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Direct and indirect effects

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Abstract

This paper evaluates the effect of a voucher award system for assignment into vocational training on the employment outcomes of unemployed voucher recipients in Germany, along with the causal mechanisms through which it operates. It assesses the direct effect of voucher assignment net of actual redemption, which may be driven by preference shaping/learning about (possibilities of) human capital investments or simply by costs of information gathering. Using a mediation analysis framework based on sequential conditional independence assumptions and semiparametric matching estimators, our results suggest that the negative short term and positive long term employment effects of voucher award are mainly driven by actual training participation. However, also the direct effect of just obtaining the voucher is negative in the short run. This points to potential efficiency losses of voucher award systems if individuals decide not to redeem, as employment chances are lower than under non-award in the short run and under redemption in the long run, making non-redemption the least attractive option.

Keywords

Mediation analysis, voucher award, training programmes, direct effects, indirect effects, causal mechanisms, causal channels, matching estimation.

JEL Classification

J64, J68, C21, C31.

1 Introduction

In January 2003, the Federal Employment Agency in Germany reformed the allocation of vocational training programmes, which are a corner stone of German active labour market policies (ALMPs). An assignment system based on vouchers replaced the direct assignment of unemployed individuals to vocational training by caseworkers. The aim of the reform was to increase the involvement of training participants in the training decision as well as to increase the competition among training providers. In contrast to the pre-reform rules, under which essentially the caseworker decided about the placement into vocational training, voucher recipients may now choose both the training provider as well as the course (subject to some restrictions concerning the course objective, content, and duration).

Based on rich administrative data, this paper investigates the effect of voucher award on re-employment and other labour market outcomes focussing on the distinct causal mechanisms through which this effect may operate. Specifically, we analyse whether in addition to the voucher's impact through its redemption (i.e. participation in vocational training) there exists a direct effect of mere voucher award (i.e. without participation). The latter may be driven by motivational effects, preference shaping, and awareness building w.r.t. (the availability of) ALMPs, that could affect labour market behaviour in one or the other direction. For instance, the award of a voucher could increase the awareness of and the preference for possibilities to build up human capital and therefore immediately reduce job search intensity. The same effect occurs if the award of the voucher leads to high information costs when looking for an appropriate provider and course. On the other hand, if ALMPs are perceived to be a burden, a threat, or unlikely to be effective, and if voucher award increases the awareness of potential obligations to participate in future (unattractive) ALMPs, then an immediate increase in job search intensity may be expected. It therefore appears to be an interesting and open issue whether the impact of getting a voucher is solely rooted in its actual use or whether a 'direct' effect, whose direction is a priori ambiguous, exists, too. In particular, this allows judging whether it is the quality of the training providers that (through voucher redemption) drive the voucher effect or whether other effects (through preference shaping, awareness building, costs of information gathering, etc.) are important as well.

We use a formal mediation framework (see for instance the seminal paper by Baron and Kenny, 1986) to identify the causal mechanisms of interest and to this end consider redemption of the voucher as a so-called mediator, i.e. an intermediate outcome on the causal path of voucher award to the individual labour market outcomes. Besides the impact of voucher redemption, we are particularly interested in the so-called controlled direct effect (see for instance Pearl, 2001), i.e. the employment effect of voucher award in the absence of actual redemption.[1] Causal mechanisms are, however, not easily identified. Even if the vouchers were randomly assigned, this would not imply randomness of the mediator (see Robins and Greenland, 1992).

To tackle the endogeneity of voucher award and redemption, a particular conditional independence assumption is invoked for identification. It requires (i) that voucher award is independent of potential employment outcomes (under (non-)award and (non-)redemption of the vouchers) conditional on observed covariates and (ii) that voucher redemption is independent of the potential outcomes conditional on the covariates and voucher award. These conditional independence assumptions are related to those invoked in the nonparametric mediation literature for identifying controlled direct effects (see for instance Petersen, Sinisi, and van der Laan, 2006, and VanderWeele, 2009), in the dynamic treatment effect literature for assessing sequences of treatments (see for instance Robins, 1986, 1989, Robins, Hernan, and Brumback, 2000, Lechner, 2009, and Lechner and Miquel, 2010) and in the multiple treatment effect framework (see Imbens, 2000, and Lechner, 2001). For estimation, we use semi-

1 We refer to Pearl (2001) for a discussion about differences between controlled and natural direct effects.

parametric radius matching with bias adjustment (Lechner, Miquel, and Wunsch, 2011) on the propensity score of (joint) voucher award and redemption given the covariates.

The results suggest that among voucher recipients, voucher award has a negative average (total) employment effect in the first three years after voucher receipt and a small positive one thereafter, with an increased employment probability of roughly 2 to 3 percentage points in each month four years after receiving the voucher. This implies that the initially negative lock-in effect of the voucher award system (likely due to decreased job search) is compensated by higher placement probabilities in later periods. Concerning the causal mechanisms, voucher redemption (and thus, actual participation in or at least starting a vocational training) has similar negative short term and positive long term effects (again, on the population of voucher recipients) as voucher award, being slightly more pronounced respectively. It is therefore voucher redemption which primarily drives the total effect on voucher recipients.

In contrast, the direct effect on voucher recipients, assessed as difference in mean potential outcomes between voucher award and non-redemption vs. non-award (and non-redemption), is insignificant in most of the fourth year. This suggests that in the long run, mere voucher receipt does not affect employment success (e.g. through a change in preferences). There nevertheless occurs a negative direct effect over the first three years, suggesting that voucher award decreases job search intensity even despite non-redemption. This points to potential efficiency losses of voucher award systems if individuals decide not to redeem the vouchers, as employment chances are lower than under non-award in the short run and under redemption in the long run. Therefore, voucher award together with non-redemption appears to be the least attractive option, which needs to be taken into account when designing such a voucher award system.

The main contribution of this study is that it assesses various causal mechanisms of a specific voucher based active labour market policy in addition of the total (gross) effect of the programme, which to the best of our knowledge has not been done before. Our research focus is therefore different to Doerr, Fitzenberger, Kruppe, Paul, and Strittmatter (2014) and Heinrich, Mueser, Troske, Jeon, and Kahvecioglu (2010), who evaluate the effectiveness of various vocational training programmes under voucher systems, but do not consider the direct effect of voucher award. It also differs from Doerr and Strittmatter (2014) and Rinne, Uhlendorff, and Zhao (2013), who compare the effectiveness of vocational training under a voucher and a direct assignment regime, but do not separate award and redemption effects either. Our paper is one of the few (but growing number of) empirical economic studies aiming at disentangling direct and indirect effects of policy interventions (see Flores and Flores-Lagunes, 2009, Heckman, Pinto, and Savelyev, 2013, and Huber, Lechner, and Mellace, 2014, for further examples).

The remainder of this paper is organized as follows. Section 2 outlines the institutional background of the award of vouchers for ALMP in Germany. Section 3 presents the econometric framework, namely the definition of the effects of interest, the identifying assumptions, and a brief description of the propensity score matching estimator. Section 4 introduces the data. In Section 5, we provide descriptive statistics and discuss the plausibility of the identifying assumptions in the light of the data. Section 6 presents the estimation results. Section 7 concludes. Three appendices provide further details on the data, on the estimation, and on the results obtained.

2 Institutional background of voucher provision

Vocational training programmes constitute a corner stone of ALMPs in Germany. Their main objective is to adjust the skills of unemployment individuals to changing labour market requirements and/or changed individual conditions (for instance health issues). Vocational training primarily consists of three categories: classical vocational training, training in so-called practice firms, and retraining. Classical vocational training is either organized in classrooms or on the job. Examples are courses in IT-based accounting or customer orientation and sales. Training in practice firms aims at simulating a (real) work environment. Retraining leading to a degree (also called degree courses) has a longer duration of up to three years with the goal to accomplish a (new) vocational degree within the German apprenticeship system. It covers, for example, the full curriculum of a vocational training for an elderly care nurse. Between 2000 and 2002, average annual expenditures for vocational training exceeded seven billion Euros (source: Labour Market Reports, Federal Employment Agency of Germany).

In January 2003, a voucher-based allocation system for the provision of vocational training was introduced. It aims at promoting the responsibility of training participants as well as market mechanisms among training providers. Potential training participants receive a vocational training voucher, which permits choosing the training provider and course. As explained in Doerr and Strittmatter (2014), several rules apply. First, the voucher specifies the objective, content, and maximum duration of the course. Second, it can only be redeemed within a one-day commuting zone. Third, the validity of training vouchers varies between one week and three months. Fourth, no sanctions (e.g. cuts in unemployment benefits) are imposed in case of non-redemption.

3 Econometric framework

3.1 Potential outcomes and causal effects

Let D denote a binary indicator for voucher award, the so-called treatment variable, and Y the labour market outcome of interest, e.g. employment. Furthermore, let M be a binary indicator for voucher redemption (which implies participation in vocational training). This is the major mediator through which the indirect effect of D on Y operates. To define the effects of interest, we use the potential outcome framework, see for instance Rubin (1974), and denote by Y^d the potential outcome as a function of voucher award $d \in \{1,0\}$.[2] The (total) average treatment effect on the treated (ATET) of voucher award is therefore given by $\Delta = E(Y^1 - Y^0 | D = 1)$. Secondly, for investigating distinct causal mechanisms, we denote by $Y^{d,m}$ the potential outcome as a function of both voucher award and redemption, $d, m \in \{1,0\}$. Note that the two types of denoting potential outcomes are linked, namely: $Y^d = Y^{d,M^d}$, where M^d is the potential redemption state under voucher award $D = d$. Therefore, the ATET may be expressed as:

$$\Delta = E(Y^{1,M^1} - Y^{0,M^0} | D = 1). \tag{1}$$

In our application, $M^0 = 0$ for everyone, because vouchers cannot be redeemed if not assigned, so that $\Delta = E(Y^{1,M^1} - Y^{0,0} | D = 1)$. In contrast, M^1 might be either 1 or 0, depending on whether an individual decides to redeem a received voucher or not. The ATET therefore provides the "reduced form" effect of award (not actual redemption), which may operate through redemption (given that M^d changes with the value of d for at least some or all individuals) or also bear a direct component.

