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What Works for Disadvantaged Unemployed: Private or Public ALMP Services? Evidence from Poland

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Purpose: This paper aims to assess the effectiveness of public vs. private active labour market policies (ALMP) for disadvantaged unemployed. The literature on the efficiency of contracting-out ALMP services by public institutions is not consistent. Formalism and limited scope of possible actions in public institutions stand in the way to activation of the less promising unemployed. On the other hand, contracted-out companies work as a black-box and are paid a success fee.

Design/Methodology/Approach: We used (conducted by accident) Randomised Control Trial evaluation of the contracting-out ALMP services program in Poland. We used public registers data from Poland.

Findings: We found a strong positive effect of support provided by contracted private agencies. This effect was found to be the strongest among the most dismissed groups of the unemployed.

Practical Implications: Study shows that success-fee for external institutions and lack of hard frames assures incentives compatibility that results in real improvement of the unemployed situation, even a year after the end of the intervention. This conclusion should result in adjustments in active labour market policy.

Originality/Value: The uniqueness of the study lies in scale of the RCT experiment and robustness of conclusions. The golden standard of evaluation allowed for gaining indisputable evidence on the effectiveness of particular ALMP instrument. Without this kind of analysis, public decisions can turn out to be wrong and lead to the closure of effective programmes (as it happened with contracting-out ALMP services), and investment in programmes that do not provide any effective support for those in need.

Keywords: Unemployed, RCT, Experiment, ALMP, Labour Offices.

JEL codes: J23, J24, J21.

Paper type : Research article.

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1. Introduction

All over the world, but particularly in the European Union, considerable sums are allocated to active labour market policies (ALMPs) every year, however efficiency of public spending is neither secured, nor properly evaluated. In Poland, in the years 2010-2015, 1.6 billion euros were spent annually to support this aim (Eurostat, 2017). However, the impact of these investments is not sufficiently scrutinized. Programmes are usually evaluated using misleading methods (the before-and-after method, comparisons of basic statistics across regions, or simply enumerating the services provided). This leads to lack of evidence on real efficiency of ALMPs. Only a few evaluation studies, out of a dozen requested by Labour Offices and institutions distributing European funds, rely on counterfactual methods. The huge amounts and importance of ALMP programs for the economy and society make the issue of the effectiveness of programs supporting the activation of the unemployed, one of the overriding problems of the labour market policy.

In this paper, we present evidence for the impact on unemployed of contracting out ALMP services, from the very first RCT evaluation of the ALMP programme in Poland³. The programme evaluated relied on private agencies who provided contracted services for the unemployed. The programme was run from 2015 to 2017 in the Małopolskie Voivodeship (province) of Poland.

Poland's programme of contracting-out counselling services for the unemployed reflects a common trend of increasing reliance on private service providers. The majority of ALMP interventions are conducted by Labour Offices (LOs); however, as Finn (2011) points out, they are increasingly outsourced to private companies. While many studies assess the effectiveness of various ALMPs, relatively few compare the effectiveness of ALMP interventions provided by public institutions to those outsourced to private companies. This study thus contributes to the literature by evaluating a large-scale programme of this type. The major difference between the public and private providers are:

- 1) flexibility of ALMP tools by private providers (while public LOs can use limited catalogue of interventions);
- 2) incentives compatibility, as the income for out-sourced companies depends on professional success of unemployed.

³*Interestingly, this RCT evaluation was not planned. Randomized choice of participants was used to avoid selection of the most difficult cases for the programme, and this was enforced by the Regional Labour Office. This opened ex post a possibility to evaluate the programme through an experimental study. The possibility of RCT evaluation was discovered during World Bank training: Capacity Building workshop on Impact Evaluation of Employment Programs, June 5-8, 2017.*

The intervention assumed lack of formalism and detailed controlling - contracted companies were just forced to assure basic catalogue of services for each unemployed, these were:

- 1) assessment of the professional potential and plan for the career;
- 2) individual counselling;
- 3) access to computers with internet;
- 4) access to the information on the progress;
- 5) flexible forms of contracting unemployed. Indeed, contracted companies relayed mainly on careful tutoring and identifying and overcoming barriers to employment, on the individual level.

