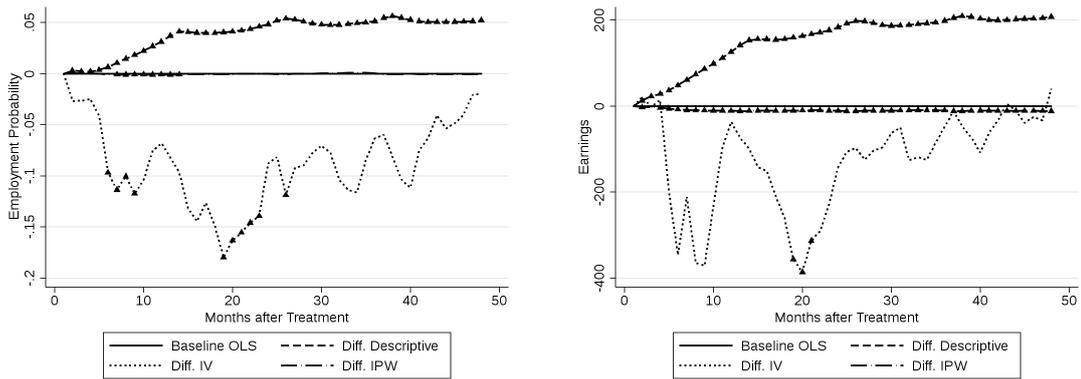
where x_k indicates the specific control variable. We perform a joint F-test for the null hypothesis that $\hat{\beta}_1$ and $\hat{\beta}_3$ equal zero. In Table 5 we report the summarized results of the test for each of the twelve treatment times. Overall we run 1,368 regressions whereof the test indicates 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.

Figure 1: Effect of a voucher award on employment and earnings averaged over elapsed unemployment durations until treatment.



Triangles and diamonds indicate significant effects.

Figure 2: Differences in the estimated effects of a voucher award on employment and earnings averaged over elapsed unemployment durations until treatment.



Triangles and diamonds indicate significant effects.

Figure 3: Fraction of individuals in training after the award of a voucher.

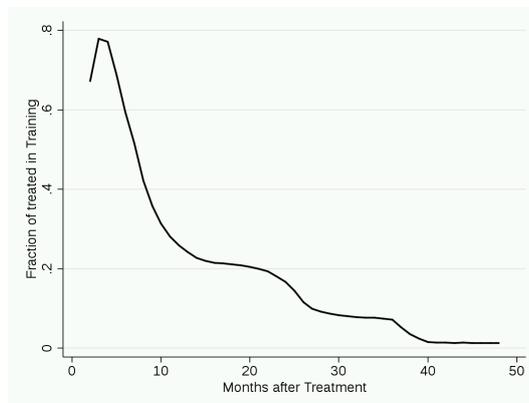


Figure 4: Comparison of average employment and average earnings between treatment and matched control group averaged over elapsed unemployment durations until treatment.

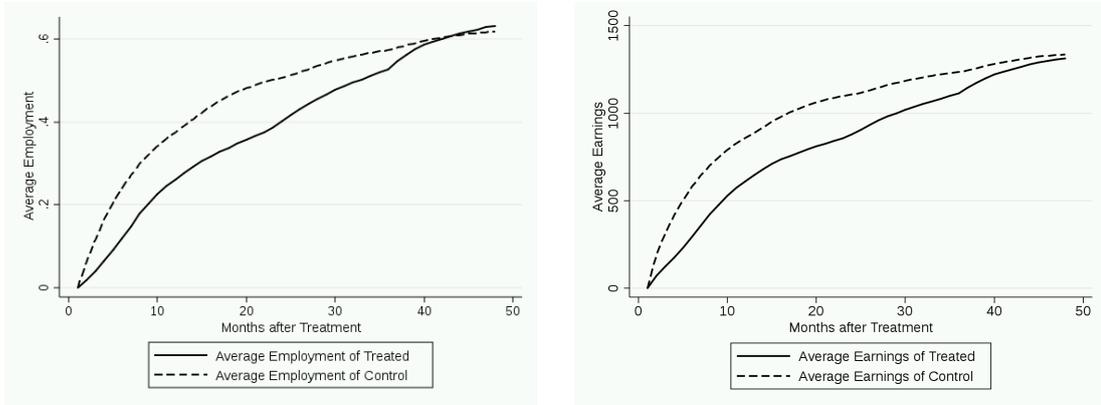


Figure 5: Heterogeneous effects on employment and earnings by skill group (OLS) averaged over elapsed unemployment durations until treatment.

