Impact Evaluation of a Training Measure Provided by Public Employment Offices in Slovakia Using Propensity Score Matching

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Abstract:
The submitted paper brings information from administrative data on registered unemployed people in Slovakia. This data are employed to evaluate a training programme which is a part of the portfolio of active labour market measures provided by the Slovak public employment offices. To evaluate the impact of the measure, propensity score matching was used with a combination of a nearest neighbour and exact matching approach. Negative effects were observable on individuals' chances of getting a job during the period of 24 months after the programme. These results were confirmed also by the regression analysis. Provided training programs, on average, decreased individuals' earnings. Moreover, estimated results are not homogenous across time periods. Estimations seem to be sensitive to overall changes in implementation of rules as well as the impact of economic crisis. Training programs seemed to have a positive impact on participants´ earnings in the initial periods of implementation and negative impact in the later periods, after 2009.

Keywords:
Propensity score matching. Counterfactual impact evaluation. Active labour market policy.

1 Description of the measure and its implementation

Inquiring the impact of a training program presents a perfect example when academic, micro-economic theory meets the demands of an everyday, project management, practice. As in the case of every investment, also investing into human capital raises the question of related returns.

The measure which is being evaluated in this paper is one of the measures within the portfolio of active labour market measures provided by the Central Office of Labour, Social Affairs and Family (COLSAF). This is the implementation agency of the Ministry of Labour, Social Affairs and Family and thus the centralised provider of publicly funded employment services. The name of the measure is "Education and preparation of the job seeker to find a job on the labour market" and it was, strictly speaking, the only training measure in the portfolio of active labour market measures.

Under this measure, training is provided to registered unemployed people. There are no further restrictions either of the target group or of the thematic focus of the training provided.

This means that the same training programs can be provided on the day after registration as well as to the long-term unemployed. It also means that the content can be provided on several levels of skill complexity and in several areas. The decision on the content of the training was in the competences of Local Labour Offices, but in the middle of 2010, a centralised public procurement procedure was introduced.

The impact of the measure will be followed on the income of trained individuals after finishing of the training. For this purpose, administrative data on the registered unemployed linked with social insurance data will be explored. Impact on income of participants finishing the training in the period between January 2007 and December 2011 will be evaluated.

Our special concern is the heterogeneity in the impact of the training measure in time. During the period of interest, the measure implementation changed two times. First, it was in May 2008, when an updated version of the Law on Employment Services was introduced (Act No. 5/2004 Coll.). The second break was in the middle of 2010, when there was a change in the national project under which the training programs were implemented. This change resulted in a rapid decrease in the total number of training programs provided and thus in a further decline in the accessibility of training programs to the registered unemployed.

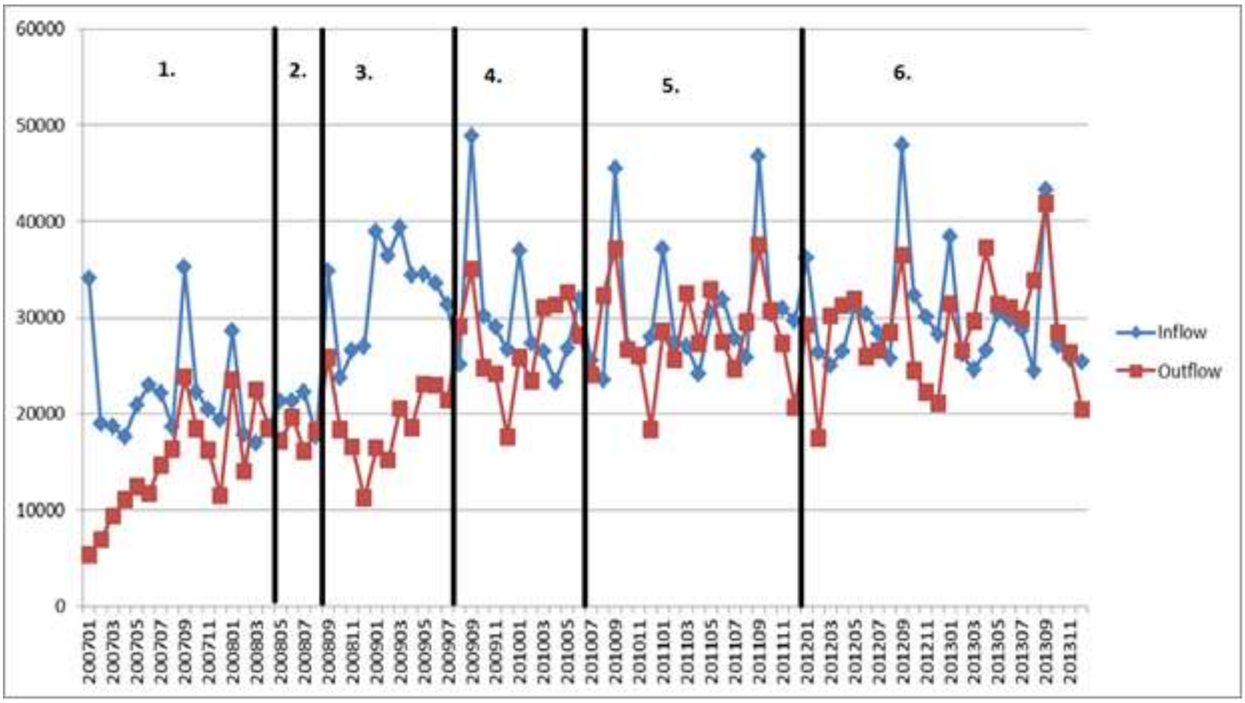
Table 1: Number of training participants

