

BEAUTIFUL SERBIA

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ABSTRACT

This paper evaluates *Beautiful Serbia*, an active labor market program operating in Serbia and Montenegro since January 2004, administered and co-financed by the United Nations Development Program. Program participants proceed through two stages: a vocational training stage and a temporary employment stage in private firms contracted for refurbishment projects. Accession to the second stage is competitive, and participants receive a market wage.

We evaluate the program impacts on unemployment probabilities, employment probabilities, employment structure and a range of individual welfare indicators applying matching techniques to a rich survey data set covering the universe of participants and a sample of non-participants.

Our findings suggest that both vocational training and temporary employment have a positive net impact on individuals. However, on the basis of cost-benefit analysis, we conclude that only the temporary employment is efficient. Employment effects of the vocational training are not sufficiently large to recover the costs of the program.

Keywords: active labor market policy, program evaluation, matching, cost-benefit analysis, Serbia and Montenegro

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1 INTRODUCTION

Beautiful Serbia (BS) represents an active labor market program (ALMP) operating in Serbia and Montenegro since January 2004, administered and co-financed by the United Nations Development Program (UNDP). The program has been implemented with the support and co-financing from the Ministry of Labor, Employment and Social Policy (MoLESP) and is fully incorporated into the National Employment Service (NES).

The BS program consists of two main components: first, the provision of vocational training to long-term and otherwise disadvantaged unemployed individuals and second, their subsequent temporary employment by contracted firms to refurbish public buildings and spaces. The intended outcomes are not only net job creation, but also improved quality of life in municipalities covered by the program, and an increased capacity of MoLESP, NES and target municipalities to conceive, implement and monitor active labor market programs.

This report evaluates the net impact of the BS program on participants, employing standard econometric procedures. The primary objective is to assess the program's effectiveness in terms of increased employability, better integration into the labor market and relative wage gains of participants. The difference between various participants' outcomes with and without the program is estimated using a so-called quasi-experimental approach, i.e. participants are compared only with comparable non-participants by employing a matching procedure. Furthermore, the report evaluates general effects of the program on the improvement of living conditions in the municipalities covered by the program and the program's overall efficiency using the tools of cost-benefit analyses.

The remainder of this report is organized as follows. Chapter 2 gives a brief overview about the BS program within the context of the situation in the construction sector and the general labor market in Serbia and Montenegro. Chapter 3 discusses the data of the empirical analysis. After explaining the evaluation strategy in chapter 4, the program impacts are quantified in chapter 5. Chapter 6 provides a cost-benefit analysis. Finally, chapter 7 summarizes and gives policy recommendations.

2 “BEAUTIFUL SERBIA”

The BS program was intended to replicate the UNDP-supported program Beautiful Bulgaria, which is currently implemented in 102 Bulgarian municipalities.¹ The success of this program led to the assumption that the design of the project can be adapted to Serbia and Montenegro and it will successfully work also there.

The BS program consists of two different and almost independent components: a vocational training stage and (subsequent) temporary employment in the construction sector. The program has been implemented in Belgrade during 2004, in Niš during 2004 and 2005, and in Zrenjanin during 2005. Besides UNDP and MoLESP, also the city beneficiaries as well as the governments of Canada, the Netherlands, Austria, and Greece financially supported the BS program.

The training measure within the BS program lasts for three months and is full-time. It provides certified vocational training for the constructional sector as mason, carpenter or painter. Its intended target group consists of long-term and otherwise disadvantaged unemployed persons, identified as such by the NES. However, no sanctions are applied if a person refuses to participate. Therefore, participation in the training measure can be considered as voluntary. The compensation for participants during the training period amounts to about 30 percent of the average national wage.²

Subsequently, the training participants are intended to work in temporary jobs provided by contracted firms to refurbish public buildings and spaces. However, the contracted firms themselves select employees hired within the projects of the BS program. Moreover, the wages for those workers are set competitively by the firms. The firms get a lump-sum payment for the project and are in return required to employ a specific share (40–60 percent) of workers who are identified by the NES as previously unemployed and otherwise disadvantaged within the project. Therefore, it could well be the case that former training participants are identified by the NES, selected by the firms, accept the competitive wage and thus work within the program’s refurbishment

1 See the Beautiful Bulgaria program’s website www.beautifulbulgaria.com for more information.

2 Participants that were entitled to any kind of support before the training started receive 110 percent of this amount during the period of training.

projects. But it is neither necessarily the case that training participants later on work in the program's refurbishment projects, nor that previously unemployed workers hired for these projects have participated in the program's training measure before.

In total, the BS program provided a three month vocational training to 238 unemployed persons.³ More than half of them were subsequently hired by contracted firms within the program's refurbishment projects, together with a similar number of workers who did not attain training. 321 men were temporarily hired and the program generated 1014 monthly salaries paid out through 16 contracted companies. Overall, 35 public buildings and spaces were refurbished: 22 buildings in Belgrade, 11 buildings in Niš, and 2 buildings in Zrenjanin.⁴ In principle, the BS program was available for both men and women, but in fact only men participated.

As the training measures and subsequent temporary employment opportunities within the BS program both relate to the construction sector, the situation and development of this sector of the Serbian and Montenegrin economy should be taken into consideration. Table 2 indicates that although the number of employees in the construction sector declined between 1997 and 2003 by about 25 percent, the same is true for the number of employees in the whole economy of Serbia and Montenegro. Hence, the share of employees in the construction sector was rather stable during this period. The GDP of the construction sector more than quintupled between 1997 and 2002, while it more than septupled for the whole Serbian and Montenegrin economy. Therefore, the share of the construction sector in total GDP declined.

The construction sector shows a high incidence of project-based jobs. Therefore, seasonal employment is a frequent employment outcome for workers in this sector. During the season, workers are paid somewhat higher wages to compensate for the off-season period without earnings. Wage payments in cash are quite common. This latter fact suggests the conjecture of a high incidence of informal work in the construction sector.

3 Actually 252 Persons were enrolled, but only 238 Persons completed the training.

4 See Table 1 for a detailed list of buildings that were refurbished. Additionally, the number of previously unemployed workers that were hired, the number of salaries paid to these workers, and the total costs of the executed works associated with the respective project's site are depicted.

In general, working in the shadow economy seems to be a widespread phenomenon in Serbia and Montenegro and at least to some extent accepted.⁵

The Serbian and Montenegrin economy is considered to pass through a transitional phase. The country has initiated a package of economic reforms aimed at restructuring and liberalizing the economy. Some positive results already materialized, but the process of ongoing reforms is also associated with growing poverty and rising unemployment. For instance, the Serbia and Montenegro Statistical Office (2004) reports that on average more than 560,000 people were registered as unemployed in 2003. This translates into an unemployment rate of 15.2 percent, defined as the percentage of unemployed within the economically active population. A share of 76.4 percent of these men and women had already been unemployed for more than one year.⁶

These figures point to a high importance of employment opportunities within the Serbian and Montenegrin population. In fact, the goal “good employment opportunities” is ranked second out of a number of parameters that the desired society should have according to UNDP (2004). Only “decent living standards” seem to be more important, but e.g. parameters such as “political stability”, “social justice” or “the rule of law” are given lower priority.

Overall, the mentioned facts about the labor market raise the issue of active labor market programs in Serbia and Montenegro as temporary measures to alleviate the unemployment impact of the ongoing reform process, at least until the conditions of a rapid and sustained economic growth are established.

5 This paragraph is based on information obtained in personal interviews with Mihail Arandarenko and Nenad Moslavac. Both of them are regarded as experts of the Serbian and Montenegrin labor market.

6 Furthermore, Arandarenko (2004, Table 9) states that the increase in the unemployment rate of Serbia and Montenegro amounts to 73 percent between 1993 and 2000.

3 DATA

The data of this report is based on surveys conducted by GfK Belgrade in October and November 2005. Table 3 shows the number of interviews planned and realized for each of the relevant groups. A sizeable number of persons could either not be found or refused to participate in the face-to-face interviews. As a result, only about 60 percent of the initially planned interviews were accomplished.

In total, one can distinguish six different groups within the 402 realized interviews. The first three groups are participants in the BS program: they either participated in the training stage (58 persons), in the temporary employment stage (29 persons), or in both stages (81 persons). Therefore, we observe a total number of 168 participants in our data. The comparison group consists of individuals who were officially registered at the NES in January 2004 and did not participate in the BS program at all. This group consists of 195 persons. Regular workers in the contracted firms (the so-called benchmark group) were employed by the contracted firms already before the BS program had started and amount to 27 persons. Finally, information is available on 12 contracted firms that won the construction tenders and operated the refurbishment projects.

Therefore, a total number of 363 observations on participants and non-participants in the BS program is included in our data. However, only 288 observations were kept due to missing values in the individual employment history (32 observations), in the previous unemployment duration of the individual (35 observations) and in the last income from other sources of support (1 observation). Additionally, 7 persons who did not participate in the BS program ended up in the statuses 'pensioner' or 'student', respectively, and were excluded from the comparison group as they do not seem to be closely attached to the labor market.

Table 4 shows the final distribution of the total number of observations across the six different groups of participants, non-participants, regular workers, and contracted firms. Table 5 illustrates the distribution of observations on (non-)participants across the two stages of the BS program (training and temporary employment).

4 METHODOLOGY

Given the information available, the primary objective of this report is to evaluate the program effectiveness and efficiency. For this purpose, we seek to compare labor market statuses (unemployment probability, employment probability), wages and subjective welfare (e.g., social contacts, health status) between those who participated in the BS program and those who did not.

For a correct assessment of program effects, it is important and necessary to “compare the comparable” (Heckman *et al.*, 1999). This means that we need to compare the program participants – the so-called *treatment group* – only to those non-participants who could have participated in the program as well, i.e., had an equal chance to be selected for participation in the program as those who were actually treated. A comparison group designed in this way is referred to as *control group* in the evaluation literature.

In what follows, we describe the main methodological problems to construct the treatment and control groups in context of the BS program, and the solution concepts we apply.

4.1 EVALUATION PROBLEM

Evaluation generally has to deal with a serious problem if the effects of participating in a specific program should be quantified compared to that what would have been without doing so. This problem naturally arises because it is impossible to observe individuals in two different states of nature (participation and non-participation) at the same time and place. Therefore, it is the principle task of any evaluation study to find a credible estimate for the counterfactual state of nature.

There are basically two methods to estimate the counterfactual situation: randomized experiments and non-experimental (also called quasi-experimental) methods. In principle, randomized experiments provide the easiest solution to recovering the desired counterfactual. In randomized experiments, individuals eligible for participation are randomly assigned to a treatment and control group. Since these groups differ from each other (on average) neither in observable nor in unobservable characteristics and the

control group can be considered as “identical” to the treatment group, the average difference in outcomes between the two groups provides a simple answer to the counterfactual question.⁷

While the BS program has not been designed as a randomized experiment, the data for the evaluation analysis was constructed to mimic an experimental situation. For each member of the treatment group, a matched partner with the same observable characteristics was drawn from the official unemployment registry. The intention was to create a control group which would resemble the treatment group as much as possible. The individual characteristics available for this matching procedure were age, education, and place of residence (Belgrade, Niš, or Zrenjanin). The matches had to be based on only very few controls, due to a lack of comprehensive or up-to-date unemployment registries.

However, if additional characteristics did play a role for determining the chances to participate in the BS program, one could not consider the treatment and control groups as “identical” like in a randomized experiment. In this case, a simple comparison of mean outcomes in the two groups would not be sufficient. Moreover, the substantial differences between the number of planned and accomplished interviews in both groups could make this approach useless since the selection of the control group was based on planned rather than on accomplished interviews.

To assess whether program participation can be regarded as quasi-random in our data, we need to compare the characteristics of participants and non-participants. Considering the two-stage procedure of the BS program, one may in fact distinguish four different “treatments”, or “programs”. This distinction allows measuring the specific effects of the program’s two individual stages as well as the impact of the combination of the two stages.

The first treatment (henceforth referred to as treatment 1) is participating in the BS program at all, which covers individuals who participated either in the training stage, the temporary employment stage, or in both stages. The second treatment (henceforth

⁷ Often randomized experiments are politically or socially not feasible. Moreover, they are in practice not entirely free of complications: see Heckman and Smith (1995) for a discussion of the advantages and disadvantages of the randomization approach.

treatment 2) is participating in the complete BS program. This treatment covers only participants in *both* the training and the temporary employment stage. The third possibility is participating in the training stage of the program only (treatment 3). Finally, individuals may participate in the temporary employment stage of the program only (treatment 4). In all cases, the potential control group consists of individuals who did not participate in any part of the BS program.

Table 6 shows the number of observations included in the treatment and control group for each of those four definitions. In addition, Figure 1 clarifies the structure of the various treatment and control groups which are analyzed in what follows.

Initially, we perform statistical tests of the hypothesis of random assignment to participation, i.e., random differences between the treatment and control group). In particular, we test statistically whether the means of important socio-demographic characteristics are significantly different between treatment and potential control groups. If the hypothesis of random assignment is rejected, it may be misleading to compute net effects of the program as the difference in the average outcomes between participants and non-participants.

Tables 7-10 show the results of these tests for our four treatment and comparison groups. The tests indicate that regarding any of the treatments, the treatment and comparison groups are significantly different in the means of important characteristics.

More precisely, it appears that the treatment groups are substantially better positioned in the labor market than the potential control groups. Across all treatments, members of the treatment groups experienced significantly shorter spells of previous unemployment, were significantly more often employed during the last three years, and are more actively searching for a job than members of the potential control groups. Moreover, treated individuals tend to be on average younger, less likely to be married, more likely to live in Belgrade, and less likely to be disabled. As these characteristics will probably positively affect employability, one would expect that a simple comparison of mean outcomes between participants and non-participants overestimates the impacts of the BS program on labor market outcomes.

Based on these findings we conclude that the hypothesis of random differences between the treatment and comparison group *can* be rejected for all four program types. Therefore, we have to apply a non-experimental method accounting for the individual probabilities of program participation, in order to construct proper control groups and to calculate unbiased impacts of participation in the different stages of the BS program.

4.2 MATCHING APPROACH

Nowadays the most common technique to solve the evaluation problem when the participants and non-participants are not randomly assigned to a labor market program is the matching approach. It mimics a randomized experiment *ex post* by constructing a control group that resembles the treatment group as much as possible. In particular, after matching the members of the control group, considering their observable characteristics, have a probability to be selected for participation in the program comparable to the members of the treatment group.

We observe in our data many variables presumably influencing both the selection into the program as well as labor market outcomes. Hence, it appears reasonable to assume that selection into the program and labor market outcomes are independent conditional on these observables.⁸ Under this assumption we apply *one-to-one nearest neighbor matching with replacement*. This method consists of two steps: (1) an estimation of the individual probabilities to participate in the program or not, depending on a set of observable characteristics; (2) matching of participants and non-participants on the basis of these estimated probabilities. One-to-one matching implies that each member of the treatment group is matched with a single member from the control group. Furthermore, nearest neighbor matching implies that the pairs are matched according to the minimum distance of the predicted probabilities of program participation. Finally, matching with replacement means that the data on individuals in the control group may be used more than once, provided that they are the nearest neighbor of an individual in the treatment group.

⁸ This is the so-called conditional independence assumption, which ensures that the matching approach indeed mimics a randomized experiment *ex post*.

We begin with a discussion of the determinants of program participation to be estimated in the first step. The impact of individual characteristics on the likelihood of participating in the BS program is estimated employing standard (probit) regressions on the treated and non-treated. The estimated coefficients reveal insights about the factors influencing the selection into the treatment. But they may also capture factors driving attrition from the survey, i.e. factors explaining differential non-response rates in the respective treatment and control groups.

Our preferred specification of the regression model includes a full range of explanatory variables, which are defined in Table 11. Note that we include individuals' place of residence through a dummy variable that takes the value of one for individuals living in Belgrade and zero otherwise. This variable is supposed to measure regional variation in program participation rates. It will also capture most of the variability in the year of program entry, since all participants in Belgrade entered the BS program in the same year (in 2004). Therefore, information about when the program had started is not included in our specification of the probit model.⁹

Tables 12a and 12b exhibit the estimation results for the various treatments.¹⁰ For all program types, the signs of the estimated impacts are the same. The estimated age pattern implies that program participation rates are lower for older people. Being married, being disabled, receiving benefits, as well as having participated in any ALMP measure generally reduce the probability of treatment. Moreover, the probability of treatment is higher if a person lives in Belgrade, belongs to the ethnic group Roma, is a homeowner, has low education, was previously unemployed for four years or less, has

⁹ We have tried several specifications of the probit model. The results did not change qualitatively. For instance, including the number of (small) children living in the household does not change the predictions since all individuals in our sample are men for whom age and marital status already capture most of the effect possibly associated with children. Our chosen specification appears to deliver the best overall predictions of program participation rates.

¹⁰ In technical terms, the reported coefficients represent so-called marginal effects. Marginal effects reveal the percentage change of the program participation rate in response to a one percentage point change in the explanatory variable, respectively the percentage change of the program participation rate if a dummy variable changes from value zero to value one, holding the value of all other explanatory factors constant.

been employed during the last three years, has actively searched for a job, has got a high desire for a job, or high chances to find a job.¹¹

Considering the statistical significance of the above mentioned general effects, individuals with comparatively shorter previous unemployment durations and persons who are more actively searching for a job are significantly more likely to participate in any part of the BS program or in the complete BS program. Married men and individuals who have already participated in any ALMP before are significantly less likely to participate in these treatments.

