Do More Placement Officers Lead to Lower Unemployment? Evidence from Germany

Jens Hainmueller[*], Barbara Hofmann[†], Gerhard Krug[‡], Katja Wolf[§]

28. Februar 2009

Preliminary version, please do not quote

Abstract: In this paper we examines the effect of a pilot project of the German Federal Employment Agency, where in 14 German local employment offices the caseload (number of unemployed per caseworker) were significantly reduced. Since the participating local offices were not chosen at random, we have to take into account potential selection bias. Therefore, we rely on a combination of matching and a difference-in-differences estimator. We use two indicators of the offices' success (unemployment rate, growth of the number of SC-III clients). Our results indicate a positive effect of a lower caseload on both outcome variables.

1. Introduction

In the last decades there was a large interest in empirical studies evaluating the causal effects of active labour market policy (ALMP) (for an overview see Kluve (2006)). Most of these studies focus on the effect of programmes like training, employment subsidies, start-up assistance or job creation schemes. However, active labour market policy also includes employment services. The main task of employment services is to match vacant jobs with

[*]Institute for Quantitative Social Science - Harvard University
[†]IAB Institute for Employment Research
[‡]IAB Institute for Employment Research
[§]IAB Institute for Employment Research

job seekers. A crucial aspect in this context is the caseload which specifies the number of job seekers who are assigned to one caseworker. Empirical evidence of an increase of the number of caseworkers in an employment office on its performance is sparse and results are mixed: For Germany, Schiel et al. (2006) find evidence for positive effects of a lower caseload on the future employment chances of job seekers. On the other hand Froelich et al. (2007) find no significant effects of the caseload for job seekers in Switzerland. For welfare recipients in the United States Hill (2006) even reports negative significant effects of a lower caseload.

To see whether caseload actually makes a difference regarding the performance of employment offices, in 2006 the German Federal Employment Agency (Bundesagentur für Arbeit) significantly increased the number of placement officers in a selected set of 14 of its 779 local employment offices. In participating offices the ratio of officers to unemployment insurance (UI) benefit recipients was lowered to 1:50 while it remained at roughly 1:100 in the non participating offices. In this paper we evaluate the effects of this increase on labour market outcomes at the regional level. Specifically, we ask whether the participating offices improved their performance with respect to reintegrating registered unemployed and programme participants into the labour market.

The participating local offices were not randomly selected, but chosen by the Federal Employment Agency. To take into account potential selection bias, we rely on a combination of matching and a difference-in-differences estimator. The participating offices are matched to comparable non-participating offices using a large set of covariates that capture the assignment mechanism. In the matched data we then conduct a difference-in-differences estimation to control for time-invariant regional characteristics.

We find that the pilot project resulted in an improvement of the performance of the participating local employment offices. The unemployment rate in the pilot project offices decreased one percentage point in the first year after programme start. Half of this decrease is due to the lower caseload. This is robust to additional checks that investigate whether the increase in the performance of the participating offices is caused by negative spill-over effects on neighbouring non participating offices. Our results indicate that no such negative spill-over effects occurred.

The remainder of the paper is organised as follows: First, we describe the pilot project in more detail. Section three explains our empirical strategy. Section four describes the data. Section five explains the implementation of the matching estimator and presents balance checks and the findings for the treatment effects. Section six concludes.

2. Pilot project "Activation of clients and improvement of integration services"

In 2006 the Federal Employment Agency (FEA) decided to run a pilot project in order to assess the effectiveness of additional placement officers. The pilot project consisted of a substantial increase of the number of placement officers in 14 out of 779 local employment offices chosen in December 2006.[1] During January and April 2007 around 400 additional placement officers were hired and trained. The idea was to set a caseload of one placement officer per 70 unemployment insurance (UI) benefit recipients. However, in the first months of 2007 there was a significant decrease in unemployment in Germany. Thus, the factual caseload at the official start of the pilot project (1 May 2007) in the participating communities was 1:50, while in the other non participating local employment offices the caseload was on average 1:100.[2]

The 14 pilot project offices were not randomly drawn out of the pool of local employment offices, but according to a well documented set of criteria allowing us to accurately capture the assignment mechanism with our set of covariates. The criteria for selecting the participating offices were as follows: first, at least one local office had to be chosen from each of the ten regional directorates (see footnote 1). Second, there was a tendency to chose local employment offices from regions with rather better labour market conditions than the regional average. Third, the local employment offices had to be able to provide the facilities

[1] The FEA is organised in ten regional directorates, that consist of 178 employment agencies. Each employment agency is organised by several local employment offices and one main local employment office, which differs slightly in terms of its structure as some organisational processes are concentrated here (e.g. human resources department). In the empirical analysis, we will take this difference into account.

