Can Subsidized Employment Tackle Long-Term Unemployment?
Experimental Evidence from Macedonia[*]

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Abstract

This paper examines the impact of a randomized experiment in Macedonia in which program applicants were assigned to receive a job interview and the employers were subsidized when job and worker were matched. The interview led to more than 30 percentage point increase in employment in the first 6 months, and the average effect persists, at around 25 percentage points after the subsidies has expired. The young, female, inexperienced and low skill workers are the main beneficiaries of the policy.

JEL codes: O15; J08; J68.
Keywords: Active Labor Market Policy; Unemployment; Wage Subsidies; Job Search.

1 Introduction

In the presence of labour market frictions, firms may prefer not to hire workers whose productivity is uncertain. Under these conditions, unemployment will be higher especially among individuals with lower ability to signal productivity. Wage subsidies are therefore introduced to provide a solution to this issue by lowering the cost of hiring for a limited amount of time.[1] At the same time, subsidies present the potential to generate long run impacts on employment beyond the duration of the subsidy, by increasing the experience and productivity of participants and by allowing firms to know about the quality of the workers (Bell et al., 1999).

Thanks to these advantages, wage subsidies have become a popular policy option to raise employment among the youth and disadvantaged groups, especially in developed countries (Kaldor, 1936; Layard and Nickell, 1980; Katz, 1998). However, in this setting, these programs have been largely ineffective in tackling unemployment (Burtless, 1985; Dubin and Rivers, 1993; Betcherman et al., 2004), especially when offered without providing support for job development, training, and job search assistance (Katz, 1998). In addition, evidence on the effectiveness of wage subsidies in generatoing long-term effects is mixed. In Germany, Kvasnicka (2009) finds no impact of temporary jobs on long-term employment. Similar conclusion is reached by Autor and Houseman (2010) for Detroit, US. In Canada, Card and Hyslop (2005) finds short-term impacts on employment of wage subsidies, but the effect has completely faded out 1.5 years after the end of the subsidy. On the contrary, Pallais (2014) finds an impact of an initial job on subsequent employment in an online market.

More recently, interest on wage subsidies has risen in transition and developing countries (Almeida et al., 2012). However, the limited evidence related to these programs suggest that wage subsidies might not be highly effective in developing countries, especially in generating additional employment under standard labor market conditions (McKenzie, 2017).

Firstly, the presence of burdensome labor regulations associated with hiring workers through the program may disincentivize firms to participate. Galasso et al. (2004) study the Argentina's Proempleo Experiment, which offered welfare recipients a wage subsidy voucher valid for up to 18 months, paying the firm up to $150 per month. The program was not successful since it required formal registration of any worker hired through the program and a large penalty in case the firm would have fired the worker. Therefore very few workers in the treatment group were actually employed. A similar finding is also observed by Levinsohn and Pugatch (2014) in South Africa.

Secondly, the effect of the subsidies tend to be centralized in the short run only, with beneficial effects fading off in the long run. In Jordan, Groh et al. (2016) studies the effect of providing wage vouchers among recent college graduates using a randomized experiment. They observe a large effect on employment (around 38%) during the duration of the voucher (6 months). However, this effect vanishes in the long-run, with firms firing workers or workers quitting their job beyond

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[1]In response to a wage subsidy, workers might also increase their reservation wage, so that the cost of labor need to to fall by the full amount of the subsidy (Levinsohn and Pugatch, 2014).

the validity of the voucher. This suggests wage subsidies could be beneficial only as a temporary instrument to increase employment. This effect is also observed when the subsidy is provided to firms instead of workers. De Mel et al. (2010, 2016) look at the effect of providing wage subsidies to microenterprises in Sri Lanka to incentivized the hiring of new workers. They find a significant increase in employment in response to the subsidy, but in the long run no lasting effect is observed on employment, firm profitability and sales. The nature of the temporary effect of wage subsidies is also evident when looking at programs supporting firms during temporary demand and/or liquidity shocks. Bruhn (2016) studies a wage subsidy program in Mexico during the global financial crisis and finds a significant effect on employment (6 to 13 percent) and on post-crisis growth.

This paper analyzes the impact of the Subsidized Employment Program (SEP) in the Republic of North Macedonia. The main goal of this program is to improve the employment prospects of disadvantaged groups in the population, especially (but not only) young adults who are recipients of Social and Financial Assistance (SFA), by providing temporary subsidies to employers who hire employees from these groups. All potential employees need to be registered at the National Employment Agency (NEA) as unemployed active employment seekers. The employment subsidy can take three possible formats, and participating firms have the option to choose which one they prefer. These options involve a mix of wage subsidy, and tax incentives for hiring program benefi-ciaries, which can both have different levels and durations in different modalities, in exchange for guaranteed employment for a certain amount of time. The exact mix of benefits and employment guarantees depend both on the employers choice of modality and the characteristics of the worker being hired. The SEP started to be advertised in June 2015 together with the opening of the ap-plication process. Both potential employers and potential employees had to file an application in which they had to respectively indicate the needed qualifications for the job and their qualifica-tions. After collecting the applications, the NEA matched potential employees to each potential job based on the skills required to obtain the job and the applicant's qualifications.

Among all qualified applicants, a subset of job seekers was randomly allocated to a treatment group, while the remaining applicants to a control group. The treatment consists in a job interview with the employer, which was scheduled by NEA officials at a mutually convenient time. If the employer decided to hire any of the applicants, successful interviewees were offered the job and the employer would receive the wage subsidy and other benefits depending on the chosen program modality at the time of the application. Baseline data was collected on potential participants and non-participants in the SEP and their households, between October 2015 and February 2016, while the interview process took place in August-September 2015 . A follow-up survey was conducted roughly 18 months later, between May and August of 2017.

We find that, in the short-run, SEP produces a large increase in the employment of individuals selected for SEP jobs. This is not surprising since, those unemployed individuals who get a job offer after the interview have the possibility of becoming automatically employed. This effect declines over time, mainly because some individuals in the non-SEP program manage to secure

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employment (this is a time of output and employment growth in Macedonia). Nevertheless, it remains positive and large, even 2 years after SEP individuals entered the program.

The remainder of the paper is organized as follows. In Section 2 we describe the program and the experimental design, while in Section 3 we present the dataset. In Section 4 we discuss the empirical strategy, and Section 5 presents the results. Section 6 concludes.

2 Background and experimental design

The Subsidized Employment Program is a social assistance program launched by the Macedonian Ministry of Labor and Social Policies in the summer of 2015. It consists of a wage subsidy given to potential employers for hiring a new employee from specific targeted groups of beneficiaries which include recipients of Social Financial Assistance (henceforth, SFA), other vulnerable groups such as recipients of Permanent Financial Assistance (PFA) or Conditional Cash Transfer (CCT) for secondary education, as well as other social assistance beneficiaries whose family monthly income per capita was lower than half of the average net salary.[2] Although people of any age from these vulnerable groups were eligible for the program, young people between 16 and 29 years of age were deemed as a priority group. Appendix A.1 detail the requirements to define potential beneficiaries.