Members of the ethnic group “Roma”, people living in Belgrade, homeowners, and men with a high desire for a job have a significantly higher probability to participate in any part of the BS program (treatment 1). The probability of this treatment is significantly lower if a person has changed his place of residence in the past five years.

An interesting pattern arises with respect to the employment history of a given individual in the last three years: while the fact of having been employed significantly increases the probability of participation, this probability significantly decreases in the share of employment during this period. Adding up the two effects reveals that they cancel out if the individual was employed for about 18 months (or half of the period under consideration). A longer period of employment within the last three years thus reduces the probability of participating in the BS program at all.

Considering participation in the training stage or the temporary employment stage of the BS program only, the probability of treatment is significantly higher for members of the ethnic group “Roma”, people living in Belgrade, homeowners, and persons who were previously unemployed for at most twelve months. The probability of participation in training only (treatment 3) is significantly higher for married men, for persons residing in Belgrade, and for individuals who were previously unemployed between 13 and 36 months or have high chances to find a job. A significant positive influence on the probability of participation in temporary employment only (treatment

11 The variable ‘disabled’ is excluded in the probit equation of treatment 2, since no treated individual is disabled. Therefore, 13 persons of the control group were also excluded because of their disability. The variable ‘ALMP participation before?’ is excluded in the probit equation for treatment 4, since no treated individual has participated in any ALMP measure before. Therefore, 11 persons of the control group were also excluded because of their previous ALMP participation.

4) is found for individuals who actively search for a job. The employment history of a given individual in the last three years significantly influences the probability of treatment in either the training or the temporary employment stage: persons with higher shares of employment during this period are significantly less likely to be treated. However, having been employed at all significantly increases the probability of treatment only for the temporary employment stage.

In sum, the probit results raise suspicion that the BS program has *not* reached its intended target group of long-term unemployed and otherwise disadvantaged people very well. For instance, short-term unemployed persons are more likely to participate in the program across all definitions of treatment. However, this interpretation should be treated with some caution, given that our sample is presumably not representative of the entire unemployed population in Serbia and Montenegro.

In a second step, we implement the one-to-one nearest neighbor matching principle by using the estimated parameters on display in Tables 12a and 12b to predict the probability to participate in a treatment – the so-called *propensity score* – for each individual in the treatment and comparison groups. The propensity scores are used to match participants with comparable non-participants. For each treated individual, we look for the one individual among the non-participants who is the closest neighbor in terms of the predicted probability of being treated. In other words, for each pair of participant and non-participant the absolute difference in terms of the estimated propensity to participate in a certain treatment is minimized.

Because the sample sizes, especially of the non-participants, are relatively small, we opt for matching with replacement. This means we allow for the possibility that different participants are matched with the *same* non-participant. To ensure that the matched pairs have reasonably similar probabilities to be treated, we exclude participants for whom the predicted probability to be in the program is larger than for any individual in the comparison group. In this way we achieve so-called *common support*.

We must stress that the general precondition for a ‘good’ matching is not fulfilled in our data. The ratio between the number of treated and the number of suitable controls is in many instances close to one (or even above). In other words, there are only as many (or

even fewer) observations in the comparison group as in the treatment group. Furthermore sample sizes are in general comparatively small. We therefore expect statistically significant treatment effects (at conventional levels) to appear only very rarely. In this sense, the results we present below will lack robustness.

We now illustrate the outcome of the matching procedure. Figure 2 shows a histogram of the propensity scores for participants and non-participants in any stage of the BS program. The figure depicts the number of observations in twenty intervals of width 0.05 in the possible range from 0 to 1. Obviously, the distributions differ between participants and non-participants. While most of the non-participants exhibit propensity scores close to 0, the majority of participants exhibit propensity scores of 0.6 and above.

It seems that the individuals surveyed as potential controls for the evaluation exercise are not randomly selected with regard to the characteristics determining program participation. Overall, the non-participants tend to have characteristics that make them systematically less likely to be selected for participation in the BS program compared to individuals who received the treatment. To form a proper control group for the evaluation of program impacts, one needs to exclude those individuals among the non-participants who appear to be too different in terms of their propensities to receive the treatment.

Among the program participants, 11 participants are off support, i.e., have a higher propensity score than the individual with the highest estimated propensity score among the non-participants, and thus need to be excluded. Table 13 displays how often the same non-participants were used as matching partners. In total, we create 131 matched pairs by using information on 131 participants, but only on 61 non-participants.

Figures 3-5 illustrate the distributions of the propensity scores for the remaining types of treatment. In all cases, the histograms are markedly different comparing participants to non-participants. The propensity scores for non-participants are normally smaller than 0.5, and very often close to 0, whereas the propensity scores of participants are more evenly distributed, and frequently in the range above 0.5. To achieve common support we need to exclude five (three) observations when assessing participation in the complete BS program (in the temporary employment stage only). Tables 14-16 display

how often the same non-participants were used as matching partners. In total we use 61 matched pairs (incorporating 33 non-participants) for the analysis of participation in the complete BS program, 48 matched pairs (incorporating 25 non-participants) for the analysis of participation in the training stage only, and 25 matched pairs (incorporating 20 non-participants) for the analysis of participation in the temporary employment stage only.

If the matching approach is successful in mimicking a randomized experiment, any differences in observable characteristics between the treatment and control groups should disappear. Tables 17-20 summarize the characteristics of the matched program participants and non-participants. They indicate that the constructed treatment and control groups indeed have basically identical socio-demographic characteristics.¹² This shows that our matching approach has successfully imitated a randomized experiment, which will allow evaluating program impacts by comparing mean outcomes between the treatment and control groups.

¹² After matching, individuals with high chances to find a job are somewhat overrepresented among the participants in the complete BS program. Individuals from the ethnic group "Roma" are overrepresented among the participants in training only. These exceptions are altogether negligible.

5 PROGRAM IMPACTS

There are a number of outcomes a specific program can be evaluated at. We will assess the impacts of the BS project on five different outcomes: unemployment probability, employment probability, wages, subjective welfare and local communities. Additionally, the impacts on (un-)employment probabilities are assessed for different subgroups of participants. This procedure reflects the conjecture of heterogeneous impacts across the following dimensions: local labor markets (Belgrade vs. Niš/Zrenjanin) and previous unemployment duration (short-term vs. long-term unemployed).

When interpreting the evaluation results, it should be always kept in mind that there is only a short time between the end of the BS program and the survey dates in October and November 2005. The maximum length of the observation window after completion of program participation is one year. For many participants the observation period is even shorter. This means that basically this report cannot assess any long-term effects of the BS program.

5.1 UNEMPLOYMENT PROBABILITY

In our data, we are not able to trace the employment history of a given individual. Therefore it is not possible to observe the exact end date of the unemployment spell under consideration. Instead, we base the subsequent analysis on the labor market status at the time of the face-to-face interview. This means we assess program impacts on the *probability* of being unemployed at a given date (the survey date) rather than on the *duration* of unemployment.

In the following, we focus on the *average treatment effects on the treated* considering the probability of unemployment at the respective survey date. The average treatment effect on the treated (ATT) measures the average effect of the intervention on the group of individuals who participated in the program. For example, in the present context the ATT represents the difference between the actual unemployment rate of participants post program and the counterfactual unemployment rate of participants supposing they

would not have received the treatment. Importantly, the ATT captures the causal effect of the program on the analyzed outcome.

Table 21 reports the ATT for the four distinct program types. For a comparison, we also report the mean differences in outcomes based on unmatched samples of participants and non-participants. The findings suggest that any participation in the BS program (treatment 1) reduces the probability of being unemployed by about 15 percentage points, compared to a situation of not participating in the program.

However, there is evidence that this effect is not primarily based on participation in the complete BS program (treatment 2). Individuals who participate in both stages are only about 5 percentage points less likely to be unemployed at the survey date than matched non-participants.

On the other hand, participants in one stage of the BS program only (training *or* temporary employment) experience comparatively strong reductions in unemployment probabilities. While participants in training only are about 19 percentage points less likely to be unemployed at the survey date than matched non-participants, this figure amounts to 24 percentage points for participants in temporary employment only.

One possible explanation for the especially large positive impact of the latter program is that participants in temporary employment are *chosen by the contracted firms*. This supposedly induces a *positive selection* of previously unemployed persons into the temporary employment stage. Whether this kind of selection occurs among participants of the training stage only is less clear. On the one hand participation in this treatment involves quite high opportunity costs. Since training is conducted full-time, participants cannot engage in informal activities during the program and potentially forego three months of wages. On the other hand, participants may also be positively selected. Considering training as an investment into human capital would attract individuals expecting a relatively high return.

To sum up, participation in the complete BS program turns out to be able to reduce unemployment only by a comparatively small degree. The impacts on unemployment seem to be much more substantial for participants in *only one* stage (*either* training or temporary employment).

Importantly, the application of the matching procedure changes the assessment of the program impact not only quantitatively but also qualitatively. In three cases (treatments 1-3), even the signs of the difference in unemployment probabilities change when comparing the matched samples of treated and controls instead of the unmatched samples of participants and non-participants. In one case (treatment 4), the estimated reduction in unemployment probabilities through the program is substantially larger when looking at the matched samples. The reason for these differences is that the unmatched sample is contaminated by a special selection pattern which leads to the inclusion of many non-participants with especially low labor market prospects. Thus a plain comparison of mean outcomes without matching would be clearly misleading in our context.

It is also important to stress that none of the estimated impacts of the program on unemployment probabilities is significant in a statistical sense.¹³ We thus suggest understanding the notion of substantial reductions of unemployment rates associated with program participation with some caution. Altogether, due to the small size of the program (leading to small sample sizes) we only manage to present *weak* evidence that participation in the stages of the BS program reduces the probability of being unemployed.

5.2 EMPLOYMENT PROBABILITY

Our analysis with respect to employment outcomes is again based on the labor market status at the time of the face-to-face interviews, since it is not possible to trace the employment history of individuals. We thus evaluate program impacts on the *probability* of being employed at a given date (the survey date).

Considering total employment rates, the analysis mirrors the previous analysis of unemployment rates.¹⁴ However, our data allow distinguishing between four distinct

13 Standard errors of the estimated ATT were obtained by bootstrapping. Throughout the evaluation analysis, statistical significance of the program effects is tested at a 95 per cent confidence level. Statistical significance in this sense requires that the probability to estimate a non-zero program impact when the actual program effect is zero is less than five per cent.

14 The estimated overall employment effects of a treatment are not exactly the inverse of the estimated unemployment effects of the same treatment. Individuals have the option to withdraw from the labor market, i.e., they may be neither employed nor unemployed according to our definitions.

types of employment: employment in regular jobs and self-employment (analyzed jointly to achieve sufficient sample sizes), employment in seasonal jobs and employment in a job within an active labor market program implemented by the NES (henceforth referred to as ALMP jobs).

Table 21 shows the ATT with regard to the different employment outcomes, for the four distinct program types. Overall, it appears that the BS program has generally raised employment levels among participants. In detail, participation in any stage (treatment 1) increases the chances of working in all categories of employment. The impact on the total employment rate, which can be calculated as the sum of the ATT for the three different employment types, amounts to almost 15 percentage points. The strongest positive impact is on employment in a regular job – the probability of being employed in this type of employment is found to be by about 10 percentage points higher than without program participation. Nevertheless, the share of employment in a regular job (19.08 percent) remains smaller than the share of employment in a seasonal job (25.95 percent).

Considering the participants who completed both stages of the BS program, we observe only moderate overall employment effects: completion of the program makes the employment probability by about 3 percentage points larger. While the program raises the probability of being employed in a regular job by about 8 percentage points, it reduces the probability of being employed in a seasonal job by about 10 percentage points. Overall, the employment impact of this treatment is nevertheless positive, since program participation leads to an about 5 percentage point higher probability of being employed in an ALMP job. These findings may indicate (1) that individuals who go through the complete BS program are not especially successful on the labor market, and (2) that participation in the complete program may be the starting point of a career in ALMP measures.

In contrast, individuals who only participate in one of the two program stages turn out to be particularly successful. Participants in the training stage only are about 19 percentage points more likely to be either regularly employed, self-employed, or employed in a seasonal job than matched non-participants. They also exhibit a smaller

propensity to be dependent on a publicly provided ALMP job after the BS program. Participation in the temporary employment stage only generates the largest impact on regular employment, self-employment and seasonal employment. This treatment group is 28 percentage points more likely to be employed in either of these jobs than matched non-participants. Again, the treatment also leads to a reduction in the probability of ALMP employment afterwards.

In summary, it appears that positive employment impacts of the BS program primarily occur when participating in *only one* of the program's two stages (*either* training or temporary employment). This finding becomes even more apparent when considering jobs in the first labor market. While the probability of being in regular employment, self-employment or seasonal employment becomes much higher for participants in training only or temporary employment only, this probability becomes even smaller for individuals participating in both training and temporary employment.

Again, the application of the matching procedure substantially changes the measured program outcomes. Even the sign of the estimated treatment effect reverses in some cases. Finally, we once more have to stress that due to the nature of our data, the reported ATT are in general statistically insignificant. Therefore, only *weak* evidence in favor of the impression that participation in the BS program increases the probability of employment is presented here.

5.3 SUBGROUP ANALYSIS

Participation in the BS program may have heterogeneous impacts in the population. In this section, we therefore assess the specific treatment effects for subgroups of participants distinguished by certain individual characteristics. Specifically, we study differential program effects regarding the dimensions place of residence (Belgrade vs. Niš or Zrenjanin) and previous unemployment duration. In the latter analysis, we will distinguish between the program impact on the short-term unemployed, i.e., individuals previously unemployed for up to one year, and on the long-term unemployed, i.e., individuals with previous unemployment durations of more than one year. Regarding outcomes, we consider both unemployment and employment probabilities.

Place of Residence

Since one could support the conjecture that the labor market in Belgrade is substantially different from that one in Niš or Zrenjanin, we split our sample in individuals who live in Belgrade and those who do not. These two subgroups are analyzed as above, i.e. the matching procedure is applied separately for each subgroup and impact analyses are subsequently performed to derive ATT.

Specifically, our sample is split into 114 individuals living in Belgrade and 174 individuals residing in Niš or Zrenjanin. Table 22 shows the distribution of observations across training and/or temporary employment participation for individuals living in Belgrade. Table 23 displays the same distribution for persons with their place of residence in Niš or Zrenjanin. Note the comparatively small size of the non-participant group for individuals from Belgrade. The reduction of sample sizes means that the findings presented in this section ought to be interpreted with particular care. We cannot expect that any of our findings will be significant in a statistical sense.

After applying our matching procedure separately to the two subgroups, ATT are calculated as usual. Since small sample sizes lead to especially fragile patterns, we only analyze the potential impacts of participating in the BS program at all, or in the complete program (treatments 1 and 2).¹⁵ The first two columns of Table 24 display the estimated results for individuals from Belgrade. We observe that both treatments reduce the unemployment rates of participants. This is the same qualitative finding as in the full sample.

Furthermore, irrespective of the type of treatment, the decline in unemployment rates is of similar magnitude. In the tendency, program participation appears to reduce the probability of being employed in a seasonal job, whereas it raises the probability of being employed in a regular job or being self-employed. This observation is in contrast to the ATT estimated on the full sample.

The last two columns of Table 25 show the corresponding findings for individuals from Niš or Zrenjanin. It seems that participation in the BS program at all (treatment 1)

¹⁵ The number of individuals observed in the training stage or in the temporary employment stage is at most 24, which is too small for any serious evaluation exercise.

neither increases nor decreases the probability of being unemployed. The same holds for the probability of being employed. Still, for individuals who complete both stages of the program, the results show the common pattern of declining unemployment probability and increasing employment probability.

Overall, the observed program impacts appear to be somewhat smaller for individuals from Niš or Zrenjanin than for individuals from Belgrade. But one should recall that the BS program has started only in 2005 for most of the participants in Niš and Zrenjanin. Therefore, the impact analysis for this subgroup should be considered as *very preliminary* and of minor explanatory power since the potential period between the end of the program and the survey date is very short.

Previous Unemployment Duration

The BS program was intended to target long-term unemployed individuals. However, it turns out that also persons who were unemployed for relatively short durations received training and/or were temporarily employed. In this section, we analyze whether the previous unemployment duration influences the impacts of the BS program on employment and unemployment. For this purpose, we split our sample in two subgroups following the common understanding that short-term unemployed are persons being unemployed for less than one year. Hence, the first group contains individuals who were previously unemployed for at most twelve months. The second group consists of the long-term unemployed who were previously unemployed for more than one year.

According to these definitions, our sample is split into 69 observations on short-term unemployed individuals and 219 observations on long-term unemployed individuals. Table 25 shows the distribution of observations across training and/or temporary employment participation for the short-term unemployed. Table 26 displays the same distribution for the long-term unemployed. Due to insufficient sample sizes, we restrict the evaluation at this stage to participation in the BS program at all (treatment 1). The respective ATT for the short-term and the long-term unemployed are shown in Table 27.