The extended notation allows us to define further parameters of interest, namely the average effect of voucher award and redemption vs. no award and no redemption, again among voucher recipients:

$$\theta = E(Y^{1,1} - Y^{0,0} | D = 1). \tag{2}$$

The difference to the ATET (Δ) is that here, the redemption is prescribed to correspond to voucher award. Note that only in the special case of perfect compliance, i.e. everyone's redemption decision corresponds to the voucher award (i.e. $M^d = d$ for $d \in \{1,0\}$), is θ equal

[2] By defining the potential outcomes this way, we implicitly impose the Stable Unit Treatment Value Assumption (SUTVA), see Rubin (1980).

to Δ. Again, part or all of the impact may be due to redemption or to a direct effect of award. In a next step we disentangle the latter two components and first consider the so-called controlled direct effect, see for instance Pearl (2001):[3]

$$\gamma = E(Y^{1,0} - Y^{0,0} \mid D = 1). \tag{3}$$

This is the impact of training voucher award among voucher recipients net of actual redemption, i.e. under prescribed non-redemption for everyone. Finally, the effect of redemption is identified by

$$\delta = E(Y^{1,1} - Y^{1,0} \mid D = 1). \tag{4}$$

Here, the effect of redemption vs. non-redemption is investigated when a voucher was awarded. Note that γ and δ sum up to θ, which can be seen from adding and subtracting $Y^{1,0}$ in the expectation of expression (2).

3.2 Identifying assumptions

To identify the effects of interest, we impose (sequential) conditional independence of the potential outcomes on the one hand and voucher award and redemption on the other hand (Assumptions 1 and 2 below). This requires that we observe all factors that are jointly related (i) with *D* and the potential outcome under non-treatment and (ii) with *M* and the potential outcome under non-treatment. We henceforth denote the vector of observed covariates by *X*. Furthermore, a particular common support restriction is also needed (Assumption 3 below) which implies that suitable comparisons in terms of observed covariates exist across various combinations of *D* and *M*.

[3] A related parameter is the so-called natural direct effect in the nomenclature of Pearl (2001) or the pure/total direct effect in the nomenclature of Robins and Greenland (1992) and Robins (2003), which is defined upon potential mediator states rather than prescribed mediator values: $E(Y^{1,M^1} - Y^{0,M^1} \mid D = 1)$, $E(Y^{1,M^0} - Y^{0,M^0} \mid D = 1)$. The latter two parameters and γ are equivalent only in the special case that there are no interaction effects between *D* and *M* on the outcome *Y*. Identification and estimation of natural direct effects has been considered in Pearl (2001), Robins (2003), Flores and Flores-Lagunes (2009), Imai, Keele, and Yamamoto (2010), and Huber (2014), among many others.

Assumption 1: $\{Y^{1,1}, Y^{1,0}, Y^{0,0}\} \coprod D \mid X = x$ for all *x* in the support of *X*.

Assumption 1 states that the potential outcomes are jointly independent of voucher award conditional on *X*. This assumption rules out unobserved confounders affecting award and the potential outcomes after controlling for the covariates. Together with the first part of Assumption 3, it is sufficient for identifying the ATET. In contrast, the identification of θ, γ, and δ requires a further conditional independence assumption.

Assumption 2: $\{Y^{1,1}, Y^{1,0}, Y^{0,0}\} \coprod M \mid X = x, D = d$ for $d \in \{1,0\}$ and all *x* in the support of *X*.

If Assumption 2 holds, redemption is independent of the potential outcomes conditional on the covariates and voucher award. Assumptions 1 and 2 are closely related to conditions (4) and (5) in Petersen, Sinisi, and van der Laan (2006) for the identification of the controlled direct effect. They are also related to conditions (1) and (2) in VanderWeele (2009), again for identifying the controlled direct effect, and conditions (a) and (b) of the Weak Dynamic Conditional Independence Assumption in Lechner (2009) and Lechner and Miquel (2010) for the evaluation of dynamic treatments. The difference to the latter papers is, however, that they allow for different sets of covariates to control for confounding of *D* and M (where the covariates for *M* may be affected by *D*), whereas we (similarly to Petersen, Sinisi, and van der Laan, 2006) assume the same *X* for *D* and *M*. Further below, we argue that this appears reasonable in our application.

Assumption 3: $\Pr(D=1 \mid X=x) < 1$ and $0 < \Pr(M=1 \mid D=1, X=x) < 1$ for all *x* in the support of *X*.

The first part of Assumption 3 requires that no combination of covariates perfectly predicts voucher award, otherwise no comparable observations (in terms of conditioning variables *X*) without award (and thus, without redemption) exist, implying that Δ, θ and γ (which involve $Y^{0,0}$) cannot be identified. The second part requires that conditional on voucher award, no combination of covariates perfectly predicts redemption or non-redemption, otherwise δ (which involves both $Y^{1,1}$ and $Y^{1,0}$) cannot be identified.

Note that Assumptions 1 and 2 together imply the following conditional independence restriction: $\{Y^{1,1}, Y^{1,0}, Y^{0,0}\} \coprod \{D, M\} \mid X = x$ for all x in the support of X. This means that technically, the various combinations of D and M may (despite their sequentiality) be treated as distinct treatments in the identification of θ, γ, and δ based on conditioning on X. We can therefore analyse the effects of the various combinations $(D=1, M=1)$, $(D=1, M=0)$, and $(D=0, M=0)$ in a standard multiple treatment effect framework (for assessing a treatment that can take more than two value), as outlined in Imbens (2000) and Lechner (2001). It follows that

$$\begin{aligned}
&E(Y^{1,M^1} \mid D=1) = E(Y^1 \mid D=1) = E(Y \mid D=1),\\
&E(Y^{0,M^0} \mid D=1) = E(Y^{0,0} \mid D=1) = E(Y^0 \mid D=1) = E[E(Y \mid D=0, X=x) \mid D=1],\\
&E(Y^{d,m} \mid D=1) = E[E(Y \mid D=d, M=m, X=x) \mid D=1],
\end{aligned}$$

where the second and third lines are implied by Assumption 1 and Assumptions 1 and 2, respectively.[4]

However, directly controlling for the possibly high dimensional vector X when estimating $E[E(Y \mid D=0, X=x) \mid D=1]$ and $E[E(Y \mid D=d, M=m, X=x) \mid D=1]$ may be prone to the curse of dimensionality problem in nonparametric estimation. Rosenbaum and Rubin (1983) show that one may instead condition on the treatment propensity scores, in our case $p(x) = \Pr(D=1 \mid X=x)$ and $p_{dm}(x) = \Pr(D=d, M=m \mid X=x)$, respectively, which balance the distributions of X. Therefore, it also holds that

$$\begin{aligned}
&E(Y^{0,M^0} \mid D=1) = E\{E[Y \mid D=0, p(x)] \mid D=1\},\\
&E(Y^{d,m} \mid D=1) = E\{E[Y \mid D=d, M=m, p_{dm}(x)] \mid D=1\}.
\end{aligned}$$

[4] The derivation of these results is standard (e.g. Heckman, Ichimura, and Todd, 1998, Section 3) and therefore omitted.

This has the practical advantage that the vector of covariates is reduced to a single variable and, thus, circumvents the curse of dimensionality, at least if the propensity scores are well approximated by parametric probability models. The effects of interest may then be estimated by propensity score matching on estimates of $p(x)$ and $p_{dm}(x)$, respectively.

3.3 Estimators

Estimation of the various effects of interest (see Section 3.1) is based on radius matching on the propensity score with bias adjustment. Estimation is semi-parametric in the sense that the propensity scores $p_x(x)$ and $p_{dm}(x)$ are parametrically specified by probit models, while the models for the conditional expectations of the outcomes are unrestricted. Therefore, propensity score matching flexibly allows for effect heterogeneity in *X* and is more robust in terms of model specification than fully parametric methods.

To be concise, we use the matching algorithm of Lechner, Miquel, and Wunsch (2011), which is more precise than standard nearest-neighbour matching because it incorporates the idea of radius matching (e.g. Dehejia and Wahba, 2002). Furthermore, the procedure uses the initial matching weights for a (weighted) regression adjustment for bias reduction in a second step (see Abadie and Imbens, 2011). Therefore, the estimator satisfies a so-called double robustness property, meaning that it is consistent if either the matching step is based on a correctly specified selection model or the regression model is correctly specified (e.g., Rubin, 1979; Joffe et al., 2004). Moreover, the regression adjustment should reduce small sample as well as asymptotic biases of matching. Huber, Lechner, and Wunsch (2013) investigate the finite sample properties of this radius matching with bias adjustment algorithm along with many other matching type estimators and find it to be very competitive.

We match on the linear index of the probit specification of the propensity score and use a data-driven approach for the choice of the radius size. That is, we set the latter to 90% of the 0.9^{th} quantile of the distance between matched treated and control observations occurring in standard nearest-neighbour matching.[5] Other choices of the radius size did not affect the results importantly. Concerning inference, Abadie and Imbens (2008) show that bootstrap-based standard errors may be invalid for matching based on a fixed number of comparison observations. However, our matching algorithm is smoother than the latter approach because it (by the nature of radius matching) uses a variable number of comparisons that furthermore are distance-weighted within the radius and on top applies the regression adjustment. The bootstrap is therefore likely a valid inference procedure in our context. To be precise, inference is based on bootstrapping the respective effect 999 times and using the standard deviation of the bootstrapped effects as an estimate of the standard error in the t-statistic.

4 Empirical implementation

This section describes the data and the selection of our estimation sample.