2. Literature Review

Theoretically, there are many potential benefits, and reasons for the growing interest, in contracting-out ALMP services. First, it is recognized that private companies are more flexible and service-oriented than public institutions (Bernhard and Wolff, 2008) and have a stronger motivation to invest in cost-saving technologies and innovations that improve the quality of services provided (Bennmarker *et al.*, 2013). Secondly, public institutions can use private companies to expand the range of services provided, use the specialist skills of potential suppliers, and provide access to services not available in the public sector (Finn, 2011). State agencies are not subject to pressure from competition, which means that they are not motivated to control costs, provide high-quality services or respond to the needs and requirements of consumers (Grout and Stevens, 2003). The efficiency of public versus private support schemes was examined by Hasluck *et al.* (2013); Hales *et al.* (2003); Bernhard and Wolff (2008); Behaghel *et al.* (2014); Bennmarker *et al.* (2013); Laun and Thoursie (2014). Results are not consistent and show that ALMP instruments exhibit different levels of efficiency under different support regimes.

The efficiency of using private contractors in the example of Employment Zones in Great Britain has been demonstrated by, among others, Hasluck *et al.* (2003); Hales *et al.* (2003); Brutell (2005). According to Hasluck *et al.* (2003) unemployment in Employment Zones fell faster than in the other areas compared. Also, people from the Employment Zone who found a job were less likely to return to unemployment. Hales' research (2003) found that, a year after eligibility, respectively 34% and 24% of people in Employment Zones and compared areas were in work for a certain period. As pointed out by Rehwald *et al.* (2017) and Bennmarker *et al.* (2013), private providers have more frequent meetings and deliver prompter, more intense and employment-oriented services. Moreover, job-seekers were more satisfied with private providers (compared to public ones), applied for more positions, and were more often invited to interviews. People using Public Employment Services (PES), however, spend more time on job training and internships.

On the other hand, a huge body of work on the effectiveness of outsourcing ALMP services in comparison to the provision of services by public job centres indicates the advantage of the latter. Public service schemes are at least as good as private service provision (Behaghel *et al.*, 2014; Bennmarker *et al.*, 2013; Krug and Stephan, 2013; Laun and Thoursie, 2014). Behaghel *et al.* (2014) examined the effectiveness of job search counselling by dividing the unemployed into three groups: a group of people getting standard services provided by Public Employment Services (PES) and two groups that were randomly assigned to the public and private intensive ALMP programmes. Both intensive programmes proved to have a positive impact. However, the services of public providers were more effective than those of private contractors, particularly in the short term. Also, the study showed that a private programme is not cost-effective, leads to an increase in costs per job-seeker, while a public programme reduces costs.

In some studies, the authors found contracting-out had a conditional positive effect, but only for groups of people facing the greatest difficulties in finding a job. For example, Bernhard and Wolff (2008), using propensity score matching, showed that contracted-out employment services tend to be more effective for particular social groups (e.g., migrants and females over 49 from Western Germany, and people without qualifications and men under 25 from Eastern Germany). Winterhager (2006a; 2006b) came to similar conclusions. In Western Germany, only women, older people and those entering the labour market gained an advantage using private companies as ALMP services provider. For others, use of private service providers resulted in lower chances in getting a job.

Laun and Thoursie (2014) did not prove any difference between the situations when employment services are delivered through public and private organizations. Rehwald *et al.* (2017) also found no difference in labour market outcomes; however, private ALMP proved to be more expensive. Bennmarker *et al.* (2013) also did not prove any difference in the probability of finding a job in any of three groups they studied (unemployed under the age of 25, immigrants, disabled). The above studies indicate that, contrary to the expectations of researchers, the services provided by public institutions are generally more effective compared to those provided by private companies. If the study showed contracting-out had a positive impact, it was usually for groups of people facing the biggest problems in finding a job. As Finn (2011) points out, managing the system of subcontractors is a complex task, and takes time to learn to manage efficiently. The author suggests that financial benefits from contracting-out services may appear in time. The success of this kind of policy depends on mechanism design and the implementation of contractual arrangements.

3. The Intervention: Contracting-out Services for Unemployed

The intervention known as “contracting-out counselling services” was one of the first projects in Poland that allowed outsourcing of private counselling services by Labour Offices. The programme provided a lot of flexibility in performing

contracted tasks. Private companies were paid a success fee and were free to decide what kind of services to provide to each unemployed person, while beneficiaries were given a profile by the LO. The programme was designed for unemployed people who were to some extent disadvantaged in the labour market (with less promising profiles)⁴. Eligibility criteria were as follows:

- being unemployed for more than 12 months in the two years before the start of the programme;
- being in profile II or III, according to the official profiling tool.