	2007	2008	2009	2010	2011	2012	2013
Number of participants	9 472	12 208	17 901	8 822	1 433	1 708	1 694

Source: COLSAF

Another circumstance which could be affecting the impact of the training measure was the economic crisis. Its effects are observable mainly after September 2008, when the inflow of the unemployed into the database of the registered unemployed jumped up, while the outflow of the unemployment dropped. This inconsistency between inflow and outflow of the registered unemployment remained present until July 2009. During that period, the total number of the registered unemployed jumped up. After that date, the inflow and outflow of the registered unemployment remains closely related. The following figure displays inflows and outflows of the registered unemployment as well as the time periods identified.

Figure 1: Inflow and outflow of the registered unemployment and time periods identified in the analysis



Source: COLSAF

To summarize:

1. The first period lasts from the beginning of the reference period (January 2007) until the new version of the Law on Employment Services was introduced in practice (April 2008).
2. The second period starts after the introduction of the updated Law on Employment Services until the economic crisis becomes observable in September 2008.
3. The third period is the period of inflation of registered unemployment until July 2009.
4. The fourth period is the post-crisis period until the new national projects were introduced in July 2010.
5. The fifth period lasts until December 2011, after this date we are not able to follow the outcome of participants for a sufficiently long period.
6. The sixth period is excluded from the analysis because of the lack of information about the outcome.

2 Previous studies

There are few previous evaluations of this exact measure. (Bořík, et al., 2013) evaluated the outcome of participants finishing the training programs before the end of 2009. They conclude that this measure has positive impact on employment as well as income of participants two years after finishing the training. Furthermore, they point at relatively higher positive impact of the measure in Bratislava in comparison to the rest of Slovakia. This study used similar data exploring descriptive statistics to assess the impact of the measure. No counterfactual approach was applied.

The results of the study are in contrast with later evaluations published in (Štefánik, et al., 2014) and (Štefánik, 2014). These studies showed negative effects of the measure provided during 2011 on the chances of the unemployed to be placed in a job. Using a counterfactual approach, namely the propensity score matching, they observed higher negative impact in Bratislava than in the rest of Slovakia.

Contradictory evidence on the impact of the measure exists in earlier studies evaluating different periods of the implementation of training programs. Therefore, our question here is whether there is heterogeneity in the impact of the measure implemented in different time periods.

There is a rich supply of empirical studies using counterfactual methodology to evaluate impacts of training measures from abroad. Based on a meta-analysis of more than 100 similar studies, (Card, et al., 2009) conclude that on-the-job training programmes are more effective in the medium run than in the short run. For Slovenia, (Juznik Rotar, 2012) reports positive treatment effects of a youth training programme on employment of participants in the short run. Positive significant effects of training programmes on employment of participants in the medium–run are reported also for Germany or Austria, negative ones for Sweden.

3 Quantifying the returns to a human capital investment – impact evaluation of a training measure

The question of returns into education, training and human capital investment in general is one of the prominent topics in economic theory. Since (Mincer, 1974), this question has moved more into the field of microeconomics. The methodological improvements in assessing the returns to educational activities and training are surprisingly rich. This area was one of the most dynamic parts of the microeconomic literature, especially microeconomic methodology. Approaches taking advantage of a regression based analysis, such as the original model of Mincer, had to face some severe methodological objections. One way of solving some of the objections is abandoning the regression based approach and moving to the analysis based on matching. (Dehejia, et al., 1999) and (Heckman, et al., 1997) were ones of the first studies which went along this alley.

The principle of matching takes advantage of the analogy with a random experiment. The question about returns to a training measure is linked with the question of the outcome of participants if they did not participate. Obviously, we are not able to observe the outcome of both situations - the outcome of a participant after participation as well as the outcome of the same participant if he did not participate. To be able to quantify the effect of the training, we need to impute the information about participants' outcome if they did not participate. In a random experiment framework, the missing information is gained from the control group outcome. Because the treatment, as well as the control group, is selected randomly, no problem of selection bias should (ideally) occur. Unfortunately, experimental data are scarce and there were no experiments organised to quantify the effects of the training measure of our interest.