4.1 Data

Our analysis is based on administrative data provided by the Federal Employment Agency of Germany, namely the Integrated Employment Biographies (IEB).[6] The latter contain information on all individuals in Germany who received a voucher between 2003 and 2004, along with subsequent participation in vocational training programmes. That is, the precise award and redemption dates for each voucher as well as the start and end dates of vocational trainings are observed. Furthermore, the data include detailed daily information on employment subject to social security contributions, the receipt of transfer payments during un-

5 If there is no comparison observation within the radius, then the nearest neighbour is matched.

6 The IEB is a rich administrative database and the source of the sub-samples of data used in all recent studies that evaluate German ALMP programmes (e.g., Biewen, Fitzenberger, Osikominu, and Paul, 2014, Lechner, Miquel, and Wunsch, 2011, Lechner and Wunsch, 2013, Rinne, Uhlendorff, and Zhao, 2013). The IEB is a merged data file containing individual records collected 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). IAB (Institut für Arbeitsmarkt- und Berufsforschung) is the abbreviation for the research department of the German Federal Employment Agency.

employment, job search, and participation in various active labour market programmes (type, duration), rich individual information (among others education, age, gender, marital status, profession, and nationality), and regional (labour market) characteristics. Thus, we are able to control for a wealth of personal characteristics and detailed labour market histories (e.g., type of employment, industry, occupational status, earnings) for all individuals receiving a voucher and thus capture the key confounders in such settings as identified by Lechner and Wunsch (2013). Furthermore, we also make use of a control sample of unemployed individuals without voucher award (and redemption) during the years 2003 and 2004. It also originates from the IEB and is constructed as a three per cent random sample of individuals who experience at least one transition from employment to non-employment (of at least one month) in 2003.[7]

4.2 Sample definition

The evaluation sample is constructed as an inflow sample into unemployment. It consists of individuals who became unemployed in 2003 after having been continuously employed for at least three months and were awarded a voucher before 2005. Entering unemployment is defined as the transition from (non-subsidised, non-marginal, non-seasonal) employment to registered non-employment of at least one month. We focus on individuals who are eligible for unemployment benefits at the time of inflow into unemployment. Thus, this sample focuses on the main target groups of these programmes. To exclude individuals eligible for specific labour market programmes targeting youths and individuals eligible for early retirement schemes, we only consider persons aged between 25 and 54 years at the beginning of their unemployment spell under consideration.

The treatment is defined as the first voucher award in the years 2003 and 2004, the mediator as voucher redemption for participation in some vocational training course. Redemp-

[7] We account for the different sampling probabilities whenever necessary by using sampling weights. Note that these probabilities differ for unemployed obtaining a voucher versus not obtaining a voucher, while they are identical within each of the two groups.

tion needs to take place within 3 months after award, otherwise the voucher expires. One concern regarding the treatment and mediator definitions is the timing with respect to the elapsed unemployment duration prior to award and redemption. Frederiksson and Johansson (2008) argue that in countries such as Germany, nearly all unemployed persons would receive ALMPs if their unemployment spells were sufficiently long. Individuals who find jobs quickly are less likely to be assigned to and receive training, because the treatment definition is restricted to unemployment periods. Accordingly, ignorance of the elapsed unemployment duration at treatment start could lead to a higher share of individuals with better labour market characteristics among non-treated than among treated. To address this problem, we randomly assign pseudo treatment start dates to each individual in the control group. Thereby, we recover the distribution of the elapsed unemployment duration at voucher award from the treatment group (similar to, e.g., Lechner and Smith, 2007). For comparability of the treatment definitions of the treated and non-treated groups, we only consider individuals who are unemployed at their (pseudo) voucher award. Following similar arguments, the same approach is applied to the (pseudo) voucher redemption time.[8] This makes to groups of individuals with redeemed and expired vouchers comparable with respect to their duration of unemployment.

4.3 Descriptive statistics

The baseline sample includes 93,016 (or 600,842 weighted) observations.[9] 41,138 observations are awarded a voucher in 2003 or 2004, whereas 51,878 are not. Of the former

[8] 592 individuals with expired vouchers are dropped because of the definition of the pseudo voucher redemption time.

[9] The IAB provided a data set that contains 230,842 (or 3,638,851 weighted) observations. This sample is representative for the inflow of unemployed in the years 2003 and 2004 subject to the following sample restrictions: previous employment of at least 3 months, some contact with the employment agency within the first three months of unemployment, unemployment durations of at least one month, eligible for unemployment benefits, and aged between 25 and 54 years. We do not consider treatments after 2004, because in January 2005 a large labour market reform took place in Germany (Hartz IV reform). Thus, we restrict our sample to individuals who become unemployed in the year 2003. This enables us to consider for all unemployed a potential treatment within the first twelve months of their unemployment spell. Further, we drop individuals with marginal, seasonal, or subsidised employment before their last unemployment spell. This leaves 124,696 observations. Another 31,680 observations are dropped because of the definition of the pseudo voucher award and redemption times. See the descriptive means of the initial and final sample in Table A.2 in Appendix A.

group, 33,077 individuals redeem their voucher, whereas 8,061 do not redeem it. Table 4.1 reports the means of selected observed characteristics across groups defined in terms of treatment and mediator states (see Table A.1 for a more extensive set of variables): voucher awarded, no voucher awarded, voucher redeemed, voucher expired (note that the last two groups are subsamples of the first group). Pairwise standardized mean differences (see Rosenbaum and Rubin, 1985) are also shown as measures of covariate balance. Information on individual characteristics refers to the time of inflow into unemployment. Only for the elapsed unemployment duration and the remaining eligibility for unemployment benefits, we consider the measurement at the time of the (pseudo) voucher award.

The descriptive statistics in Table 4.1 reveal that voucher recipients (1) and non-recipients (2) differ importantly in several socio-economic characteristics such as age, health, education, and profession. In particular, those awarded a voucher are younger, healthier, better educated, and have higher paying jobs. However, elapsed time in unemployment duration is higher for recipients and accordingly the remaining eligibility for unemployment benefits is lower. Regional differences appear to be limited. The regional differences are also more pronounced.

When comparing the samples of unemployed with redeemed (3) to those with expired vouchers (4), differences in socio-economic variables are often small, with the important exception that the latter group more likely suffers from incapacities (and health problems in general), which may to an important extent drive non-redemption. Furthermore, while the employment histories are quite comparable, non-redeemers have higher elapsed unemployment durations and thus a lower eligibility to unemployment benefits at voucher award than redeemers.

Table 4.1: Means and standardized biases of selected variables

Voucher	*… awarded*		*… redeemed*		*Comparisons of groups*			
	yes	no	yes	no				
	(1)	(2)	(3)	(4)	(1) - (2)	(1) - (3)	(1) - (4)	(3) - (4)
	Subsample means				Standardized differences			
Individual characteristics								
Age	39.03	41.75	39.01	39.11	31.44	0.27	1.10	1.38
Children under 3 years	0.43	0.35	0.43	0.41	15.06	0.58	2.39	2.97
Health problems	0.02	0.06	0.02	0.03	20.08	2.08	7.35	9.38
Incapacities	0.12	0.19	0.11	0.17	19.53	3.70	13.75	17.43
No German citizenship	0.07	0.10	0.07	0.08	10.35	0.60	2.40	3.00
No schooling degree	0.04	0.07	0.04	0.04	13.74	0.33	1.34	1.67
University entry degree (Abitur)	0.23	0.17	0.24	0.23	17.21	0.40	1.66	2.06
Elementary occupation	0.07	0.10	0.07	0.07	11.15	0.21	0.87	1.09
Craft, machine operators & related	0.29	0.35	0.29	0.28	13.60	0.37	1.52	1.89
Clerks	0.25	0.16	0.25	0.25	23.08	0.02	0.10	0.12
Individual labour market history								
Half months employ. in last 2 years	45.17	44.30	45.19	45.12	12.33	0.21	0.83	1.04
Half months OLF in last 2 years	1.59	2.19	1.59	1.63	11.74	0.17	0.67	0.84
Cumulative earnings in last 4 years	91’258	84’199	91’126	91’799	14.71	0.27	1.10	1.37
Months of remaining UE benefits	8.90	10.95	9.14	7.92	31.44	4.09	16.42	20.54
Elapsed unemployment duration	4.46	3.76	4.19	5.56	21.04	8.08	30.56	38.81
Regional characteristics								
Share of empl. in construction ind.	0.06	0.06	0.06	0.06	7.17	2.64	11.20	13.84
Share of vacant fulltime jobs	0.78	0.78	0.78	0.77	3.94	2.84	10.63	13.46
Population per km^2	965	868	919	1156	5.72	2.69	10.02	12.69
Unemployment rate (in %)	12.33	12.53	12.33	12.36	3.69	0.13	0.52	0.65
Observations	41'138	51'878	33'077	8'061				
Sum of weighted observations	41'138	559'704	33'077	8'061				

Note: See Rosenbaum and Rubin (1985) for a definition of the standardized difference. They consider an absolute standardized difference of more than 20 as being “large”. The full set of results is contained in Table A.1 in Appendix A.

Figures 4.1 and 4.2 show the evolvement of two key outcome variables over time, namely employment and registered unemployment (further outcome variable are presented in Appendix A). Not surprisingly, over a horizon of 4 years (48 months) after voucher award, employment rates reach about 60% and registered unemployed falls below 20% for all groups.

Comparing the development within the different groups, there is an obvious difference depending on the time horizon. In the short-run, the groups participating at least partly in a training programme appear to experience so-called lock-in effects, i.e. they take up fewer jobs than the two other groups. In the longer run, this effect disappear and the groups subject to the programmes experience higher employment rates (and similar unemployment rates) than the two non-participating groups. The econometric analysis below will reveal how much of these differences can be attributed to the effects of the treatment (obtaining a voucher) and the mediator (redeeming it).

Figure 4.1: Mean employment

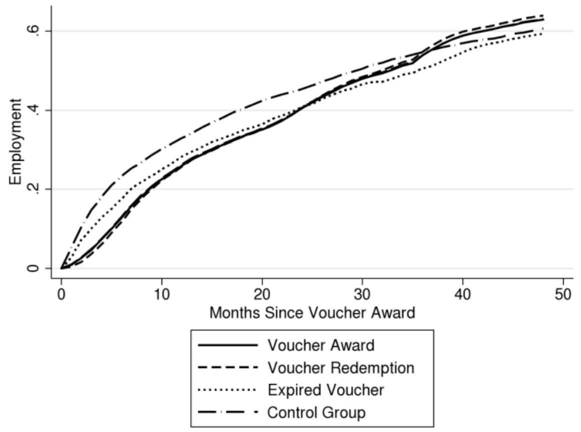
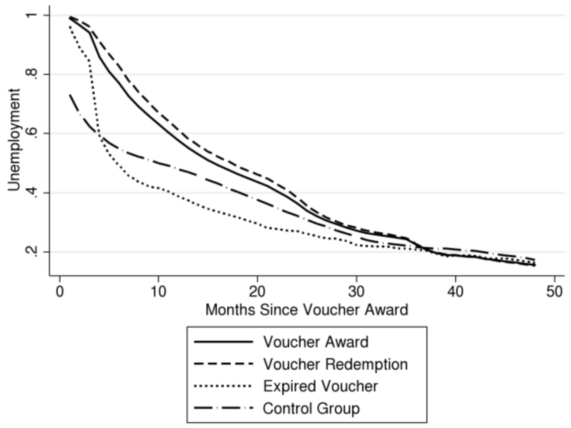


Figure 4.2: Mean unemployment



Note: Group means adjusted to the population of individuals awarded with a voucher.