In order to participate into the program, both potential employers and potential employees had to file and submit an application to the National Employment Agency specifying, in the case of potential employers, the desired characteristics of the workers for any new job vacancy they would like to open; and in the case of potential employees, the level of education, qualifications and previous experience. At the time of filing the application, potential employers could choose among three possible modalities which differ in the duration and extent of the wage subsidies and other benefits as well as in the employers' obligations. Figure 1 summarizes the subsidy design, the benefits and the obligations under the three schemes.

Under the first modality, potential employers receive a six-months wage subsidy with the obligation to keep the worker for additional six months under the same conditions. There are two types of subsidy level: one lower for the beneficiaries without qualifications and the other higher for beneficiaries with higher educational degree or for the ones that are going to be employed for performing more complex working tasks. The amount of the lower subsidy is 14,900 MKD per month per employee, while the amount of the higher subsidy is 17,000 MKD per month per employee. In addition to the wage subsidy, employers also receive an additional 5,000 MKD per month per employee for the first 6 months to compensate the training and material costs of the newly hired worker.

The second modality provides a less generous six-months wage subsidy as compared to the first modality but also exempts the employers from payment of social security contributions for

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[2]Beneficiaries of PFA are usually people that up to the age of 18 had a status of children without parents and parental care. For more information regarding the CCT for secondary education, see Armand et al. (2018).

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the newly hired worker for five years since the date of signing the employment contract. Again, there are two types of subsidy level which depend on the qualifications of the worker. The amount of the lower subsidy is 10,500 MKD per month per employee, while the amount of the higher subsidy is 11,900 MKD per month per employee. This modality does not envisage an obligation for the employer to keep the hired worker after the end of the six months period.

Finally, the third modality provides a six-months wage subsidy and a three years exemption from payment of social security contributions and personal income tax for the newly hired employee. The subsidy level is equal to the one under the second modality (i.e., 10,500 MKD per month per employee for low skill workers and 11,900 MKD per month per employee for high skill workers) and the employer has an obligation to keep the worker for additional twelve months since the end of the first six months under the same conditions.

Although the benefits and costs of hiring and keeping a worker are distributed differently across time in the three modalities, we believe that the large majority of employers choose the first modality. We base this observation on a firm survey that we conducted in order to understand the types of benefits (and their distribution over time) that the employers received for hiring a worker through the program. Among all the employers we surveyed, none reported that they received an exemption from payment of social contributions and personal income tax for longer than six months. Therefore, in the rest of the paper, we refer to the timing of the program as the one originating from the first modality and interpret the results accordingly.

The SEP started to be advertised in June 2015 together with the opening of the application process. As anticipated above, both potential employers and potential employees had to file an application in which they had to respectively indicate the needed qualifications for the job and their qualifications. After collecting the applications, the National Employment Agency created lists of job seekers for each available job by matching potential employees to each potential job based on the skills required to obtain the job and the applicant's qualifications. After employee's and employer's applications were collected between June and August 2015, the matching process resulted in a total of 107 potential employees for 26 job vacancies whose skills were compatible with the employer's requirements. Since for most of the available jobs there were several qualified applicants, we implemented an experimental design that randomly assigned about half of job seekers to a treatment group (53 individuals), and the remaining half of applicant to a control group (54 individuals). The treatment consists in a job interview with the employer, which was scheduled by NEA officials at a mutually convenient time. If the employer decided to hire any of the applicants, successful interviewees were offered the job and the employer would receive the wage subsidy and other benefits depending on the chosen program modality at the time of the application. The interview process took place in August-September 2015, and successful applicants could start to work immediately after that date depending on their mutual agreement with the employer.

With a sample of this size, we aim to detect large impacts of SEP on employment. In our power calculations, we consider employment rates of either 10% or 20% in this population and, with about 50 treatment and 50 control individuals, we should be able to detect an impact of being

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offered a SEP interview on employment rates of at least 17 to 22 percentage points. These are large numbers, and imply even larger impacts of getting a SEP job (as opposed to an interview) on employment probabilities.[3] Nevertheless, as we document below, the program did produce large increases in the employment of individuals in the treated group in the short and in the medium run. Although the longer run impacts are positive but more modest, we show that employment effects are persistent among the treated individuals and that the shrinking estimated effects of the program on employment rates are due to individuals in the control group eventually finding a job rather than treated individuals loosing the job after the end of the subsidy.

3 Data

3.1 Data collection and sample

We use two main sources of data in our paper. First, to study the impact of SEP on employment and to construct a measure of previous work experience, we rely on administrative data from the NEA. This dataset reports any employment spell for individuals registered with the NEA since 1980. In our main results in the paper, we convert employment spells into a series of monthly employment indicators and look at the difference in employment status between treated and untreated individuals over the period July 2015- November 2017.

The second source of data are two applicant and household level surveys, which we use to explore the potential mechanisms behind our main results. The first survey was administered between October 2015 and February 2016 while the second took place between May and August 2017. As already discussed in the previous section, the SEP program started to be phased in between August and September 2015. The original goal was to collect information on the targeted population before the job interview were scheduled but, due to administrative delays, the first wave of the data collection actually took place after a significant fraction of treated individuals took up a subsidised employment job. Therefore, we do not regard this as a baseline survey but rather as a short-term survey about the impact of SEP.

Instead, the second wave of data, which was collected more than one and a half years after the introduction of SEP, provides information on the medium-term outcomes and characteristics of program participants after the last wage subsidy was paid to the employer and (depending on the chosen modality) after the employer's contractual obligations are fulfilled.

The individual survey comprises extensive information about the applicant's education, labour supply and income histories, mental health conditions as well as on-the-job, cognitive and non-cognitive skills.[4] We measure non-cognitive skills using two well known scales: the Big-5 questionnaire (Goldberg, 1992) and the 12-item grit scale (Duckworth and Quinn, 2009). Cognitive

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[3]If we assume that 1 out of 4 interviewees gets a SEP job, we would be able to detect very large impacts of getting a SEP job on employment, of about 60 to 80 percentage points.

[4]On-the-job skills are partly adapted from the World Bank's STEP survey (World Bank, 2016). We measure mental health condition using the Centre for Epidemiological Studies Depression scale (Eaton et al, 2004)

skills are assessed using the Abbreviated Raven's test of progressive matrices (Bilker et al, 2012). The household survey, which was administered to the head of household, contains information about demographics, education and employment of every household member, the household's participation in social assistance programs, household expenditure, ownership of durables and conditions of the dwellings.