A robust data set on the registered unemployed was made available for the purpose of this analysis. As this is observational data, we need to deal with the problem of selection bias. Selection bias is likely to occur because of different characteristics of participants in comparison to the rest of the registered unemployed[1]. In this article, we will use two methods of dealing with selection bias when estimating the treatment effect of the training measure.

3.1 Regression approach (OLS)

First, we will use the regression approach, where we rely on the ability of a regression equation to control side effects. The equation can be formalized as follows:

$$Y = \beta_0 + \beta_1 I + \beta_2 X + \mu$$

where Y stands for the outcome if individuals; I is a dummy referring to whether the individual participated in the training or not; X is a vector of all observable characteristics of individuals. Observable characteristics bring information on:

- Duration of unemployment (date of entering, length of the evidence, ...)
- Individual characteristics (gender, age, region, level and field of education, ...)
- Previous participation in other ALMM
- Previous work experience (days of previous work experience, economic sector and occupation, ...)
- Family background (children, marital status, ...)
- Declared skills (PC skills, languages, ...)

Coefficient β_1 estimated for variable I represents the quantification of the treatment effect of the training measure on the outcome Y after controlling for X. The equation was estimated on all registrations of the unemployed in the database, which at the beginning of the selected reference period was 1 758 578. Ordinary least squares (OLS) estimations were used.

This quantification is used only to confirm the quantification acquired using the propensity score matching approach. Because the assumptions behind regression analysis are stronger than the assumptions behind propensity

[1] There is no formal restriction for a registered unemployed to be eligible for participation in the training, but as the accessibility of training programs is extremely low, self-selection may play a role here. Only around 1.5% of the eligible registered unemployed received some training.

score analysis, we will not report the regression analysis results in such detail as the propensity score analysis results.

3.2 Propensity score matching (PSM)

In this paper, the most intuitive form of matching will be performed; this is the one which is the most alike to an experimental setting. For each participant, one member of the control group will be selected out of the database of non-participants. The nearest (non- participant) neighbour of each participant on the propensity score variable will be selected for the control group. A probit model will be used to construct the propensity score variable. The model can be formalized as follows:

$$\Pr(I_i = 1 \mid Xi) = \beta_0 + \beta_2 X + \mu$$

where I refers to the participation in the training and X are all observable characteristics, similarly as in the regression approach. A probit model was estimated separately on the data from 46 Local Labour Offices (regions). A matched control group member was also selected only within the same Local Labour Office as his twin participant.

There are two main assumptions behind propensity score matching (Caliendo, et al., 2005). First, it is the assumption of unconfoundedness, saying that after ensuring the balance on observable characteristics, non-participants outcomes' have the same distribution that participants would have experienced if they had not participated. The information on the balance improvement due to the ex-post selection of the control group can be found in the Appendix. In Appendix 2, we may follow the means and proportions of selected variables grasping some of the observable characteristics of individuals in the analysis. The first column from the left displays the proportions and means of selected variables for the control group members. The second column displays the descriptive statistics for participants followed by the statistics of the whole database without participants. The column on the total right displays the improvement of the balance on each of the variables achieved by selecting the control group ex-post. The most important variables are the propensity score variable and the date of entering unemployment. Balance on both of these variables increased significantly (99.84% or 99.32%, respectively).

Another evidence to support the unconfoundedness assumption is the predictive power of the probit model predictions. The model proved to be strong in predicting participation in the measure. It was able to predict participation of over 99 percent of cases and in over 81 percent of participants.