[2] Taking registered unemployed who do not receive any benefits into account (e.g. women who want to rejoin the labour force after longterm maternity leave) the caseload was 1:80 and respectively 1:170 in non-pilot project local employment offices.

needed to accommodate the new staff so office space was an important consideration. Finally, some anecdotal evidence suggests that soft factors (e.g. personal contact) might have played a role in the decision process as well.

It is important to note, that apart from the decrease in the caseload, to our knowledge no other major changes accompanied the participation in the pilot project, especially there was no change in the integration budget. In other words the overall office budget to implement its employment and training measures aiming at the integration of jobseekers into the labour market remained constant. Yet, there was one change of conditions for the pilot project offices, namely a) they had to sign new target agreements and b) a separate controlling tool was implemented. We consider these changes as part of our treatment.

3. Methods

In order to identify the effect of the pilot project we use the potential outcome framework (cf. e.g. (Holland, 1986) or (Rubin, 1974)), where the effect of interest is analyzed in comparison to the counterfactual situation, namely the situation if the treatment group would not have received treatment. Let $mdst = 1$ if the local employment office is a pilot project office and $mdst = 0$ if it was not chosen to be a pilot project office and y an indicator of its performance, the outcome of interest. Let y_i^0 be the potential outcome of unit i in case of treatment and y_i^1 the potential outcome of unit i in case of no treatment. For each unit the causal effect of the treatment is defined as the difference between its potential outcomes, only one of which is observed.

We focus on the average treatment effect on the treated (ATT) which is defined as the difference in the expected outcomes under the treatment and control condition integrated over the distribution of the treated units:

$$ATT \equiv E(y^1 - y^0 \mid mdst = 1)$$

which is identified under the assumption of selection on observables and common support. The common support assumption is innocuous in our data. The 14 participating offices fall well within the characteristics of the non-participating offices on our covariates.

To adjust for the potential selection bias, we rely on a combination of matching and a difference-in-differences estimation (DiD-matching Heckman, Ichimura, and Todd (1998)) that exploits the fact that the outcome variables are observed both in the pre-treatment period right before the start of the program y_0 and in the post-treatment period y_1.

Apart from the observed confounding variables, this design also accommodates the presences of time-invariant unobserved confounders. The key identifying assumption of the DiD matching estimator is

$$E(y_1^0 - y_0^0|x, mdst = 1) = E(y_1^0 - y_0^0|x, mdst = 0)$$

meaning that conditional on the observed covariates the before and after differences in outcome are independent of the treatment assignment. In other words, the before and after difference in the outcome of the controls is equal to the before and after difference in the outcome for the treated had they not been treated. By taking the differences in the outcomes before and after treatment, we eliminate time-constant unobserved factors (Smith and Todd (2005)).

The ATT^{DiD} can be estimated as:

$$\widehat{ATT}^{DiD} = \frac{1}{n^1} \sum_{i \in I_1 \cap CS} \left((y_{1i}^1 - y_{0i}^0) - \sum_{j \in I_0 \cap CS} w(i,j)(y_{1j}^0 - y_{0j}^0) \right)$$

where CS refers to the common support, I_0 and I_1 the control group and the group of the pilot project offices, respectively. The number of pilot project offices in the common support region is denoted by n^1 and $w(i,j)$ is the weight of office j if it is matched to the pilot project office i.