5 The selection processes

5.1 Variables

Our identification strategy requires observing all variables that jointly affect (i.e. confound) voucher award and the outcome and/or voucher redemption and the outcome in a relevant way. It is therefore essential to understand which factors affect both voucher award and redemption.

Concerning voucher award, the analyses of Biewen, Fitzenberger, Osikominu, and Paul (2014) and Lechner and Wunsch (2013) based on German labour market data suggest that so-called pre-treatment outcomes (e.g. lagged employment and wages measured prior to the intervention or treatment of interest), the prior benefit receipt history, socio-economic factors, and local labour market characteristics are important confounders that need to be controlled for when evaluating ALMPs. This information is available in our data. In particular, the individuals' labour market histories are observed up to four years prior to unemployment and regional factors can also be controlled for at the level of the local employment agency districts.

While Doerr, Fitzenberger, Kruppe, Paul, and Strittmatter (2014) argue voucher award involves a similar selection process as assignment of ALMPs in general, they also point out that it is left to the discretion of the caseworker. An advantage of our data is that it also contains information collected by the caseworker as base for counselling and assignment decisions, namely: information on the job-seeker's current and previous health status (physical and mental), proxy variables indicating whether an unemployed person lacks motivation (e.g. whether she/he dropped out of a past program or benefits were withdrawn), and former sanctions.

Concerning actual redemption, Kruppe's (2009) analysis of redemption behaviour points to the fact that individuals with bad labour market prospects are less likely to redeem their vouchers. On top of this limited evidence in the literature, we suspect that the latter is affected by similar factors (although perhaps in different way) as voucher assignment. In particular, the previous labour market history, socio-economic characteristics like education and age, and local labour market conditions should importantly influence an unemployed individual's decision to participate in a vocational training, as a function of the (personal assessment of the) expected benefits. Furthermore, (physical and mental) health and personality traits associated with motivation and compliance in the counselling process (approximated by benefit withdrawal and programme drop out) are likely to affect participation.

Given that vouchers have to be redeemed within three months, time-varying (or dynamic) confounding of redemption due to important changes in control variables after voucher award but prior to voucher redemptions should not be an issue. To verify this argument, we estimated the effects of voucher assignment on a range of covariates measured at redemption time, which were all close to and not statistically different from zero. We therefore control for the same set of covariates measured at the same point in time to tackle selection into both voucher award and redemption, namely gender, age, family background, health and incapacities, nationality, school and vocational education, occupation, complete employment and welfare history of the last four years, past programme and sanction experience, timing and region of unemployment, economic indicators at the level of the local employment agency (see Table B.1 in Appendix B for the full set of control variables used).

5.2 Empirical results

Table 5.1 shows the results of two propensity score estimations that relate to the selection into treatment and mediator states for selected variables (see Table B.1 in Appendix B for a full set of results). They are based on a probit model.

Table 5.1: Selected average marginal effects from propensity score estimation

	Award Probability		*Redemption Probability*	
	Marg. Eff. (in %)	Std. Error	Marg. Eff. (in %)	Std. Error
	(1)	(2)	(3)	(4)
Individual characteristics				
Age	-.034***	(.0001)	.068*	(.0004)
Older than 50 years	-9.86***	(.0031)	-4.90***	(.0163)
Children under 3 years	1.14***	(.0014)	.187	(.0048)
Health problems	-3.78***	(.0028)	-4.48***	(.0122)
Incapacities	-2.95***	(.0016)	-6.27***	(.0057)
No German citizenship	-1.65***	(.0022)	-.274	(.0078)
No schooling degree	-2.80***	(.0027)	-.406	(.0106)
University entry degree (Abitur)	.743***	(.0021)	.812	(.0059)
Elementary occupation	.299	(.0026)	.385	(.0100)
Craft, machine operators & related	.743***	(.0022)	.768	(.0080)
Clerks	3.98***	(.0022)	1.173*	(.0072)
Individual labour market history				
Half months empl. in last 2 years	-.037	(.0003)	.179	(.0012)
Half months OLF in last 2 years	-.071**	(.0004)	-.033	(.0013)
Remaining unempl. insurance claim	.150***	(.00005)	.131***	(.0002)
Cum. half months empl. in last 4 y.	.034***	(.0001)	.074***	(.0002)
Cumulative earnings in last 4 years	.00001***	($2.1 \cdot 10^{-8}$)	-.00003***	($6.9 \cdot 10^{-8}$)
Regional characteristics				
Share of empl. in construction	5.17	(.0606)	-4.89	(.1987)
Share of vacant fulltime jobs	.512	(.0074)	13.2***	(.0218)
Population per km^2	.0004***	($6.3 \cdot 10^{-7}$)	-.001***	($1.9 \cdot 10^{-6}$)
Unemployment rate (in \%)	-.037	(.0003)	.131	(.0009)
Unconditional probability…	6.85%		80.4%	
Sample size (weighted)	93'016 (600'842)		41'138 (41'138)	

Note: Asterisks indicate significant marginal effects at the 1% (***), 5% (**), and 10% (*) level, respectively. Probit model used. Heteroscedasticity robust standard errors are in parentheses. The complete set of variables is contained in Table B.1 in Appendix B.

By and large these results confirm the impression obtained by the univariate comparisons of Table 4.1. Again, it appears that the group receiving a voucher has overall better labour market prospects than the control group, with regional characteristics playing only a very limited role. For the second comparisons, such regional characteristics seem to play a larger role, while individual differences exist, but are far less pronounced than for the first comparison. The clear exemption to this general finding is the state of health which appears to be a key factor related to voucher redemption in the sense that bad health substantially reduces the probability to redeem a voucher.

6 Results

The propensity score estimates outlined Table 5.1 serve as inputs into the matching procedures. When performing matching, three potential concerns should be addressed: i) insufficient support in the propensity scores across treatment states; ii) large weights (in the estimation) of observations with extreme propensity scores (which should entail some form of trimming); and iii) the failure of matching to remove differences in the covariates that are relevant in the propensity score estimation. In our application, insufficient support and extreme observations are not an issue problem, as can be seen from the distributions of the propensity scores in the different groups (details in Figures B.1 and B.2 in the Appendix B). Furthermore, all important covariates are well balanced such that no substantial differences remain after matching (for details see Table B.2 in Appendix B).[10]

Figures 6.1 and 6.2 provide the estimates of the average employment and unemployment effects for voucher recipients, namely the (total) impacts of voucher award vs. non-award (Δ), and the effects of voucher award and redemption vs. non-award (θ), voucher award without redemption vs. non-award (γ), and voucher award with redemption vs. voucher award without redemption (δ). Concerning employment, we consider only (non-marginal, non-subsidised) employment of at least one month. The lines reflect the effect magnitudes on the probability to be employed or unemployed in a particular month after voucher

[10] Note that the balancing property of propensity score matching is automatic if the propensity score is correctly specified. Thus, these statistics essentially serve as tool to check the specifications of the two probit models estimated.

assignment over 4 years (48 months). Superimposed symbols on the lines (diamonds) indicate effects that are (pointwise) statistically significantly different from zero.

The results in Figure 6.1 suggest that awarding a voucher has a negative (total) employment impact among voucher recipients in the first three years, in particular in the initial months where the employment probability decreases by as much as 10 percentage points. This dip points to the so-called lock-in effect likely due to reduced job search in response to (anticipated) participation in a vocational training. However, after a gradual fade-out of the negative impact over roughly three years, the employment probability is statistically significantly increased by approximately 2 to 3 percentage points in the fourth year. The positive employment effect appears quite stable and may therefore even last in the longer run. This suggests that the voucher award system successfully compensates the initial lock in effect by a higher placement success in later periods.

Figure 6.1: Employment

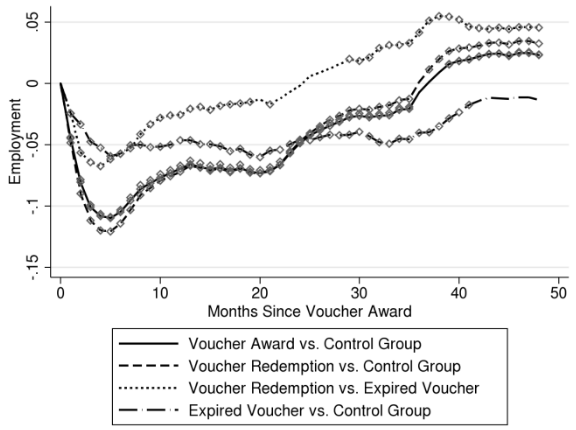


Figure 6.2: Registered unemployment

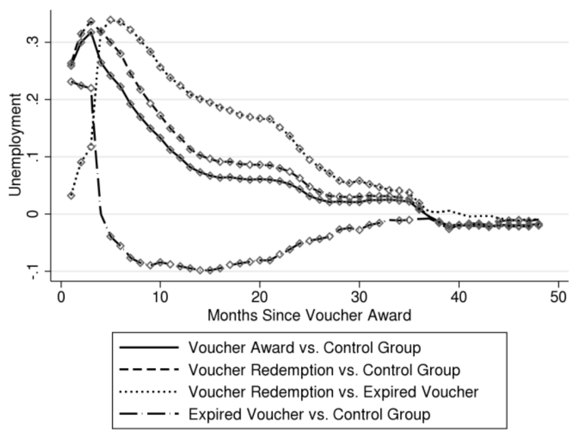


Figure 6.2 shows that the time patterns for unemployed are (as expected) reversed, but initially larger in magnitude. This is due to the fact that in the short run the award of a voucher reduces also the drop-out from the labour market as shown by the effect on the labour market state out-of-labour force (see Figure C.2 in Appendix C for details). Similar in mag-

nitude (but with opposite sign) to the employment effect, in the long-run registered unemployment is reduced somewhat.