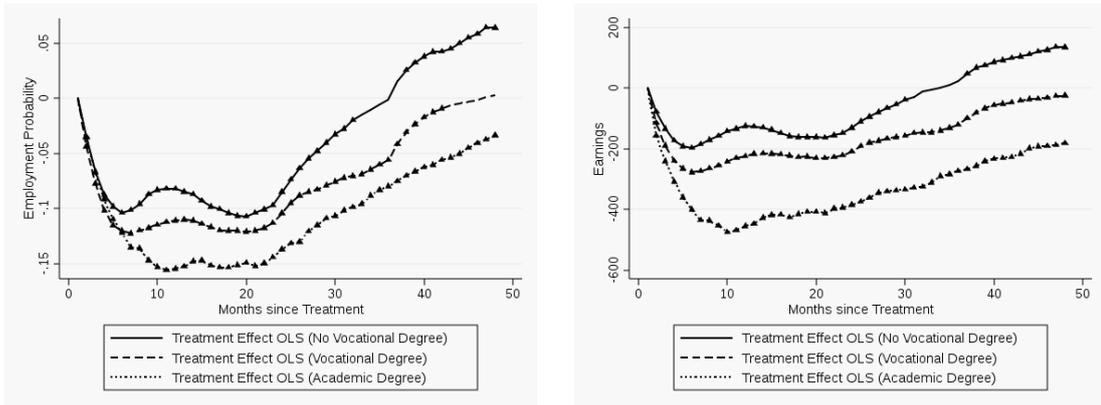


Figure 6: Comparison of average employment of treated and matched control group by skill group averaged over elapsed unemployment durations until treatment.

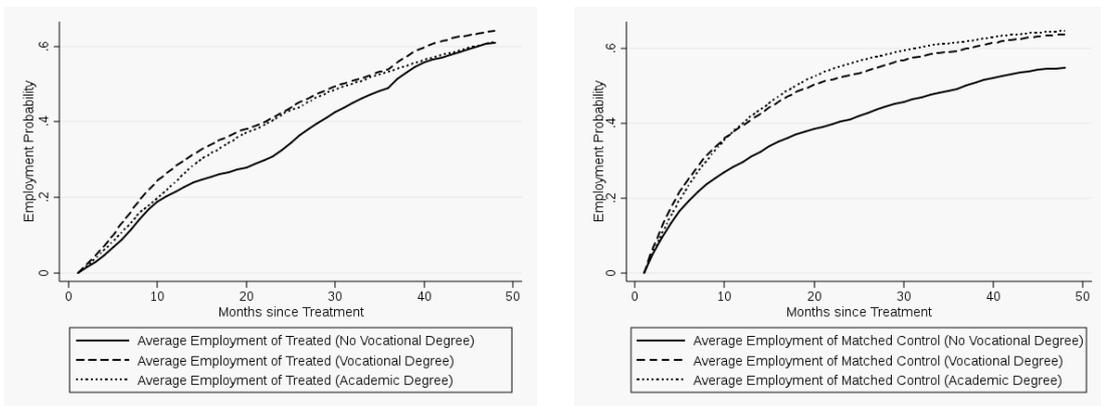


Figure 7: Effect of a voucher award on employment and earnings for individuals without vocational degree averaged over elapsed unemployment durations until treatment.

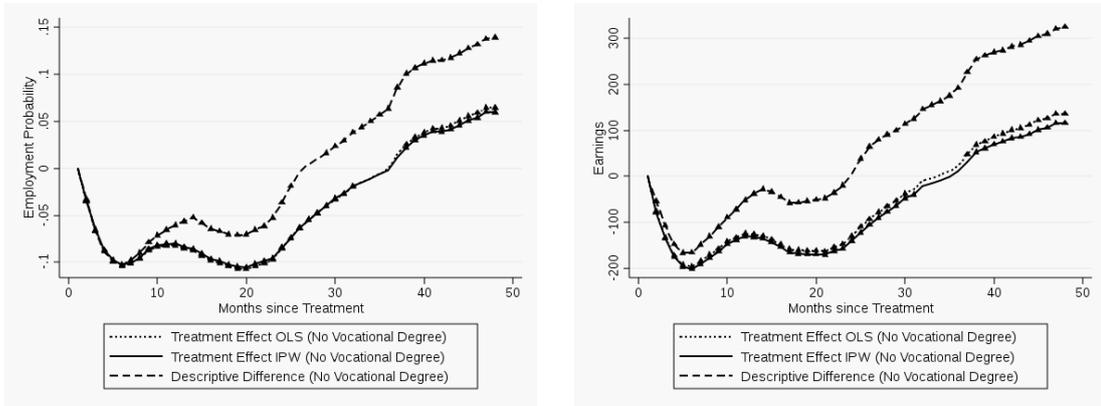


Figure 8: Effect of a voucher award on employment and earnings for individuals with vocational degree averaged over elapsed unemployment durations until treatment.