Interventions had already taken place twice: in the Małopolskie Voivodeship these were in January 2015 – November 2016, and April 2016 – December 2017. The programme was suspended after the first two interventions. The decision resulted from an internal report that reviewed basic statistics and official opinions across Poland. In fact, in many regions, basic data suggested that the programme was unsuccessful, but this could have been entirely due to poor selection for the programme. Anecdotal evidence is that LOs in some regions were assigning the most difficult cases to demonstrate that private agencies could not outperform their services. If this was true, the programme would appear to be highly ineffective, especially when the whole evaluation was based on simple comparisons between the situations of the unemployed receiving the LOs' services and those who were assigned to private agencies for support.

As mentioned above, in Małopolskie Voivodeship the programme was implemented in a specific manner. The Regional Labour Office (RLO) required local offices to randomly assign participants and even offered a tool for random assignment (an Excel file with step-by-step instructions). The randomization process was implemented and monitored by officials from the RLO. In this way, contracted private agencies were provided with cases similar to those that were left within local LOs. Random assignment was conducted at the Powiat (district) level and within groups defined by participants' age (under and over 25) and unemployment profile (II and III).

This paper provides an evaluation of the first edition of the intervention (2015-2016) that was conducted in four Powiat LOs. The programme was supervised, and the RLO collected the data in Kraków. The data were collected in late 2017, allowing

⁴*In Poland each unemployed person is, based on information provided, given one of three profiles (on the basis of the quantitative score). Briefly, profile I includes highly motivated people, with professional qualifications and appropriate skills, assumed not to need intensive support. LOs offer them some forms of support. Profile II includes those who have some professional skills but for whom there is no demand in the labour market. The LO can offer these people a wide range of ALMPs. Those who are given profile III, meanwhile, have a poor chance of entering the labour market – for many possible reasons. LOs can offer them very little help. (MLSP, 2014).*

evaluation of the outcomes of the programme during 12 months after the official end of the first session.

4. Data and Methods

4.1 Methodological Issues

The main source of data gathered to assess the efficiency of contracting-out ALMP is the Syriusz System. Syriusz serves as a fundamental tool for Powiat LOs and is used to store data about the unemployed. However, Syriusz has hardly ever been used for quantitative analysis as it is difficult to extract data from it. In cooperation with our partners from Regional Labour Offices, we extracted data using SQL source codes. The initial data base was spitted into number of not consistent files, with not consistent coding of events, dates and even personal data. The extracted data needed substantial cleansing, recoding merging and constructing of final variables for the analysis.

Unemployed participants in the project were randomly assigned from the list of all eligible unemployed registered in LOs. We identified 2,171 beneficiaries of the intervention (Treatment group) who were randomly assigned and 18,979 individuals who were randomly assigned to the Control group. As outcomes we measured:

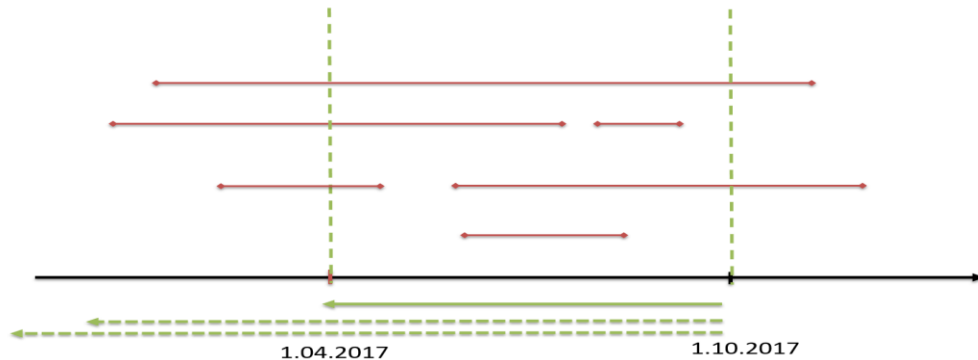
- duration of registered unemployment for the last six, nine and 12 months before data collection (before October 2017);
- the share of unemployed who again registered in LO during the last six, nine and 12 months before data collection.