Investigating the causal mechanisms underlying the total employment effect (with essentially symmetric results for registered unemployment), it becomes apparent that it is predominantly redemption (e.g. participation in/start of a vocational training) which drives the results. In fact, the estimated effect of voucher award and redemption vs. no award (θ) closely follows the overall impact of voucher award, albeit it is somewhat more negative in earlier and more positive in later periods. In contrast, the direct effect of voucher award without redemption (γ) is insignificant and close to zero in most of the fourth year. This suggests that in the long run, mere voucher assignment does not affect for instance the preferences for human capital investments in a way that they influence employment success.

We nevertheless find a negative direct effect in the short run: Even without actual redemption, voucher award decreases the employment probability in the first three to 3.5 years and therefore appears to reduce job search activities. This may be rooted in the learning and decision process about the supply of vocational training. Individuals may initially reduce job search intensity in response to voucher award and consider the programmes available. Some of them may then not be satisfied with the programmes available and therefore decide not to redeem the voucher. Rather, they try to find employment again.

We would expect this initial *direct* lock-in effect to be less severe than for the total effect (which also includes the impact of actual redemption leading to training participation), as individuals should be sooner available for intensive job search when foregoing redemption. Indeed, we find that in the initial periods, the estimated γ is considerably less negative than the estimates of Δ and in particular of θ (redemption vs. non-award). Accordingly, the estimate of δ (redemption vs. award without redemption) is initially negative (as $\delta = \theta - \gamma$) and significantly so. However, with regard to later periods, redemption pays off for the population of all voucher recipients: After roughly two years, the estimates of Δ and θ dominate those of γ and the estimates of δ are significant and non-negligible (up to 5 percentage points) in later periods.

Besides employment and unemployment, we considered several other outcome variables (presented in Appendix C). One of these additional variables is a measure for employment stability, i.e. being employed for at least 6 months. For the latter variable, the outcome evaluation window only starts in month 7 after voucher award. The estimates of the (total) impact of voucher award vs. non-award (Δ) and voucher award and redemption vs. non-award (θ) on stable employment are qualitatively similar to those on employment, albeit the former become significantly positive at a later point in time and are of a somewhat smaller magnitude. In contrast to Figure 6.1, the estimate of the direct effect (γ) now remains statistically significantly negative until the end of the evaluation window (implying that the adverse effect of not redeeming a voucher vs. not getting one is more severe for stable employment), even though it shows an upward tendency.

Furthermore, we investigated the effects on full time employment, defined as an indicator for working on a position with 100% fulltime equivalent. Again, the results are qualitatively similar to the employment effects of Figure 6.1, including an insignificant direct effect in the fourth year after voucher award. Similar conclusions apply to the effects on monthly earnings: After an initial lock in phase, the estimates of Δ, θ, and δ are moderately positive (between 30 and 70 EUR) and statistically significantly so in the fourth year, while that of γ approaches zero.

7 Conclusion

Using rich administrative labour market data from Germany, we evaluated the effectiveness of awarding vouchers for vocational training programmes to unemployed individuals.

We found an overall negative short term but positive longer run effect on the employment of voucher recipients. As a key contribution, we also investigated alternative causal mechanisms through which the overall effect materializes, using a sequential conditional independence assumption. In particular, we considered the direct employment effect of voucher assignment (net of actual redemption), which may for instance be driven by decreased job search intensity during the assessment of training options, or increased awareness about (and a changed preference for) human capital investments in general.

The direct effect turned out to be insignificant in the longer run, but negative in the short run (albeit less so than the overall impact), pointing to a decreased search intensity shortly after voucher assignment (despite non-redemption). In contrast, the effect of actual voucher redemption (vs. non-award and non-redemption) closely follows the overall effect, albeit it is somewhat more negative in earlier and more positive in later periods. Comparing the latter to the direct effect suggests that conditional on voucher assignment, redemption (and thus, actual programme participation) entails a more severe negative (or lock in) effect on voucher recipients than under non-redemption, which is intuitive because individuals not redeeming are sooner available for the labour market. In the longer run, however, redemption pays off and significantly increases the employment probability by roughly two to three percentage points when compared to non-award in the fourth (and last observed) year after voucher assignment.

From a policy perspective, our results suggest that the introduction of a voucher award system, which was embraced for promoting the self-responsibility of training participants and competition among training providers, may come with an efficiency loss in the case that individuals do not make use of the awards: non-redemption entails lower employment chances than redemption in the long run and non-assignment in the short run and therefore appears to be the least attractive option. This needs to be taken into account when designing the provision of active labour market policies through a voucher award system.

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Appendix A: Descriptive statistics

Table A.1 contains the descriptive statistics for the full set of variables used in the estimation of the propensity scores.

Table A.1: Means and standardized biases for all control variables

Voucher	*… awarded*		*… redeemed*					
	yes	no	yes	no	Standardized difference			
	(1)	(2)	(3)	(4)	(1) - (2)	(1) - (3)	(1) - (4)	(3) - (4)
Female	0.46	0.43	0.46	0.46	4.91	0.11	0.44	0.54
Age	39.03	41.75	39.01	39.11	31.44	0.27	1.10	1.38
Older than 50 years	0.01	0.12	0.01	0.02	43.59	1.02	3.79	4.80
Children under 3 years	0.43	0.35	0.43	0.41	15.06	0.58	2.39	2.97
Married	0.30	0.27	0.30	0.32	8.09	0.75	3.06	3.82
Health problems	0.02	0.06	0.02	0.03	20.08	2.08	7.35	9.38
Received sanctions	0.01	0.01	0.01	0.01	0.35	0.07	0.27	0.33
Incapacities	0.12	0.19	0.11	0.17	19.53	3.70	13.75	17.43
Proxy for motivation lack	0.09	0.09	0.09	0.10	2.43	0.66	2.63	3.29
No German citizenship	0.07	0.10	0.07	0.08	10.35	0.60	2.40	3.00
No schooling degree	0.04	0.07	0.04	0.04	13.74	0.33	1.34	1.67
University entry degree (Abitur)	0.23	0.17	0.24	0.23	17.21	0.40	1.66	2.06
No vocational degree	0.21	0.22	0.21	0.21	3.34	0.26	1.08	1.34
Academic degree	0.12	0.09	0.12	0.11	7.58	0.35	1.47	1.82
White-collar	0.39	0.49	0.39	0.38	20.74	0.24	0.98	1.22
Elementary occupation	0.07	0.10	0.07	0.07	11.15	0.21	0.87	1.09
Skilled agriculture & fishery workers	0.01	0.02	0.01	0.01	7.14	0.35	1.50	1.84
Craft, machine operators & related	0.29	0.35	0.29	0.28	13.60	0.37	1.52	1.89
Clerks	0.25	0.16	0.25	0.25	23.08	0.02	0.10	0.12
Technicians & assoc. professionals	0.16	0.12	0.16	0.16	9.97	0.27	1.08	1.35
Professionals & managers	0.12	0.10	0.12	0.12	5.86	0.10	0.41	0.51
Half months empl. in last 6 months	11.95	11.92	11.95	11.96	7.29	0.27	1.14	1.41
Employed before 4 year	0.67	0.67	0.68	0.66	0.02	0.62	2.51	3.13
Half months empl. in last 2 years	45.17	44.30	45.19	45.12	12.33	0.21	0.83	1.04
Half months unempl. in last 6 months	0.10	0.09	0.10	0.10	1.96	0.25	1.04	1.28
Half months unempl. in last 2 years	0.48	0.49	0.48	0.46	0.38	0.22	0.93	1.15
H. mo. since last unempl. in last 2 y.	45.91	44.87	45.91	45.92	14.28	0.03	0.14	0.17
No unempl. in last 2 years	0.89	0.89	0.89	0.89	0.19	0.15	0.63	0.78
Unemployed in last 2 years	0.04	0.04	0.04	0.04	2.99	0.16	0.68	0.84
# unemployment spells in last 2 years	0.15	0.15	0.15	0.14	0.09	0.31	1.31	1.62
Any program in last 2 years	0.05	0.05	0.05	0.05	2.06	0.14	0.59	0.73
Half months OLF in last 2 years	1.59	2.19	1.59	1.63	11.74	0.17	0.67	0.84
# OLF spells in last 2 years	0.15	0.21	0.15	0.15	13.81	0.04	0.15	0.19
Half months since last OLF in last 2 y.	45.43	43.97	45.43	45.44	17.77	0.03	0.12	0.15
Half months OLF in last6 months	0.02	0.05	0.02	0.02	8.21	0.25	1.09	1.34
No OLF in last 2 years	0.87	0.82	0.87	0.87	12.57	0.03	0.10	0.13
OLF in last 2 years	0.11	0.12	0.11	0.11	3.94	0.38	1.54	1.91
Remaining unempl. insurance claim	25.35	20.32	25.57	24.44	35.62	1.65	6.55	8.20
Cum. half months empl. in last 4 y.	80.92	79.48	81.06	80.34	6.56	0.64	2.58	3.22
Cumulative earnings in last 4 years	91258	84199	91126	91799	14.71	0.27	1.10	1.37
Cumulative benefits in last 4 years	3.10	3.45	3.07	3.18	4.36	0.27	1.10	1.37
Eligibility unempl. benefits	8.90	10.95	9.14	7.92	31.44	4.09	16.42	20.54
Elapsed unempl. duration	4.46	3.76	4.19	5.56	21.04	8.08	30.56	38.81

Table A.1 to be continued.