As in most applications X contains several characteristics which makes exact matching difficult, matching techniques that are based on a reduction of X to one dimension, e.g. propensity score matching or mahalanobis distance matching, have been developed in order to balance the distribution of the covariates in the treatment and the matched control group. Here we rely on Genetic Matching (GM) as developed by Diamond and Sekhon

(2006). Formally, each treated unit is matched to B nearest neighbors according to the following generalized Mahalanobis distance distance metric

$$d(X_i, X_j) = \{(X_i - X_j)'(S^{-1/2})'VS^{-1/2}(X_i - X_j)\}^{1/2}$$

where V is a $(k \times k)$ positive definite weight matrix with zero in all elements except the main diagonal and $S^{1/2}$ is the Cholesky decomposition of S, the variance-covariance matrix of X, the $(N \times k)$ matrix of covariate characteristics. Notice that this metric generalizes the conventional Mahalanobis distance distance metric which we obtain when setting each of the k parameters in the diagonal of V equal to 1.

In GM, the weights in the diagonal of V are chosen by an evolutionary algorithm (Sekhon and Mebane (1998)) such that covariate balance between the treatment and the control group is maximized. We define the balance score in the objective function as the lowest p-value across covariate-by-covariate paired t-tests for differences in means and bootstrapped Kolmogorov-Smirnov tests for the equality of distributions. Diamond and Sekhon (2006) and Sekhon (2006) present evidence from Monte Carlo simulations that show the good properties of this balance score. The key advantage over conventional matching techniques is that GM often leads to higher levels of covariate balance especially in cases where there are few treated units where the stochastic balancing of the propensity score may not lead to satisfactory balance.

Finally, notice that we estimate the treatment effect using regression adjustment with a set of key covariates in the matched data-set as recommended in Abadie and Imbens (2006, 2007) in order to account for any bias that may result from the presence of matching discrepancies.

4. Data

4.1. Outcome Variables

The data source of our analysis is the Dataware House (DWH) of the Federal Employment Agency (FEA), the FEA human resource department and the Federal Statistical Office.

Since we want to estimate the effect of the pilot project on the performance of the pilot project offices, all of our data are aggregated at a regional level. The local employment offices are in charge of the group of so-called Social Code (SC) III clients, who are (mostly) entitled to unemployment insurance benefits.[3] Therefore, our outcome variables focus on these SC-III clients. They are registered as unemployed or as participants of active labour market programmes. As indicators of the offices' success we use the following two variables: a) the SC-III unemployment rate and b) the growth of the number of SC-III clients. As participants of active labour market programmes are not counted as registered unemployed we use the second indicator to assure that a decrease in the regional unemployment rate is not caused by an increased programme intensity.

The SC-III unemployment rate is defined as the number of registered SC-III unemployed relative to the official reference figure, which is the total labour force. Applying DiD-Matching, we take the difference between this unemployment rate in April 2008 and April 2009:

$Y^a :=$ unemployment $rate^{SCIII}_{April08}$ - unemployment $rate^{SCIII}_{April07}$

Concerning the second indicator, we use the difference between the percentage growth of the number of SC-III clients before and after programme start. To avoid potential seasonal effects we compute each growth rate for twelve months, i.e.:

$$Y^a := \frac{N^{SCIII-clients}_{April08} - N^{SCIII-clients}_{May07}}{N^{SCIII-clients}_{May07}} - \frac{N^{SCIII-clients}_{April07} - N^{SCIII-clients}_{May06}}{N^{SCIII-clients}_{May06}}$$

4.2. Covariates

In addition to the difference-in-differences approach, which accounts for unobservable time-invariant factors, we use a large number of observable control variables influencing the selection of the pilot project offices and our outcome variables. In our case we cannot exactly reproduce the decision process of the FEA. However, since we have a rich administrative data set at our disposal, we are able to control for many factors that are correlated to the outcome, which finally makes it plausible that the CIA holds. The variables listed in table 1 are included in the matching algorithm. Most of the variables

[3] In contrast, Social Code II clients receive means-tested unemployment benefit II.

are measured at the local employment office level. Some of them represent the economic performance of the region or the demand side of the local labour market, like average wages, employment growth or commuting. Other variables focus on the composition of the SC III-clients, like age, gender, nationality or qualification and its dynamic. In this context we further include a typology of SC-III-clients of the FEA regarding their future employment prospects. Additionally, we account for variables measuring the tightness of the local labour market, like vacancy rate, unemployment rate and growth of unemployment. Seasonal aspects are captured by the variable "standard deviation of unemployment rate". As each local employment office operates not only on a local labour market but is part of a larger regional labour market we include regional variables at the employment agency level.