We extracted a set of personal characteristics to check random assignment quality and estimate differences in the impact of the programme for different groups of unemployed. Duration of registered unemployment was measured as the total of all sub-periods for each individual, as shown in Figure 1. In each case, when, for example, looking at the last six months before data collection, only the duration between the two dashed lines was counted as the outcome. Thus, we extracted from the data the number of days for participants who registered before the six-month period and continued to be unemployed; for those who registered several times during that period; and also for those who registered just before data collection and remained on the register.

Four Powiat LOs participated in the project in the first round. Random assignment was conducted separately in each LO using the following strata:

- Young profile II unemployed (under 25);
- Older profile II unemployed (25 and over);
- Young profile III unemployed (under 25);
- Older profile III unemployed (25 and over).

Figure 1. Examples of calculations of the duration of registered unemployment for the last six, nine and 12 months before data collection (October 2017)



Thus, participants were randomly selected from 16 separate lists (four strata in each Powiat LO). In some strata, the low number of individuals did not allow quantitative analysis, especially among young unemployed with profile III (the smallest stratum had 48 participants only). Also, as the number of participants from each stratum was determined before the intervention, the assignment of participants was not proportional to the target population structure. In the two smallest strata, Treated (T) is larger than Controls (C), while in other cases C is larger than T (see detailed data in Table 1).

Table 1. Number of Treated (T) and Controls (C) in each randomization stratum

<i>Powiat code</i>	<i>Randomization group</i>	<i>C</i>	<i>T</i>	<i>C+T</i>	<i>T as a share of C+T</i>	<i>% of the total sample</i>
1201	under 25, profile II	297	30	327	9.2%	1.5%
	under 25, profile III	7	41	48	85.4%	0.2%
	over 25, profile II	999	128	1127	11.4%	5.3%
	over 25, profile III	305	150	455	33.0%	2.2%
1206	under 25, profile II	383	36	419	8.6%	2.0%
	under 25, profile III	46	56	102	54.9%	0.5%
	over 25, profile II	1892	144	2036	7.1%	9.6%
	over 25, profile III	1246	166	1412	11.8%	6.7%
1211	under 25, profile II	768	40	808	5.0%	3.8%
	under 25, profile III	186	55	241	22.8%	1.1%
	over 25, profile II	2037	167	2204	7.6%	10.4%
	over 25, profile III	1362	171	1533	11.2%	7.2%
1261	under 25, profile II	225	116	341	34.0%	1.6%
	under 25, profile III	59	123	182	67.6%	0.9%
	over 25, profile II	6296	374	6670	5.6%	31.5%
	over 25, profile III	2871	374	3245	11.5%	15.3%
Total		18979	2171	21150	10.3%	100.0%

Source: Own calculations based on the Syriusz data.

In principle, random assignment was conducted using the list of all eligible unemployed and prepared templates provided by the Regional Labour Office in Kraków. This algorithm of assignment aimed to limit the possibility of assigning participants based on their probability of success in the labour market. LOs were allowed to replace randomly assigned participants only in cases when the latter did not agree to participate in the project. Replacements were taken from a randomly sorted list of all those eligible. Unfortunately, most of the source Excel sheets are missing. The lists of randomly assigned participants were not collected immediately after randomization and had to be recovered from historical Syriusz data from October-November 2017. Reconstruction of initial random assignment lists was the most demanding task of analysis.

In the first stage of our analysis, we checked if the participants were indeed randomly assigned by comparing their key characteristics between Treated and Controls. Using Syriusz data we checked balance across Treated and Controls using education level (recoded into four levels), gender (0/1), civil status (married/single), disability (0/1), number of children, age, and number of days on the LO register over the two years before the project. Covariates imbalance analysis is described under “Robustness checks” below.

5. Results

Our analysis focuses on a comparison of the number of days in registered unemployment after the intervention. For some comparisons, we also use a simplified binary indicator showing a share of Treated and Controls who again registered as unemployed after the intervention. We looked at these outcomes for three periods: for the 12, nine and six months before the final data collection date (November 2017). We constructed our outcome variables backward from the final data collection date because the exact final date of the intervention was slightly different for each participant.