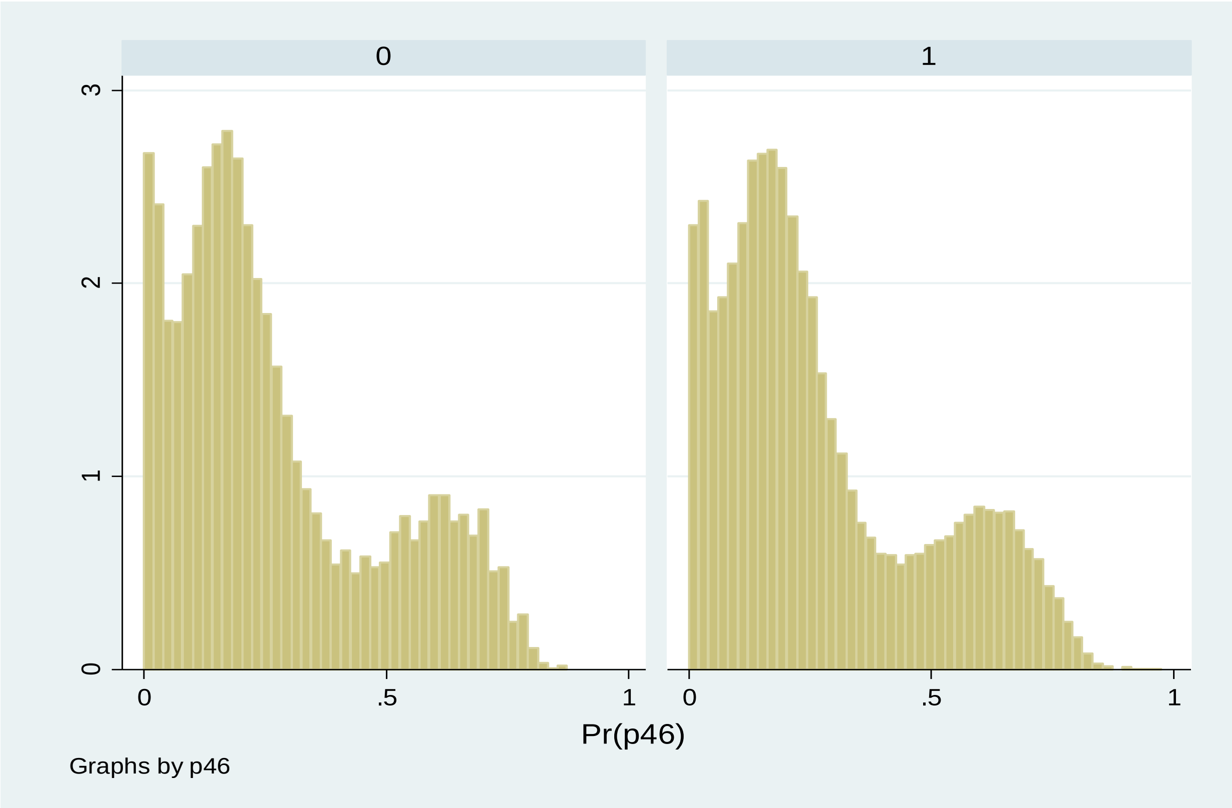
Table 2: Probit model diagnostics

Log likelihood	-189 020
Prob > chi2	0.000
Pseudo R2	0.7519
N	671 013
Sensitivity	42.61%
Specificity	99.98%
Positive predictive value	81.69%
Negative predictive value	99.86%
Correctly classified	99.84%

Source: Authors´ calculations

The second assumption behind propensity score matching is the assumption of common support, stating that there is an overlap in the characteristics of participants and non-participants. In other words, for each analysed participant, there is a non-participant that is sufficiently similar. Existence of common support can be observed from the distribution of the propensity score variable.

Figure 2: Distribution of the propensity score variable in the control group (left) and the group of participants (right)



Source: Authors´ calculations

As can be observed from Figure 2, the shape of the propensity score variable distribution is similar in the control group and the group of participants. This means that there were a sufficient number of individuals similar to participants for the whole spectrum of participants. If there were violation of the common support assumption, the shape of the distributions would differ between these two groups in that part of the distribution.

In this paper, we will also report the so called treatment effects on the treated (ATT). These are counted from the differences in income between the participant and its control. This can be formalized as follows:

$$\Delta_{ATT} = E(Y^1 | D=1) - E(Y^0 | D=1)$$

Because we do not have the information about participants if they did not participate $(Y^0 | D=1)$ we are imputing this information with information about the outcome of similar individuals. Similarity is defined on observable characteristics using the propensity score matching approach. The final formalization of ATT is, therefore, as follows:

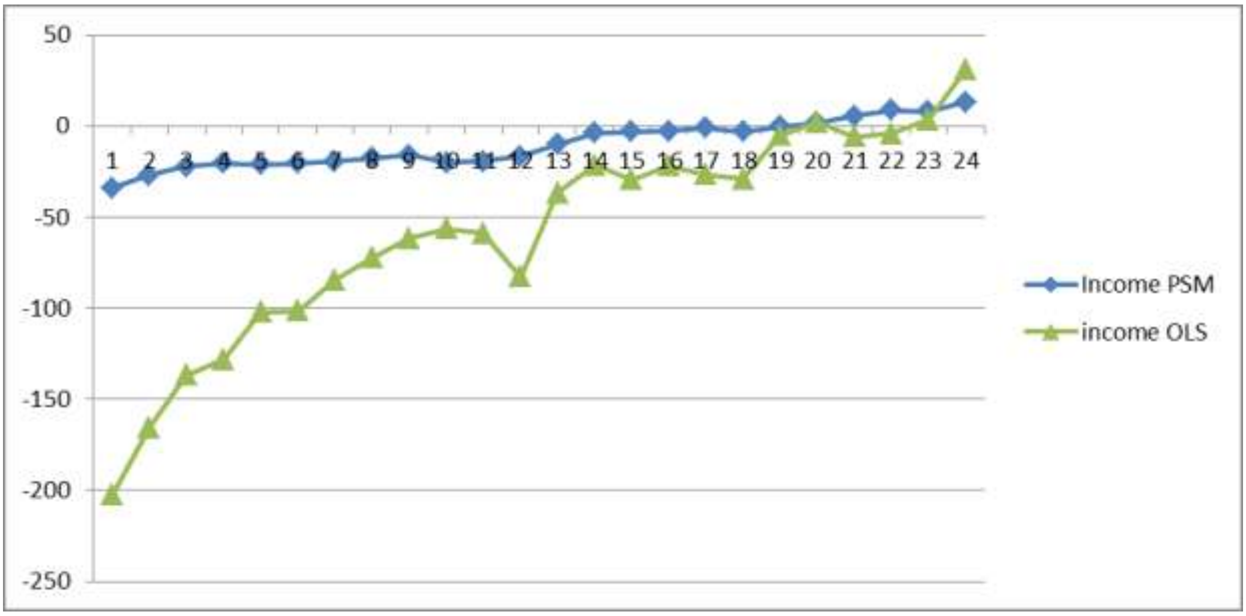
$$\Delta_{ATT} = E(Y^1 | D=1) - E(Y^0 | D=0)$$