Table 1: Matching variables

Local level:	employment growth (2004-2006)
	population density per km^2 (2005)
	average wages (fulltime)
	percentage growth of average wages (2000 - 2006)
	commuters: Net commuting per employee (place of residence)
	commuters: Net commuting per employee (place of work)
	vacancy rate (Dec 2006)
	percentage growth of stock of unemployed (Dec 2005 - Dec 2006)
	number of SC-III clients (Dec 2006)
	share of SC-III benefit recipients of all SC-III clients
	mean of unemployment rate (Jan - Dec 2006)
	standard deviation of unemployment rate (Jan - Dec 2006)
	mean share of training participants (Jan - Dec 2006)
	share clients type "activating" (Dec 2006)
	share clients type "advancing" (Dec 2006)
	share clients type "caring" (Dec 2006)
	share clients type "market" (Dec 2006)
	share clients type missing (Dec 2006)
	share clients above 50 years (Dec 2006)
	share clients below 25 years (Dec 2006)
	share clients without school degree (Dec 2006)
	share clients male (Dec 2006)
	share clients German citizenship (Dec 2006)
	percentage growth of number of clients above 50 years (Dec 2005 - Dec 2006)
	percentage growth of number of clients below 25 years (Dec 2005 - Dec 2006)
	percentage growth of number of clients without school degree (Dec 2005 - Dec 2006)
	percentage growth of number of clients male (Dec 2005 - Dec 2006)
	percentage growth of number of clients German citizenship (Dec 2005 - Dec 2006)
	mean integration rate (Jan - Dec 2006), controlling indicator
	mean unemployment duration (Jan - Dec 2006), controlling indicator
	mean difference between regional and local unemployment rate (Jan - Dec 2006)
Regional level:	employment growth (2004-2006)
	mean sanction rate (Jan - Dec 2006)
	caseload (Dec 2006)
	percentage growth of numbers of vacancies April (2006 - April 2007)
	share of SC-II unemployed of all unemployed (Dec 2006)
	mean of unemployment rate (Jan - Dec 2006)

5. Results

5.1. Implementation

We imposed three matching restrictions: First, since head local employment offices differ from the other local employment offices (see footnote 1), we matched exactly on the organisational structure, i.e. a head local employment office could only be matched to a head local employment office. Analogously regular local employment offices were only matched to regular local employment offices.

Second, potential spill-over effects might bias the estimation of the effect of interest, the ATT. In order to exclude potential spill-over effects, we did not use any local employment office that are located in an employment office district where a pilot project office was located ("neighbor offices") as control.[4]

Third, we excluded a very small set of local employment offices from the pool of potential controls due to their geographic peculiarity (i.e. islands) or data problems (Borkum, Norderney, Westerland, Rügen, Juist and Lauterecken).

We are left with a donor pool of potential controls consisting of 549 local employment offices and of 162 main local employment offices. The results reported in this paper are based on 1:1-matching. Additionally, we tested 1:2- and 1:3-matching and the results are virtually identical and available upon request.

5.2. Matching quality

In order to assess the matching quality, we use three indicators: the standardised difference (i.e. the difference in means between the two groups scaled by the standard deviation), the p-values of bootstrapped covariate-by-covariate Kolmogoroff-Smirnov tests for the equality of distributions, and the p-values of paired covariate-by-covariate t-tests of mean differences. All three are informative on covariate differences (mean or distribution) between treatment and potential control group before and treatment and control group after matching, respectively.

[4] Further below we will use these neighbour offices in order to explicitly analyse potential spill-over effects in section 5.4.

Before matching, there were significant differences in the mean between treated and potential controls regarding six of the covariates, e.g. the regional employment growth or integration rate (cf. column 4 table 4). According to the t-tests of mean differences, there are no significant differences between treated and control group after matching (cf. column 4 table 5). More or less the same is true for differences in the distributions of the covariates. After matching there are no significant differences (cf. column 5 table 4 and column 5 table 5).