Table A.1 continued …

Voucher	… awarded		… redeemed		Standardized difference			
	yes	no	yes	no				
	(1)	(2)	(3)	(4)	(1) - (2)	(1) - (3)	(1) - (4)	(3) - (4)
Start unempl. in January	0.08	0.10	0.08	0.08	6.33	0.17	0.69	0.86
Start unempl. in February	0.08	0.09	0.08	0.08	3.94	0.07	0.27	0.34
Start unempl. in March	0.09	0.09	0.09	0.10	1.18	0.45	1.80	2.25
Start unempl. in April	0.09	0.09	0.09	0.09	1.10	0.23	0.94	1.16
Start unempl. in June	0.07	0.08	0.07	0.08	4.96	1.13	4.41	5.53
Start unempl. in July	0.09	0.08	0.09	0.10	2.35	0.50	2.03	2.53
Start unempl. in August	0.10	0.08	0.10	0.09	5.79	0.79	3.32	4.10
Start unempl. in September	0.12	0.08	0.13	0.10	14.88	1.65	7.12	8.76
Start unempl. in October	0.11	0.08	0.11	0.10	7.99	0.19	0.79	0.98
Start unempl. in November	0.07	0.08	0.07	0.07	3.33	0.15	0.60	0.75
Start unempl. in December	0.05	0.08	0.05	0.05	12.21	0.42	1.67	2.09
Baden-Württemberg	0.09	0.11	0.09	0.12	5.26	1.82	7.04	8.86
Bavaria	0.15	0.14	0.15	0.14	3.83	0.93	3.91	4.84
Berlin, Brandenburg	0.10	0.09	0.09	0.12	2.08	2.09	8.01	10.09
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.08	0.09	0.08	0.07	4.28	0.89	3.78	4.67
Hesse	0.07	0.07	0.07	0.07	0.50	0.51	2.04	2.55
Northrhine-Westphalia	0.23	0.21	0.24	0.21	5.49	1.25	5.23	6.47
Rhineland Palatinate, Saarland	0.06	0.05	0.06	0.09	4.54	2.93	10.69	13.59
Saxony-Anhalt, Saxony, Thuringia	0.11	0.15	0.12	0.09	9.54	1.97	8.65	10.61
Share of empl. in the production	0.25	0.25	0.25	0.25	3.88	0.86	3.49	4.35
Share of empl. in the construction	0.06	0.06	0.06	0.06	7.17	2.64	11.20	13.84
Share of empl. in the trade industry	0.15	0.15	0.15	0.15	1.38	1.24	5.12	6.36
Share of male unempl.	0.56	0.56	0.56	0.56	1.44	0.05	0.21	0.26
Share of non-German unempl.	0.14	0.14	0.14	0.15	4.03	2.37	9.75	12.13
Share of vacant fulltime jobs	0.78	0.78	0.78	0.77	3.94	2.84	10.63	13.46
Population per km^2	965	868	919	1156	5.72	2.69	10.02	12.69
Unemployment rate (in \%)	12.33	12.53	12.33	12.36	3.69	0.13	0.52	0.65
Observations	41'138	51'878	33'077	8'061				
Sum Weighted Obs	41'138	559'704	33'077	8'061				

Note: See Rosenbaum and Rubin (1985) for a definition of the standardized difference.

Table A.2: Difference in means of some important control variables in the initial and final sample.

	Means in the		Difference
	…initial sample	…final sample	
	(1)	(2)	(3)
Female	0.42	0.43	-0.01
Age	38.77	41.56	-2.79
Children under 3 years	0.41	0.36	0.05
Married	0.48	0.27	0.21
Received sanctions	0.03	0.01	0.02
Incapacities	0.21	0.19	0.03
Proxy for motivation lack	0.14	0.09	0.05
University entry degree (Abitur)	0.17	0.17	-0.01
Vocational degree	0.67	0.69	-0.02
Academic degree	0.08	0.09	-0.01

Note: The descripitive statistics for the initial sample are based on the 230,842 observations initially provided by the IAB. The descriptive of the final sample are calculated after the sample selection (see discussion Section 4.3).

Figures A.1 to A.3 contain the evolvement of the matched levels of the outcome variables considered, while Figures A.4 to A.8 show the respective (unadjusted) differences.

Figure A.1: Mean of stable employment

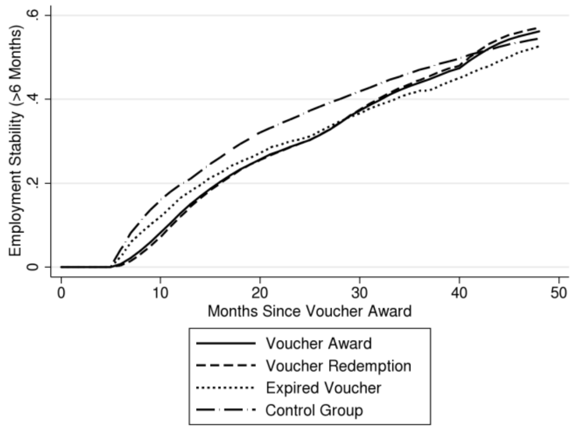


Figure A.2: Mean of full time employment

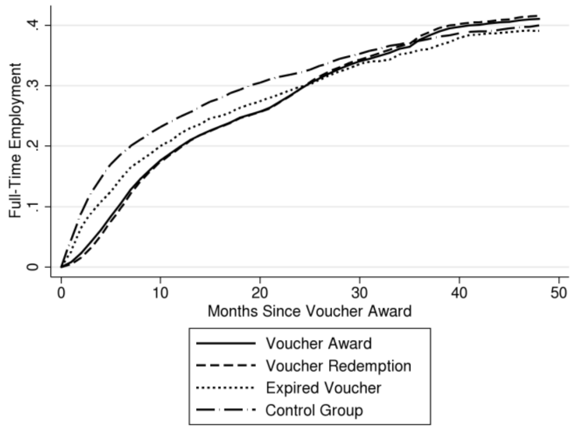


Figure A.3: Mean monthly earnings

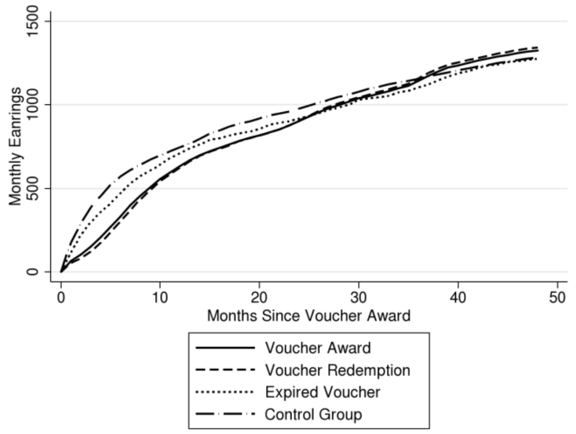


Figure A.4: Differences of employment

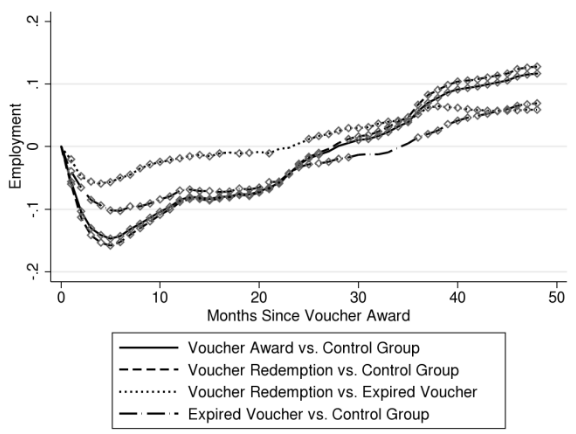


Figure A.5: Differences of stable employment

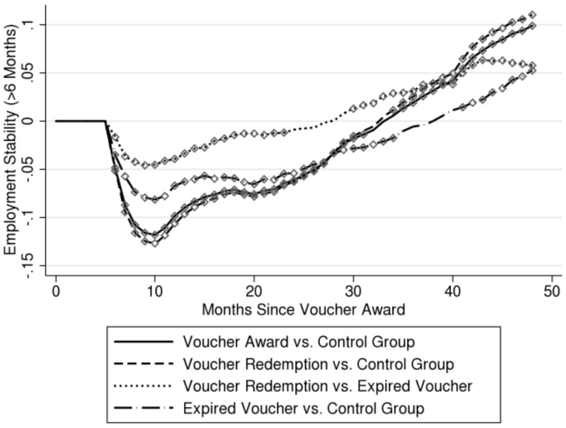


Figure A.6: Differences of full time employment

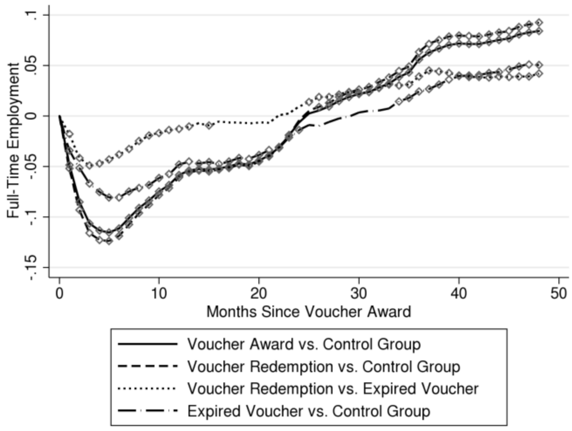


Figure A.7: Differences of monthly earnings

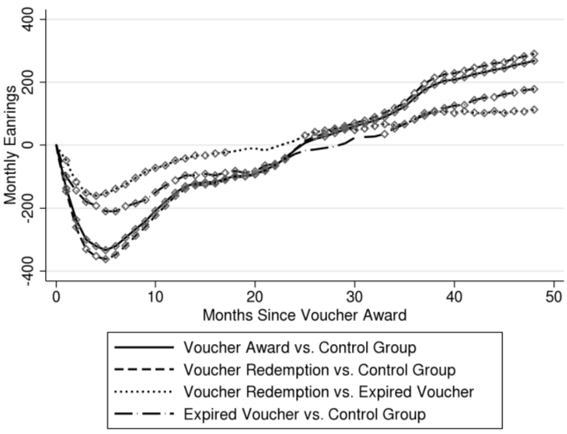
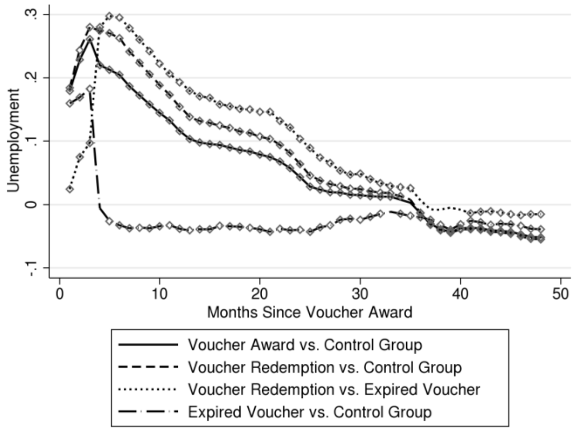


Figure A.8: Differences of unemployment



Appendix B: Matching

Table B.1 shows the full set of average marginal effects for the two relevant propensity scores.