Outcome will be measured based on the information provided from the database on social insurance. This was provided for each participant for each month of the period 2007-2013. This allows us to construct a snapshot indicator of income at the end of each month.

4 Results

First of all, we will report the comparison of results acquired by the regression approach (OLS) and the propensity score matching (PSM). Figure 4 displays estimated coefficients for the whole reference period during up to 24 months after the participation in the training ended. The x axis shows the month after the training. Lines represent the values of coefficients (treatment effects) counted from the difference in earnings of participants and non-participants. The interpretation of the coefficients is straightforward. Twelve months after the training, participants earned 82.64 euro less than non-participants according to the regression based (OLS) estimates. According to the PSM estimates, the average difference was only 16.44. In both cases, the acquired coefficients are statistically significant. Detailed results can be found in Appendix 3.

Figure 3: Comparison of OLS and PSM results in months after the training



Source: Authors´ calculations

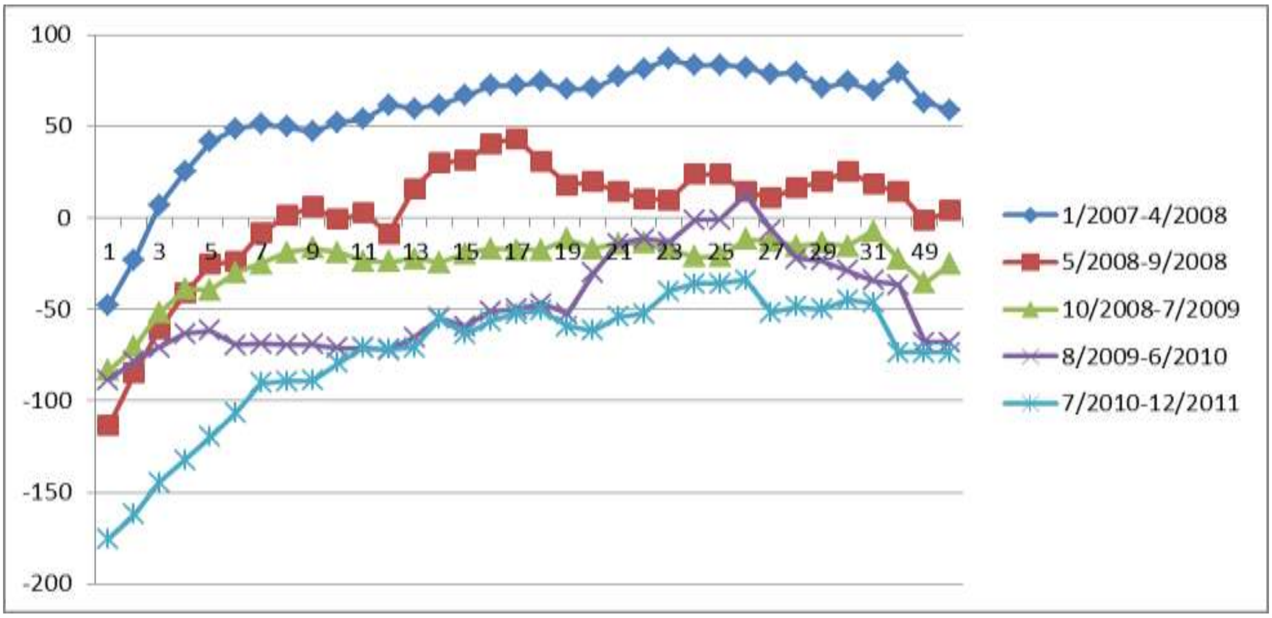
As can be observed, the PSM coefficients are more conservative in the way they quantify impact of training on income of participants, especially in the short run period up to 12 months after the training. In the medium term (12-24 months), estimated coefficients are more consistent. Moreover, the trend observable on results acquired by both approaches is consistent.

Because of technical reasons, regression analysis did not allow disaggregation of the results based on the periods identified. Therefore, we report only the PSM treatment effects estimates based on the period of implementation. Based on these results, training programs provided under the evaluated measure seem to have had positive impact before the economic crisis and especially before the introduction of the updated Law on Employment Services in May 2008.