Table 2 compares an average number of days in registered unemployment before and after the intervention. Calculations are based on the whole available sample, which gives a total of 21,150 observations (2,171 Treated and 18,979 Controls). The first comparison in the table confirms that Treated and Controls both came from the same group of unemployed and were randomly assigned. This step was particularly important as it justified the process of randomization. In fact, before the intervention, there is no difference in the number of days in registered unemployment between the two groups. However, there are statistical differences when the same groups are compared after the intervention. Treated unemployed spend on average 30 days less during the last 12 months before data collection. The difference is statistically significant (at the 1% significance level) and, thanks to a large sample, precisely estimated. The difference is smaller for the shorter periods of nine and six months before data collection – 19 days less and 12 days less, respectively, with all differences estimated precisely.

Table 2. Average number of days in registered unemployment before and after intervention

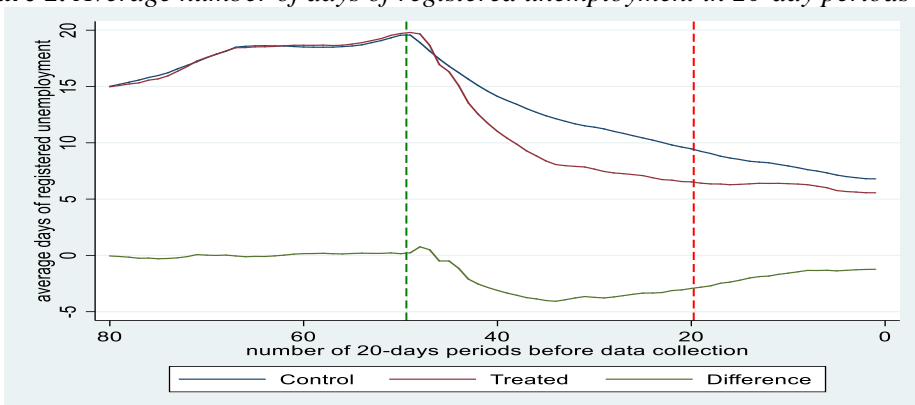
Group	Before the	After the intervention		
	24 months	6 months	9 months	12 months
Controls	630.2	65.0	101.3	140.8
Treated	630.8	53.1	81.8	110.9
Difference (T-C)	0.5 (2.7)	-11.9 (1.9)	-19.5 (2.7)	-29.9 (3.6)

Source: Own calculations based on the Syriusz data. Standard errors are in parentheses.

The results above suggest that the impact of the intervention is positive, but tends to diminish over time. Figure 1 compares average days in registered unemployment in 20-day periods before and after the intervention until data collection. The intervention started on the 15th of January 2015, which is close to 1,000 days (or 50 20-day periods) before data collection on the 1st of October 2017 (988 days exactly). It finished in October 2016. The starting date of the intervention is shown by a dashed green line, the end date is by a dashed red line (as mentioned above, not all participants started and finished on exactly the same day). In the dataset, fewer than 10 participants out of more than 2,171 Treated were still receiving support after the official end of the project (red vertical line in figure 2).

Figure 2 also shows the difference in the average days between Treated and Controls (Treated minus Controls). The impact of the intervention is positive and decreases the number of days in registered unemployment. The difference in number of days is close to zero at the beginning and increases to reach a maximum of four days per 20-day period on average around 34th and 35th period. After that it declines, but at the time of data collection it is still negative, meaning that Treated are in registered unemployment for a smaller number of days on average.

Figure 2. Average number of days of registered unemployment in 20-day periods



Source: Own calculations based on the Syriusz data. See Table A1 with detailed data in the Annex.

The overall impact can also be shown by comparing the percentages of people who, after the intervention, at any point registered again as unemployed (or did not leave the register). During the 12 months before data collection, 53.8% of Controls registered as unemployed compared to 44.5% among the Treated, giving a difference of more than nine percentage points. These numbers do vary, however, within groups of participants.

Figure 3 compares the percentages of people who registered as unemployed among Treated and Controls on average, but also within four groups in which random assignment was conducted: young (under 25) and older (over 25) unemployed classified as profile II or III (see Table 3 for detailed results). These groups are important as there are different LO actions prescribed for each group. The crucial difference is that profile III unemployed typically do not receive any support unless there is a special programme like the one we are analysing here. Thus, for this group, the impact evaluation compares (an almost total) lack of support with the outsourced “black box” support received during the intervention. The profile II group receives typical support from LOs. Thus, for this group, the impact evaluation compares support from LOs to the support provided by private agencies.