Table B.1: Average marginal effects from propensity score estimation

	Award Probability		*Redemption Probability*	
	Marg. Eff. (in %)	Std. Error	Marg. Eff. (in %)	Std. Error
	(1)	(2)	(3)	(4)
Female	.393***	(.0015)	.337	(.0049)
Age	-.034***	(.0001)	.068*	(.0004)
Older than 50 years	-9.86***	(.0031)	-4.90***	(.0163)
Children under 3 years	1.14***	(.0014)	.187	(.0048)
Married	.912***	(.0016)	-.243	(.0051)
Health problems	-3.78***	(.0028)	-4.48***	(.0122)
Received sanctions	-.0384	(.0068)	1.02	(.0226)
Incapacities	-2.95***	(.0016)	-6.27***	(.0057)
Proxy for motivation lack	.243	(.0022)	-2.53***	(.0071)
No German citizenship	-1.65***	(.0022)	-.274	(.0078)
No schooling degree	-2.80***	(.0027)	-406	(.0106)
University entry degree (Abitur)	.743***	(.0021)	.812	(.0059)
No vocational degree	1.57***	(.0017)	-.007	(.0054)
Academic degree	-.893***	(.0029)	1.25	(.0081)
White-collar	-.586***	(.0018)	-.202	(.0060)
Elementary occupation	.299	(.0026)	.385	(.0100)
Skilled agriculture & fishery workers	-1.52***	(.0049)	2.723	(.0222)
Craft, machine operators & related	.743***	(.0022)	.768	(.0080)
Clerks	3.98***	(.0022)	1.173*	(.0072)
Technicians & assoc. professionals	2.56***	(.0024)	.052	(.0079)
Professionals & managers	1.54***	(.0029)	-0.69	(.0090)
Half months empl. in last 6 months	-.606*	(.0019)	-.126	(.0071)
Employed before 4 year	-.094	(.0020)	-.145	(.0069)
Half months empl. in last 2 years	-.037	(.0003)	.179	(.0012)
Half months unempl. in last 6 months	.018	(.0013)	-.288	(.0042)
Half months unempl. in last 2 years	.295***	(.0007)	.343	(.0027)
H. mo. since last unempl. in last 2 y.	.154***	(.0002)	.082	(.0007)
No unempl. in last 2 years	.204	(.0042)	1.68	(.0148)
Unemployed in last 2 years	-.015	(.0039)	2.13	(.0139)
# unemployment spells in last 2 years	.741**	(.0032)	2.11*	(.0117)
Any program in last 2 years	2.33***	(.0030)	-.169	(.0101)
Half months OLF in last 2 years	-.071**	(.0004)	-.033	(.0013)
# OLF spells in last 2 years	.695	(.0048)	.038	(.0182)
Half months since last OLF in last 2 y.	.002	(.0002)	-.122*	(.0007)
Half months OLF in last 6 months	-.005	(.0027)	.294	(.0112)
No OLF in last 2 years	1.67***	(.0062)	-1.75	(.0228)
OLF in last 2 years	.583	(.0037)	-.178	(.0132)
Remaining unempl. insurance claim	.150***	(.00005)	.131***	(.0002)
Cum. half months empl. in last 4 y.	.034***	(.0001)	.074***	(.0002)
Cumulative earnings in last 4 years	.00001***	($2.1\cdot10^{-8}$)	-.00003***	($6.9\cdot10^{-8}$)
Cumulative benefits in last 4 years	-.060***	(.0001)	.014	(.0004)
Eligibility unempl. benefits	-.083***	(.0002)	-.153***	(.0006)
Elapsed unempl. duration	.409***	(.0003)	-1.62***	(.0009)

Table B.1 to be continued.

Table B.1 continued …

	Award Probability		Redemption Probability	
	Marg. Eff. (in %) (1)	Std. Error (2)	Marg. Eff. (in %) (3)	Std. Error (4)
Start unempl. in January	.438	(.0028)	3.20***	(.0100)
Start unempl. in February	.679**	(.0029)	3.04***	(.0100)
Start unempl. in March	1.74***	(.0029)	2.50***	(.0095)
Start unempl. in April	2.00***	(.0029)	3.23***	(.0097)
Start unempl. in June	.518*	(.0029)	.835	(.0101)
Start unempl. in July	1.80***	(.0029)	2.33**	(.0096)
Start unempl. in August	2.71***	(.0030)	4.97***	(.0097)
Start unempl. in September	4.28***	(.0030)	5.75***	(.0093)
Start unempl. in October	2.90***	(.0029)	2.18**	(.0094)
Start unempl. in November	.765***	(.0030)	1.74*	(.0104)
Start unempl. in December	-1.33***	(.0030)	.938	(.0111)
Baden-Württemberg	-2.58***	(.0034)	-5.63***	(.0109)
Bavaria	-.700***	(.0028)	.098	(.0089)
Berlin, Brandenburg	-1.98***	(.0041)	-.031	(.0132)
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	-1.10***	(.0031)	1.191	(.0105)
Hesse	-1.16***	(.0032)	-2.534***	(.0101)
Northrhine-Westphalia	-.119	(.0024)	.125	(.0075)
Rhineland Palatinate, Saarland	.039	(.0036)	-10.3***	(.0094)
Saxony-Anhalt, Saxony, Thuringia	-2.65***	(.0001)	3.39***	(.0123)
Share of empl. in production	2.09*	(.0121)	17.3***	(.0398)
Share of empl. in construction	5.17	(.0606)	-4.89	(.1987)
Share of empl. in trade industry	-30.0***	(.0469)	19.5	(.1583)
Share of male unemployed	-6.48***	(.0244)	14.5*	(.0802)
Share of non-German unemployed	-5.01***	(.0154)	-8.11*	(.0495)
Share of vacant fulltime jobs	.512	(.0074)	13.2***	(.0218)
Population per km^2	.0004***	($6.3\cdot10^{-7}$)	-.001***	($1.9\cdot10^{-6}$)
Unemployment rate (in \%)	-.037	(.0003)	.131	(.0009)
Unconditional probability…	6.85%		80.4%	
Sample size	93'016		41'138	
Weighted sample size	600'842		41'138	

Note: Asterisks indicate significant marginal effects at the 1% (***), 5% (**), and 10% (*) level, respectively. Heteroscedasticity robust standard errors are in parentheses.

Figures B.1 and B.2 show the distributions of the two relevant propensity scores among the relevant treated or mediated and respective control groups. Overlap problems are not visible.

Figure B.1: Histogramme of propensity by voucher awards status (yes=1)

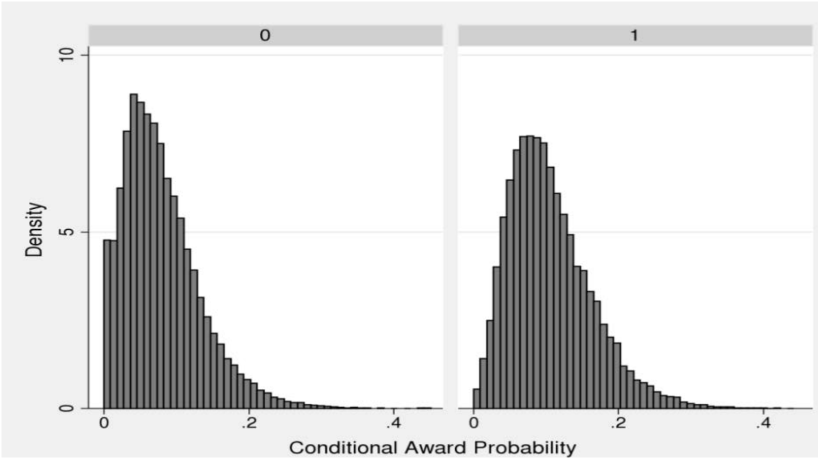


Figure B.2: Histogramme of propensity by voucher redemption status (yes=1)

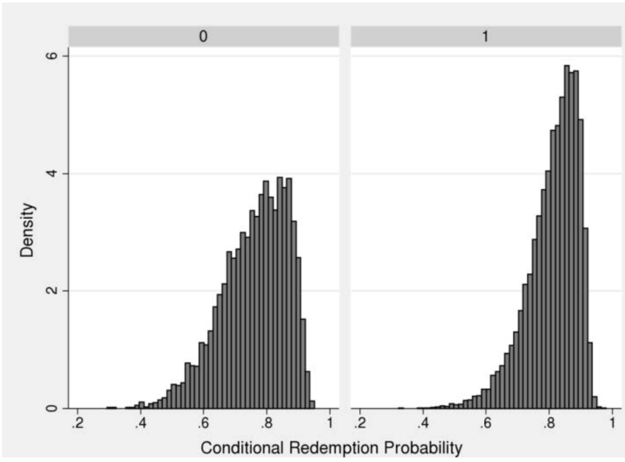


Table B.2 shows the means and the standardized biases of the covariates after matching.