Our short-term survey was administered to the 107 individuals that originally applied to the program and that were selected, among the universe of applicants, as potential candidates based on the required qualifications and characteristics for a given post. Our sample in the medium-term assessment is smaller (91 individuals), given that about 15 percent of the sample was lost due to attrition.

Regarding our administrative data on the employment history of job seekers, there are two main issues that we need to address. First, for about a third of the sample we do not observe any employment record. Although applicants to SEP should be registered at the NEA as active job seekers, they need to check-in periodically in order to confirm their employment status and they are automatically deleted from the list of job seekers if they do not comply with such requirement. We do not recode the employment status for such individuals as they might not be entirely idle and thus we drop them from the sample in our main employment results. In the Appendix we provide a detailed discussion about attrition.

The second limitation is that for 19 individuals we are unable to retrieve the firm to which such job seekers were originally matched.[5] This is unfortunate since, without information on the matching firm for every individual, we either restrict further our already limited sample or we exclude the possibility of including firm fixed effects in our empirical strategy. Given the small sample size, we decide to include these individuals in the main results that we present in the paper. In the appendix, we present some robustness checks in which we exclude individuals without a valid matching firm and show that results are essentially unchanged.

3.2 Summary Statistics

Table 1 shows some descriptive statistics for the baseline sample, separately for each treatment group (columns 1 and 2). In column 3, we test for imbalances in individual characteristics by reporting the mean difference between the treatment and the control groups, together with the corresponding standard error. We combine data from the applications to the program (age, gender and education), the administrative employment records (past work experience) and the short-term

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[5]None of these job seekers got a SEP job but slightly more than half participated in an interview with the employer. A plausible explanation for the lack of information on the firm identifier for these 19 individuals is that the matching of job seekers and firms proceeded in stages until vacancies were filled, and that in subsequent stages the firm information got lost. In the first stage, a priority group of job seekers with adequate qualifications for the post was identified among applicants in the same municipality of the firm. If this first stage did not result in a match, applicants with adequate characteristics for the post but from other municipalities entered a second randomisation round and lottery winners were sent to an interview with the employer. Importantly, none of the job seekers in the subsequent randomisation rounds was selected to participate in a lottery in the first round, so that each individual in the sample only received one treatment.

survey (ethnicity) to test the extent to which the randomisation was successful.[6]

Program participants are on average 45 years old and approximately 60 percent of them are male. Macedonians are the most widely represented ethnic group (55 percent of total applicants), while ethnic Albanians represent roughly 30 percent of program participants.[7] On average, 31 percent of applicants have at most a primary school degree while 69 percent have completed secondary school or university. Slightly more than half of program participants had at least one job in the past and they cumulated only about 4.5 years of past work experience before participating into SEP. Since applicants to SEP are on average 45 years old, this suggests that long unemployment spells, erratic participation in the labor market and possibly high levels of informality are extremely common within this population.

As can be seen in the third column of Table 1, there are no statistically significant differences between applicants in the treatment group and those in the control group. Therefore, our sample is balanced in terms of demographics, education and previous work experience. In this section, we only rely on these demographic characteristics to asses the balance. When presenting the main results for the employment history (see Section 5), we will show that the pre-program employment rates of the treatment and of the control group are not statistically different from each others.

4   Empirical strategy

Our experimental design matched firms with potential workers, but randomly assigned the opportunity to interview with the firm only to a subset of workers, the treatment group. We start by assessing the effect of being offered an interview by comparing individual's labor market outcomes for individuals in the treatment group versus indidivuals in the control groups. We estimate the following model:

$$Y_{ij} = \alpha + \beta T_{ij} + \lambda' X_i + \varepsilon_{ij}, \tag{1}$$

where Y_{ij} is the outcome of interest for applicant i matched to job j, T_{ij} is an indicator variable taking value 1 if the applicant was assigned to the treatment group, X_i is a vector of individual control variables, and ε_{ij} is an i.i.d. error term. As participation in the interview does not guarantee that the worker will be hired, the parameter of interest, β, represents the intent-to-treat (ITT) parameter for this particular outcome.

Since the program aims at increasing employment by providing subsidized jobs, it is important to focus not only on the effect of participating in the job interview, but also on the effect of being hired as part of the program. To measure this effect, we focus on the following specification:

$$Y_{ij} = \delta + \gamma D_{ij} + \theta' X_i + v_{ij}. \tag{2}$$

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[6]The sample size, which is reported in column 4, is lower than 107 for age, gender and education because of non-response. As already discussed in Section 3.1, for about a third of the sample employment records were not available so that we can measure previous work experience only for those individuals with valid employment data.

[7]Since the program was targeted to recipients of SFA, Albanians, Roma and other ethnic groups, which are on average poorer than Macedonians, are over-represented in our sample as compared to nationwide statistics.

where D_{ij} is an indicator variable equal to 1 if individual i was offered job j. Since D_{ij} is correlated with unobserved individual or job characteristics, we follow an Instrumental Variable estimation strategy and use the random assignment to the interview, T_{ij}, as an instrument for D_{ij}. The coefficient of interest, γ, can be interpreted as the impact of obtaining a SEP job among those who obtained a SEP job after the interview or, in other words, the effect of the treatment on the treated (TOT).[8]

Our main outcome of interest is the employment status of the individual over the period July 2015-November 2017, which we measure using administrative data from the NEA on each registered employment spell of the applicant. We convert employment spells into monthly employment status indicators, and estimate β and γ for each month within our period of interest. Since, SEP was phased-in between September and October 2015, we should observe the employment trajectories of treated and untreated applicants to start diverging around this date, with no significant difference in the two months preceding the start of the program. Apart from the employment status from administrative records, in the paper we also look at the effect of the program on other outcome of interest, such as household expenditure and the employment status of the spouse, if present, and of other family members.

Estimates of equations (1) and (2) allow testing for no impact on the outcomes variables of attending the job interview ($H_0 : \beta = 0$) and obtaining a SEP job ($H_0 : \gamma = 0$). Due to the small sample size of the study, relying only on heteroskedasticity-robust inference could be misleading as the sensitivity of estimates and standard errors to outliers is higher (see, for instance, Imbens and Rubin, 2015). We therefore supplement individual t-test statistics based on heteroskedasticity-robust inference with inference based on permutation tests (Fisher, 1937; Pitman, 1937). This method allows constructing test statistics with finite sample exact size and resilient to outliers, therefore more robust for small samples. Following this procedure, we build the distribution of the test statistic under the null hypothesis by computing the test statistic in all possible combinations in which we could assign the workers in the study to the treatment or the control group. To compute the test statistic in each permutation, we estimate equation (1) with no control variables. We use 1000 replications and we present p-values of the permutation tests in tables.[9]

5 Results

In this section, we report empirical results about the impact of SEP on the targeted population. We start by presenting estimates of the program's effect on employment by comparing the evolution over time in the employment rates of individuals in the treatment group vis a vis the control group (Section 5.1). This represents the more important result of the paper, as it informs about

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[8]The identifying assumption is that the interview did not have an impact on the individual's labor market outcome other than through the subsidized employment job. This might be a questionable assumption if, for example, the interview increased the applicant's motivation to search for more jobs, or if it improved interview skills.