The ATT estimated on the subgroup of long-term unemployed are consistent with our earlier findings that participation in the BS program has a positive labor market effect: it

reduces the probability of being unemployed by more than 20 percentage points, while the probability of being employed in a regular or seasonal job, or being self-employed increases by almost 9 percentage points. It furthermore raises the probability of being employed within a program implemented by the NES by almost 8 percentage points.

However, the BS program does not seem to generate these positive effects among participants who were short-term unemployed before entering the treatment. In contrast, the program lifts the probability of being unemployed by 12 percentage points. At the same time, it reduces the probability of being employed in a first labor market job by a substantial margin. A slightly positive employment impact in the domain of seasonal employment does not compensate this effect.

A possible explanation for the poor performance of the program when considering the short-term unemployed is stigmatization. Participation of a short-term unemployed individual in a program targeting long-term unemployed perhaps sends a bad signal to potential employers. Moreover, supposing that short-term unemployed have relatively good chances to find employment (or would not need an ALMP program to find a job), program participation may imply a lock-in effect: a reduced level of search activities during the program extends the average period out of the first labor market. Assuming further that reemployment probabilities rapidly decline with the duration of unemployment, program participants may be worse off than short-term unemployed who are not distracted by an ALMP from engaging in job search.

5.4 WAGES

In this section, we return to the full sample in order to study the impact of the BS program on individual revenue. The analysis is restricted by the information contained in the survey data. First, we only observe wages, not income. This implies that we do not observe the income from self-employment, so that this type of employment is excluded from the subsequent analysis. Second, while the samples of matched participants and non-participants are the same as before, wage data are missing for a relatively large number of observations.¹⁶ This means that the estimated program effects on wages are even less robust than the ATT on unemployment and employment probabilities discussed in sections 5.1 and 5.2, respectively.

Table 28 shows the effect of program participation on wages conditional on being employed at the date of the survey. Hence, we do not consider the additional wage gain associated with the fact that program participants have a higher chance to be employed relative to comparable non-participants. This strategy is justified on the grounds that our data does not contain the income of individuals who are unemployed at the survey date.

An interesting picture arises which appears to be consistent through the four distinct treatments. First, program participants who become employed in a seasonal job exhibit higher wages than comparable non-participants. The wage gain ranges from 5.2 percent to 19.6 percent depending on the treatment. Considering that construction work is typically seasonal employment, this may indicate that the BS program actually raises the productivity of workers. In this regard, it is probably revealing that the wage increase is particularly large for those program participants who go through the training stage. Individuals who receive training only and manage to obtain a seasonal job exhibit the largest wage increase. For individuals who complete both stages of the program, i.e., training and temporary employment, the estimated wage increase is larger than for those individuals who participate in the temporary employment stage only.

Second, the positive wage effect of the BS program only occurs for seasonal jobs, but not for regular jobs. Program participants who obtain a regular job earn at least 20 percent

¹⁶ For participants in temporary employment only, we are left with zero wage observations for treated individuals in regular or ALMP jobs.

less compared to similar non-participants. This could suggest that while program participation helps individuals to obtain regular jobs (compare the findings above) it does not markedly improve productivity in this type of employment. This finding is perhaps not too surprising considering that many of the regular jobs are situated outside the construction sector to which the program is targeted.

Finally, participation in BS program seems to strongly increase the wages of those individuals who obtain an ALMP job afterwards. In fact, the average income of successful program participants in these jobs is higher than the income obtained in any other employment category. Compared to similar non-participants who obtain ALMP jobs, the wage increase is in the range of 30 percent (treatment 3) to 118 percent (treatment 2).

While these particular numbers should be considered with extreme caution due to the small sample sizes on which they are based, the general pattern could indicate that for reasons not obvious to the analysts, participation in the BS program is a stepping stone to extend or renew eligibility for certain benefits paid by the unemployment system. Such an explanation would appear consistent with the positive program impacts on the probabilities of being employed in an ALPM job afterwards, as they were observed in section 5.2. Note, however, that the causality may also be reverse: the high wage to be earned in ALMP jobs could attract individuals with a choice to prefer them over other types of employment.

In total, the evidence presented in Table 28 suggests that the BS program impacts slightly positively on wages. Taking all individuals who obtain a job through the program together, the estimated wage increase is about 8 percent. A more disaggregated analysis of individual treatments, however, suggests that the average wage increase could be much smaller.

5.5 SUBJECTIVE WELL-BEING

In addition to labor market outcomes, one may assess the quality of a labor market program in terms of its impact on individual (or subjective) welfare. Even if a program does not immediately raise employment probabilities of individuals, a social planner may find it beneficial if it manages to improve their personal situation. For example, a program could reduce the psychic costs of being unemployed by strengthening self-confidence or social contacts of the program participants.

Our data includes a set of questions relating to items that give a reasonable picture of how the personal situation of the interviewed has changed over time. Specifically, individuals were asked to compare their situation at the time of the interview with that in the beginning of 2004 (Belgrade) or in the beginning of 2005 (Zrenjanin and Niš), considering various aspects of life: self-confidence, the desire to find a job, social contacts, qualification and skills, health, the possibility to find a regular job, and the family income situation. In each domain, respondents had to judge whether their situation has strongly or somewhat improved, has stayed more or less the same, or has strongly or somewhat deteriorated.

This information is important, since it may allow measuring the impact of the BS program on subjective welfare. Furthermore, the responses concerning personal changes with regard to “qualification and skills” and “job chances” may deliver valuable subjective information whether or not the BS program raised employability.

Figures 6–9 compare the distributions of the individual judgments on all aspects of life covered by the data, for participants in the various treatments distinguished in our analysis. The figures are based on the respective samples of matched participants and non-participants. For individuals who participate in the BS program at all, the treatment seems to generate positive impacts on all items (Figure 6). A similar improvement is apparent considering individuals who completed both stages of the BS program (Figure 7) or the training stage only although health status seems to be virtually unaffected in this case (Figure 8). Overall, the improvement in the personal situation of individuals who participated in the temporary employment stage only is weaker. Especially, it

appears that participation in this stage only does neither improve the desire to find a job nor health (Figure 9).

For a more formal analysis, we apply the matching approach to the subjective data. As the outcome variable, we define a dummy variable that takes the value of one if individuals judge that their personal situation has strongly or somewhat improved, and takes the value of zero otherwise. In this way, the ATT measures the change in the percentage share of individuals judging their personal as improved because of program participation.

Table 29 summarizes our findings. In general, program participation seems to have substantially improved the personal situation with regard to all aspects of life considered. Among the individuals who participated in any part of the BS program (treatment 1), the share with improved job desire, social contacts, qualification and skills is substantially higher than among similar individuals who did not participate. These improvements are even significant in a statistical sense. In comparison to the other indicators of changes in the personal situation, the program appears to have little impact on health status. Considering the empirical observation that transitions from unemployment to employment tend to be associated with an improvement in health and given that the program tends to have an employment effect, this finding may appear surprising. A possible explanation is that the BS program targets employment in the construction sector, which is known to provide relatively unhealthy working conditions.

The positive program effects appear to be even stronger for individuals who complete both stages of the BS program. Any of the ATT is positive and statistically significant. The strongest absolute effects occur in the domains of qualification and skills, job desire and social contacts. Participation in the training stage only also positively influences all measures considered (except health), although statistical significance is generally not achieved due to small sample sizes. Again, the treatment effects appear to be especially large in the domains of qualification and skills, and job desire.

In line with the impression derived from Figure 9, the ATT shown in Table 29 indicate that the program impact on individuals who participate in the temporary employment stage only, although generally positive, is relatively weak. In particular, there is no

substantial improvement in the domains of qualification and skills as well as job desire, which is a remarkable contrast to the other treatments considered.

Taken together, the positive program effects considering individuals' subjective assessment of conditions of life appear to be larger than the program impacts when considering their objective labor market status. This suggests that the BS program may improve subjective welfare through other channels than the labor market. In this regard, it is especially relevant that program participation leads to improved self-confidence and social contacts. It also looks as if the program boosts job desire, provided that individuals go through the training stage. The absence of this effect for individuals who go through the temporary employment stage only is perhaps surprising. An explanation could be that the unemployed in this particular treatment obtained their job through a regular hiring process. The fact that they succeeded to obtain the temporary employment contract in a competitive labor market indicates that they are positively selected in terms of their initial job desire. The matching procedure could not control such a mechanism.

Finally, it is remarkable that individuals' own impressions about the changes in their personal status that occurred in the course of the observation window are very much in line with the actual program impact on labor market outcomes. Irrespective of the treatment considered, the participants feel that they have improved employment chances compared to a time prior to the treatment. The evidence for a positive (negative) program impact on employment (unemployment) rates, discussed in sections 5.1 and 5.2, shows that this is actually the case.

Likewise, participants in the training stage of the BS program, no matter whether they subsequently participate in the temporary employment stage or not, more frequently report that their qualification and skills improved during the observation period. This evidence is basically consistent with the wage effects of the program discussed in section 5.4, which suggest that the vocational training stage actually endows individuals with relevant human capital provided that they get employed in a seasonal (presumably construction sector) job. Not surprisingly, the impression of improved skills does not appear among individuals who only pass the temporary employment stage of the BS

program. This stage does not include any particular vocational training apart from learning on-the-job, which is probably not too relevant considering the relatively simple tasks performed by the temporary workers.

5.6 LOCAL COMMUNITIES

The refurbishment of public buildings and spaces within the BS program also impacted on the involved local communities. In our data, we observe some variables that relate to the impact of the BS program on the local communities from the perspective of the involved persons and the contacted firms in the refurbishment projects. This allows deriving some *qualitative* results on this topic.

Figure 10 displays the impressions of twelve contracted firms that conducted the refurbishment projects. These firms were asked about the projects' contributions on four indicators that relate to the program impact on the local communities. In general, the contracted firms perceive the BS program as positively affecting the local communities. More than 80 percent of the firms state that the program has contributed to a large or to some extent in strengthening partnerships at the local level. 75 percent see contributions of the program to the environmental improvement in the local communities. Furthermore, two-thirds of the contracted firms view the program as a contribution to carrying out publicly beneficial areas of activity and even to the social and political stability *of the country as a whole*. It seems natural to assume that the contracted firms would have supported also the notion of program contributions to the social and political stability *of the local communities*.

Figure 11 reveals that the participants in the temporary employment stage view their work in general as useful for the local communities.¹⁷ Almost 90 percent of the 94 previously unemployed workers who took part in the temporary employment stage consider it 'useful' or 'very useful' for the local community, while all surveyed regular workers in the contracted firms (the benchmark group) support this notion. The share of previously unemployed workers who consider the work as 'not useful at all' amounts to only about 3 percent.

¹⁷ Participants of the training stage only were not asked this question.

In sum, our *qualitative* assessment of the impacts on the local communities points to a positive perception of the BS program in this regard. However, it is not possible to *quantitatively* evaluate program impacts on the local communities with our data.

6 COST-BENEFIT ANALYSIS

Conducting cost-benefit analysis is considered to be important for evidence-based policy-making, which is based on facts rather than on theory or ideology. Assuming that the benefits and costs of a given ALMP are correctly measured, the program is justified on efficiency grounds if the former exceed the latter, and should otherwise be abandoned unless other justifications (e.g., equity reasons) can be found for it.¹⁸

Therefore, we try to assess the potential costs and benefits of the BS program in this section. We conduct our analysis by comparing revenue and expenditure associated with participants and matched non-participants from the perspective of the public budget. For this purpose, direct costs of the vocational training and/or net costs of temporary employment, unemployment benefits, social security contributions, and income taxes are considered.

For an ideal efficiency analysis of the BS program, one would trace individual labor market histories over time, in order to associate the costs and benefits of each possible program episode and each possible labor market outcome with the specific labor market episodes. Yet in our data labor market status is known at only two points in time: (i) in January 2004 by construction both participants and non-participants must have been unemployed, and (ii) in October or November 2005 the current labor market status at the survey date can be observed. Between these dates we have to rely on assumptions to reconstruct individual labor market histories.

Table 30 displays the stylized sequence of events we assume for the participants in the different types of treatment and for the individuals in the respective control groups. As mentioned above, each individual was unemployed in January 2004. We assume that all individuals start participation in vocational training in April 2004 and that the training lasts for three months. Subsequently, temporary employment is supposed to begin in July 2004 with an average duration of three months.¹⁹ Thus, individuals who participate

¹⁸ See Kluve and Schmidt (2002).

¹⁹ The average duration of temporary employment amounts to 3.41 months in our data. However, it is reasonable to assume an average duration of three months since most of the employers report durations between 2 and 4 months with a peak at 3 months. Only one firm specifies this duration to be 11 months.

in the training stage of the BS program only are assumed to finish treatment in June 2004, whereas participants in both training and temporary employment and participants in temporary employment only are assumed to finish the treatment in September 2004.

It is furthermore assumed that all participants are unemployed until the treatment starts. For those individuals changing into employment after the treatment, we assume that they reach their final labor market status at exactly half of the period between the end of the treatment and the survey date, fixed at October 2005. Since we will not discount any of the payment streams (a justified simplification given the relatively short time frame of the cost-benefit analysis), this procedure is equivalent to assuming a constant transition rate from unemployment to employment. Put differently, for those individuals who reach an employment state, the hazard of remaining in the unemployment state linearly declines to zero from the end of the treatment to the survey date.

In detail, we assume that participants in the training stage only are on average unemployed until mid January 2005, while participants in the temporary employment stage (with or without previous training) on average change employment status at the beginning of April 2005. A similar assumption is made for the respective control groups. Those controls exiting the unemployment state after January 2004 are supposed to access on average their job when one half of the observation window has passed, i.e., in December 2004.

Starting from this stylized sequence of events, we need to associate fiscal costs and benefits with particular program or labor market episodes. Table 31 summarizes the specific monthly amounts of spending and revenues we assume to be associated with each possible state.

First, we calculate the average costs of the vocational training measure. According to our information, in total 150,000 USD was spent for the training stage of the BS program. Since 238 persons completed the three-month vocational training, average monthly costs per participant are 210.08 USD or 177.41 EUR.

During the temporary employment stage, the program generates costs as well as benefits. On the one hand, temporary workers receive a competitive wage from the contracted firms and therefore pay income taxes and social security contributions. Those

payments constitute a fiscal gain. We estimate that this gain amounts to 22.41 EUR per program participant and month, which is 50 percent of the average gross monthly wage earned in temporary employment.²⁰ On the other hand, although the refurbishment projects were awarded to the firms in public tenders and competition is supposed to lead to market-based prices, it seems very likely that the requirement to employ 40–60 percent previously unemployed workers induces somewhat higher prices than those of comparable projects without those specific requirements. The contracted firms will at least want to recover their additional costs.

To approximate these costs, we assume that the prices of the refurbishments projects increase by the wage costs of the additional workers who had to be employed for instructing and monitoring the previously unemployed workers. According to the employer survey, the contracted firms employed on average 0.17 *additional* workers per previously unemployed worker. We assume that these workers are paid the average wage of the surveyed regular workers in the contracted firms. This leads to the estimate that each program participant in the temporary employment state causes additional wage costs of 49.62 EUR per month. By balancing the supposed public revenue and expenditure streams, we find that fiscal costs during the temporary employment are 27.41 EUR per participant per month.

For episodes outside the program we need to estimate the costs and benefits associated with the possible labor market states: unemployment and regular, seasonal or ALMP employment. For episodes of unemployment, we assume that the average fiscal costs per capita and month amount to 7.25 EUR. This figure equals the average benefit received as reported by the unemployed in our data, and is consistent with complementary UNDP information.

During episodes of regular or seasonal employment fiscal benefits arise from paid income taxes and social security contributions.²¹ We assume that these payments add up to 50 percent of the gross monthly wages earned in these jobs. We further assume an

20 Employees' and employers' social security contribution rates add up to 36 percent of gross salary. Income taxes are 14 percent of gross salary. Furthermore, we assume that all temporary employed indeed pay taxes and social security contributions.

21 We lack precise information on ALMP jobs. For simplicity, we assume that these jobs generate neither costs nor benefits or, equivalently, that the respective amounts cancel each other.

incidence of informal work of 14.29 percent: in our data only 85.71 percent of the workers in regular and seasonal jobs actually report to pay income taxes and social security contributions. Moreover, we adjust for frequent short non-employment periods in seasonal jobs by assuming that these jobs pay a wage actually during only 75 percent of a year.

Given that the BS program took place in Belgrade only in 2004, our supposed stylized sequence of events seems to be most adequate for analyzing this particular labor market. Therefore, we will present a separate costs-benefit analysis for Belgrade based on the specific ATT discussed in section 5.3. Note that the estimates for the fiscal benefits and costs associated with the various labor market states slightly change when considering Belgrade only, since average monthly wages as well as monthly unemployment benefits are somewhat higher (see Table 31).

Table 32 presents the results of the cost-benefit analysis. The cost-benefit measure is the difference of the average net fiscal costs calculated for matched participants and non-participants. The numbers represent the total costs accumulated during the observation window from January 2004 to October 2005.