Table B.2: Means and standardized biases after matching

Voucher	*… awarded*		*… redeemed*					
	yes	no	yes	no	Standardized difference			
	(1)	(2)	(3)	(4)	(1) - (2)	(1) - (3)	(1) - (4)	(3) - (4)
Female	0.459	0.458	0.459	0.454	0.23	0.04	0.85	0.89
Age	39.03	39.32	39.02	38.97	3.72	0.12	0.76	0.64
Older than 50 years	0.014	0.023	0.013	0.013	6.87	0.68	0.17	0.51
Children under 3 years	0.426	0.416	0.427	0.428	1.95	0.36	0.49	0.13
Married	0.302	0.286	0.3	0.307	3.54	0.52	0.94	1.45
Health problems	0.022	0.03	0.02	0.019	5.00	1.43	1.93	0.50
Received sanctions	0.008	0.009	0.008	0.007	1.48	0.02	0.71	0.73
Incapacities	0.118	0.133	0.11	0.113	4.55	2.42	1.52	0.90
Proxy for motivation lack	0.094	0.098	0.092	0.094	1.36	0.49	0.09	0.40
No German citizenship	0.069	0.076	0.067	0.066	2.90	0.46	1.15	0.69
No schooling degree	0.037	0.044	0.037	0.037	3.12	0.21	0.52	0.31
University entry degree (Abitur)	0.234	0.212	0.235	0.24	5.37	0.34	1.42	1.08
No vocational degree	0.207	0.209	0.206	0.202	0.70	0.25	1.09	0.85
Academic degree	0.115	0.106	0.115	0.116	2.68	0.24	0.50	0.27
White-collar	0.388	0.419	0.389	0.39	6.18	0.18	0.30	0.12
Elementary occupation	0.065	0.071	0.065	0.065	2.19	0.10	0.25	0.15
Skilled agriculture & fishery workers	0.009	0.01	0.009	0.009	1.30	0.23	0.04	0.27
Craft, machine operators & related	0.285	0.308	0.286	0.285	5.02	0.21	0.01	0.22
Clerks	0.253	0.219	0.253	0.256	7.91	0.05	0.71	0.66
Technicians & assoc. professionals	0.159	0.147	0.158	0.16	3.24	0.20	0.20	0.39
Professionals & managers	0.123	0.119	0.123	0.124	1.28	0.06	0.33	0.28
Half months empl. in last 6 months	11.95	11.95	11.95	11.95	0.09	0.24	0.09	0.33
Employed before 4 year	0.672	0.675	0.675	0.678	0.57	0.54	1.35	0.81
Half months empl. in last 2 years	45.17	45.01	45.18	45.16	2.43	0.14	0.25	0.39
Half months unempl. in last 6 months	0.103	0.09	0.104	0.112	2.18	0.19	1.54	1.35
Half months unempl. in last 2 years	0.479	0.489	0.482	0.479	0.54	0.14	0.00	0.14
Half mo. since last unempl. in last 2 y.	45.91	45.79	45.91	45.96	1.95	0.04	0.68	0.72
No unempl. in last 2 years	0.888	0.886	0.887	0.889	0.55	0.10	0.43	0.53
Unemployed in last 2 years	0.036	0.039	0.037	0.035	1.52	0.07	1.05	1.11
# unemployment spells in last 2 years	0.147	0.152	0.148	0.146	0.92	0.21	0.31	0.52
Any program in last 2 years	0.051	0.052	0.052	0.054	0.29	0.28	1.20	0.92
Half months OLF in last 2 years	1.594	1.667	1.589	1.639	1.54	0.09	0.96	1.06
# OLF spells in last 2 years	0.146	0.153	0.146	0.148	1.75	0.06	0.45	0.39
Half months since last OLF in last 2 y.	45.43	45.254	45.428	45.461	2.44	0.03	0.44	0.47
Half months OLF in last 6 months	0.018	0.02	0.019	0.017	0.74	0.26	0.57	0.82
No OLF in last 2 years	0.868	0.86	0.868	0.866	2.25	0.04	0.49	0.44
OLF in last 2 years	0.108	0.109	0.107	0.109	0.54	0.29	0.56	0.85
Remaining unempl. insurance claim	25.35	23.90	25.51	25.69	10.37	1.20	2.43	1.26
Cumul. half months empl. in last 4 y.	80.92	80.61	81.03	81.01	1.40	0.53	0.42	0.10
Cumulative earnings in last 4 years	91258	88541	91205	92225	5.61	0.11	1.97	2.08
Cumulative benefits in last 4 years	3.095	3.305	3.078	3.086	2.68	0.23	0.12	0.11
Eligibility unempl. benefits	8.902	9.26	9.079	9.059	6.04	3.02	2.67	0.33
Elapsed unempl. duration	4.462	4.223	4.267	4.2	7.08	5.88	7.56	1.96
Start unempl. in January	0.079	0.085	0.079	0.079	2.11	0.00	0.17	0.17
Start unempl. in February	0.079	0.08	0.079	0.083	0.46	0.13	1.36	1.50
Start unempl. in March	0.092	0.09	0.091	0.092	0.80	0.42	0.02	0.44
Start unempl. in April	0.088	0.094	0.089	0.092	1.76	0.32	1.23	0.91
Start unempl. in June	0.068	0.069	0.066	0.062	0.61	0.72	2.44	1.72
Start unempl. in July	0.089	0.085	0.088	0.093	1.55	0.36	1.25	1.62
Start unempl. in August	0.095	0.095	0.097	0.095	0.07	0.59	0.26	0.85
Start unempl. in September	0.121	0.106	0.125	0.119	4.78	1.27	0.47	1.74
Start unempl. in October	0.105	0.102	0.106	0.102	1.21	0.07	1.08	1.15
Start unempl. in November	0.068	0.071	0.068	0.07	1.03	0.07	0.86	0.93
Start unempl. in December	0.05	0.059	0.05	0.047	3.65	0.32	1.65	1.32

Table B.2 to be continued.

Table B.2 continued …

Voucher	*… awarded*		*… redeemed*					
	yes	no	yes	no	Standardized difference			
	(1)	(2)	(3)	(4)	(1) - (2)	(1) - (3)	(1) - (4)	(3) - (4)
Baden-Württemberg	0.094	0.098	0.091	0.098	1.42	1.24	1.19	2.43
Bavaria	0.15	0.143	0.152	0.149	1.97	0.65	0.13	0.78
Berlin, Brandenburg	0.098	0.096	0.094	0.099	0.68	1.43	0.20	1.63
Hamburg, Mecklenburg Western Pomerania, Schleswig Holstein	0.076	0.079	0.077	0.072	1.07	0.55	1.59	2.14
Hesse	0.066	0.067	0.065	0.068	0.55	0.47	0.80	1.27
Northrhine-Westphalia	0.23	0.225	0.234	0.235	1.30	0.84	1.13	0.29
Rhineland Palatinate, Saarland	0.064	0.064	0.059	0.057	0.00	1.97	2.89	0.92
Saxony-Anhalt, Saxony, Thuringia	0.114	0.122	0.119	0.115	2.50	1.46	0.29	1.16
Share of empl. in the production	0.248	0.247	0.249	0.248	1.41	0.64	0.03	0.61
Share of empl. in the construction	0.062	0.063	0.063	0.062	2.50	1.98	0.68	1.31
Share of empl. in the trade industry	0.15	0.15	0.15	0.15	0.67	0.84	0.09	0.75
Share of male unempl.	0.561	0.561	0.561	0.561	0.77	0.01	1.85	1.83
Share of non-German unempl.	0.143	0.142	0.142	0.143	1.10	1.76	0.07	1.83
Share of vacant fulltime jobs	0.776	0.778	0.778	0.781	1.35	1.94	4.36	2.44
Population per km^2	965	935	933	950	1.76	1.89	0.88	1.01
Unemployment rate (in %)	12.33	12.39	12.33	12.28	1.02	0.05	1.02	0.98

Note: See Rosenbaum and Rubin (1985) for a definition of the standardized difference.

Appendix C: Additional results

Figures C.1 to C.5 contain the results for the additional outcome variables considered.

Figure C.1: Part-time employment

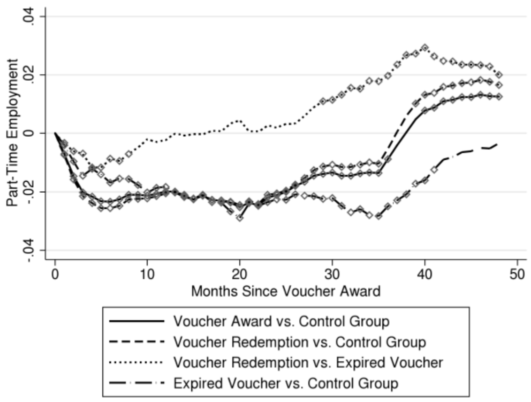


Figure C.2: Out of labour force

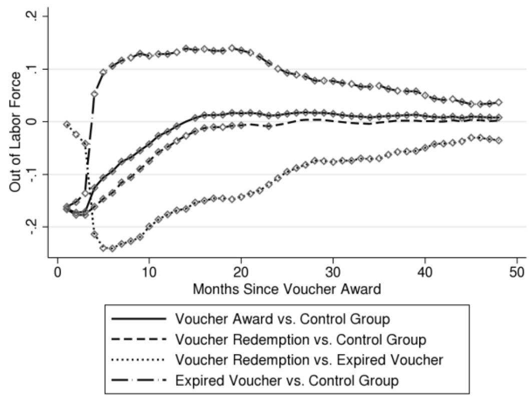


Figure C.3: Results for stable employment

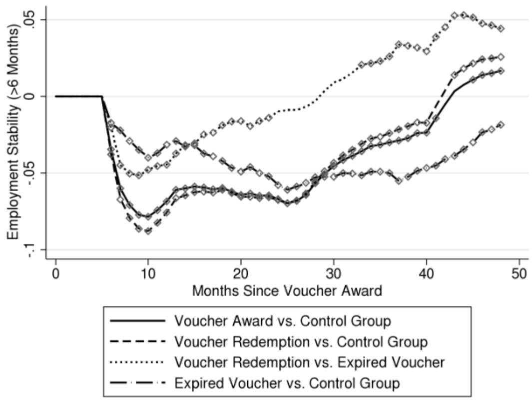
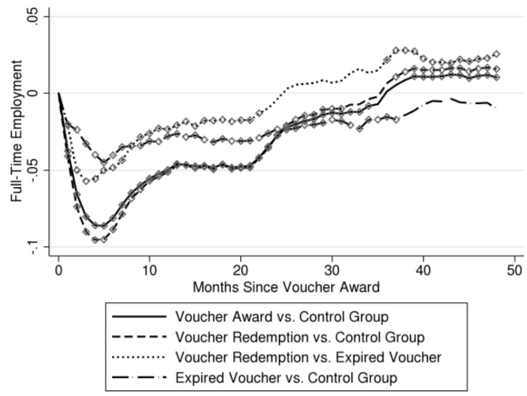
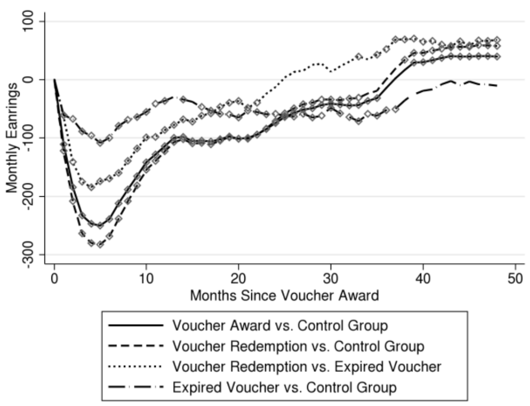


Figure C.4: Results for full time employment



Note: Stable employment is defined as being at least 6 month employed.

Figure C.5: Results for monthly earnings



Note: Earnings in EUR at 2005 prices.