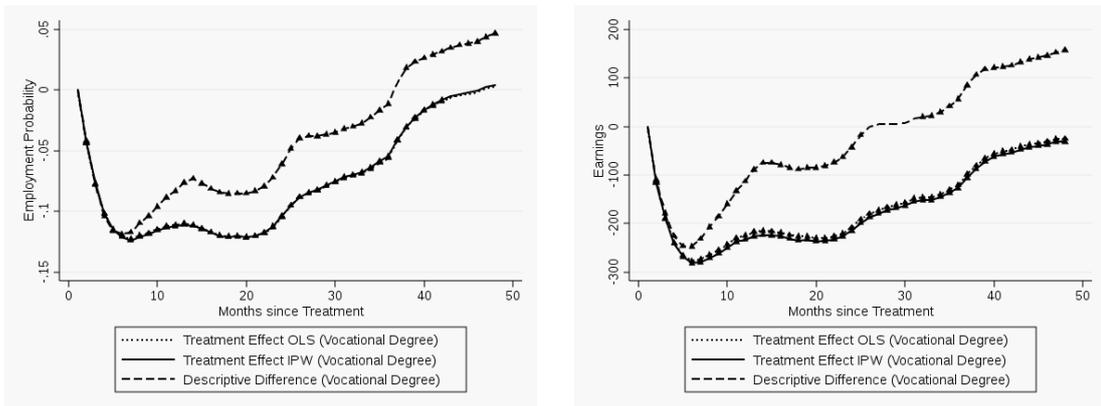


Figure 9: Effect of a voucher award on employment and earnings for individuals with academic degree averaged over elapsed unemployment durations until treatment.

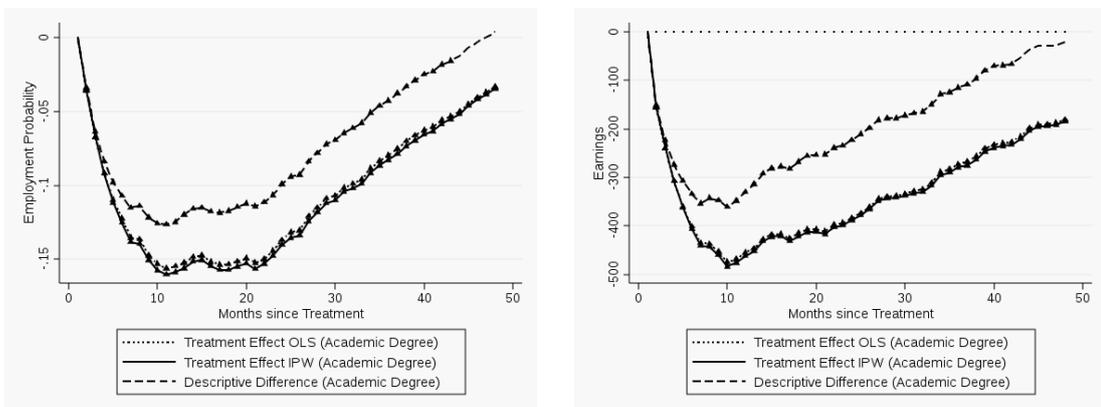


Figure 10: Heterogenous effects on employment and earnings with regard to the type of training (OLS) averaged over elapsed unemployment durations until treatment

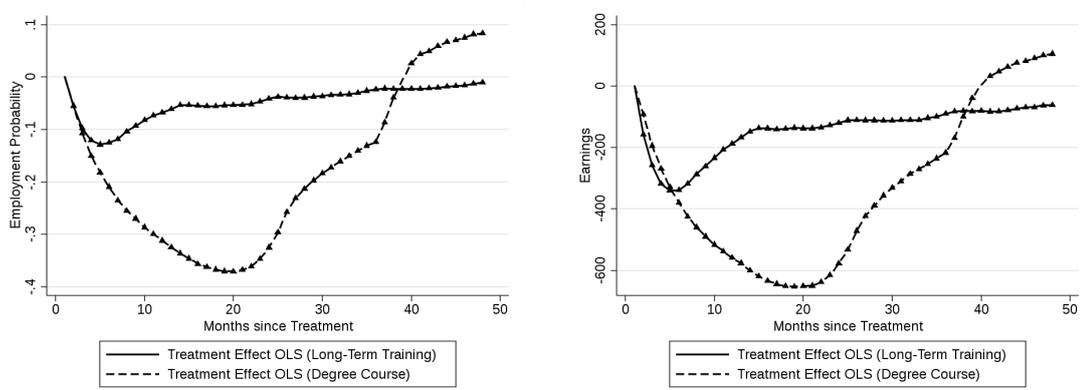


Figure 11: Effect of a voucher award on employment and earnings for individuals participating in long-term courses averaged over elapsed unemployment durations until treatment.