In the first period, we can observe an initial negative effect, which lasts for 2-3 months. After the third month, significant positive earnings effects are observable. Similar situations are well described in the literature as the lock-in effect of training programme participation. Because of participating in training programs, individuals do not search for a job as actively as they would if they did not participate, which results in initially lower employment and thus also earning effect.

In the second period, the initial negative effect lasts for much longer. During the first 12 months after the training, individuals show only insignificant or significant negative effects. Positive earnings effect of the training programs is visible only in the medium run, after 12 months. Similar results can also be found quite often in similar empirical studies.

Figure 4: Average treatment effects on the treated acquired by PSM by period of implementation



Source: Authors´ calculations

During the period of unemployment inflation, when as a consequence of the economic crisis, inflows into unemployment remained above the outflows from unemployment, earnings effects of the training remained insignificant also in the medium run.

The two last periods, after the initial hit of the economic crisis and also after the change of the national project implementation, negative significant earnings effects of the trainings are provided in the short-term as well as medium term after the training.

4 Discussion

Findings of this paper point at the fact that the treatment effects of a particular training measure went into negative numbers for training programs which were implemented after the hit of the economic crisis. This is, nevertheless, only pure description of a particular situation. The analysis behind our findings cannot be used to describe or assess any relation the economic crisis had on the impact of provided training programs. Such relation, in general, would be a matter of external validity to the evidence brought in this paper. This is because we are processing only one observation (one training measure and one economic crisis) and we are not controlling even those of the relevant factors we would be able to observe.

Instead, we are only stating that the impact of economic crisis coincided with a decline of impact of a particular training measure. The single fact that there is heterogeneity in the treatment effects, measured for different periods of implementation of the same measure, should be relevant for policy makers responsible for the implementation of this measure. To answer what reasons are behind this heterogeneity remains a challenge for a future analysis inspired by the results presented here.

In the context of existing evaluations of this training measure, results presented here bring relevant and consensual evidence. They show that evaluation pointing at positive treatment effects of training programs implemented before 2010 (Bořík, et al., 2013) can be in line with reality to the same extent as later evaluations pointing at negative treatment effects of training programs implemented in 2011 (Štefánik, 2014) (Štefánik, et al., 2014).

The methodology of counterfactual impact evaluation has been widely elaborated in the recent years. There is a large pool of counterfactual techniques at hand for researchers nowadays. These techniques can be divided into those which rely on information from observable characteristics and those which work with the error term to grasp the unobservable (Caliendo, et al., 2005). Both techniques used in this paper are from the first type of techniques; they rely on observable characteristics. This is not a bad choice with respect to the robustness and quality of the data provided, but the results would gain some additional significance if they were confirmed also by a technique relying on the unobservables.

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APPENDICES:

Appendix 1: Inflow and outflow of unemployment and outflow of training by months of the reference period (Figure1)

	Inflow	Outflow	Training Outflow
200701	34114	5330	2
200702	18980	6926	47
200703	18633	9361	73
200704	17635	11082	97
200705	20843	12397	309
200706	22971	11709	608
200707	22169	14663	649
200708	18648	16383	441
200709	35197	23877	670
200710	22256	18455	1311
200711	20404	16206	1348
200712	19405	11490	1020
200801	28572	23501	418
200802	17806	13983	335
200803	16966	22510	234
200804	18699	18447	380
200805	21328	17171	664
200806	21272	19631	1291
200807	22237	16081	1466
200808	17642	18253	1322
200809	34775	25790	2073
200810	23710	18339	3282
200811	26535	16549	1877
200812	27040	11233	438
200901	38989	16435	249
200902	36446	15127	357
200903	39383	20608	2165
200904	34427	18543	7201
200905	34506	23106	3872
200906	33562	23020	1515
200907	31304	21398	516
200908	25138	29107	534
200909	48846	35054	1460
200910	30209	24797	2311
200911	28996	24162	4131
200912	26663	17648	900
201001	36946	25889	339
201002	27305	23457	218
201003	26515	30972	304
201004	23261	31288	805
201005	26744	32585	1618
201006	31894	28216	1518
201007	25491	24026	1689
201008	23558	32324	1908
201009	45518	37152	2072
201010	27059	26726	1784
201011	25933	26030	966
201012	27905	18390	273
201101	37149	28555	136
201102	27412	25587	261
201103	26973	32527	191
201104	24136	27330	162
201105	30477	32942	164
201106	31907	27397	202
201107	27862	24677	135
201108	25890	29500	132
201109	46762	37538	94
201110	30366	30667	158
201111	30893	27278	293
201112	29658	20693	501