[9]Following the same procedure and using permutations of the outcome variables instead of the treatment assignment leads to the same conclusion.

the successfulness of the program in achieving its primary goal of increasing the employment prospects of individuals in long-term unemployment. In Section 5.2, we present some exploratory results about the heterogeneity of the program's effect on employment. Section 5.3 shows the impact of SEP on other individual and household level outcomes.

5.1 Impact of SEP on employment

In Panel A of Figure 2 we compare the percentage of employed individuals among those who were offered a job interview and those who were not over the period of analysis.[10] The solid line refers to the treatment group, while the dashed line refers to the control. We start by noting that, prior to the beginning of the program (September 2015), the employment rates of the two groups were extremely similar and actually very close to zero. This reassures us about the fact that the main outcome variable in our analysis is balanced across treatments. Right after the start of the program, we observe a marked spike in the employment rates of treated individuals, with an increase of about 35 percentage points as compared to the control group. As time goes by, employment rates in the treatment arm remain high, and actually slightly increase to about 40 percent. Thus, it appears that individuals that received the intervention managed to secure a job even after employers stopped to receive the wage subsidy (which occurred 6 months after the hired worker started working) and were freed from the contractual obligations of the program (which occurred 12 months after the worker started working). As for the control group, employment rates remained flat at zero until the end of 2015 and started to increase only at the beginning of 2016, eventually reaching an average of about 20 to 25 percent two years since the start of SEP. Even after this relatively long period, it appears to exist a substantial gap in the probability of being employed among treated and untreated individuals.

To assess whether the observed employment differences across treatment arms are statistically significant from zero, we complement Panel A of Figure 2 with the ITT estimates from the estimation of equation (1). We present these both in the first three columns of Table 2 and in Panel A of Figure 3. As expected given the random assignment, differences in the employment rates of the treatment and control groups are not statistically different from zero in the months before SEP started (July and August 2015). However, since September 2015 the employment trajectories of the two groups started to diverge. In the first six months, the ITT estimates show statistically significant increases in the employment of interviewees of about 30 to 36 percentage points. The program effects remain large and statistically different from zero until the first months of 2017, about 18 months after the start of SEP and about 6 months after the end of the contractual obligations of most employers.

Since March 2017, ITT estimates are no longer significant (with the exception of November 2017, the last month for which we have employment data). Given the small sample size, we are underpowered to detect such effects but these remain economically large and constant over time

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[10]As discussed in Section 3, employment rates at the monthly frequency have been constructed by converting employment spells from the NEA's administrative data.

at about 17 percentage points. Moreover, as noted in Figure 2, the declining magnitude of the ITT estimates over this period is a result of individuals in the control group taking up employment offers rather than individuals in the treatment group loosing their jobs. This provides evidence that the effects of SEP lasted way beyond the end of the program, and that the employment gap with respect to individuals that were not offered the job interview in the first place remains substantial.

As discussed in Section 4, we also rely on inference based on permutations tests in order to address potential concerns about the sensitivity of our estimates to outliers in small sample sizes. We show this in the third column of Table 2 which reports the p-values of the permutation based tests. As can be seen, results are very similar to those based on heteroskedasticity-robust inference, suggesting that the program effects are not driven by outliers and are robust even in our relatively small sample. Although the ITT estimates reported in Table 2 and in Figure 3 are drawn without adding any control variable to our estimating equation, our results are robust to the inclusion of individual demographic characteristics. Moreover, they are robust to the exclusion of those observations without a valid firm identifier (see the discussion in Section 3.1). We report the estimates corresponding to these robustness checks in Table B4 and B5 in the Appendix.

While the ITT estimates are informative to understand the impact of being offered a job interview on employment, we would also like to know what is the impact of being offered, and taking up, a SEP job. Before presenting our IV estimates, as before we start by representing the employment dynamics for three groups of program participants: those in the treatment group who were offered a SEP job after the interview (labelled "Treatment Job"); those in the treatment group who were offered the interview but did not get the job (labelled "Treatment No Job"); and those in the control group who were not offered the interview (labelled "Control"). The monthly employment rates of these three groups are represented in Panel B of Figure 2.

We observe again that individuals in all three groups are essentially unemployed just before the start of the program. Within the first two months since SEP started, the employment rates of those offered a job jump to about 83%. They subsequently decline slightly to 67% roughly 6 months after the start of SEP. Although this period coincides with the time the SEP subsidy expires for those employers hiring a worker right at the start of the program, we think it is unlikely to be a consequence of the end of the subsidized period for two reasons. First, according to the employer's contractual obligations, the contract can not be terminated for another 6 months. Second, when we asked employers about the reason for job termination in our exploratory firm survey, two of the three job terminations were due to workers voluntarily quitting the job. Nevertheless, the employment dynamics observed in Figure 2 suggest that about 67% of individuals that were offered a SEP job managed to secure the job until the last month in our data, two years after the program started.[11]

The employment rates of the other two groups are much lower than those of individuals who

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[11]Individuals that were offered a SEP job might not necessarily work in the same firm two years after the start of SEP. We claim that these individuals secured their original employment because we do not observe discontinuities in the employment spells of such workers, which would occur if they moved to a job with a different firm.

were offered the job. By the end of our period of analysis, they are equal to about 25% and essentially undistinguishable between each other. However, interestingly individuals who were offered a job interview but who were not selected for the post seem to find another job faster than individuals who were not offered an interview. This is surprising since, while the random assignment should ensure that individuals who were and those who were not offered the interview have similar characteristics (as seen in Table 1), as job offers are prone to selection we would expect treated individuals who were not offered the job to be, on average, of lower quality than individuals in the control group. However, one possible explanation for this fact is that the interview itself motivated this group of job seekers to pursue other employment possibilities more aggressively than those in the control group, who were never even offered an interview.

In order to estimate the impact of being offered a SEP job on future employment outcomes, we now turn to a discussion of our IV estimates based on equation (2) which we report both in Panel B of Figure 3 and in the fourth and fifth columns of Table 2. By September of 2015, we estimate that a SEP job lead to an increase in employment rates of 50 percentage points, and up to April of 2016 this increase is of about 100 percentage points. This estimate is very large and possibly upwardly biased, because the interview may have had a direct impact on employment even for those who did not get a job. However, large and statistically significant impacts are found even after the first 8 months of the program, with employment rates which are still about 58 percentage points higher by February 2017. After this period, we cannot reject that the impact of SEP is statistically different from zero (with the notable exception of the last two months in our period of analysis) but estimates remain large at about 50 percentage points.