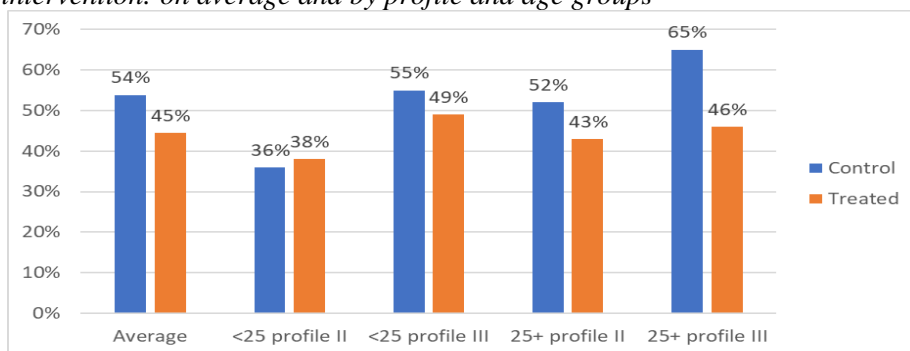
Figure 3 shows a much larger impact for the profile III unemployed, especially those over 25. On one hand this is not surprising, as without the intervention this group is receiving very limited support. On the other hand it shows that, despite the assumption that this group lacks any reasonable employment perspective, our impact evaluation demonstrates large benefits from privately provided assistance. In effect, among Treated unemployed over 25, those with profile III have a similar probability of going back on the register as those with profile II (43% with profile II return, compared to 46% with profile III). This contrasts with the unemployed in the Control group, where 65% of those over 25 and with profile III return to the register compared to only 52% among those over 25 and with profile II. This suggests that the intervention is more effective in profile III and among older unemployed, although Figure 2 also shows the intervention has positive effects even in the group under 25, but in profile III only.

A more detailed analysis suggests that the impact of the intervention increases with age, but this effect varies by unemployment profile and by gender (see Table 3 for detailed results). Figure 4 shows the average days of registered unemployment separately for those in profile II and III categories. In each category, separate lines by treatment status and gender are shown over age. For the Control group, the probability of returning to the register increases with age, while in profile II the probability of returning is smaller than in profile III, and is also larger for women. Among the Treated, the effect of age is much smaller, and differences between genders are less clear.