In general, the implementation of the BS program does not seem to be fiscally efficient since the balance of costs and benefits is always negative. However, the net fiscal costs associated with distinct treatments differ considerably in magnitude. Participation in training (with or without subsequent temporary employment) appears to be expensive (456.63-687.16 EUR per participant). It therefore may be considered as inefficient from a purely fiscal perspective. In contrast, participation in temporary employment only involves an almost negligible financial loss (11.42 EUR per participant). If one focuses solely on program participants from Belgrade, participation in the complete BS program turns out to be slightly less inefficient, while participation in the training stage or in temporary employment stage only become fiscally more inefficient.

How robust are these results? Because the period under consideration (January 2004 – October 2005) is comparatively short and considering that the importance of long-term

effects to adequately assess program efficiency is well-established in the literature,²² we decide to extend the observation window to incorporate an additional 12 months in the future. We proceed by assuming that during this period, individuals will continuously stay in the labor market status reported at the survey date. The results, also reported in Table 32, indicate that under this condition participation in the temporary employment stage only appears to be worthwhile from a fiscal point of view. Although the cost-benefit measures for the other types of treatment generally improve, participation in the complete BS program or in the training stage only still appears as relatively costly.

In sum, the results of the cost-benefit analysis may lead to the conclusion that the BS program is not a worthwhile investment from a purely fiscal perspective. In particular, participation in training (with or without subsequent temporary employment) seems to be comparatively inefficient. The induced employment effects are not large enough to balance the initial public investment into the program.

However, one can raise a number of objections against this interpretation. First, so far we cannot adequately assess any long-term effects of the program which could change our fiscal efficiency measures substantially. The ad hoc sensitivity test presented above shows that the efficiency of the BS program would improve if the positive employment effects lasted in the future.

Second, and more importantly, our analysis focuses only on the direct revenue and expenditure streams impacted by the program. In particular, the cost-benefit assessment ignores any non-monetary costs and benefits associated with a reduction in unemployment, or an increase in employment.

On the cost side, our analysis only accounts for unemployment benefits, but the social and individual welfare costs caused by unemployment are presumably much higher. Hence, to the extent that the BS program reduces unemployment, a purely fiscal perspective understates its potential benefits.

It is difficult to determine the social and individual welfare costs empirically. Instead, we may rely on a thought experiment: In order to make the Beautiful Serbia program

22 Compare Jespersen *et al.* (2004) or Lechner *et al.* (2005a and 2005b).

profitable, how large would the unobserved welfare costs of being unemployed (not counting the direct costs of unemployment benefits) need to be? To answer this question, we redo the cost-benefit analysis including some fictive amount of social welfare costs per unemployed and month and perform a grid search. When considering the complete BS program (the training stage only) we obtain that the non-monetary losses from unemployment need to be equivalent to 159.40 EUR (107.50 EUR) per unemployed and month, to ensure that these interventions reach the break even point. A much smaller amount is needed when considering the costs and benefits of the temporary employment stage only. Here, the program is profitable from a social point of view as soon as the monthly non-monetary costs per unemployed exceed 7.50 EUR.

Likewise, on the revenue side, our analysis only accounts for tax revenue and social security contributions, but the social and individual welfare gains associated with bringing people into employment are presumably much larger. Hence, to the extent that the BS program creates employment, a purely fiscal perspective again understates the potential benefits of the intervention.

7 SUMMARY AND POLICY RECOMMENDATIONS

This report evaluates the net impact of the Beautiful Serbia program. On the basis of a comprehensive data set covering almost the universe of program participants as well as a comparison group of unemployed who did not participate, we employ statistical tools for program evaluation designed to calculate the average treatment effect on the treated. The treatment effect captures the causal effect of the program. It shows how the analyzed outcome changes for program participants, compared to a situation where they would not have received the treatment.

In this study, treatment effects are assessed across a range of outcomes: unemployment probabilities, employment probabilities, structure of employment, wages, and subjective improvements in personal life, notably concerning self-confidence, social contacts, qualification and skills, and health. We also provide a qualitative assessment of the impacts on the local communities. On the basis of the estimated program effects, a cost-benefit analysis is provided which focuses on the additional public revenue and spending associated with the Beautiful Serbia program.

7.1 CAVEATS

It is important to note that the empirical findings in this study suffer from some fundamental drawbacks. A first drawback is that the Beautiful Serbia program, with around 300 participants, is a small scale intervention, which yields small sample sizes. However, in a non-experimental setting, even basic program evaluation methods require rather large data sets to generate satisfactorily robust empirical results. Since our data do not meet this condition, the treatment effects we estimate are fragile and generally not significant at conventional levels of statistical analysis. The specific design of the sample aggravates the problem. Statistical analysis reveals that the non-participants drawn as a comparison group on average do not resemble the program participants very well. Since program effects can only be evaluated by “comparing the comparable”, we must discard many potential controls, which damages the robustness of our empirical estimates even further.

A second drawback of the analysis is the comparatively short time period observed after treatment. Participants in the Beautiful Serbia program are typically not observed more than one year after completing the program. For most participants, the observed time window during which positive program effects could materialize is even shorter, especially for participants in Niš and Zrenjanin. But the evidence in the program evaluation literature points to the fact that program effects may require substantial time to fully unfold. In this study, we can only assess short-term program impacts. If the potential employment effects of the Beautiful Serbia program were mainly realized over the longer term, we would underestimate the net benefits of the intervention. Only part of the employment effects would be captured and the negative impact of being locked-in in the program (with probably reduced search activities) would be given too much weight.

Given these fundamental concerns, the estimation results presented in this report should be treated with extreme caution. In particular, one should be aware that they represent at best weak empirical evidence for the potential impacts of the Beautiful Serbia program. It is especially relevant to keep this in mind when drawing policy recommendations on the basis of this empirical analysis.

7.2 MAIN FINDINGS

Coming to a summary of our main findings, we observe that:

- **Participation in the Beautiful Serbia program provides employment for a considerable group of unemployed who would otherwise have remained out of work.** On the survey date, the unemployment rate in the treatment group was by about 15 percentage points lower (42.7 percent vs. 58.0 percent) compared to the control group of non-participants. At the same time, 52.7 percent of the participants were still employed in October 2005, whereas comparable individuals who did not receive the treatment had an employment rate of only 38.2 percent. However, it appears that the strong decline in unemployment is not primarily associated with participation in both stages of the Beautiful Serbia program. For individuals who complete the full program, the employment rate is only 5 percentage points higher than for comparable individuals who did not participate at all. The strong effects

seem to occur when participating in either the training or the temporary employment stage of the program only.

- **A considerable share of the unemployed who find employment through participation in the Beautiful Serbia program comes into a regular job.** On the survey date, the share of non-participants who are employed in a regular job (or self-employed) is about 10 percent smaller (9.2 percent vs. 19.1 percent) compared to individuals who participated in the entire program or parts of the program. Nevertheless the share of program participants who are employed in a regular job remains smaller than the share of participants employed in a seasonal job (25.6 percent). For those individuals who enter employment after participating only in the training stage or the temporary employment stage of the program, seasonal employment is a more frequent destination of exits from unemployment.
- **While the program improves employment prospects, it has on average only moderately positive wage effects. The impact on wages heavily depends on the type of employment obtained.** Program participants who become employed in a seasonal job after completing the full or parts of the Beautiful Serbia program earn a 13 percent higher wage than similar individuals who did not participate and also end up in a seasonal job. In contrast, program participants who find a regular job earn 20 percent less compared to the control group. This suggests that while program participation helps individuals obtaining regular jobs, it does not raise productivity in this specific type of employment.
- **The vocational training stage of the Beautiful Serbia program is useful because it improves qualification and skills of the participants.** While the actual skills acquired cannot be observed, indirect evidence suggests that the vocational training stage of the program makes participants more productive and therefore easier to integrate in the labor market. First, according to the self-assessment of the treatment and control groups, program participants to a much larger extent (54.7 percent vs. 17.6 percent) believe that their current qualifications and skills have improved compared to a reference point prior to the vocational training. This positive assessment does not occur for individuals who only participate in the temporary

employment stage of the program and hence do not receive the vocational training. Second, the positive program impact on wages in seasonal jobs may reflect a productivity gain, considering that the vocational training mainly provides skills related to construction work and that construction work is typically seasonal employment.

- **The Beautiful Serbia program has contributed to higher employability of the unemployed persons, at least from the subjective perspective of participants.** According to the self-assessment of the treatment and control groups, completing both stages of the Beautiful Serbia program significantly improves not only the individual desire to take up a job, but also the chances to find a job. In the group of program participants, the share of individuals with improved job desire is about 41 percent larger. The share of individuals who believe that their job chances have improved relative to the pre-program period is about 26 percent larger.
- **The program has yielded additional benefits by improving the individual well-being of participants.** The positive program impacts are especially large considering a subjective assessment of the circumstances of life at the survey date. The share of individuals in the treatment group reporting that their their personal situation has improved with regard to self-confidence, social contacts and family income is considerably larger than in the control group of non-participants. Furthermore, the Beautiful Serbia program does not appear to have a negative impact on the health status of participants, in spite of the physically exhausting jobs dominating in the construction sector.
- **In addition to the impacts for the participants, the Beautiful Serbia program has generated benefits for the local communities where the projects were carried out.** The combined evidence from the surveys among employers and previously unemployed workers who took part in the temporary employment stage of the program firmly indicates that the implemented activities are useful for the communities and improve the local living environment.
- **Despite the positive employment effects, the program appears inefficient when judged on the basis of the associated fiscal benefits and costs. A cost-benefit**

analysis reveals that the temporary employment stage of the Beautiful Serbia program is much more efficient than the vocational training stage. Comparing the expenses made for the program (direct costs of vocational training, monitoring costs during temporary employment) with the benefits due to the positive employment effects (taxes and contributions paid, lower spending on unemployment), the fiscal balance is worse for program participants than for non-participants. Net fiscal costs for program participants in the complete program total around 690 EUR in the course of an observation window from January 2004 to October 2005. For individuals who participate in the training stage only, net costs still amount to around 450 EUR. In contrast, the net fiscal costs associated with participants in the temporary employment stage of the program only (11 EUR) are almost negligible.

- **Program implementation may be justified from an efficiency perspective only if the non-monetary benefits or reducing unemployment are sufficiently large.** A purely fiscal perspective probably overestimates the net costs of the Beautiful Serbia program. First, we only measure the short-term effects of the program. If the positive employment effects last or the program has yet to unfold its full employment effect, the fiscal balance improves, as additional public revenue is generated. Second, the pure net fiscal costs ignore potential non-monetary benefits from the program. If the loss in social welfare associated with unemployment, the gain in social welfare associated with employment, or the positive externalities for communities associated with the refurbishment program are sufficiently large, the program may actually be efficient from a social planner's perspective. Still, the non-monetary benefits unaccounted for in the cost-benefit analysis need to be rather large to make the vocational training stage profitable, since the induced employment effects are not sufficiently large.

7.3 POLICY RECOMMENDATIONS

Having in mind the still tentative results of the short-term evaluation of the Beautiful Serbia program, the following recommendations may be proposed:

- **The two-stage design of the Beautiful Serbia program should be reconsidered. Splitting the program into two independent interventions - a vocational training program and a temporary employment program - could achieve a more transparent structure.** It appears that individuals who participate in both the vocational training stage and the temporary employment stage of the program do not have better employment chances than those who participate in only one of the stages. If anything, they exit unemployment at a lower rate. One possible explanation is the existence of a lock-in effect, which means that job search motivation declines as the length of program participation increases. Alternatively, it is conceivable that only individuals with particular obstacles to find a job pass through the complete program. Individuals who are comparably employable may drop out after the vocational training, or only enter through the competition for jobs in the temporary employment stage. In view of these conjectures, it may be recommended to split the program into a training program and an independent temporary employment program. This structure would help establishing clearer target groups for each intervention. The training program should aim at the unemployed with a special skill problem, whereas the temporary employment program with competitive access should aim at the unemployed who for some reason (other than qualification) have difficulties to find a job in the open labor market.
- **The training program should focus on unemployed individuals for whom a lack of specific vocational skills is a major obstacle to find employment.** Although the evaluation results suggest that the vocational training stage of the Beautiful Serbia has been effective in raising qualification and skills, the currently implemented program is relatively expensive. The net fiscal costs of the program could be reduced through better targeting to people for whom the induced improvement in employment probabilities is especially large.

- **The training program should be carefully monitored to ensure that it places enough emphasis on teaching the right skills.** Although the vocational training in the Beautiful Serbia program enhances qualification and reduces the probability to remain unemployed, better curricula could improve employment rates even further and thereby raise program efficiency in terms of fiscal costs and benefits involved. In particular, evidence gathered from employers at the temporary work stage suggests that the practical component during the vocational training stage is inadequate. This suggests that a combination of parallel classroom and workplace training (instead of two consecutive stages) may generate better results. Furthermore, more emphasis should be placed on skills enabling the participants to apply for and to find a job independently. A non-negligible fraction of participants in vocational training do not proceed to the temporary employment stage because they do not receive a job offer. This suggests that active job placement activities and job search assistance should complement the training effort. International experience and the evaluation literature support that the recommended measures are often effective.
- **The temporary employment stage in the Beautiful Serbia program seems efficient. An especially attractive feature is that it does not interfere with the labor market as participants receive competitive (rather than subsidized) wages. Nevertheless, when relying on this type of intervention, great care should be taken to avoid possible displacement and revolving door effects.** Displacement effects occur, if employers participating in the refurbishment program hire unemployed at the expense of other persons. Therefore, precedence must be given to projects that provide entirely new activities or expand existing activities. In practice, however, it will be rather difficult to identify such activities. Revolving door effects arise if employers competing for orders in the refurbishment program seek to meet the quotas for previously unemployed workers by laying off and re-employing the same employees. To avoid this strategic behavior, hirings in the contracted firms should be closely monitored.

- **Clear admission criteria are necessary to ensure that the program reaches the target group of long-term unemployed and otherwise disadvantaged people.** Our data show some indication that those individuals who actually entered into the Beautiful Serbia program had rather favorable characteristics. Shorter unemployment duration and closeness to the labor market (previous participation in active labor market policies, high job desire) have a positive influence to be treated. According to the evaluation results, admission of short-term unemployed into the program was perhaps counterproductive – employment success declined for this group. More generally, our findings hint at some selection process: caseworkers may knowingly or unknowingly interfere with program assignment. While this behavior could boost the employment impact of the program, it may exclude the truly disadvantaged unemployed for whom the social benefits of program participation are especially high. To avoid discretionary selection, program implementation should include transparent and obligatory admission rules.
- **When planning the design of a labor market program, it should be carefully considered whether it privileges or excludes certain groups in the population.** An obvious problem of the Beautiful Serbia program is that it is not neutral with regard to gender. Due to the focus on the construction sector, it could hardly reach the female unemployed. But also among the male unemployed, the design of the program probably privileged a particular group. Individuals not managing to engage in full-time training (e.g., because they could not afford the income loss when withdrawing from informal activities) were systematically excluded. This hurdle may explain why it was apparently not easy to recruit participants.
- **A detailed concept for program evaluation should be an integral part of program implementation. The performance of new labor market programs should be tested on a small scale using experimental designs.** The difficulties to obtain robust empirical results on the potential impacts of the Beautiful Serbia program show the importance of developing a good evaluation design at a very early stage, even before the program starts. Although the data collected during the current project are of high quality and allowed a speedy research process, fundamental structural problems prevent an analysis yielding more specific policy conclusions and recommendations.

The Beautiful Serbia program followed the right, prudent principle of testing innovative programs at a low scale, which avoids waste of resources in the case of failure. However, the effectiveness of small scale interventions is generally difficult to evaluate unless they are run under very controlled conditions. For future programs to be implemented, one should consider establishing such conditions by performing *randomized experiments*. In randomized experiments, individuals eligible for participation are randomly assigned to a treatment and control group. By construction, these groups differ from each other in none of the characteristics relevant for the program outcome. This allows very easy assessment of the program impacts by comparing mean outcomes in the two groups.

- **Collecting better information on the unemployed could help better controlling of active labor market policies in general.** Poor data provided by the employment agencies complicates the evaluation of the Beautiful Serbia program. It appears that the public database currently includes very little information about the unemployed individuals and even less information about employment outcomes. More detailed data on unemployment and employment histories, participation in labor market measures, and individual factors affecting employability would reduce the costs of evaluation: it allows constructing adequate control groups to benchmark program impacts and reduces the need for collecting surveys in the field. Adequate information is even more important, however, before carrying out the evaluation: it allows better identification of suitable program participants, which may improve program outcomes.
- **The scope of active labor market policies targeting employment in the construction sector should be closely linked to the pace of structural change.** At present, the economy of Serbia and Montenegro is still at a rather early stage of the transformation process. At this stage it is natural that the the construction industry plays a relatively important rule. However, as soon as the economy reaches a more stable state, it is probable that the weight of the construction sector in the economy declines. It is advised not to follow the example of other countries (notably East Germany) where government intervention fostered the construction sector was,

worsening high unemployment among construction workers when the building industry eventually recessed.