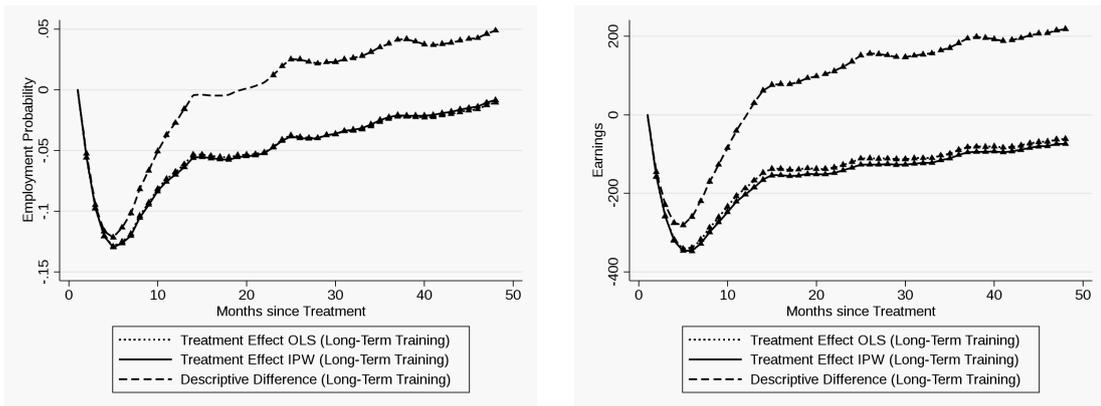


Figure 12: Effect of a voucher award on employment and earnings for individuals participating in degree courses averaged over elapsed unemployment durations until treatment.

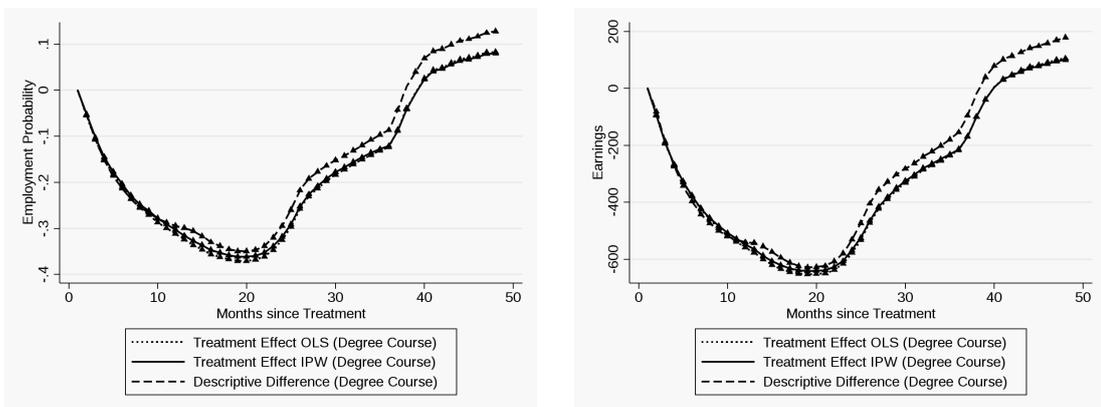


Figure 13: Comparison of average employment of treated and matched control group by course type averaged over elapsed unemployment durations until treatment.

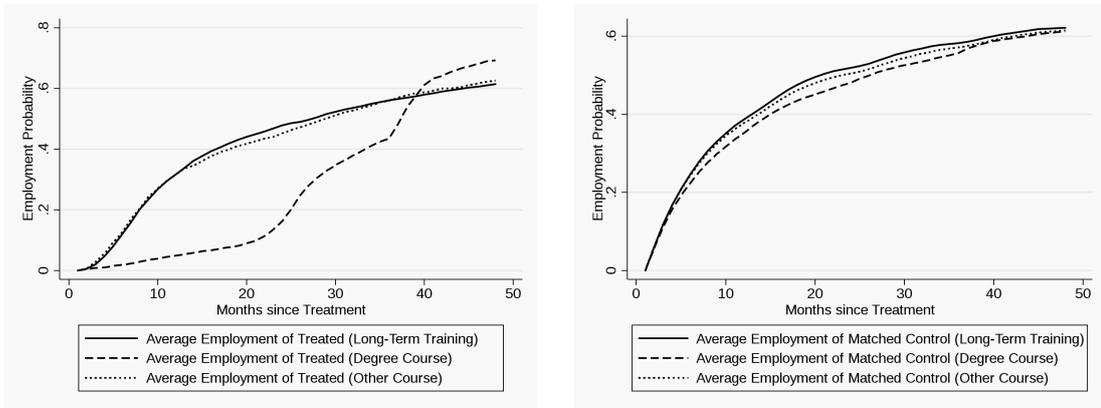


Figure 14: Heterogenous effects on employment and earnings with regard to the type of training and vocational degree (OLS) averaged over elapsed unemployment durations until treatment