5.3. Treatment effects

In this section the results of the estimation of the effect of the pilot project using the two different outcomes (Y^a and Y^b) are presented. In 2007 the German economy experienced an economic boom leading to an improvement of the performance of (nearly) each employment agency, irrespective of whether it took place in the pilot project or not: compared to the year before, more unemployed individuals could be integrated in the labour market, the average duration of unemployment was shortened and accordingly the number of SC-III unemployed decreased. The main result of this analysis though is, that among the pilot project offices, the improvement of the performance was even bigger. The following table lists the estimated $ATTs$:

Table 2: ATT of the pilot project

		pilot project offices		control offices	
	effect	before	after	before	after
Y^a	-10.7***	-20.5	-27.1	-21.3	-18.1
Y^b	-0.52***	3.03	2.01	2.92	2.43

The estimated coefficients are based on regression adjustment of Y^a and Y^b. The results are significant on a 1%-level

During the period May 2007 until April 2008, the number of SC-III clients in the pilot offices decreased by 27%. Among the controls, we "only" find a decrease in this number of 18%. Regarding the same calendar period during the previous year (May 2006 until April 2007), we do not find any difference between pilot project offices and controls. Thus, the pilot project offices could decrease the number of clients by around 10% more compared to the control group. This effect is significantly different from zero. Additionally applying regression adjustment (see section 3), the effect slightly increases to 10.7%.

Regarding the outcome SC-III unemployment rate the result also indicates an improvement of the performance of the pilot project offices: the average SC-III unemployment rate in April 2008 in the treated group was 2.0% while it was 3.0% in April 2007. According to our results, 0.5%-points of this difference can be traced back to the pilot project. This effect is statistically significant and robust towards regression adjustment.[5]

5.4. Spill-over effects

In this section we use the neighbour offices of the pilot project offices, i.e. the local employment offices that are located in an employment agency district where a pilot project office is located, to assess whether the pilot project induced negative spill-over effects. Pilot project offices might have improved their performance at the costs of their neighbour offices as they both operate on the same regional labour market. In order to estimate potential average spill-over effects, we apply the same matching procedure as described above but instead of using the pilot project offices as treated, we treat the neighbour offices as treated.[6] What we would expect in case of negative spill-over effects, once using the neighbour offices instead of the treated, is negative average treatment effects of these neighbour offices. The following table contains the results of the spill-over effect analysis:[7]

Table 3: Spill over effects of the pilot project

		neighbour offices		control offices	
	effect	before	after	before	after
Y^a	0.08	-21.53	-20.86	-21.91	-19.84
Y^b	0.06	3.26	2.58	3.31	2.67

The estimated coefficients are based on regression adjustment of Y^a and Y^b. The results are not significant on a 10%-level

According to the results in table 3 we do not find significant negative spill-over effects: the increase of the caseload in the pilot project offices did not cause a deterioration of performance of the neighbour offices.

[5]Additionally to the results presented in this paper, we used two indicators of the FEA controlling department measuring the integration rate and an aggregated unemployment duration as outcome variables. Again, the estimation indicate a significant better performance of the treatment group.

[6]Note that in this step, we excluded the pilot offices from the pool of potential controls.

[7]We used the same covariates and the same matching algorithm as before. After matching there were no significant differences between the new treatment and control group.

6. Conclusion

This paper examines the effect of a pilot project of the German Federal Employment Agency, where in 14 local employment offices the caseload were significantly reduced. As outcome variables we define the SC-III unemployment rate and the growth of SC-III clients. We rely on a combination of matching and difference-in-differences estimation to avoid potential selection bias introduced by the non-random assignment of local employment offices in the treatment group.

Our results indicate a positive effect of a lower caseload on both outcome variables. However, from the perspective of political decision makers the important question is not only if the pilot project is successful, but if the same effects could be expected if the caseload is decreased in all local employment offices. An argument in favour of transferability is that we find no spill-over effects on neighbouring regions. Thus, the positive effect from the pilot project is not at the expense of other regions. An argument against is that the observation period is characterised by an economic upswing. To what extent our results depend on these favourable economic conditions further research has to clarify.

References

Abadie, A., and G. Imbens (2007): "Simple and Bias-Corrected Matching Estimators for Average Treatment Effects," .

Abadie, A., and G. W. Imbens (2006): "Large Sample Properties of Matching Estimators for Average Treatment Effects," *Econometrica*, 74(1), 235–267.