- **If the Beautiful Serbia program is continued, the scope of the program should be increased only slowly.** While the current findings overall suggest a positive impact of the program, it is impossible to predict *a priori* how the effects would change if an identical program were to be implemented on a larger scale, for example throughout the entire Serbia and Montenegro. A larger program may generate displacement effects and also have macroeconomic repercussions which could fundamentally change program outcomes.

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ABBREVIATIONS

ALMP	active labor market program
ATT	average treatment effect(s) on the treated
BS	Beautiful Serbia
EUR.....	Euro
MoLESP.....	Ministry of Labor, Employment and Social Policy
NES	National Employment Service
SCG	Serbia and Montenegro
UNDP	United Nations Development Program
USD.....	US-Dollar

TABLES AND FIGURES

Table 1: Buildings and locations refurbished in the BS program.

Municipality	Building/Location	# previously unemployed workers	# salaries paid	Total costs of executed works
Belgrade	Serbian Crown – City Library	15	38	58,586.15 \$
Belgrade	Queen of Greece	10	30	30,610.42 \$
Belgrade	25 Knez Mihailova Street	-	-	-
Belgrade	47 Skadarska Street (73a Cara Dušana)	12	48	55,030.43 \$
Belgrade	45 Skadarska Street	12	36	37,700.17 \$
Belgrade	Jovan Smederevac House	5	20	24,256.65 \$
Belgrade	8 – 10, Carice Milice Street	15	45	32,983.17 \$
Belgrade	12, Carice Milice Street	11	33	37,625.42 \$
Belgrade	Home of Magdalena Osvald	7	21	32,877.87 \$
Belgrade	Home of Tradesman Nikola Ruso	5	15	21,786.44 \$
Belgrade	1 Gospodska Street	4	15	16,388.97 \$
Belgrade	House with the Latin Hologram	4	11	15,005.29 \$
Belgrade	Anchor House	10	18	45,540.79 \$
Belgrade	“Ministry of Education Building” – Vuk’s Legacy	4	8	17,410.86 \$
Belgrade	Restaurant “Kolarac”	10	21	49,694.01 \$
Belgrade	1a Kosančićev Venac Street	-	-	-
Belgrade	14 Zeleni Venac Street	5	17	26,580.18 \$
Belgrade	Home of Ms. Sofija Medović	18	47	49,016.36 \$
Belgrade	Home of Mr. Borivoje Đurić	18	47	32,677.57 \$
Belgrade	8 Zeleni Venac Street	6	18	16,895.68 \$
Belgrade	Home of Ivan Ognjenović	10	20	25,599.00 \$
Belgrade	Home of Smiljka Gavrilović and Draga Simić-Gavrilović	-	-	17,066.85 \$
Niš	Paintshop “Nijansa”	8	28	32,642.89 \$
Niš	Bakery “Jančić”	8	27	32,610.97 \$
Niš	Palace Jovanović	10	30	39,910.46 \$
Niš	Youth Center	15	35	75,614.05 \$
Niš	Primary School “Vožd Karadjordje”	31	163	182,008.19 \$
Niš	Association of Engineers and Technicians Building	8	16	40,109.13 \$
Niš	Bookstore “Stevan Sremac”	8	24	40,977.67 \$
Niš	“Gallery Srbija” Building	5	15	16,438.33 \$
Niš	15 and 19, Trg Kralja Milana (2 buildings)	15	30	28,412.67 \$
Niš	23, Trg Kralja Milana	8	24	40,977.67 \$
Zrenjanin	Čokliget – Kej 2. Октобра	20	40	88,367.45 \$
Zrenjanin	National Town Library “Žarko Zrenjanin”	11	33	97,436.00 \$

Source: UNDP (2005), www.beautifulserbia.org.

Table 2: The construction sector in Serbia and Montenegro (1997 – 2003).

	1997	1998	1999	2000	2001	2002	2003
Number of employees in the construction sector	127,819	132,315	119,960	110,702	103,541	97,376	94,750
Share of employees in the construction sector	5.84%	6.08%	6.02%	5.78%	5.50%	5.39%	5.45%
GDP (in million din.) in the construction sector	6,718.0	8,762.2	10,065.9	21,684.9	33,041.4	43,969.8	n/a
Share of GDP in the construction sector	7.43%	6.89%	6.16%	6.22%	5.45%	5.77%	n/a

Source: Serbia and Montenegro Statistical Office (2004).

Table 3: Planned and accomplished number of interviews.

Group	Interviews planned	Persons not found / Non-respondents	Interviews accomplished
Participants in the training stage only			58
	238	99	
Participants in both training and temporary employment stages			81
Participants in temporary employment stage only	71	42	29
Non-participants (unemployed in January 2004)	307	112	195
Regular workers in the contracted firms	40	13	27
Contracted firms	15	2	12*

Source: GfK Belgrade.

Note: * Actually 13 contracted firms were interviewed, but two of them answered at the same time because they had worked together in the BS program.

Table 4: Number of observations used in this report.

Group	# obs. available	# obs. dropped	# obs. used
Participants in the training stage only	58	10	48
Participants in both training and temporary employment stages	81	15	66
Participants in temporary employment stage only	29	1	28
Non-participants (unemployed in January 2004)	195	49	146
Regular workers in the contracted firms	27	0	27
Contracted firms	12	0	12

Source: GfK Belgrade, own calculations.

Note: Observations are dropped due to missing values in important characteristics or implausible employment statuses, respectively.

Table 5: Distribution of observations across training and/or temporary employment participation.

Participation in training?	Participation in temporary employment?		
	No	Yes	
No	146 obs.	28 obs.	174 obs.
Yes	48 obs.	66 obs.	114 obs.
	194 obs.	94 obs.	288 obs.

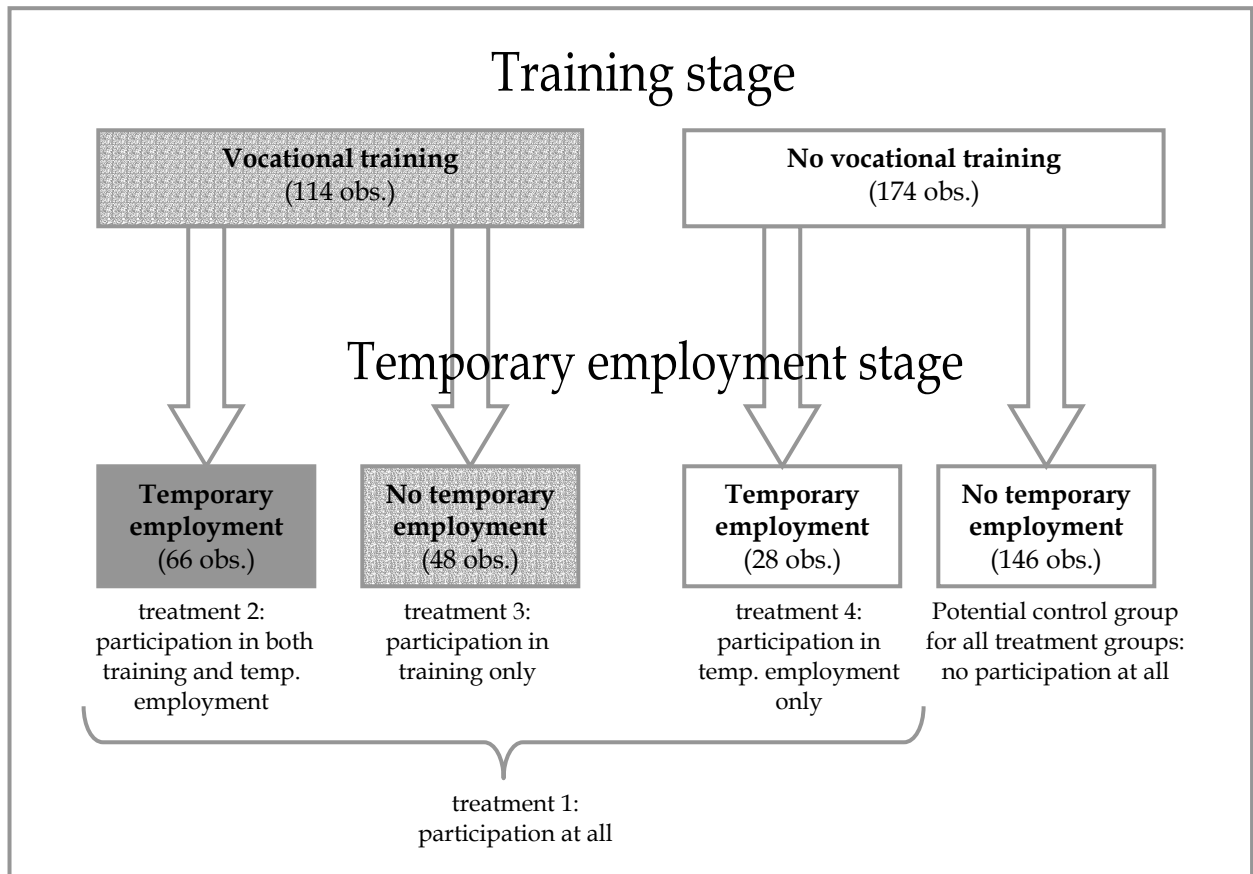
Source: GfK Belgrade, own calculations.

Table 6: Definitions of treatment and control groups.

Type of treatment	Size of treatment group	Size of potential control group
(1) Participation at all	142 obs.	146 obs.
(2) Participation the complete program	66 obs.	146 obs.
(3) Participation in the training stage only	48 obs.	146 obs.
(4) Participation in the temporary employment stage only	28 obs.	146 obs.

Source: GfK Belgrade, own calculations.

Figure 1: Definitions of treatment and potential control groups.



Source: Own illustration.

Table 7: Participation at all (1): socio-demographic characteristics of treatment and potential control group (comparison of means).

Socio-demographic characteristics	Treatment group		Potential Control group		Difference significant?		
	obs.	mean	obs.	mean	t-test statistic	p-value	
ln(Age)	142	3.41110	146	3.47620	-1.71	0.088	*
ln(Age) ²	142	11.7310	146	12.1960	-1.75	0.081	*
ln(Age) ³	142	40.6720	146	43.1820	-1.79	0.074	*
married	142	0.45070	146	0.58219	-2.24	0.026	**
Roma	142	0.16197	146	0.08219	2.08	0.038	**
Belgrade	142	0.47887	146	0.31507	2.87	0.004	***
homeowner	142	0.35915	146	0.28082	1.43	0.155	
education: primary school or less	142	0.35915	146	0.28767	1.30	0.196	
education: vocational school (3 years)	142	0.33803	146	0.41096	-1.28	0.203	
disabled	142	0.01408	146	0.08904	-2.89	0.004	***
moved in past 5 years	142	0.07042	146	0.08219	-0.37	0.708	
< 1 year previously unemployed	142	0.30986	146	0.17123	2.78	0.006	***
1-2 years previously unemployed	142	0.25352	146	0.15753	2.03	0.044	**
2-3 years previously unemployed	142	0.20423	146	0.14384	1.35	0.177	
3-4 years previously unemployed	142	0.08451	146	0.08219	0.07	0.944	
employed in last 3 years	142	0.75352	146	0.56849	3.37	0.001	***
share of employment in last 3 years	142	0.20335	146	0.19321	0.37	0.711	
receipt of benefits?	142	0.03796	146	0.08521	-2.06	0.040	**
active job search?	142	0.83803	146	0.63699	3.96	0.000	***
ALMP participation before?	142	0.04225	146	0.07534	-1.19	0.235	
high job desire?	142	0.89437	146	0.74658	3.31	0.001	***
high chances to find a job?	142	0.28169	146	0.19863	1.65	0.099	*

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The potential control group consists of individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 8: Participation in the complete program (2): socio-demographic characteristics of treatment and potential control group (comparison of means).

Socio-demographic characteristics	Treatment group		Potential control group		Difference significant?		
	obs.	mean	obs.	mean	t-test statistic	p-value	
ln(Age)	66	3.38950	146	3.47620	-1.79	0.076	*
ln(Age) ²	66	11.5820	146	12.1960	-1.81	0.072	*
ln(Age) ³	66	39.8990	146	43.1820	-1.83	0.068	*
married	66	0.31818	146	0.58219	-3.65	<0.001	***
Roma	66	0.10606	146	0.08219	0.56	0.575	
Belgrade	66	0.48485	146	0.31507	2.39	0.018	**
homeowner	66	0.31818	146	0.28082	0.55	0.582	
education: primary school or less	66	0.31818	146	0.28767	0.45	0.654	
education: vocational school (3 years)	66	0.33333	146	0.41096	-1.07	0.285	
disabled	66	0.00000	146	0.08904	-2.53	0.012	**
moved in past 5 years	66	0.09091	146	0.08219	0.21	0.834	
< 1 year previously unemployed	66	0.33333	146	0.17123	2.66	0.008	***
1-2 years previously unemployed	66	0.25758	146	0.15753	1.73	0.085	*
2-3 years previously unemployed	66	0.21212	146	0.14384	1.24	0.217	
3-4 years previously unemployed	66	0.09091	146	0.08219	0.21	0.834	
employed in last 3 years	66	0.74242	146	0.56849	2.44	0.015	**
share of employment in last 3 years	66	0.21928	146	0.19321	0.71	0.476	
receipt of benefits?	66	0.04106	146	0.08521	-1.38	0.169	
active job search?	66	0.84848	146	0.63699	3.18	0.002	***
ALMP participation before?	66	0.03030	146	0.07534	-1.26	0.207	
high job desire?	66	0.89394	146	0.74658	2.47	0.014	**
high chances to find a job?	66	0.25758	146	0.19863	0.96	0.337	

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in both training and temporary employment. The potential control group consists of individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 9: Participation in the training stage only (3): socio-demographic characteristics of treatment and potential control group (comparison of means).

Socio-demographic characteristics	Treatment group		Potential control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	48	3.41210	146	3.47620	-1.16	0.246
ln(Age) ²	48	11.7400	146	12.1960	-1.18	0.238
ln(Age) ³	48	40.7260	146	43.1820	-1.20	0.230
married	48	0.50000	146	0.58219	-0.99	0.322
Roma	48	0.20833	146	0.08219	2.41	0.017 **
Belgrade	48	0.50000	146	0.31507	2.33	0.021 **
homeowner	48	0.39583	146	0.28082	1.50	0.136
education: primary school or less	48	0.41667	146	0.28767	1.67	0.097 *
education: vocational school (3 years)	48	0.33333	146	0.41096	-0.95	0.342
disabled	48	0.02083	146	0.08904	-1.59	0.114
moved in past 5 years	48	0.04167	146	0.08219	-0.94	0.349
< 1 year previously unemployed	48	0.27083	146	0.17123	1.51	0.133
1-2 years previously unemployed	48	0.31250	146	0.15753	2.37	0.019 **
2-3 years previously unemployed	48	0.20833	146	0.14384	1.06	0.293
3-4 years previously unemployed	48	0.06250	146	0.08219	-0.44	0.660
employed in last 3 years	48	0.72917	146	0.56849	1.99	0.048 **
share of employment in last 3 years	48	0.18113	146	0.19321	-0.30	0.762
receipt of benefits?	48	0.03750	146	0.08521	-1.28	0.203
active job search?	48	0.81250	146	0.63699	2.28	0.024 **
ALMP participation before?	48	0.08333	146	0.07534	0.18	0.858
high job desire?	48	0.91667	146	0.74658	2.53	0.012 **
high chances to find a job?	48	0.37500	146	0.19863	2.50	0.013 **

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in training only.
The potential control group consists of individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 10: Participation in the temporary employment stage only (4):
socio-demographic characteristics of treatment and potential control group
(comparison of means).

Socio-demographic characteristics	Treatment group		Potential control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	28	3.46010	146	3.47620	-0.23	0.815
ln(Age) ²	28	12.0660	146	12.1960	-0.27	0.788
ln(Age) ³	28	42.4020	146	43.1820	-0.31	0.760
married	28	0.67857	146	0.58219	0.95	0.344
Roma	28	0.21429	146	0.08219	2.12	0.036 **
Belgrade	28	0.42857	146	0.31507	1.16	0.246
homeowner	28	0.39286	146	0.28082	1.18	0.238
education: primary school or less	28	0.35714	146	0.28767	0.73	0.465
education: vocational school (3 years)	28	0.35714	146	0.41096	-0.53	0.597
disabled	28	0.03571	146	0.08904	-0.95	0.345
moved in past 5 years	28	0.07143	146	0.08219	-0.19	0.849
< 1 year previously unemployed	28	0.32143	146	0.17123	1.84	0.067 *
1-2 years previously unemployed	28	0.14286	146	0.15753	-0.20	0.845
2-3 years previously unemployed	28	0.17857	146	0.14384	0.47	0.639
3-4 years previously unemployed	28	0.10714	146	0.08219	0.43	0.669
employed in last 3 years	28	0.82143	146	0.56849	2.54	0.012 **
share of employment in last 3 years	28	0.20387	146	0.19321	0.21	0.832
receipt of benefits?	28	0.03143	146	0.08521	-1.13	0.260
active job search?	28	0.85714	146	0.63699	2.29	0.023 **
ALMP participation before?	28	0.00000	146	0.07534	-1.50	0.135
high job desire?	28	0.85714	146	0.74658	1.26	0.209
high chances to find a job?	28	0.17857	146	0.19863	-0.24	0.808

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in temporary employment only.
The potential control group consists of individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 11: Explanatory variables included in the preferred specification of the regression model.