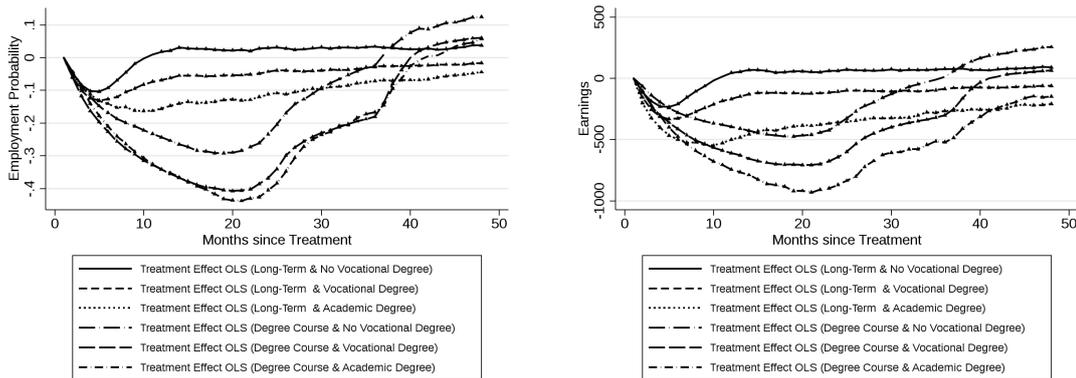


Figure 15: Heterogenous effects on employment and earnings with regard to the redemption decision (OLS) averaged over elapsed unemployment durations until treatment.

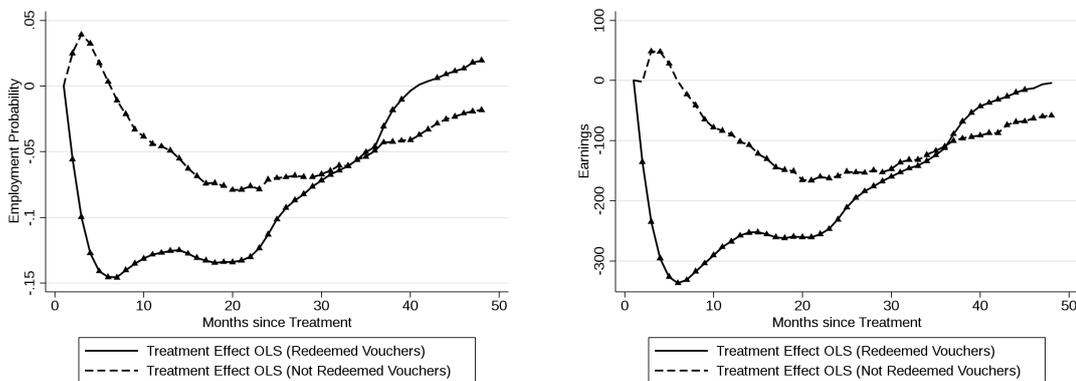


Figure 16: Effect of a voucher award on employment and earnings for individuals who redeem the voucher averaged over elapsed unemployment durations until treatment.

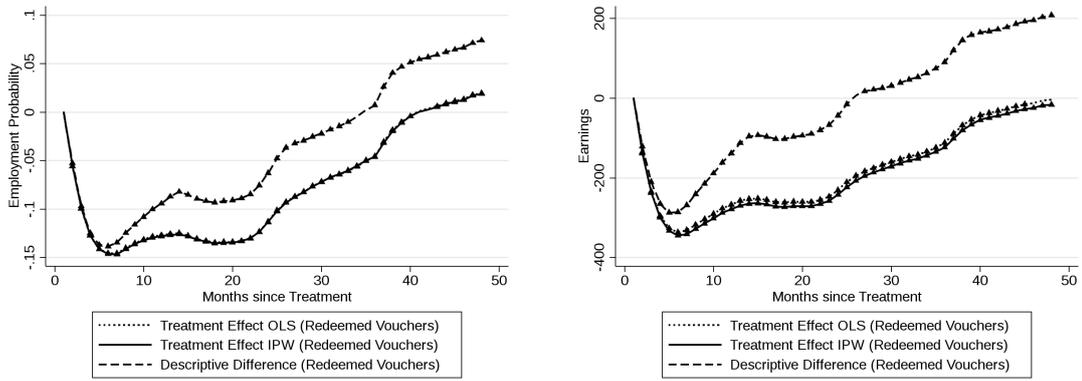


Figure 17: Effect of a voucher award on employment and earnings for individuals who do not redeem the voucher averaged over elapsed unemployment durations until treatment.