Diamond, A., and J. S. Sekhon (2006): "Genetic Matching for Estimating Causal Effects: A General Multivariate Matching Method for Achieving Balance in Observational Studies," Discussion Paper WP2006-35, Institute of Governmental Studies., http://repositories.cdlib.org/igs/WP2006-35.

Froelich, M., M. Lechner, S. Behncke, and S. Hammer (2007): "Einfluss der RAV auf die Wiedereingliederung von Stellensuchenden," SECO Publikation Arbeitsmarktpolitik 20, Staatssekretariat für Wirtschaft (SECO).

Heckman, J. J., H. Ichimura, and P. Todd (1998): "Matching as an Econometric Evaluation Estimator," *Review of Economic Studies*, 65(2), 261–94.

Hill, C. J. (2006): "Casework Job Design and Client Outcomes in Welfare-to-Work Offices," *J Public Adm Res Theory*, 16(2), 263–288.

Holland, P. W. (1986): "Statistics and Causal Inference," *Journal of the American Statistical Association*, 81(396), 945–960.

Kluve, J. (2006): "The Effectiveness of European Active Labor Market Policy," IZA Discussion Papers 2018, Institute for the Study of Labor, Bonn.

Rubin, D. B. (1974): "Estimating causal effects of treatment in randomized and nonrandomized studies.," *Journal of Educational studies*, 66, 688–701.

Schiel, S., R. Cramer, R. Gilberg, D. Hess, and H. Schröder (2006): "Evaluation des arbeitsmarktpolitischen Programms FAIR," IAB Forschungsbericht 7, IAB, Nuremberg.

Sekhon, J. (2006): "Alternative Balance Metrics for Bias Reduction in Matching Methods for Causal Inference," *Unpublished manuscript, Dept. of Political Science, UC Berkeley.*

Sekhon, J. S., and W. R. Mebane (1998): "Genetic Optimization Using Derivatives: Theory and Application to Nonlinear Models," *Political Analysis*, 7, 189–213.

Smith, J. A., and P. E. Todd (2005): "Does matching overcome LaLonde's critique of nonexperimental estimators?," *Journal of Econometrics*, 125(1-2), 305–353.

A. Appendix

Table 4: Balance before matching (Minimum P value from T-Tests is 0.01566619 Minimum P value from KS-Tests is 0)

	mean.Tr	mean.Co	sdiff.pooled	T pval	KS pval
Eastern Germany	0.36	0.23	41.59	0.37	
Employment growth (local)	−0.01	−0.02	40.12	0.13	0.31
Employment growth (region)	0.00	−0.01	99.83	0.03	0.04
Commuting I	−0.07	−0.11	26.19	0.27	0.11
Commuting II	−0.10	−0.20	49.69	0.08	0.11
Population density	365.19	392.90	−7.18	0.80	0.04
Growth of vacancies (region)	4.11	2.46	29.28	0.50	0.60
Growth of unemployed (local)	−0.14	−0.16	22.73	0.67	0.78
Growth of unemployed (region)	−0.15	−0.14	−17.22	0.70	0.92
Share SC-II unemployed (region)	0.61	0.61	−1.80	0.95	0.42
SC-III clients	3882.76	2432.91	90.71	0.02	0.00
Share SC-III benefit recipients	0.66	0.66	−5.65	0.83	0.53
Share type "activating"	0.14	0.15	−45.21	0.13	0.27
Share type "advancing"	0.23	0.20	60.29	0.10	0.18
Share type "caring"	0.39	0.39	1.50	0.97	0.17
Share type "market"	0.10	0.09	10.47	0.74	0.63
Share type missing	0.10	0.10	−20.57	0.66	0.67
Share above 50 years	0.41	0.40	28.18	0.36	0.63
Share below 25 years	0.11	0.11	−3.88	0.92	0.99
Share without school degree	0.06	0.06	−7.06	0.84	0.97
Share male	0.48	0.47	52.18	0.08	0.29
Share German	0.93	0.92	9.57	0.77	0.90
Growth above 50 years	0.10	0.10	−5.73	0.84	0.72
Growth above 25 years	0.02	0.03	−10.91	0.72	0.92
Growth without school degree	0.05	−0.02	53.26	0.13	0.29
Growth male	−0.06	−0.06	19.02	0.56	0.89
Growth German	−0.00	0.00	−39.20	0.24	0.50
Unemployment rate (local)	0.11	0.12	−29.81	0.27	0.61
Unemployment rate (region)	0.11	0.12	−26.71	0.44	0.81
Seasonal indicator	0.01	0.01	−22.74	0.56	0.43
Difference regional and local unemployment rate	−0.00	−0.00	−15.81	0.55	0.93
Share training participants	0.22	0.20	63.61	0.17	0.05
Average wage	84.41	82.76	17.13	0.64	0.90
Growth average wage	0.10	0.09	24.43	0.39	0.59
Sanction rate (region)	0.01	0.01	24.29	0.52	0.46
Vacancy rate (region)	0.10	0.09	2.75	0.94	0.61
Integration rate	0.30	0.27	100.15	0.02	0.05
Unemployment duration	182.21	182.58	−2.79	0.91	0.48
Caseload (region)	113.66	121.66	−55.09	0.16	0.26