Name of variable	Survey question	Description
$\ln(\text{Age})$		Logarithm of age (in years)
$\ln(\text{Age})^2$	What is your exact age?	Logarithm of age (in years) squared
$\ln(\text{Age})^3$		Logarithm of age (in years) cubed
married		1: if married 0: otherwise
Roma	To which ethnic group do you think you belong?	1: if Roma 0: otherwise
Belgrade	Place of residence?	1: if Belgrade 0: otherwise
homeowner	Type of dwelling?	1: if owned by respondent 0: otherwise
education: primary school or less	What is the highest level of your education?	1: if without education, up to 4 years of primary school, 5 to 7 years of primary school, or primary school 0: otherwise
education: vocational school (3 years)		1: if vocational/secondary special school (3 years) 0: otherwise
disabled		1: if yes (categories I, II, or III) 0: otherwise
moved in past 5 years	Have you changed your place of living (city) in last 5 years?	1: if yes 0: otherwise
< 1 year previously unemployed	How long were you already out of work before January 2004 (January 2005 for respondents from Zrenjanin/Niš)?	1: if duration 12 months or less 0: otherwise
1-2 years previously unemployed		1: if duration between 13 and 24 months 0: otherwise
2-3 years previously unemployed		1: if duration between 25 and 36 months 0: otherwise
3-4 years previously unemployed		1: if duration between 37 and 48 months 0: otherwise
employed in last 3 years		1: if having worked at all (at least 1 month) 0: otherwise
share of employment in last 3 years	Can you remember roughly how many months in total you did work during the years 2001, 2002 and 2003 (2002, 2003 and 2004 for respondents from Zrenjanin/Niš)?	Number of months working in the 3-year-period divided by 36 months (duration of that period)
receipt of benefits?	In that period of time before 2004 (2005 for respondents from Zrenjanin/Niš), did you receive any of the following?	1: if receipt of social assistance, unemployment benefits, or other benefits 0: otherwise
active job search?	In that period of time before 2004 (2005 for respondents from Zrenjanin/Niš), did you apply for jobs? And if so, how often on average did you apply?	1: if job application at least once per month 0: otherwise
ALMP participation before?	Had you participated in any program or measure offered by the local labor office before January 2004 (2005 for respondents from Zrenjanin/Niš)?	1: if answer yes 0: otherwise
high job desire?	How would you describe your situation on the following points in that time before January 2004 (2005 for respondents from Zrenjanin/Niš)?	1: if desire to find a job judged at least reasonable 0: otherwise
high chances to find a job?		1: if possibility to find a regular job judged at least reasonable 0: otherwise

Source: GfK Belgrade, own illustration.

Table 12a: Marginal effects of probit estimates.

Variable	(1) Participation at all			(2) Participation in the complete program		
	Coefficient	p-value		Coefficient	p-value	
ln(Age)	- 63.32293	0.188		- 107.5017	0.046	**
ln(Age) ²	19.15592	0.168		31.83987	0.042	**
ln (Age) ³	- 1.913580	0.151		- 3.118195	0.038	**
Married	- 0.913583	0.022	**	- 0.278216	0.003	***
Roma	0.246141	0.029	**	0.152229	0.295	
Belgrade	0.170177	0.023	**	0.084071	0.311	
homeowner	0.204691	0.013	**	0.095956	0.303	
education: primary school or less	0.140181	0.127		0.075012	0.452	
education: vocational school (3 years)	0.066163	0.450		0.002087	0.982	
disabled	- 0.168917	0.414				
moved in past 5 years	- 0.249096	0.048	**	- 0.164072	0.148	
< 1 year previously unemployed	0.364399	0.001	***	0.412443	0.004	***
1-2 years previously unemployed	0.327055	0.001	***	0.389935	0.003	***
2-3 years previously unemployed	0.323923	0.002	***	0.318431	0.017	**
3-4 years previously unemployed	0.299240	0.017	**	0.352578	0.026	**
employed in last 3 years	0.204193	0.036	**	0.095711	0.357	
share of employment in last 3 years	- 0.391416	0.044	**	- 0.162339	0.388	
receipt of benefits?	- 0.350458	0.141		- 0.269418	0.257	
active job search?	0.242150	0.003	***	0.186791	0.021	**
ALMP participation before?	- 0.334970	0.015	**	- 0.239506	0.047	**
high job desire?	0.183819	0.052	*	0.143088	0.135	
high chances to find a job?	0.113660	0.157		0.053720	0.546	
# obs. total		288			199	
# obs. treatment group		142			66	
# obs. control group		146			133	
pseudo R ²		0.2460			0.2390	

Source: GfK Belgrade, own calculations.

Notes: Participation at all (1): Participants are individuals who participated in training only, in temporary employment only, or in both. Non-participants are individuals who did not participate in the BS program at all.

Participation in the complete program (2): Participants are individuals who participated in both training and subsequent temporary employment. Non-participants are individuals who did not participate in the BS program at all.

Statistical significance at the 99 percent level: ***

Statistical significance at the 95 percent level: **

Statistical significance at the 90 percent level: *

Table 12b: Marginal effects of probit estimates.

Variable	(3)		(4)	
	Participation in training only		Participation in temporary employment only	
	Coefficient	p-value	Coefficient	p-value
ln(Age)	- 34.03334	0.472	0.875729	0.979
ln(Age) ²	10.51090	0.443	0.150758	0.988
ln (Age) ³	- 1.068852	0.416	- 0.053270	0.954
Married	- 0.131270	0.098	* 0.023270	0.688
Roma	0.279234	0.031	** 0.243051	0.030 **
Belgrade	0.146703	0.041	** 0.063941	0.319
homeowner	0.132619	0.088	* 0.133729	0.040 **
education: primary school or less	0.135706	0.134	0.040507	0.534
education: vocational school (3 years)	0.100333	0.232	0.038520	0.547
disabled	- 0.006940	0.967	- 0.042172	0.685
moved in past 5 years	- 0.171253	0.039	** - 0.069428	0.292
< 1 year previously unemployed	0.281639	0.027	** 0.271558	0.025 **
1-2 years previously unemployed	0.313272	0.007	*** - 0.003339	0.966
2-3 years previously unemployed	0.299060	0.011	** 0.101106	0.323
3-4 years previously unemployed	0.161901	0.291	0.109450	0.316
employed in last 3 years	0.102616	0.205	0.156531	0.011 **
share of employment in last 3 years	- 0.344552	0.058	* - 0.311638	0.034 **
receipt of benefits?	- 0.177155	0.374	- 0.175525	0.350
active job search?	0.108217	0.112	0.115685	0.031 **
ALMP participation before?	- 0.125885	0.171		
high job desire?	0.065843	0.424	0.074786	0.193
high chances to find a job?	0.190189	0.015	** - 0.045573	0.438
# obs. total		194		163
# obs. treatment group		48		28
# obs. control group		146		135
pseudo R ²		0.2655		0.2560

Source: GfK Belgrade, own calculations.

Notes: Participation in training only (3): Participants are individuals who participated in training only. Non-participants are individuals who did not participate in the BS program at all.

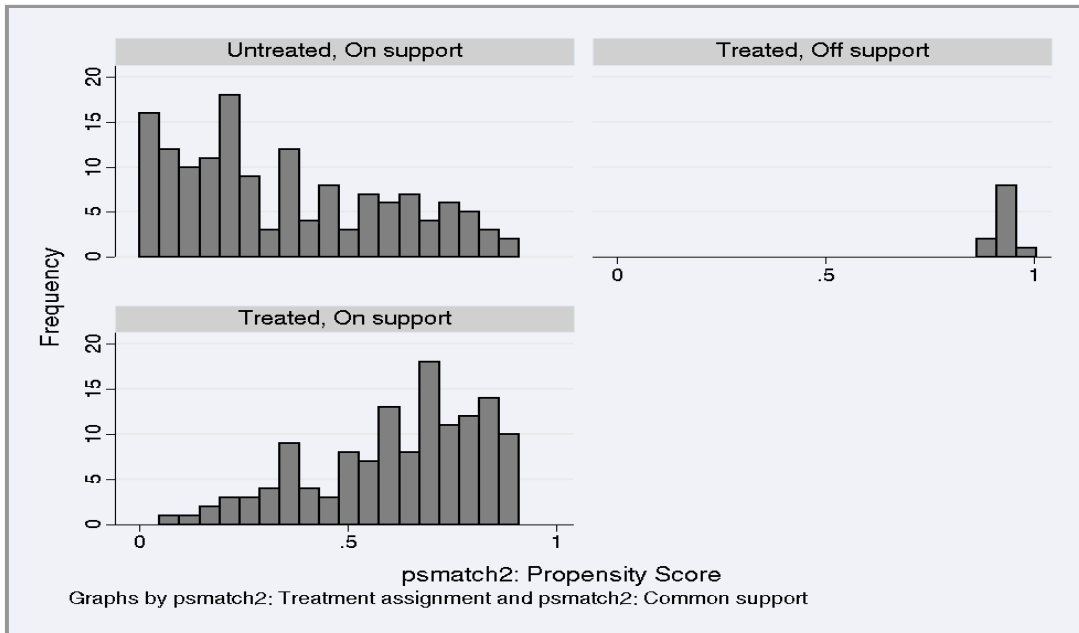
Participation in temporary employment only (4): Participants are individuals who participated in temporary employment only. Non-participants are individuals who did not participate in the BS program at all.

Statistical significance at the 99 percent level: ***

Statistical significance at the 95 percent level: **

Statistical significance at the 90 percent level: *

Figure 2: Participation at all (1): One-to-one nearest neighbor matching, distribution of propensity scores and common support.



Source: GfK Belgrade, own calculations.

Notes: Participation at all (1): The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

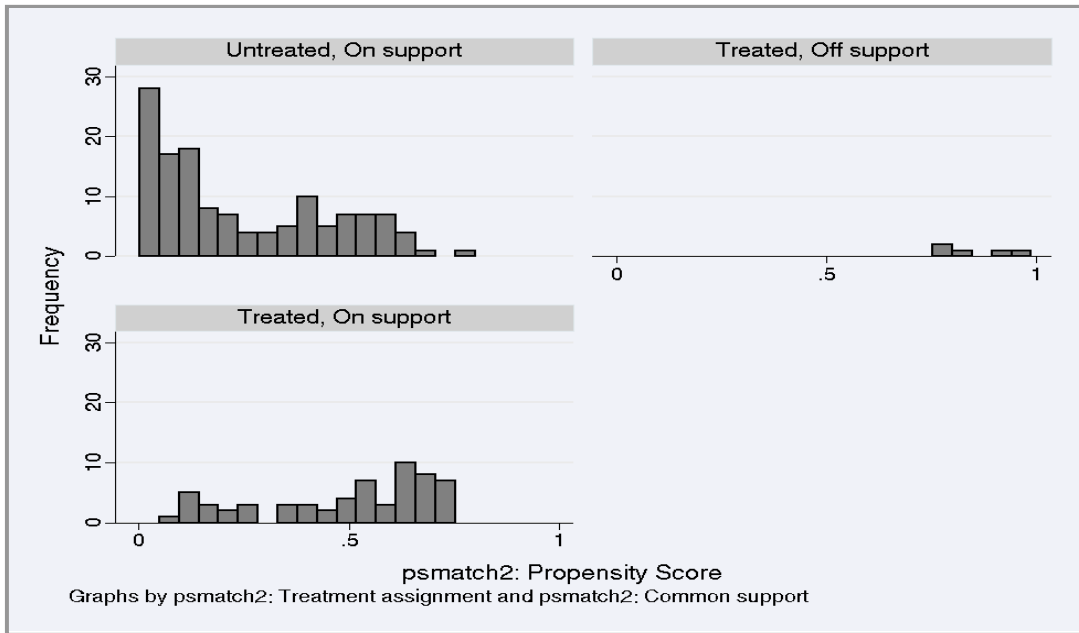
Table 13: Participation at all (1): One-to-one nearest neighbor matching with replacement, control group observations used *after* matching.

# matches per control group obs.	# obs.
1	33
2	10
3	8
4	3
5	3
6	2
7	1
8	1
Total # obs.	61

Source: GfK Belgrade, own calculations.

Notes: Participation at all (1): The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

Figure 3: Participation in the complete program (2): One-to-one nearest neighbor matching, distribution of propensity scores and common support.



Source: GfK Belgrade, own calculations.

Notes: Participation in the complete program (2): The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.

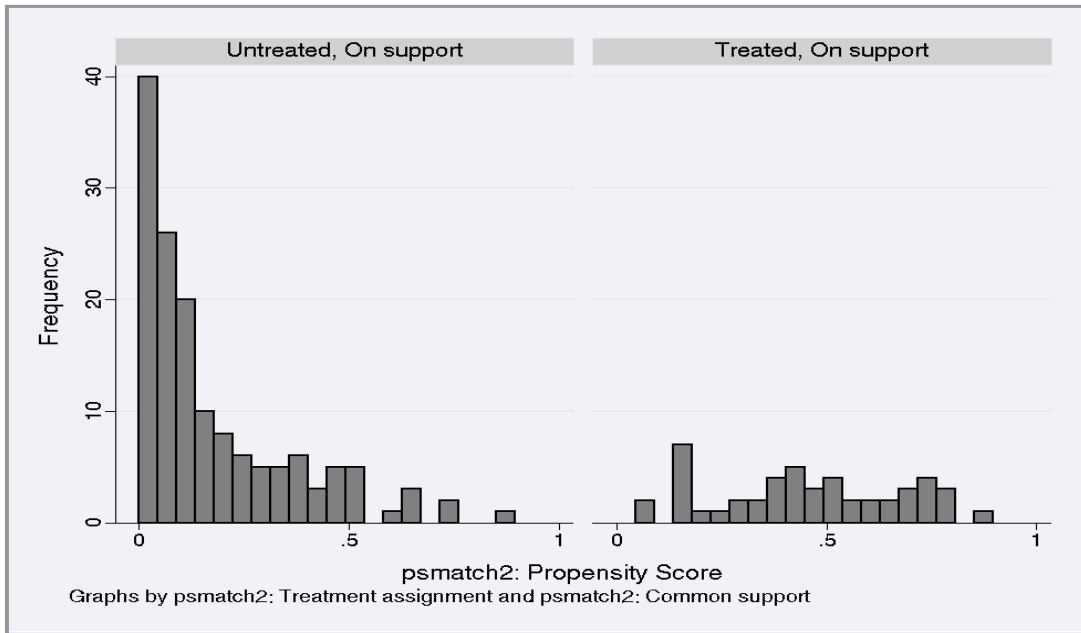
Table 14: Participation in the complete program (2): One-to-one nearest neighbor matching with replacement, control group observations used *after* matching.

# matches per control group obs.	# obs.
1	22
2	6
3	2
5	1
12	1
Total # obs.	33

Source: GfK Belgrade, own calculations.

Notes: Participation in the complete program (2): The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.

Figure 4: Participation in training only (3): One-to-one nearest neighbor matching, distribution of propensity scores and common support.



Source: GfK Belgrade, own calculations.

Notes: Participation in training only (3): The treatment group includes individuals who participated in training only. The control group consists of *matched* individuals who did not participate in the BS program at all.

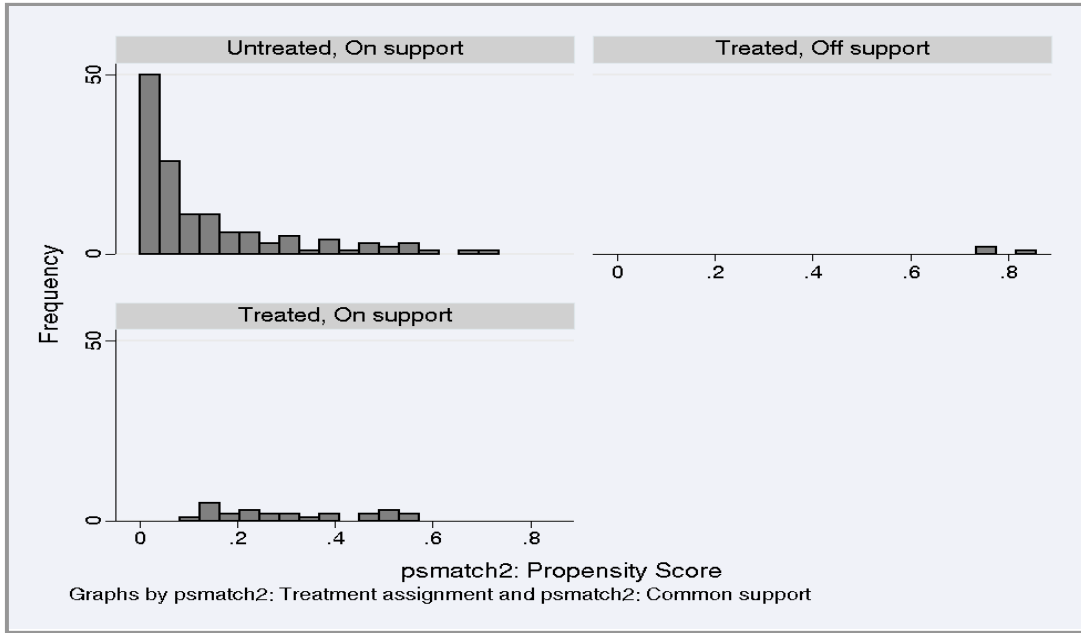
Table 15: Participation in training only (3): One-to-one nearest neighbor matching with replacement, control group observations used *after* matching.