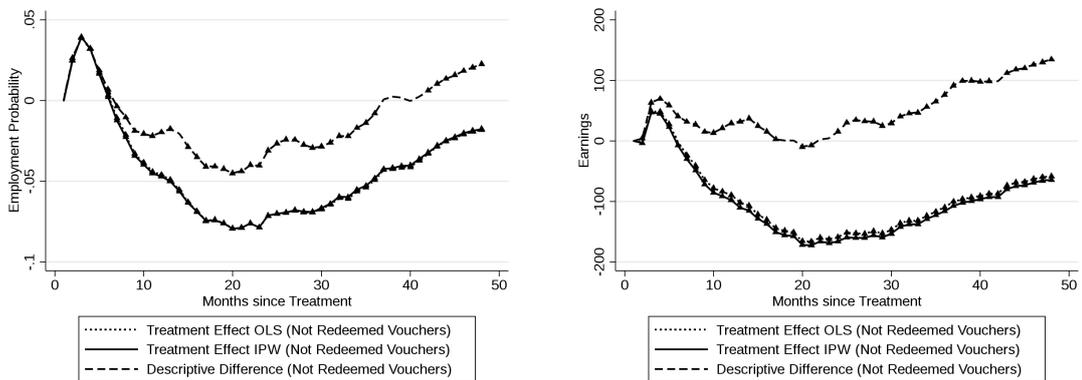


Table 1: Means and Standardized Differences (SD) for Personal Characteristics

	Treatment- group	Control- group	SD before Matching	Matched Controlgroup	SD after Matching	Voucher redeemed	Voucher expired	Degree Courses	Long-term Courses
Female	0.442	0.423	7.050	0.441	0.200	0.442	0.440	0.487	0.412
Age									
25-29 years	0.159	0.159	1.590	0.161	0.500	0.157	0.170	0.237	0.129
30-34 years	0.190	0.176	3.710	0.190	0.180	0.189	0.193	0.248	0.173
35-39 years	0.233	0.203	7.250	0.233	0.200	0.234	0.225	0.247	0.229
45-49 years	0.141	0.155	4.240	0.140	0.290	0.142	0.138	0.075	0.163
50-54 years	0.070	0.115	15.570	0.070	0.170	0.069	0.074	0.015	0.087
Nationality									
Germany	0.927	0.924	1.400	0.927	0.260	0.928	0.920	0.907	0.936
Outside EU	0.032	0.013	12.600	0.031	0.280	0.032	0.032	0.042	0.028
Missing	0.017	0.036	11.810	0.017	0.090	0.016	0.021	0.019	0.015
Marital Status									
Single	0.326	0.315	4.150	0.326	0.250	0.321	0.349	0.293	0.339
Single parent	0.072	0.059	5.340	0.071	0.180	0.072	0.068	0.099	0.062
Married	0.463	0.484	4.490	0.463	0.100	0.468	0.441	0.442	0.478
Missing	0.096	0.093	3.610	0.096	0.270	0.095	0.099	0.118	0.077
Child	0.364	0.356	2.970	0.364	0.160	0.370	0.336	0.417	0.355
Age of youngest child									
One year	0.012	0.011	2.230	0.012	0.150	0.012	0.011	0.014	0.011
Between 1 and 3 years	0.035	0.031	2.580	0.036	0.130	0.036	0.034	0.041	0.035
Between 3 and 6 years	0.066	0.062	2.100	0.066	0.120	0.067	0.060	0.086	0.062
Between 6 and 10 years	0.083	0.074	3.220	0.083	0.150	0.083	0.081	0.103	0.075
Older than 14 years	0.086	0.098	4.450	0.085	0.180	0.087	0.077	0.081	0.091
Missing	0.637	0.646	2.940	0.637	0.160	0.631	0.665	0.584	0.646
Disabled	0.020	0.027	4.190	0.020	0.170	0.019	0.026	0.006	0.024
Health									
Health problems	0.095	0.123	9.040	0.095	0.200	0.093	0.109	0.081	0.097
Health problems before unemployment	0.041	0.053	5.760	0.041	0.080	0.039	0.047	0.033	0.041
N	46,068	80,107				38,437	7,631	9,819	24,025

Omitted Categories:

Age: 40-44 years

Nationality: Member EU

Marital Status: Common law marriage

Age of youngest child: Between 10 and 14 years

Table 2: Means and Standardized Differences (SD) for Education, Occupation, and Sector