Table 3. Detailed results for the whole sample and subgroups

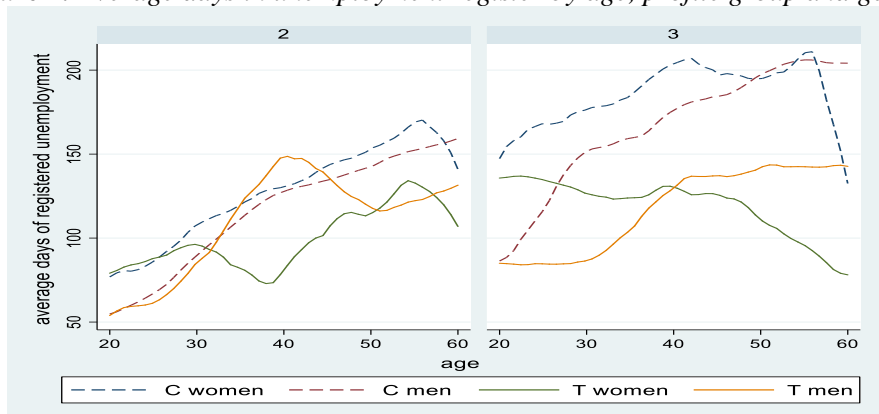
	Average number of days on the register				Percentage of those who returned to the register			
	C	T	T-C	S.E.	C	T	T-C	S.E.
Full sample								
under 25, profile II	69.4	71.4	2.1	8.6	36.4%	37.8%	1.4%	3.4%
under 25, profile III	126.5	118.4	-8.2	12.6	55.4%	49.5%	-5.9%	4.2%
over 25, profile II	130.3	108.6	-21.7	5.6	51.6%	43.2%	-8.4%	1.8%
over 25, profile III	182.5	120.9	-61.6	5.9	64.8%	45.9%	-18.9%	1.8%
Men								
under 25, profile II	55.4	61.7	6.4	10.4	32.3%	37.5%	5.2%	4.6%
under 25, profile III	88.2	83.9	-4.4	17.6	45.0%	38.6%	-6.4%	6.6%
over 25, profile II	130.7	117.9	-12.8	7.9	52.8%	45.7%	-7.1%	2.6%
over 25, profile III	183.5	131.0	-52.5	8.2	65.3%	50.9%	-14.4%	2.4%
Women								
under 25, profile II	81.4	82.9	1.5	13.6	39.9%	38.2%	-1.7%	5.1%
under 25, profile III	155.8	138.4	-17.4	16.9	63.3%	55.7%	-7.6%	5.3%
over 25, profile II	129.9	99.7	-30.2	7.9	50.5%	40.7%	-9.8%	2.5%
over 25, profile III	181.6	109.7	-71.9	8.6	64.4%	40.3%	-24.1%	2.5%
by education level								
primary or lower secondary	154.5	135.7	-18.9	6.5	59.9%	53.4%	-6.5%	2.0%
basic vocational	139.9	102.5	-37.4	6.9	54.9%	41.2%	-13.7%	2.2%
secondary vocational	134.5	80.0	-54.5	8.9	50.9%	33.6%	-17.3%	2.8%
secondary general and post-secondary	134.3	112.6	-21.7	8.8	52.3%	46.4%	-5.9%	2.8%
tertiary	132.8	105.2	-27.6	10.7	50.3%	40.6%	-9.7%	3.4%
by length of previous unemployment (24 months before the intervention)								
less than 24 months	113.4	97.4	-16.0	4.6	48.3%	42.7%	-5.6%	1.6%
full 24 months	171.8	124.8	-47.0	5.3	61.2%	46.4%	-14.8%	1.6%

Source: Own calculations based on the Siriusz data.

Figure 3. Percentages of Controls and Treated who registered as unemployed after the intervention: on average and by profile and age groups

Source: Own calculations based on the Siriusz data.

Figure 4. Average days in unemployment register by age, profile group and gender



Source: Own calculations based on the Syriusz data.

Impact of the intervention also varies by education level, with the unemployed having vocational (in particular secondary vocational) education benefiting from the strongest positive effects of the intervention. The difference between Treated and Controls in the average number of days in registered unemployment in the last 12 months before data collection was negative for all education levels, meaning the impact of the intervention was on average positive in all cases. However, this difference was around 54 days for those with secondary education, 37 days for those with basic vocational education, 28 days for those with tertiary education and around 20 days for those with post-secondary or basic or lower secondary education (see Table A3 in the Annex for detailed results).

Finally, the intervention also has more profound effects for the long-term unemployed. Among those eligible for the intervention but who were registered for a full 24 months before the intervention, in the Control group 61% again registered as unemployed during the 12 months before data collection compared to 46% in the Treated group. Among those eligible who experienced some periods of employment in the two years before the intervention, in the Control group 48% registered again compared to 43% in the Treated group. Thus, the difference is 15% for those with a long-term, uninterrupted unemployment history, while for those with a short-term unemployment history it is only 5%.

6. Robustness Checks

First, we checked whether the Treated and Control groups both had balanced characteristics regarding additional covariates. We ran 16 logit regressions separately for each stratum in which randomization was conducted. We found only small differences in some strata related to an imbalance of education level (in two strata), number of children, civil status and number of days in registered unemployment before the intervention (each in one stratum).

We conducted robustness checks to see if the results would change with additional correction using propensity score matching (PSM), balancing key characteristics between Treated and Controls. When using the full sample, we found some imbalances which are due not to poor random assignment but mainly to imbalance in the number of participants in each stratum, which results in imbalance in age, unemployment profile and related characteristics like education level (see propensity score test results in Table 4).

Table 4. Propensity score balance test

<i>Variable</i>	<i>Treated</i>	<i>Control</i>	<i>% bias</i>	<i>p>t</i>	<i>V(T)/V(C)</i>
education level	2.4993	2.7063	-15	0.000	0.92*
gender (female=1)	0.50668	0.50761	-0.2	0.934	.
married=1	0.43482	0.47753	-8.6	0.000	.
special needs	0.08291	0.08035	0.9	0.678	.
number of children	0.79322	0.7449	4	0.083	1.09*
age	40.277	42.362	-15.8	0.000	1.19*
days on register before intervention	630.76	630.25	0.4	0.853	1.02

Source: Own calculations based on the Syriusz data.