# matches per control group obs.	# obs.
1	15
2	4
4	5
5	1
Total # obs.	25

Source: GfK Belgrade, own calculations.

Notes: Participation in training only (3): The treatment group includes individuals who participated in training only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Figure 5: Participation in temporary employment only (4): One-to-one nearest neighbor matching, distribution of propensity scores and common support.



Source: GfK Belgrade, own calculations.

Notes: Participation in temporary employment only (4): The treatment group includes individuals who participated in temporary employment only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Table 16: Participation in temporary employment only (4): One-to-one nearest neighbor matching with replacement, control group observations used *after* matching.

# matches per control group obs.	# obs.
1	15
2	5
Total # obs.	20

Source: GfK Belgrade, own calculations.

Notes: Participation in temporary employment only (4): The treatment group includes individuals who participated in temporary employment only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Table 17: Participation at all (1): socio-demographic characteristics of treatment and control group *after matching* (comparison of means).

Socio-demographic characteristics	Treatment group		Control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	131	3.40150	131	3.43820	-0.62	0.535
ln(Age) ²	131	11.6670	131	11.9160	-0.61	0.539
ln(Age) ³	131	40.3520	131	41.6230	-0.61	0.544
married	131	0.45038	131	0.48092	-0.43	0.668
Roma	131	0.16031	131	0.10687	1.14	0.254
Belgrade	131	0.45038	131	0.46565	0.19	0.851
homeowner	131	0.33588	131	0.37405	-0.22	0.827
education: primary school or less	131	0.35878	131	0.31298	0.69	0.489
education: vocational school (3 years)	131	0.35115	131	0.38168	-0.65	0.520
disabled	131	0.01527	131	0.01527	-0.07	0.944
moved in past 5 years	131	0.07634	131	0.12977	-1.42	0.159
< 1 year previously unemployed	131	0.28244	131	0.24427	1.04	0.300
1-2 years previously unemployed	131	0.25191	131	0.27481	-0.34	0.733
2-3 years previously unemployed	131	0.22137	131	0.24427	-0.68	0.496
3-4 years previously unemployed	131	0.08397	131	0.04580	1.11	0.269
employed in last 3 years	131	0.73282	131	0.70229	0.82	0.415
share of employment in last 3 years	131	0.20218	131	0.18066	0.76	0.451
receipt of benefits?	131	0.04115	131	0.04924	-0.50	0.619
active job search?	131	0.82443	131	0.85496	-0.33	0.740
ALMP participation before?	131	0.04580	131	0.03817	0.15	0.883
high job desire?	131	0.88550	131	0.94656	-1.36	0.174
high chances to find a job?	131	0.26718	131	0.31298	-0.49	0.628

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 18: Participation the complete program (2): socio-demographic characteristics of treatment and control group *after matching* (comparison of means).

Socio-demographic characteristics	Treatment group		Control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	61	3.37910	61	3.4230	-0.51	0.612
ln(Age) ²	61	11.5120	61	11.8330	-0.55	0.583
ln (Age) ³	61	39.5440	61	41.3130	-0.60	0.552
married	61	0.32787	61	0.34426	-0.27	0.786
Roma	61	0.09836	61	0.04918	1.05	0.299
Belgrade	61	0.47541	61	0.54098	-0.55	0.581
homeowner	61	0.31148	61	0.42623	-1.11	0.271
education: primary school or less	61	0.31148	61	0.18033	1.58	0.117
education: vocational school (3 years)	61	0.36066	61	0.45902	-1.28	0.205
disabled						
moved in past 5 years	61	0.09836	61	0.03279	1.19	0.239
< 1 year previously unemployed	61	0.31148	61	0.29508	0.41	0.686
1-2 years previously unemployed	61	0.24590	61	0.24590	0.13	0.895
2-3 years previously unemployed	61	0.22951	61	0.22951	-0.21	0.837
3-4 years previously unemployed	61	0.09836	61	0.04918	0.80	0.424
employed in last 3 years	61	0.72131	61	0.78689	-0.52	0.607
share of employment in last 3 years	61	0.22313	61	0.24727	-0.60	0.549
receipt of benefits?	61	0.04443	61	0.02689	0.64	0.525
active job search?	61	0.83607	61	0.91803	-1.07	0.289
ALMP participation before?	61	0.03279	61	0.01639	0.45	0.653
high job desire?	61	0.88525	61	0.96721	-1.42	0.159
high chances to find a job?	61	0.26230	61	0.09836	2.08	0.040 **

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in both training and temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 19: Participation in training only (3): socio-demographic characteristics of treatment and control group *after matching* (comparison of means).

Socio-demographic characteristics	Treatment group		Control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	48	3.41210	48	3.36760	0.57	0.568
ln(Age) ²	48	11.7400	48	11.4570	0.53	0.598
ln(Age) ³	48	40.7260	48	39.3820	0.49	0.628
married	48	0.50000	48	0.35417	1.26	0.213
Roma	48	0.20833	48	0.04167	2.19	0.032 **
Belgrade	48	0.50000	48	0.64583	-1.26	0.213
homeowner	48	0.39583	48	0.56250	-1.43	0.158
education: primary school or less	48	0.41667	48	0.37500	0.36	0.720
education: vocational school (3 years)	48	0.33333	48	0.39583	-0.55	0.585
disabled	48	0.02083	48	0.02083	-0.00	1.000
moved in past 5 years	48	0.04167	48	0.04167	-0.00	1.000
< 1 year previously unemployed	48	0.27083	48	0.27083	0.00	1.000
1-2 years previously unemployed	48	0.31250	48	0.25000	0.59	0.559
2-3 years previously unemployed	48	0.20833	48	0.18750	0.22	0.826
3-4 years previously unemployed	48	0.06250	48	0.06250	0.00	1.000
employed in last 3 years	48	0.72917	48	0.66667	0.57	0.567
share of employment in last 3 years	48	0.18113	48	0.16840	0.29	0.776
receipt of benefits?	48	0.03750	48	0.00313	1.59	0.116
active job search?	48	0.81250	48	0.77083	0.43	0.666
ALMP participation before?	48	0.08333	48	0.10417	-0.30	0.764
high job desire?	48	0.91667	48	0.87500	0.58	0.566
high chances to find a job?	48	0.37500	48	0.50000	-1.07	0.288

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in training only.
The control group consists of *matched* individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 20: Participation in temporary employment only (4): socio-demographic characteristics of treatment and control group *after matching* (comparison of means).

Socio-demographic characteristics	Treatment group		Control group		Difference significant?	
	obs.	mean	obs.	mean	t-test statistic	p-value
ln(Age)	25	3.44220	25	3.44610	0.14	0.886
ln(Age) ²	25	11.9480	25	12.0020	0.09	0.925
ln (Age) ³	25	41.8190	25	42.2340	0.05	0.963
married	25	0.64000	25	0.44000	1.68	0.100
Roma	25	0.20000	25	0.28000	-0.52	0.607
Belgrade	25	0.40000	25	0.40000	0.20	0.845
homeowner	25	0.40000	25	0.48000	-0.60	0.553
education: primary school or less	25	0.36000	25	0.24000	0.87	0.388
education: vocational school (3 years)	25	0.32000	25	0.44000	-0.58	0.568
disabled	25	0.04000	25	0.00000	0.90	0.375
moved in past 5 years	25	0.08000	25	0.04000	0.46	0.647
< 1 year previously unemployed	25	0.28000	25	0.24000	0.61	0.542
1-2 years previously unemployed	25	0.12000	25	0.12000	0.23	0.820
2-3 years previously unemployed	25	0.20000	25	0.20000	-0.19	0.854
3-4 years previously unemployed	25	0.12000	25	0.08000	0.31	0.754
employed in last 3 years	25	0.80000	25	0.64000	1.42	0.161
share of employment in last 3 years	25	0.20722	25	0.13333	1.34	0.185
receipt of benefits?	25	0.03520	25	0.06400	-0.70	0.490
active job search?	25	0.84000	25	0.88000	-0.23	0.820
ALMP participation before?						
high job desire?	25	0.84000	25	0.92000	-0.67	0.503
high chances to find a job?	25	0.16000	25	0.24000	-0.51	0.609

Source: GfK Belgrade, own calculations.

Notes: The treatment group includes individuals who participated in temporary employment only.
The control group consists of *matched* individuals who did not participate in the BS program at all.

Difference statistically significant at the 99 percent level: ***

Difference statistically significant at the 95 percent level: **

Difference statistically significant at the 90 percent level: *

Table 21: Program impacts on unemployment and employment probabilities for treatment and control groups.

		(1) Participation at all		(2) Participation in the complete program		(3) Participation in training only		(4) Participation in temporary employment only	
		Treated	Controls	Treated	Controls	Treated	Controls	Treated	Controls
Unemployment	Without matching	42.96	42.47	45.45	42.47	45.83	42.47	32.14	42.47
		0.49		2.99		3.37		- 10.32	
	ATT	42.75	58.02	44.26	49.18	45.83	64.58	32.00	56.00
		- 15.27		- 4.92		- 18.75		- 24.00	
Regular job or self-employed	Without matching	20.42	9.59	22.73	9.59	14.58	9.59	25.00	9.59
		10.83		13.14		4.99		15.41	
	ATT	19.08	9.16	22.95	14.75	14.58	8.33	20.00	12.00
		9.92		8.20		6.25		8.00	
Employment	Without matching	25.35	34.93	19.70	34.93	27.08	34.93	35.71	34.93
		- 9.58		- 15.23		- 7.85		0.78	
	ATT	25.95	23.66	21.31	31.15	27.08	14.58	40.00	20.00
		2.29		- 9.84		12.50		20.00*	
ALMP job	Without matching	7.04	2.05	7.58	2.05	6.25	2.05	7.14	2.05
		4.99		5.53		4.20		5.09	
	ATT	7.63	5.34	8.20	3.28	6.25	10.42	8.00	12.00
		2.29		4.92		- 4.17		- 4.00	

Source: GfK Belgrade, own calculations.

Notes: In Percent. Bold numbers indicate mean differences or ATT, respectively.
"ALMP" refers to jobs within a program implemented by the NES.

* ATT statistically significant at the 95 percent level for at least one definition of bootstrap statistics.

- (1) Participation at all: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (2) Participation in the complete program: The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (3) Participation in training only: The treatment group includes individuals who participated in training only. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (4) Participation in temporary employment only: The treatment group includes individuals who participated in temporary employment only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Table 22: Distribution of observations across training and/or temporary employment participation *for individuals living in Belgrade.*

		Participation in temporary employment?		
		No	Yes	
Participation in training?	No	46 obs.	12 obs.	58 obs.
	Yes	24 obs.	32 obs.	56 obs.
		70 obs.	44 obs.	114 obs.

Source: GfK Belgrade, own calculations.

Table 23: Distribution of observations across training and/or temporary employment participation *for individuals living in Niš or Zrenjanin.*

		Participation in temporary employment?		
		No	Yes	
Participation in training?	No	100 obs.	16 obs.	116 obs.
	Yes	24 obs.	34 obs.	58 obs.
		124 obs.	50 obs.	174 obs.

Source: GfK Belgrade, own calculations.

Table 24: Impact of the BS program on probability of employment for treatment and control groups (*Belgrade vs. Niš/Zrenjanin*).

		Belgrade				Niš/Zrenjanin			
		(1) Participation at all		(2) Participation in the complete program		(1) Participation at all		(2) Participation in the complete program	
		Treated	Controls	Treated	Controls	Treated	Controls	Treated	Controls
Unemployment	Without matching	50.00	43.48	53.13	43.48	36.49	42.00	38.24	42.00
		6.52		9.65		- 5.51		- 3.76	
	ATT	50.00	62.50	54.17	66.67	35.09	33.33	33.33	45.83
		- 12.50		- 12.50		1.75		- 12.50	
Regular job and self-employed	Without matching	25.00	13.04	28.13	13.04	16.22	8.00	17.65	8.00
		11.96		15.09		8.22		9.65	
	ATT	25.00	9.38	25.00	8.33	14.04	15.79	16.67	4.17
		15.63		16.67		- 1.75		12.50	
Seasonal job	Without matching	19.12	23.91	12.50	23.91	31.08	40.00	26.47	40.00
		- 4.80		- 11.41		- 8.92		- 13.53	
	ATT	18.75	28.13	12.50	25.00	36.84	42.11	33.33	33.33
		- 9.38		- 12.50		- 5.26		0.00	
ALMP job	Without matching	1.47	2.17	3.13	2.17	12.16	2.00	11.76	2.00
		- 0.70		0.96		10.16		9.76	
	ATT	1.56	0.00	4.17	0.00	8.77	3.51	8.33	16.67
		1.56*		4.17*		5.26		- 8.33	

Source: GfK Belgrade, own calculations.

Notes: In Percent. Bold numbers indicate mean differences or ATT, respectively.
"ALMP" refers to jobs within a program implemented by the NES.

* ATT statistically significant at the 95 percent level for at least one definition of bootstrap statistics.

(1) Participation at all: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

(2) Participation in the complete program: The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.

Table 25: Distribution of observations across training and/or temporary employment participation for individuals who have been previously unemployed 12 months or less (*short-term unemployed*).

		Participation in temporary employment?		
		No	Yes	
Participation in training?	No	25 obs.	9 obs.	34 obs.
	Yes	13 obs.	22 obs.	35 obs.
		38 obs.	31 obs.	69 obs.

Source: GfK Belgrade, own calculations.

Table 26: Distribution of observations across training and/or temporary employment participation for individuals who have been previously unemployed more than 12 months (*long-term unemployed*).

		Participation in temporary employment?		
		No	Yes	
Participation in training?	No	121 obs.	19 obs.	140 obs.
	Yes	35 obs.	44 obs.	79 obs.
		156 obs.	63 obs.	219 obs.

Source: GfK Belgrade, own calculations.

Table 27: Impact of the BS program on probability of employment for treatment and control groups (ATT) for individuals who have been previously unemployed 12 months or less (*short-term vs. long-term unemployed*).

		Short-term unemployed		Long-term unemployed	
		(1)		(1)	
		Participation at all		Participation at all	
		Treated	Controls	Treated	Controls
Unemployment	Without matching	54.55	52.00	37.76	40.50
		2.55		- 2.74	
	ATT	64.00	52.00	34.78	55.43
		12.00		- 20.65	
Regular job and self-employed	Without matching	20.45	16.00	20.41	8.26
		4.45		12.14	
	ATT	16.00	40.00	20.65	16.30
		- 24.00		4.35	
Employment	Without matching	18.18	20.00	28.57	38.02
		- 1.82		- 9.45	
Seasonal job	ATT	16.00	8.00	30.43	26.09
		8.00		4.35	
ALMP job	Without matching	2.27	0.00	9.18	2.48
		2.27		6.70	
	ATT	0.00	0.00	9.78	2.17
		0.00		7.61	

Source: GfK Belgrade, own calculations.

Notes: In Percent. Bold numbers indicate mean differences or ATT, respectively. "ALMP" refers to jobs within a program implemented by the NES.

* ATT statistically significant at the 95 percent level for at least one definition of bootstrap statistics.

(1) Participation at all: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

Table 28: Mean wage and income differences between treatment and control groups from regular jobs, seasonal jobs and ALMP jobs *after matching*.

		(1) Participation at all		(2) Participation in the complete program		(3) Participation in training only		(4) Participation in temporary employment only	
		Treated	Controls	Treated	Controls	Treated	Controls	Treated	Controls
Regular jobs	# obs.	18	10	12	4	6	4	0	2
	Mean wage	14,458	18,075	15,771	22,000	11,833	19,000	n/a	14,875
Seasonal jobs	# obs.	32	31	13	19	12	7	9	5
	Mean wage	14,847	13,185	12,662	11,908	20,417	17,071	13,944	13,250
ALMP jobs	# obs.	6	7	4	2	2	5	0	3
	Mean wage	18,500	11,071	16,375	7,500	22,750	17,500	n/a	11,667
Any job*	Mean wage	15,113	13,895	14,602	14,640	18,109	17,687	n/a	n/a

Source: GfK Belgrade, own calculations.

Notes: ALMP jobs are jobs within a program implemented by the NES.

* Mean wages in any job are weighted averages of the mean wages in the three categories of employment when the estimated ATT serve as weights.