Source: Authors´ calculations

Appendix 2: Balance improvement after matching

		Control group	Participants	Database	Balance improvement
	N	32.651	32.651	2.354.850	
Mean	mean (date of entry)	25.12.08	26.12.08	2.9.10	99,84%
	mean(length of entry)	446,3297	542,1494	311,6741	58,43%
	mean(age)	38,49674	38,32553	34,95795	94,92%
	mean(psvar)	0,4148981	0,4173019	0,0622178	99,32%
Proportion in %	Female	45,82	52,79	53,89	-533,64%
	NP	9,79	12,48	36,42	88,76%
	Single	37,39	37,7	50,77	97,63%
		Previous occupation			
	ISCO 0	15,57	17,59	30,70	84,59%
	ISCO 1	2,9	2,93	1,58	97,78%
	ISCO 2	4,8	4,73	3,18	95,48%
	ISCO 3	14,07	13,99	7,62	98,74%
	ISCO 4	8,04	7,6	4,7	84,83%
	ISCO 5	13,36	13,7	11,69	83,08%
	ISCO 6	0,58	0,62	0,93	87,10%
	ISCO 7	15,42	15,12	13,21	84,29%
	ISCO 8	10,09	9,36	9,17	-284,21%
	ISCO 9	15,16	14,37	17,23	72,38%
		Skills			
	PC	26,14	19,3	12,46	0,00%
	Foreign language	66,16	76,16	77,78	-517,28%
	Graduate	2,76	2,73	2,54	84,21%
		Level of highest education achieved			
	No elementary	0,09	0,08	0,51	97,67%
	Elementary	18,53	19,15	24,16	87,62%
	Lower socondary	0,43	0,43	1,07	100,00%
	Vocational secondary	26,11	26,11	28,21	100,00%
	Upper socondary vocational	39,29	37,72	30,05	79,53%
	Upper secondary general	5,46	5,36	4,12	91,94%
	Post-secondary	0,22	0,11	0,16	-120,00%
	First stage university	0,44	0,44	0,99	100,00%
	Second stage university	9,4	10,56	10,59	-3766,67%
	Ph.D.	0,02	0,03	0,14	90,91%
		Field of highest education achieved			
	Field of education 1	19,26	19,94	26,26	89,24%
	Field of education 2	0,34	0,53	0,64	-72,73%
	Field of education 3	24,5	24,17	21,94	85,20%
	Field of education 4	17,3	17,15	15,68	89,80%
	Field of education 5	6,23	6,31	5,18	92,92%
	Field of education 6	0,79	1,04	1,51	46,81%
	Field of education 7	20,36	20,59	19,8	70,89%
	Field of education 8	9,14	8,58	7,57	44,55%
	Field of education 9	1,81	1,27	1,02	-116,00%
	Field of education 10	0,27	0,4	0,4	-1200,00%

Source: Authors´ calculations

Appendix 3: Comparison of OLS and PSM results, complete results of Figure 3

	OLS				PSM			
Month	Coef.	S.E.	p.	N	Coef.	S.E.	p	N
1	-202,24	2,77	0	1758578	-33,77	1,54	0	61108
2	-165,42	2,13	0	1758221	-27,13	1,73	0	61108
3	-136,71	2,14	0	1757935	-22,03	1,96	0	60357
4	-127,99	2,67	0	1757909	-20,46	2,14	0	60268
5	-102	2,24	0	1757899	-21,03	2,23	0	60217
6	-101,48	2,87	0	1757898	-20,25	2,36	0	60168
7	-84,46	2,13	0	1757897	-19,27	2,45	0	60125
8	-72,31	2,04	0	1757897	-17,62	2,55	0	60094
9	-61,63	2,03	0	1757892	-15,81	2,6	0	60066
10	-56,56	2,24	0	1757867	-20,07	2,65	0	60013
11	-58,93	2,08	0	1757835	-19,35	2,79	0	59950
12	-82,64	3,19	0	1757805	-16,44	2,96	0	59889
13	-36,57	2,57	0	1757798	-9,97	3,17	0,0017	59842
14	-21,33	2,75	0	1757690	-3,72	2,94	0,2066	59622
15	-29,48	2,93	0	1757566	-2,99	3,07	0,3305	59426
16	-21,9	2,66	0	1757487	-2,57	2,95	0,3831	59282
17	-26,52	2,42	0	1757405	-1,06	3,28	0,7457	59100
18	-29,02	2,66	0	1757296	-3,03	3,19	0,3433	58907
19	-5,17	2,37	0,0293	1757220	-0,22	3,21	0,9461	58771
20	1,94	2,24	0,3865	1757125	1,68	3,07	0,5849	58600
21	-5,77	2,32	0,013	1756987	5,52	3,24	0,0888	58368
22	-3,97	2,66	0,1362	1756843	8,83	3,2	0,0058	58122
23	3,53	2,65	0,1833	1756744	7,97	3,61	0,0273	57915
24	30,77	3,63	0	1756729	13,2	3,54	0,0002	57802

Source: Authors´ calculation

Appendix 4: Average treatment effects on the treated acquired by PSM by period of implementation, complete results of Figure 4