	Treatment- group	Control- group	SD before Matching	Matched Controlgroup	SD after Matching	Voucher redeemed	Voucher expired	Degree Courses	Long-term Courses
Education									
No schooling degree	0.042	0.070	12.330	0.042	0.130	0.042	0.044	0.045	0.039
Abitur/Hochschulreife	0.223	0.166	14.320	0.225	0.400	0.225	0.211	0.163	0.264
Missing	0.011	0.013	2.530	0.011	0.160	0.011	0.014	0.016	0.009
Vocational Training									
No vocational degree	0.220	0.237	8.320	0.219	0.400	0.220	0.223	0.365	0.159
Academic degree	0.107	0.085	7.630	0.109	0.490	0.109	0.099	0.050	0.144
Missing	0.012	0.013	2.460	0.012	0.180	0.011	0.013	0.015	0.009
Classification of Occupation									
Farmer, Fisher	0.013	0.025	8.720	0.013	0.160	0.014	0.011	0.020	0.012
Technical	0.076	0.050	10.510	0.077	0.200	0.077	0.073	0.024	0.102
Service	0.616	0.573	8.710	0.615	0.120	0.615	0.620	0.623	0.611
Other	0.004	0.005	3.690	0.004	0.160	0.004	0.004	0.006	0.003
Part-time work									
Full-time	0.812	0.797	9.030	0.813	0.260	0.813	0.811	0.784	0.839
Missing	0.066	0.077	4.380	0.066	0.340	0.065	0.070	0.076	0.056
Part-time work desired									
Desired	0.836	0.832	4.720	0.837	0.190	0.837	0.833	0.829	0.857
Missing	0.078	0.078	4.280	0.078	0.310	0.078	0.080	0.101	0.060
Kind of work									
White-collar	0.467	0.367	20.520	0.468	0.200	0.466	0.471	0.327	0.529
Missing	0.106	0.107	6.680	0.105	0.090	0.108	0.093	0.135	0.091
Azubi									
	0.031	0.018	12.510	0.031	0.290	0.033	0.022	0.053	0.012
Sector									
Agriculture	0.010	0.018	6.900	0.010	0.100	0.010	0.009	0.012	0.009
Mining	0.002	0.002	1.410	0.002	0.100	0.002	0.001	0.002	0.002
Utilities	0.002	0.002	1.160	0.002	0.130	0.002	0.002	0.001	0.002
Construction	0.075	0.116	13.690	0.076	0.170	0.076	0.074	0.065	0.081
Trade	0.166	0.150	4.420	0.166	0.100	0.165	0.172	0.159	0.169
Hotels and Restaurants	0.031	0.043	6.150	0.031	0.130	0.030	0.036	0.044	0.026
Traffic, Transportation	0.059	0.064	2.540	0.059	0.100	0.059	0.059	0.073	0.056
Financial Services	0.022	0.015	5.080	0.022	0.120	0.022	0.020	0.017	0.024
Renting	0.011	0.011	1.530	0.011	0.070	0.011	0.011	0.007	0.013
Data processing	0.158	0.134	7.000	0.158	0.200	0.157	0.163	0.105	0.185
Public Sector, Education	0.061	0.070	4.990	0.062	0.300	0.061	0.063	0.066	0.061
Health and social services	0.080	0.080	15.140	0.080	0.290	0.081	0.071	0.148	0.045
Other Services	0.044	0.048	1.910	0.045	0.180	0.045	0.042	0.056	0.042
Temporary Employment	0.045	0.054	4.650	0.045	0.140	0.045	0.045	0.036	0.048
N	46,068	80,107				38,437	7,631	9,819	24,025

Omitted Categories:

Education: Schooling degree without Abitur

Vocational Training: Vocational Degree

Classification of Occupation: Miner and Manufacturing

Part-time work: Part-time

Part-time work desired: Not desired

Kind of work: Blue-collar

Sector: Production

Table 3: Means and Standardized Differences (SD) for Employment/Unemployment/ALMP History

	Treatment- group	Control- group	SD before Matching	Matched Controlgroup	SD after Matching	Voucher redeemed	Voucher expired	Degree Courses	Long-term Courses
Noticeable problems									
Problem group	0.018	0.026	5.490	0.018	0.150	0.018	0.017	0.016	0.020
Sanction	0.012	0.033	14.800	0.012	0.100	0.011	0.015	0.015	0.009
Lack of Motivation	0.109	0.140	9.850	0.109	0.130	0.107	0.118	0.137	0.096
Incapacity	0.136	0.220	22.790	0.136	0.270	0.127	0.181	0.123	0.130
Dropout	0.012	0.057	24.790	0.012	0.220	0.012	0.014	0.016	0.010
Employment History (last 7 years), Sequences									
Mostly employed in last period									
Mostly unemployed	0.171	0.231	14.840	0.172	0.290	0.171	0.173	0.232	0.151
3 years employed (close)	0.132	0.093	12.180	0.132	0.140	0.131	0.135	0.133	0.129
3 years employed (far)	0.026	0.056	15.480	0.026	0.210	0.026	0.026	0.024	0.027
3 years unemployed (close)	0.011	0.025	10.430	0.011	0.090	0.011	0.011	0.01	0.012
3 years unemployed (far)	0.099	0.088	3.600	0.099	0.250	0.099	0.094	0.112	0.095
Mixed employment	0.048	0.062	6.260	0.048	0.160	0.048	0.049	0.053	0.046
Mostly unemployed in last period									
Mostly employed	0.016	0.032	10.690	0.016	0.110	0.016	0.015	0.014	0.016
3 years employed (close)	0.005	0.007	2.600	0.005	0.060	0.004	0.006	0.006	
3 years employed (far)	0.001	0.004	5.830	0.001	0.130	0.001	0.001	0.001	0.001
Program History (last 3 years), Sequences									
Often in programs	0.012	0.035	15.580	0.012	0.290	0.012	0.012	0.013	0.012
No programs	0.911	0.763	40.830	0.910	0.410	0.912	0.908	0.907	0.911
History of Wages While Employed									
Real wage (t-1)	66.988	57.966	30.110	67.056	0.230	66.917	67.396	57.808	71.112
Real wage (t-2)	60.794	46.693	40.150	60.899	0.290	60.702	61.304	50.341	65.238
Real wage (t-3)	54.718	42.984	30.860	54.672	0.190	54.674	54.989	43.747	59.157
Real wage (t-4)	49.739	42.424	18.960	49.582	0.390	49.611	50.472	38.923	54.026
Real wage (t-5)	45.031	39.380	14.530	44.912	0.300	44.970	45.401	34.607	49.214
Real wage (t-6)	41.384	36.707	12.490	41.290	0.240	41.289	41.907	31.300	45.364
Real wage (t-7)	39.256	35.398	11.250	39.170	0.220	39.070	40.271	29.026	43.097
N	46,068	80,107				38,437	7,631	9,819	24,025