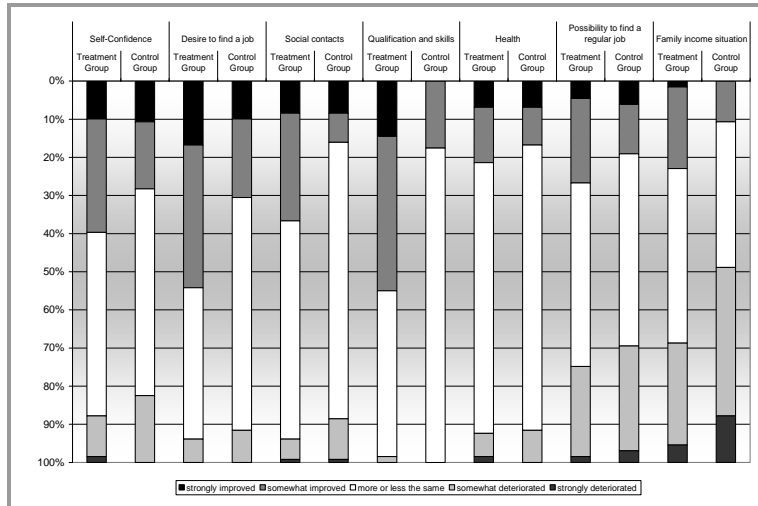
- (1) Participation at all: The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (2) Participation in the complete program: The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (3) Participation in training only: The treatment group includes individuals who participated in training only. The control group consists of *matched* individuals who did not participate in the BS program at all.
- (4) Participation in temporary employment only: The treatment group includes individuals who participated in temporary employment only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Mean wage difference statistically significant at the 99 percent level: ***

Mean wage difference statistically significant at the 95 percent level: **

Mean wage difference statistically significant at the 90 percent level: *

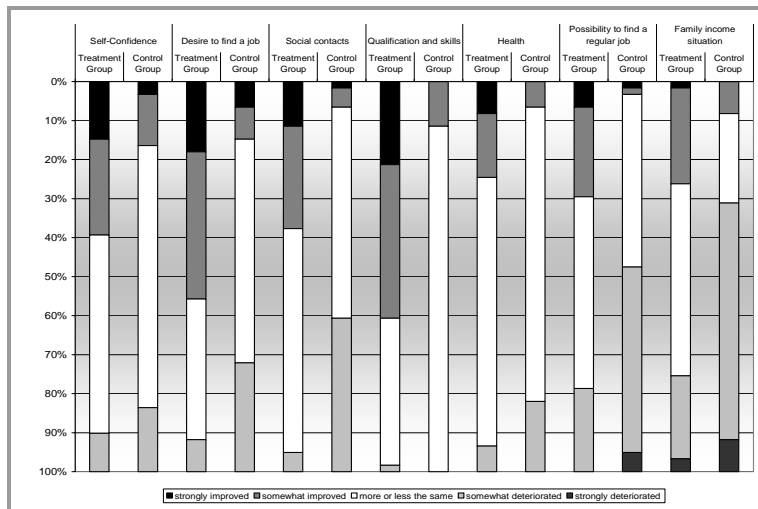
Figure 6: Participation at all (1): Distribution of subjective welfare indicators.



Source: GfK Belgrade, own calculations.

Note: Participation at all (1): The treatment group includes individuals who participated in training only, in temporary employment only, or in both. The control group consists of *matched* individuals who did not participate in the BS program at all.

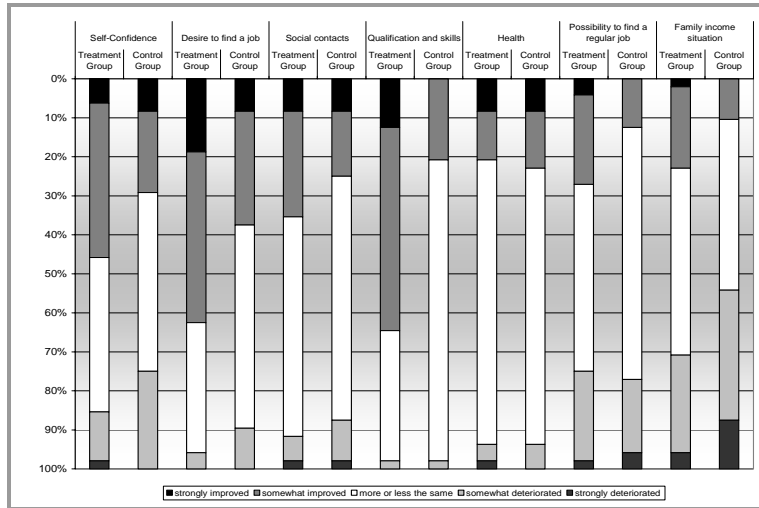
Figure 7: Participation in the complete program (2): Distribution of subjective welfare indicators.



Source: GfK Belgrade, own calculations.

Note: Participation in the complete program (2): The treatment group includes individuals who participated in both training and subsequent temporary employment. The control group consists of *matched* individuals who did not participate in the BS program at all.

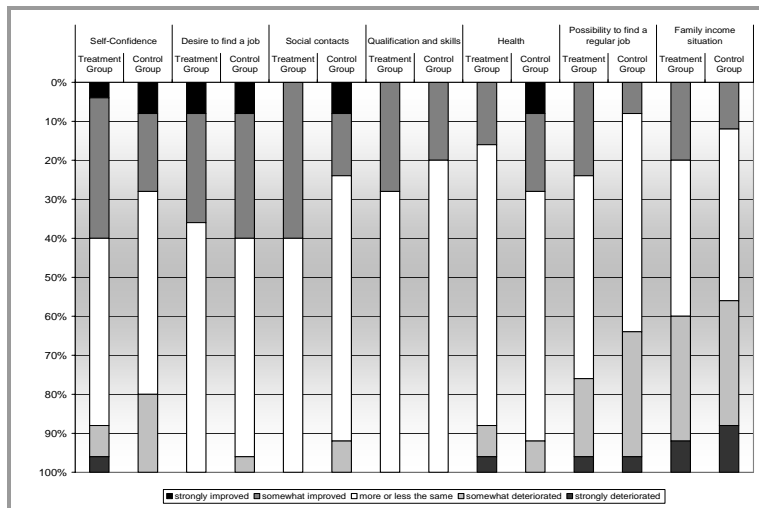
Figure 8: Participation in training only (3): Distribution of subjective welfare indicators.



Source: GfK Belgrade, own calculations.

Note: Participation in training only (3): The treatment group includes individuals who participated in training only. The control group consists of *matched* individuals who did not participate in the BS program at all.

Figure 9: Participation in temporary employment only (4): Distribution of subjective welfare indicators.



Source: GfK Belgrade, own calculations.

Note: Participation in temporary employment only (4): The treatment group includes individuals who participated in temporary employment only. The control group consists of individuals who did not participate in the BS program at all.

Table 29: Improvement of subjective welfare indicators (treatment vs. control groups).

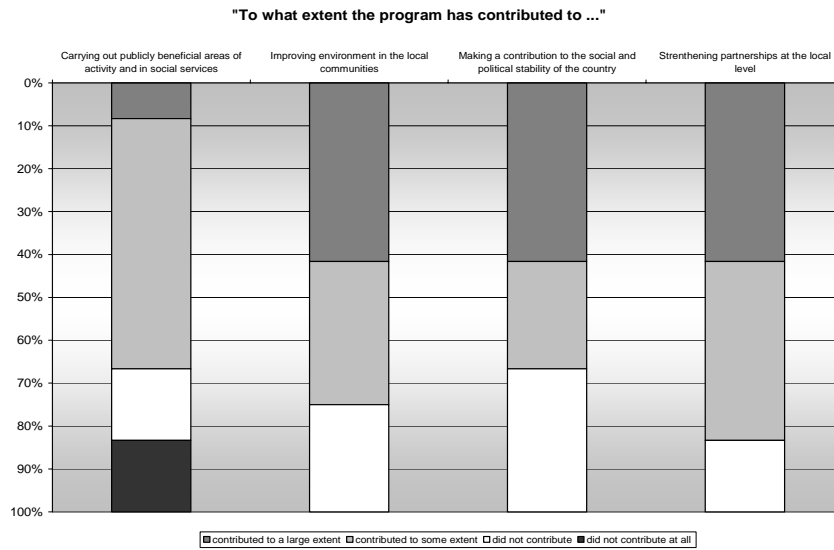
		(1) Participation at all		(2) Participation in the complete program		(3) Participation in training only		(4) Participation in temporary employment only	
		Treated	Controls	Treated	Controls	Treated	Controls	Treated	Controls
Self-confidence	Without matching	40.14	22.60	37.88	22.60	45.83	22.60	35.71	22.60
		17.54		15.28		23.23		13.11	
	ATT	39.69	28.24	39.34	16.39	45.83	29.17	40.00	28.00
		11.45		22.95*		16.67		12.00	
Job desire	Without matching	54.23	26.03	54.55	26.03	62.50	26.03	39.29	26.03
		28.20		28.52		36.47		13.26	
	ATT	54.20	30.53	55.74	14.75	62.50	37.50	36.00	40.00
		23.66*		40.98*		25.00		- 4.00	
Social contacts	Without matching	36.62	11.64	37.88	11.64	35.42	11.64	35.71	11.64
		24.98		26.24		23.77		24.70	
	ATT	36.64	16.03	37.70	6.56	35.42	25.00	40.00	24.00
		20.61*		31.15*		10.42		16.00	
Qualification and skills	Without matching	54.23	10.96	59.09	10.96	64.58	10.96	25.00	10.96
		43.27		48.13		53.62		14.04	
	ATT	54.96	17.56	60.66	11.48	64.58	20.83	28.00	20.00
		37.40*		49.18*		43.75*		8.00	
Health	Without matching	21.13	13.70	24.24	13.70	20.83	13.70	14.29	13.70
		7.43		10.54		7.13		0.59	
	ATT	21.37	16.79	24.59	6.56	20.83	22.92	16.00	28.00
		4.58		18.03*		- 2.08		- 12.00	
Job chances	Without matching	26.76	10.27	28.79	10.27	27.08	10.27	21.43	10.27
		16.49		18.52		16.81		11.16	
	ATT	26.72	19.08	29.51	3.28	27.08	12.50	24.00	8.00
		7.63		26.23*		14.58		16.00	
Family income situation	Without matching	23.24	10.27	25.76	10.27	22.92	10.27	17.86	10.27
		12.97		15.49		12.64		7.59	
	ATT	22.90	10.69	26.22	8.20	22.92	10.42	20.00	12.00
		12.21*		18.03*		12.50		8.00	

Source: GfK Belgrade, own calculations.

Notes: In Percent. Bold numbers indicate average treatment effects on the treated (ATT).

* ATT statistically significant at the 95 percent level for at least one definition of bootstrap statistics.

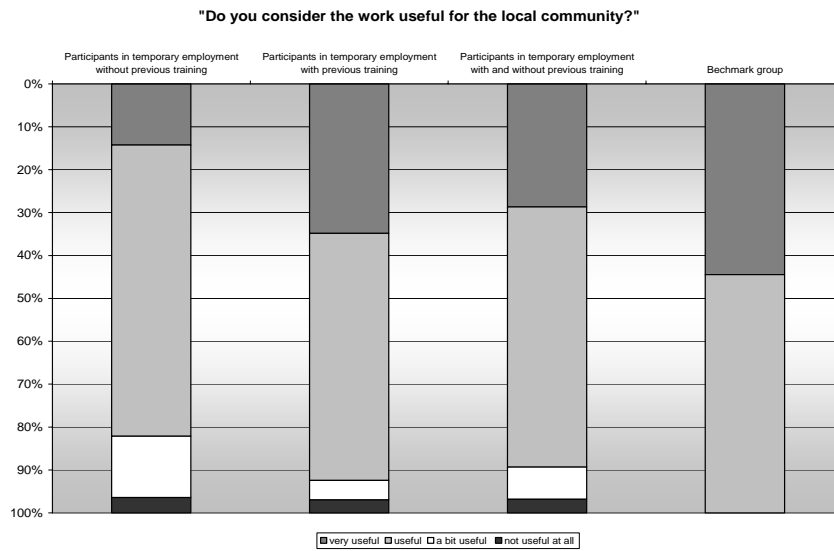
Figure 10: Impact on local communities: Impression of contracted firms.



Source: GfK Belgrade, own calculations.

Note: The figure displays data on 12 contracted firms.

Figure 11: Impact on local communities: Impression of program participants in temporary employment (previously unemployed and benchmark group).



Source: GfK Belgrade, own calculations.

Note: The figure refers to 28 persons who participated in the temporary employment without previous training and 66 persons with previous training. Therefore, the total number of observations on participants in temporary employment amounts to 94. Additionally, 27 persons belong to the benchmark group.

Table 30: Stylized sequence of events for the cost-benefit analysis.

Treatment	Period of time (number of months)	Treatment group	Control group
(2) complete program	January 2004 – March 2004 (3 months)	unemployed	unemployed
	April 2004 – June 2004 (3 months)	vocational training	unemployed
	July 2004 – September 2004 (3 months)	temporary employment	unemployed
	October 2004 – November 2004 (2 months)	unemployed	unemployed
	December 2004 – March 2005 (4 months)	unemployed	observed labor market status
	April 2005 – October 2005 (7 months)	observed labor market status	observed labor market status
(3) training stage only	January 2004 – March 2004 (3 months)	unemployed	unemployed
	April 2004 – June 2004 (3 months)	vocational training	unemployed
	July 2004 – November 2004 (5 months)	unemployed	unemployed
	December 2004 – January 15, 2005 (1.5 months)	unemployed	observed labor market status
	January 16, 2005 – October 2005 (9.5 months)	observed labor market status	observed labor market status
(4) temporary employment stage only	January 2004 – June 2004 (6 months)	unemployed	unemployed
	July 2004 – September 2004 (3 months)	temporary employment	unemployed
	October 2004 – November 2004 (2 months)	unemployed	unemployed
	December 2004 – March 2005 (4 months)	unemployed	observed labor market status
	April 2005 – October 2005 (7 months)	observed labor market status	observed labor market status

Source: Own illustration.

Table 31: Average monthly costs and/or benefits associated with specific labor market statuses as well as with training and temporary employment.

Labor market status	Monthly costs and/or benefits		Remarks
Vocational training	Direct costs	177.41 EUR	Total costs of training amount to 630.25 USD per participant. *
Temporary employment	Additional costs per unemployed worker (monitoring etc.)	49.62 EUR	Additional costs are calculated as average wages of workers who additionally had to be hired by the contracted firms. **
	Income taxes and social security contributions	22.41 EUR (24.98 EUR)	50 percent of average gross monthly income in this type of job (14 percent income tax flat rates, 36 percent employee's and employer's social security contribution rate).
Unemployment	Unemployment benefits	7.25 EUR (7.57 EUR)	Average monthly unemployment benefits per unemployed person according to our data.
Regular job and self-employment	Adjusted income taxes and social security contributions	81.47 EUR (95.85 EUR)	50 percent of average gross monthly income in this type of job (14 percent income tax flat rates, 36 percent employee's and employer's social security contribution rate). Adjusted for informal jobs: 85.71 percent of jobs are with contract and pay social security contributions according to our data.
Seasonal job	Adjusted income taxes and social security contributions	53.36 EUR (70.40 EUR)	50 percent of average gross monthly income in this type of job (14 percent income tax flat rates, 36 percent employee's and employer's social security contribution rate). .Adjusted for informal jobs: 85.71 percent of jobs are with contract and pay social security contributions according to our data. Adjusted for seasonality: We assume that people actually work only 75 percent of the year.

Source: GfK Belgrade, UNDP Serbia and Montenegro, own calculations.

Notes: Numbers in brackets indicate the respective amounts assumed for Belgrade only.

Assumed exchange rates: 1 € = 85.41 Din.
1 € = 1.1842 USD

* This information was provided by UNDP Serbia and Montenegro.

** Average wages of additional workers are calculated by multiplying the average monthly gross wage of a member of the benchmark group times the average number of additional workers according to the employers' survey.

Table 32: Results of the cost-benefit analysis: average individual cost-benefit measures.

	Belgrade, Niš and Zrenjanin		Belgrade only	
	Treated	Controls	Treated	Controls
(2) complete program	- 491.08 EUR	196.07 EUR	- 473.65 EUR	142.68 EUR
	- 687.16 EUR		- 616.32 EUR	
	(- 168.75 EUR)	(496.97 EUR)	(- 129.70 EUR)	(389.17 EUR)
	(- 665.73 EUR)		(- 518.87 EUR)	
(3) training only	- 382.51 EUR	74.12 EUR	- 353.15 EUR	229.21 EUR
	- 456.63 EUR		- 582.36 EUR	
	(- 106.39 EUR)	(241.97 EUR)	(- 102.15 EUR)	(499.08 EUR)
	(- 348.37 EUR)		(- 601.23 EUR)	
(4) temporary employment only	40.94 EUR	52.36 EUR	73.66 EUR	241.03 EUR
	- 11.42 EUR		- 167.37 EUR	
	(502.41 EUR)	(297.18 EUR)	(840.86 EUR)	(594.80 EUR)
	(205.23 EUR)		(246.06 EUR)	

Source: GfK Belgrade, own calculations.

Notes: Cost-benefit measures are aggregated over the entire period which is analyzed (January 2004 – October 2005) and averaged over the respective group under consideration.

Bold numbers indicate the average cost-benefit difference between treated and non-treated individuals.

Numbers in brackets represent the results of the same analyses if the period under consideration is extended for additional 12 months *ceteris paribus*, i.e. if this period starts in January 2004 and only ends in October 2006.