However, a PSM estimate for the full sample gives results that are very close to those obtained with experimental data without PSM (see Table 5). This suggests that these small imbalances in covariates do not affect estimates of treatment effect.

Table 5. Propensity score matching adjusted estimates of the average number of days in registered unemployment (in comparison to results in Table 2)

<i>Period</i>	<i>Sample</i>	<i>Treated</i>	<i>Controls</i>	<i>Difference</i>	<i>S.E.</i>	<i>T-stat</i>
6 months	Unmatched	54.7	67.2	-12.6	1.9	6.46
	ATT	54.7	68.8	-14.1	3.0	4.71
9 months	Unmatched	84.2	104.6	-20.5	2.8	7.22
	ATT	84.2	107.8	-23.7	4.4	5.41
12 months	Unmatched	113.8	145.3	-31.5	3.7	8.48
	ATT	113.8	149.0	-35.2	5.7	6.14

Note: Single nearest-neighbour method. Sample size of 19,796 observations is lower than for the main estimates in Table 2 due to missing data on some covariates.

Source: Own calculations based on the Syriusz data.

Finally, we conducted PSM on key covariates (education level, gender, civil status, number of children, special needs status, number of days in registered unemployment before the intervention) separately in each stratum and compared them to treatment effects estimates before matching. The results were nearly identical with a PSM-adjusted ATT estimate of around 34.3 days less in registered unemployment during the last 12 months before data collection compared to the unadjusted ATT estimate of 31 days.

7. Discussion

This paper compares the effectiveness of public instruments with private services. The results clearly show the positive impact of the latter. The reasons for that can be found in differences in flexibility of tools and system of counsellor incentives. LOs have very limited freedom in offering services. For the profile II unemployed, an LO can use available (but precisely defined) instruments, however for profile III unemployed the LO is hardly allowed to provide any services. At the same time, contracted private providers had freedom with the tools they used and were just forced to assure basic catalogue of services for each unemployed. The intervention assumed lack of formalism and just basic controlling of agencies. Not less important is incentive compatibility of counsellors, as the revenue for out-sourced companies depended on real professional success of unemployed. Incentive compatibility mechanisms are almost lacking in the public LOs.

The strongest positive effect of support provided by contracted private agencies was found to be among the most dismissed groups of the unemployed. The positive effects of outsourcing services on LOs clients are especially strong for older unemployed people and those with profile III. Moreover, the results suggest that the programme is more effective for women and unemployed people with vocational education, especially secondary vocational education. As mentioned, within this group, under present law, LOs have very limited instruments at their disposal. The strong positive effect lessens when we compare the impacts within the profile II group of unemployed. This group receives regulated by law support from LOs, with all available instruments for active labour market policies. Thus, for this group, the impact evaluation allows us to compare the support from LOs to the support provided by private agencies. The positive effect of the contracted-out services compared to that of normal public services is not obvious within this group.

The results contribute to the understanding of how likely people from disadvantaged groups (profile III) are to succeed in the labour market. It appears that assuming their little potential and not offering them help is not efficient. The programme proved that for groups categorized by LOs as having a little chance of success in finding employment, private contractors get results. This group does not respond to standard ALMP instruments; instead it needs to be guided in a non-standard way towards finding suitable employment. This might be a group for which proper counselling provides a chance of returning to legal employment. It would appear that with this group the authorities decided to “cut their losses” prematurely.

Moreover, this paper should be important for stakeholders in two general respects. First, it shows how powerful (and doable) RCT evaluation is on the labour market, where programme evaluations are usually limited to before-and-after comparisons or even focus on input data only, e.g., a list of services provided. Second, it shows that the Ministry in Poland suspended a highly effective programme, basing its decision on a misleading report that cannot be considered an evaluation. This paper

demonstrates that finding effective programmes in active labour market policies is possible, but requires careful evaluation using RCTs or at least counterfactual, quasi-experimental methods. Without these, public decisions can turn out to be wrong and lead to the closure of effective programmes, and investment in programmes that do not provide any effective support for those in need.

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