Omitted Categories:

Mostly employed in last Period: Mostly Employed

Mostly unemployed in last period: 3 years unemployed (far) and Mixed Employment

History of programs (last 3 years): Seldom in programs

Omitted Categories:

Industries: Manufacturing industry

Table 4: Means and Standardized Differences (SD) for Regional Characteristics

	Treatment - group	Control- group	SMD before Matching	Matched Controlgroup	SMD after Mat ding	Voucher redeemed	Voucher expired	Degree Courses	Long-term Courses
Unemployment and Population									
Unemployment rate	12.134	12.841	13.472	12.162	0.552	12.195	11.842	12.674	12.392
Share of male unemployed	0.565	0.561	10.863	0.565	0.338	0.564	0.568	0.563	0.565
Share of German unemployed	0.857	0.871	15.879	0.857	0.455	0.858	0.850	0.868	0.857
Share of vacant fulltime jobs	0.794	0.789	7.646	0.794	0.242	0.794	0.796	0.790	0.793
Population per km^2	565.215	534.265	4.297	566.585	0.191	543.161	680.007	519.359	604.57
Industries									
Management of forests and agriculture	0.012	0.014	18.108	0.012	0.554	0.012	0.011	0.013	0.012
Fishing	0.000	0.000	9.247	0.000	0.314	0.000	0.000	0.000	0.000
Mining	0.005	0.005	3.814	0.005	0.178	0.005	0.005	0.005	0.005
Energy and water supply	0.010	0.010	3.705	0.010	0.238	0.010	0.010	0.001	0.001
Construction	0.064	0.067	15.418	0.064	0.461	0.064	0.062	0.066	0.064
Trade	0.150	0.150	2.888	0.150	0.154	0.150	0.149	0.149	0.150
Hotels and Restaurants	0.028	0.028	3.200	0.028	0.267	0.028	0.028	0.029	0.028
Transport and Communications	0.056	0.057	9.414	0.056	0.426	0.056	0.055	0.057	0.056
Bank and insurance business	0.038	0.037	8.666	0.038	0.268	0.038	0.039	0.037	0.038
Real estate activities	0.118	0.116	5.886	0.118	0.180	0.118	0.120	0.116	0.120
Public administration and defence	0.065	0.067	13.020	0.065	0.361	0.065	0.065	0.066	0.065
Education	0.040	0.043	13.113	0.040	0.590	0.040	0.040	0.041	0.041
Healthcare and social sector	0.118	0.117	2.711	0.118	0.128	0.117	0.118	0.118	0.118
Services	0.047	0.047	4.115	0.047	0.288	0.047	0.048	0.047	0.048
Production at the householdlevel	0.001	0.001	14.498	0.001	0.556	0.001	0.001	0.001	0.001
Extraterritorial organisations and bodies	0.001	0.001	3.209	0.001	0.317	0.001	0.001	0.001	0.001
Other	0.000	0.000	4.765	0.000	0.131	0.000	0.000	0.000	0.000
N	46.068	80.107				38.437	7.631	9.819	24.025

Table 5: Balancing Test (Smith and Todd, 2005)

Elapsed Unempl. Duration (in months)	Weighted Obs	Treated	Number of Parameters	# sign.
1	1,897,665	8,259	114	7
2	1,796,172	4,151	114	1
3	1,639,687	4,254	114	6
4	1,503,814	4,214	114	5
5	1,394,775	4,160	114	5
6	1,307,297	3,964	114	5
7	1,229,031	3,628	114	4
8	1,164,495	3,362	114	3
9	1,105,852	2,930	114	3
10	1,051,222	2,763	114	3
11	1,002,824	2,408	114	2
12	952,258	1975	114	4
			1,368	48