5.2 Heterogeneity

Both our ITT and TOT estimates suggest that the SEP program successfully improve the employment status of those job seekers that received the intervention, and that the effect persists even after the end of the program. In order to better understand the reason why the impact of SEP lasted way beyond the end of the program, we conduct an exploratory analysis trying to identify which types of participants benefited the most from the program. Although with our sample we are probably underpowered to detect small heterogenous effects on employment rates, we believe that a simple comparison of the employment dynamics of job seekers with different characteristics is informative to understand the channels through which the program might operate.[12]

We start by comparing the employment rates among the treatment and the control group for male and female job seekers (panel A of Figure 4). Two main patterns emerge. First, by looking at the employment rates in the control group, it seems that male workers would be more likely to be employed than female workers in the absence of the program. Second, while the initial increase in the employment of those who were offered the interview was about 40 percentage points for both males and females, the pattern of employment of females slightly increases thereafter and

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[12]In the Appendix, we present further results on heterogeneity. Tables B6, B7, B8 and B9 report ITT and TOT estimates of the impact of SEP on employment for the subgroups represented in the four panels of Figure 4.

remained substantially larger than that of female who were not offered the job interview. On the other hand, the employment rates for males in the treatment and in the control group are essentially the same two years after the start of SEP.

In panel B of Figure 4 we split the sample among individuals below the average age of 45 (labelled "Young") and those above 45 years old (labelled "Old"). It is apparent that the effects of the program are mainly concentrated on the relatively younger individuals who, after two years since the start of the program, have employment rates that are about 35 percentage points larger than those of individuals of the same age who did not participate in the interview. Such effects are statistically different from zero throughout the entire period of analysis (see Table B7 in the Appendix).

Similar patterns emerge if we divide the sample into those individuals with and without previous work experience (panel C of Figure 4); or individuals with lower or higher qualifications (panel D of Figure 4).[13] The impact of SEP operates mainly by increasing the employment rates of less skilled and less experienced workers. The most notable effect is among individuals with no previous experience, whose employment rates two years after the start of the program are about 40 percentage points larger than workers with similar characteristics in the control group. As for more qualified or more experienced types of workers, SEP had a more limited initial effect and, in the long run, employment rates converged to those of qualified or experienced workers that did not received the intervention.

Overall, we conclude from this analysis that the main beneficiaries of SEP are groups of workers with lower average participation rates in the Macedonian labor market, such as women, the young, unexperienced and unskilled workers. In the absence of the intervention, the employment rates of such workers would be rather low, being about 20 to 25%, as can be seen by looking at the employment dynamics of these groups in the control group (Figure 4). It is thus possible that SEP, which was targeted towards marginalized individuals in Macedonia, created job opportunities that these workers would not have otherwise sought, and that treated individuals were able to secure such jobs by accumulating work experience and skills for a rather sustained period of time. To shed more light on potential mechanisms, in the next section we show estimation results on other outcomes from our survey of program participants.

5.3 Other Outcomes

In Table 3, we present additional results about the impact of SEP on other outcomes based on data from the first and second waves of the beneficiary and household level surveys. We interpret the estimates on the first wave of data as the short-term effects of the program (top panel of Table 3), and those on the second wave as the long-term effects (bottom panel of Table 3). As before, we

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[13]We define a worker to be unexperienced if, prior to the start of the program, there are no employment spells associated with the worker in the administrative data. Workers in the "low skill" group are those who, at the time of the application to the program, listed more basic occupations among those they would be qualified to do (such as driver or janitor); instead, workers in the "high skill" group listed more advanced occupations, such as construction engineer, technician or administrative clerk.

show both the ITT and TOT estimates corresponding to the estimation of equation (1) and (2), respectively.

We start by presenting estimation results on several types of workers' skills. As the survey contains many measures capturing individual abilities, we follow Anderson (2008) and group such measures into three indices: a cognitive skill index, a noncognitive skill index, and a work-related skill index. The cognitive skill index is based on the Abbreviated Raven's test of progressive matrices. The noncognitive skill index combines the Big-5 personality trait test and the 12-item grit scale. The work-related skill index is constructed from self-reported indicators for the individual reading, writing, using math, using a pc in the last twelve months and an indicator for speaking english. Indices are standardized to be mean zero in the control group.

The ITT estimates show that, in the short run, the treatment group has, on average, larger scores on the three skill indices, with differences with respect to the control group that are statistically different from zero. Similarly, the TOT estimates suggest that being offered a SEP job leads to improvements in worker's skills. The positive and significant effects on work-related skills is not surprising, since employment by itself presumably affects the frequency of reading, writing and counting activities. The impact of SEP on cognitive and noncognitive skills are more puzzling. It is possible that the program, by rising employment levels of the treated individuals and the frequency with which they perform more intellectual activities, also boosted their ability to perform well in cognitive tests, at least in the short run.[14] As for the noncognitive skills, the program might improve some personality traits, such as extraversion or agreeableness, through higher employment and increased interactions with co-workers or customers. In the long run, with the employment rates of the control group progressively increasing, the effects for cognitive and work-related skills decrease and are no longer statistically different from zero, but we still observe better noncognitive outcomes for treated individuals as compared to those of individuals who did not receive the intervention.

The second set of outcomes on which we focus are related to household income. As the quality of expenditure data is not ideal, we report results on household ownership of durables, which we use as a proxy for income, and on receipt of social financial assistance (SFA). As before, we reduce the dimensionality of the durable measures by creating an index from a list of 25 durable items, as described in Anderson (2008). We do not observe that the program lead to statistically significant increases in household economic conditions neither in the short nor in the long run, despite the fact that the program should have increased labor earnings through increased employment. However, we also observe significant reductions in both the probability of receiving SFA and in the amount of the subsidy for the treatment group, especially in the short run. This is expected since such benefits are given to individuals in long term unemployment, and are automatically removed when recipients find employment in a formal job. The null effect on income might thus be explained by the fact that, on average, SEP substituted social benefits with increased labor income.

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[14]Given the lack of baseline data on cognitive and noncognitive skills, we can not exclude that such effects are due to pre-existing differences in such skills between the treatment and the control group.

Finally, we show results on the mental status of program participants. Several studies have shown that long unemployment spells are associated with increased unhappiness and worse mental conditions (Clark and Oswald (1994) and Winkelmann and Winkelmann (1998)). Thus, we are interested in understanding if SEP had an effect on participant's mental health, which we measured through the Centre for Epidemiological Studies depression scale. We standardize this score to be mean zero in the control group, and construct an additional indicator for clinical depression based on criteria from the psychological literature (Radloff (1977)). Table 3 show that, in the short run, treated and untreated individuals do not present statistically significant differences in depression outcomes as a result of the SEP program. However, in the longer run individuals in the treatment group are more likely to be depressed and have larger depression scores than individuals in the control group. It is possible that this effect is due to individuals that participated in the job interview but were not selected for the post which, together with a prolonged period of unemployment, might have worsen their well-being.