Table 5: Balance after **1:1** Matching (Minimum P value from T-Tests is 0.1505891 Minimum P value from KS-Tests is 0.26)

	mean.Tr	mean.Co	sdiff.pooled	T pval	KS pval
Eastern Germany	0.36	0.21	28.73	0.15	
Employment growth (local)	−0.01	−0.01	−4.74	0.88	1.00
Employment growth (region)	0.00	−0.00	33.39	0.22	0.57
Commuting I	−0.07	−0.05	−13.01	0.59	0.84
Commuting II	−0.10	−0.07	−13.78	0.53	0.84
Population density	365.19	310.85	13.86	0.37	0.27
Growth of vacancies (region)	4.11	3.87	2.75	0.80	0.99
Growth of unemployed (local)	−0.14	−0.15	8.11	0.77	0.85
Growth of unemployed (region)	−0.15	−0.14	−13.87	0.55	0.86
Share SC-II unemployed (region)	0.61	0.62	−22.22	0.19	0.88
SC-III clients	3882.76	3600.74	14.52	0.62	0.27
Share SC-III benefit recipients	0.66	0.67	−18.75	0.22	0.53
Share type “activating”	0.14	0.14	−22.28	0.53	0.84
Share type “advancing”	0.23	0.22	14.75	0.59	0.86
Share type “caring”	0.39	0.40	−10.69	0.56	0.27
Share type “market”	0.10	0.09	24.83	0.20	0.26
Share type missing	0.10	0.09	13.94	0.38	0.87
Share above 50 years	0.41	0.40	28.93	0.31	0.27
Share below 25 years	0.11	0.11	3.55	0.90	0.86
Share without school degree	0.06	0.05	19.71	0.44	0.55
Share male	0.48	0.48	23.74	0.25	0.87
Share German	0.93	0.93	−0.90	0.97	0.85
Growth above 50 years	0.10	0.10	−14.01	0.63	0.56
Growth above 25 years	0.02	0.03	−7.09	0.80	1.00
Growth without school degree	0.05	0.02	19.13	0.24	0.54
Growth male	−0.06	−0.05	−2.73	0.91	1.00
Growth German	−0.00	−0.00	4.98	0.87	0.55
Unemployment rate (local)	0.11	0.11	−9.17	0.55	0.99
Unemployment rate (region)	0.11	0.11	2.82	0.76	0.99
Seasonal indicator	0.01	0.01	15.19	0.45	0.83
Difference regional and local unemployment rate	−0.00	0.00	−33.70	0.23	0.50
Share training participants	0.22	0.22	11.77	0.60	0.56
Average wage	84.41	85.76	−10.68	0.61	0.85
Growth average wage	0.10	0.10	−13.63	0.61	0.85
Sanction rate (region)	0.01	0.01	6.85	0.73	0.99
Vacancy rate (region)	0.10	0.09	9.83	0.55	0.87
Integration rate	0.30	0.29	33.84	0.15	0.58
Unemployment duration	182.21	175.91	53.34	0.18	0.50
Caseload (region)	113.66	115.15	−7.59	0.73	0.83