Month	1/2007-4/2008				5/2008-9/2008				9/2008-7/2009				8/2009-6/2010				9/2010-12/2011			
	ATT	S.E.	p.	N	ATT	S.E.	p.	N	ATT	S.E.	p.	N	ATT	S.E.	p.	N	ATT	S.E.	p.	
1	-47,8	4,27	0,000	8083	-113,67	8,26	0,000	4743	-83,51	3,29	0,000	23545	-88,51	4,55	0,000	14138	-175,3	6,49	0,000	11121
2	-23,12	5,22	0,000	8083	-84,76	8,71	0,000	4743	-70,48	3,52	0,000	23545	-78,51	4,97	0,000	14138	-161,98	6,64	0,000	11121
3	6,69	5,59	0,231	8083	-60,99	9,3	0,000	4743	-52	3,92	0,000	23545	-70,79	5,26	0,000	14138	-144,78	6,87	0,000	11121
4	25,17	6,24	0,000	8083	-41,22	9,98	0,000	4743	-39,04	4,14	0,000	23545	-63,32	5,33	0,000	14138	-132,3	8,38	0,000	11121
5	41,59	6,35	0,000	8083	-25,29	10,47	0,016	4743	-39,52	4,35	0,000	23545	-61,61	5,62	0,000	14138	-119,71	7,17	0,000	11121
6	48,74	6,62	0,000	8083	-23,71	10,56	0,025	4743	-29,74	4,45	0,000	23545	-69,58	6,03	0,000	14138	-106,41	7,66	0,000	11121
7	50,96	6,82	0,000	8083	-8,25	10,41	0,428	4743	-25,19	4,65	0,000	23545	-68,7	6,16	0,000	14138	-90,19	7,53	0,000	11121
8	49,87	7,07	0,000	8083	1,15	10,83	0,916	4743	-18,96	4,73	0,000	23545	-69,61	6,37	0,000	14138	-89,14	8,4	0,000	11121
9	47,06	7,33	0,000	8083	5,94	11,02	0,590	4743	-16,44	5,3	0,002	23545	-69,24	6,48	0,000	14138	-89,13	8,26	0,000	11121
10	51,99	7,28	0,000	8083	-0,73	10,32	0,943	4743	-18,79	4,65	0,000	23545	-71,29	6,42	0,000	14138	-79,44	8,44	0,000	11121
11	53,71	7,45	0,000	8083	2,49	10,85	0,818	4743	-23,64	5,41	0,000	23545	-71,28	6,64	0,000	14138	-70,95	8,65	0,000	11121
12	61,53	7,73	0,000	8083	-8,73	21,66	0,687	4743	-24,06	4,94	0,000	23545	-72,06	6,77	0,000	14138	-71,94	8,81	0,000	11121
13	59,69	8,78	0,000	8083	16,01	11,88	0,178	4743	-22,37	4,88	0,000	23545	-65,44	7,42	0,000	14138	-71	8,94	0,000	11121
14	61,67	7,54	0,000	8083	30,2	12,07	0,012	4743	-24,28	5,93	0,000	23545	-54,7	7,16	0,000	14138	-55,25	9,03	0,000	11121
15	66,7	7,46	0,000	8083	31,29	11,11	0,005	4743	-19,56	5,26	0,000	23545	-59,12	9,21	0,000	14138	-63,65	12,59	0,000	11121
16	72,24	7,56	0,000	8083	40,38	11,44	0,000	4743	-17,06	5	0,001	23545	-51,14	9,3	0,000	14138	-56,48	9,16	0,000	11121
17	72,4	7,79	0,000	8083	43,3	11,42	0,000	4743	-17,41	5,18	0,001	23545	-50,14	11,32	0,000	14138	-52,48	8,98	0,000	11121
18	74,17	7,91	0,000	8083	30,46	11,69	0,009	4743	-17,72	5,35	0,001	23545	-46,79	8,32	0,000	14138	-50,73	9,15	0,000	11121
19	70,3	8,98	0,000	8083	18,09	13,02	0,165	4743	-11,63	5,6	0,038	23545	-52,59	11,05	0,000	14138	-59,35	9,24	0,000	11121
20	70,81	8,88	0,000	8083	19,72	10,8	0,068	4743	-16,83	5,56	0,003	23545	-30,18	10,4	0,004	14138	-61,59	9,01	0,000	11121
21	77,12	7,9	0,000	8083	14,29	11,28	0,205	4743	-14,21	5,59	0,011	23545	-14	8,07	0,083	14138	-54,12	9,92	0,000	11121
22	81,51	8,05	0,000	8083	10,31	11,99	0,390	4743	-13,7	5,6	0,014	23545	-11,37	7,76	0,143	14138	-52,55	9,88	0,000	11121
23	86,54	8,12	0,000	8083	9,82	12,19	0,421	4743	-17,19	5,96	0,004	23545	-13,64	7,89	0,084	14138	-40,09	8,95	0,000	11121
24	83,34	8,29	0,000	8083	23,61	11,56	0,041	4743	-21,26	6,25	0,001	23545	-1,24	8,84	0,888	14138	-36,03	9,61	0,000	11121

Source: Authors´ calculations