6   Conclusion

This paper studies the impact of a subsidized employment program in Macedonia targeted to marginalized individuals in long term unemployment. Participating employers are given a wage subsidy that roughly reduced by half the yearly cost of a newly hired worker's wage and compensated the firm for the training costs. We evaluate the effectiveness of the program in increasing employment of participating job seekers by randomly varying the access to a job interview with the potential employer.

We find that, in the short run, the SEP program increased the employment rates of individuals who were offered the job interview by more than 30 percentage points. The effect persisted even after the end of the subsidized period and of the employer's contractual obligations, at about 25 percentage points two years after the start of the program. Using the random assignment to the interview as an instrument for being offered a SEP job, we estimate even larger employment impacts, of about 45 percentage points over the counterfactual employment rates of the control group, for those job seekers who were offered the subsidized employment. Such large impacts are mainly concentrated on job seekers with lower counterfactual participation rates in the labor market, such as women, young, unexperienced and unskilled individuals.

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Figure 1: The SEP design

	Employer's benefits per newly-hired worker	Employer's obligations
Modality 1	6-month gross wage subsidy with two levels depending on qualifications (14900 / 17000 MKD per month) Subsidy for training of employee	Hire the worker for additional 6 months under the same conditions
Modality 2	6-month net wage subsidy with two levels depending on qualifications (10500 / 11900 MKD per month) 5-year exemption from social security contributions	No obligation beyond the 6-month period
Modality 3	6-month net wage subsidy with two levels depending on qualifications (10500 / 11900 MKD per month per employee) 3-year exemption from social security contributions and personal income tax	Hire the worker for additional 12 months under the same conditions for the first 6 months

Notes. The figure summarizes the three modalities introduced by the SEP program. Different modalities present different benefits and obligations for the employer.

Figure 2: Employment rate dynamics by treatment group

A. Treatment vs control
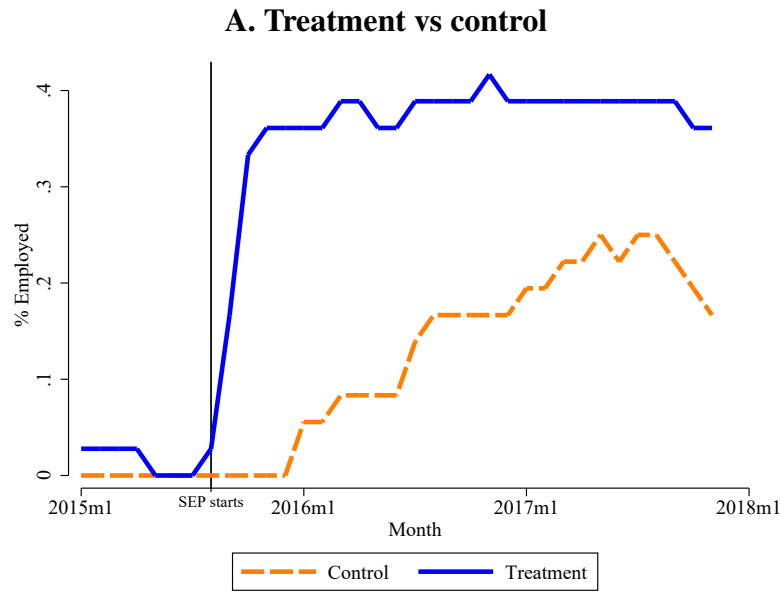
B. Interviewee offered SEP job, interviewee not offered SEP job, control
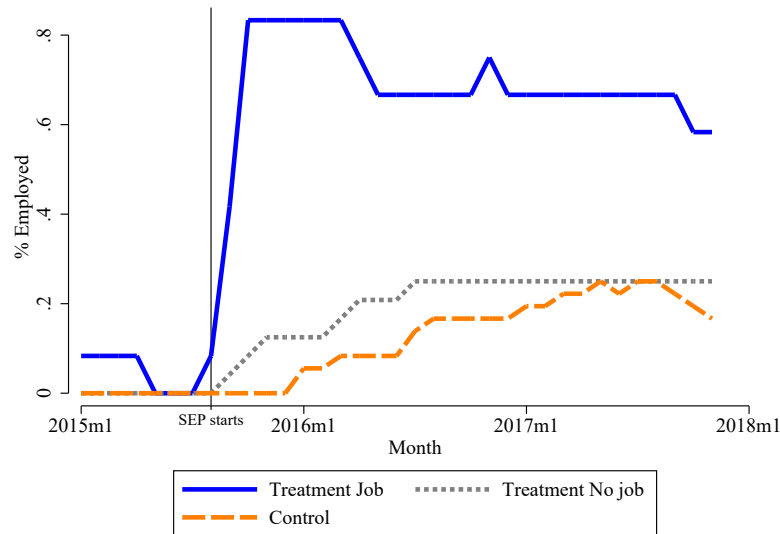
Notes: (1) Panel A shows the employment dynamics of individuals in the treatment (solid line) and control (dashed line) group. (2) Panel B shows the employment dynamics of individuals in the treatment group who were offered a SEP job (solid line), those in the treatment group who were not offered a SEP job (dotted line) and those in the control group (dashed line). (3) The vertical line in both panel A and panel B indicates the month SEP started (September 2015). (4) Employment rates at the monthly frequency are computed by converting employment spells from National Employment Agency's administrative data. (5) The estimation sample includes 72 individuals.

Figure 3: Estimated impact of SEP on employment

A. ITT estimates
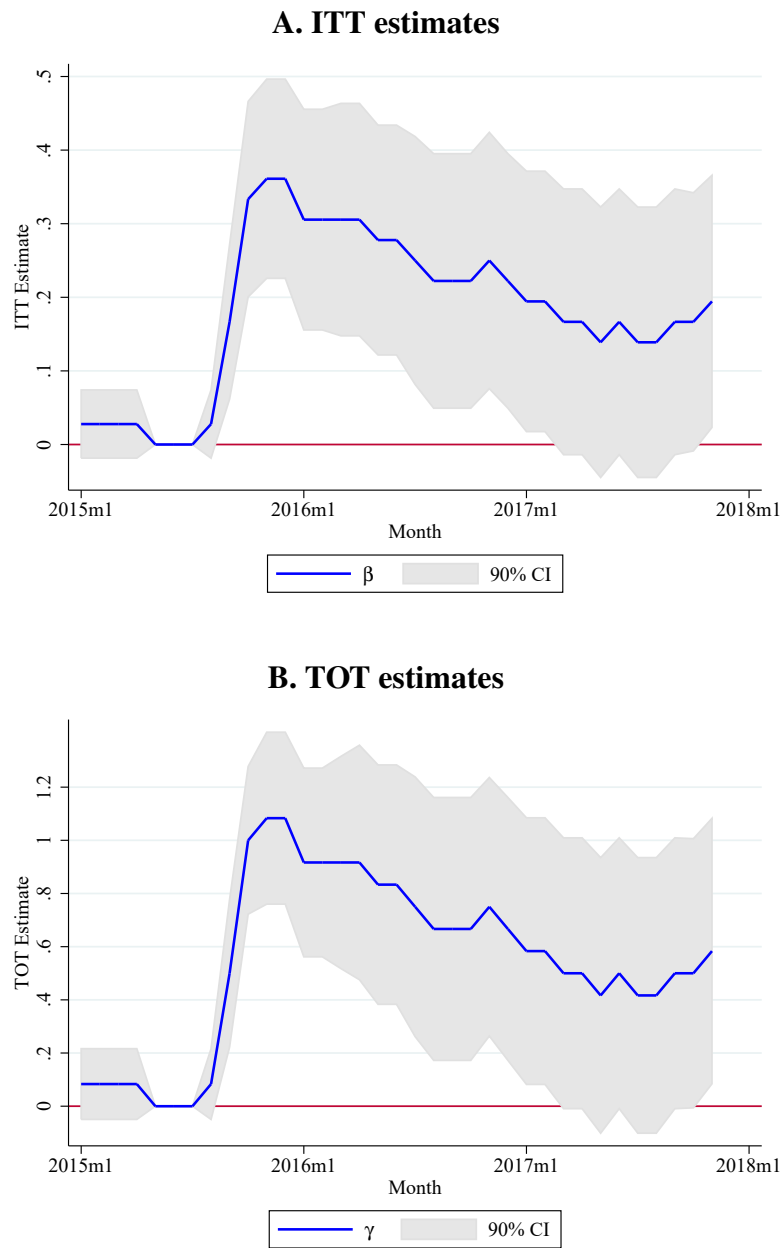
B. TOT estimates

Notes: (1) The solid line in panel A shows estimates of β from estimation of equation (1). (2) The solid line in panel B shows estimates of γ from estimation of equation (2). (3) In both figures, bands around the solid lines are 90 % confidence intervals. (4) The estimation sample includes 72 individuals.

Figure 4: Heterogeneous effects of SEP on employment, by treatment group
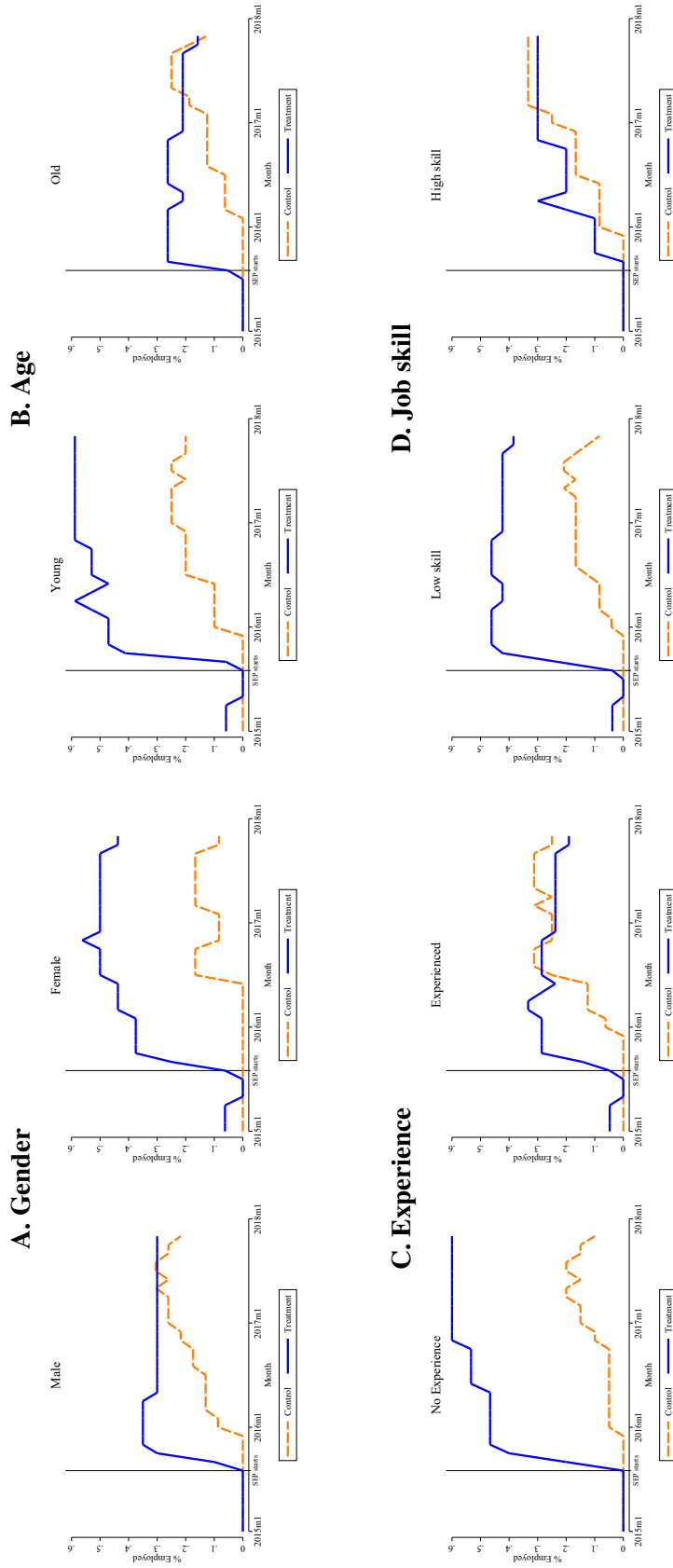
Notes: (1) Employment dynamics of individuals in the treatment (control) group are represented with the solid (dashed) line. Vertical lines indicate the month SEP started (September 2015). (2) In Panel A the sample is split among male (N=43) and female (N=28). (3) In Panel B the sample is split among individuals 45 years old or younger (labelled "Young", N=37) and individuals older than 45 (labelled "Old", N=35). (4) In panel C the sample is split among individuals without previous work experience (N=35) and with previous experience (N=37). Previous work experience is an indicator equal to 1 if the individual had at least one employment spell before September 2015 within the administrative data. (5) In panel D the sample is split among individuals with low qualifications (N=50) and high qualifications (N=22), as determined by the reported type of occupation they would be qualified to cover at the the time of the application.

Table 1: Baseline characteristics by treatment group

	Control (C) (1)	Treatment (T) (2)	Difference (T-C) (3)	N (4)
Age	44.58 (11.72)	45.06 (11.47)	0.47 (2.25)	106
Male	0.67 (0.47)	0.55 (0.50)	-0.13 (0.10)	105
Macedonian	0.54 (0.50)	0.58 (0.50)	0.05 (0.10)	107
Albanian	0.31 (0.47)	0.28 (0.45)	-0.03 (0.09)	107
Other	0.15 (0.36)	0.13 (0.34)	-0.05 (0.06)	107
Primary Education	0.34 (0.48)	0.29 (0.46)	-0.05 (0.09)	102
Secondary Education or Above	0.66 (0.48)	0.71 (0.46)	0.05 (0.09)	102
Has Worked in the Past	0.47 (0.51)	0.58 (0.50)	0.11 (0.12)	72
Years of Past Work Experience	3.61 (1.21)	5.41 (1.40)	1.80 (1.85)	72

Notes: * p<0.10, ** p<0.05, *** p<0.01. Numbers in parentheses are robust standard errors for the differences in column (3) and standard deviations elsewhere. Column (4) reports the number of observations.

Table 2: Impact of SEP on employment, ITT and TOT estimates

Dependent variable	ITT			TOT	
	Coeff.	Std. error	Perm. test (p-value)	Coeff.	Std. error
	(1)	(2)	(3)	(4)	(5)
Employed in 2015/7	0.000	(0.000)	1.000	-	-
Employed in 2015/8	0.028	(0.028)	1.000	0.083	(0.080)
Employed in 2015/9	0.167**	(0.063)	0.021	0.500***	(0.167)
Employed in 2015/10	0.333***	(0.080)	0.000	1.000***	(0.167)
Employed in 2015/11	0.361***	(0.081)	0.000	1.083***	(0.194)
Employed in 2015/12	0.361***	(0.081)	0.001	1.083***	(0.194)
Employed in 2016/1	0.306***	(0.090)	0.005	0.917***	(0.213)
Employed in 2016/2	0.306***	(0.090)	0.001	0.917***	(0.213)
Employed in 2016/3	0.306**	(0.095)	0.001	0.917***	(0.239)
Employed in 2016/4	0.306**	(0.095)	0.003	0.917***	(0.265)
Employed in 2016/5	0.278**	(0.094)	0.009	0.833**	(0.270)
Employed in 2016/6	0.278**	(0.094)	0.012	0.833**	(0.270)
Employed in 2016/7	0.250**	(0.101)	0.028	0.750**	(0.293)
Employed in 2016/8	0.222**	(0.104)	0.066	0.667**	(0.297)
Employed in 2016/9	0.222**	(0.104)	0.056	0.667**	(0.297)
Employed in 2016/10	0.222**	(0.104)	0.053	0.667**	(0.297)
Employed in 2016/11	0.250**	(0.104)	0.037	0.750**	(0.292)
Employed in 2016/12	0.222**	(0.104)	0.052	0.667**	(0.297)
Employed in 2017/1	0.194*	(0.106)	0.119	0.583*	(0.301)
Employed in 2017/2	0.194*	(0.106)	0.113	0.583*	(0.301)
Employed in 2017/3	0.167	(0.108)	0.193	0.500	(0.306)
Employed in 2017/4	0.167	(0.108)	0.209	0.500	(0.306)
Employed in 2017/5	0.139	(0.110)	0.284	0.417	(0.311)
Employed in 2017/6	0.167	(0.108)	0.206	0.500	(0.306)
Employed in 2017/7	0.139	(0.110)	0.302	0.417	(0.311)
Employed in 2017/8	0.139	(0.110)	0.315	0.417	(0.311)
Employed in 2017/9	0.167	(0.108)	0.206	0.500	(0.306)
Employed in 2017/10	0.167	(0.105)	0.185	0.500*	(0.304)
Employed in 2017/11	0.194*	(0.103)	0.103	0.583*	(0.299)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Column (3) presents the p-value of a permutation test (see section 4). Columns (4)-(5) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history for 72 individuals.

Table 3: Short and long term impacts of SEP on other outcomes

	Control (C) (1)	Treatment (T) (2)	ITT (T-C) (3)	TOT (4)	N (5)
Wave 1: short-term impact					
Skills					
Cognitive Skill Index	0.00	0.45	0.45*	1.60*	95
	(1.00)	(1.31)	(0.24)	(0.89)	
Noncognitive Skill Index	0.00	0.23	0.23**	0.85*	106
	(0.59)	(0.50)	(0.11)	(0.42)	
Work Skill Index	0.00	0.25	0.25**	0.93*	107
	(0.69)	(0.57)	(0.12)	(0.50)	
Income					
Durables Index	0.00	0.05	0.05	0.20	104
	(0.35)	(0.36)	(0.07)	(0.27)	
SFA Benefit (Denars)	2823.6	2121.51	-702.09*	-2657.91**	103
	(1492.16)	(2169.26)	(368.99)	(1277.58)	
Received SFA last month	0.92	0.68	-0.24***	-0.91***	103
	(0.27)	(0.47)	(0.08)	(0.25)	
Depression					
Depression Score	0.00	-0.23	-0.23	-0.84	105
	(1.00)	(0.94)	(0.19)	(0.70)	
Depressed	0.22	0.23	0.00	0.02	107
	(0.42)	(0.42)	(0.08)	(0.31)	
Wave 2: long-term impact					
Skills					
Cognitive Skill Index	0.00	0.04	0.04	0.13	87
	(1.00)	(0.99)	(0.21)	(0.75)	
Noncognitive Skill Index	0.00	0.28	0.28**	1.04*	93
	(0.61)	(0.62)	(0.13)	(0.50)	
Work Skill Index	0.00	0.18	0.18	0.67	93
	(0.61)	(0.48)	(0.11)	(0.44)	
Income					
Durables Index	0.00	-0.04	-0.04	-0.15	90
	(0.25)	(0.20)	(0.05)	(0.20)	
SFA Benefit (Denars)	2009.05	1460.23	-548.82	-2195.28	86
	(1804.39)	(1774.58)	(386.23)	(1277.58)	
Received SFA last month	0.69	0.52	-0.17	-0.67*	86
	(0.47)	(0.51)	(0.11)	(0.37)	
Depression					
Depression Score	0.00	0.64	0.64*	2.41	93
	(1.00)	(2.40)	(0.38)	(1.63)	
Depressed	0.00	0.13	0.13***	0.50***	93
	(0.00)	(0.34)	(0.05)	(0.23)	

Notes: * p<0.10, ** p<0.05, *** p<0.01. (1) The top (bottom) panel of the table shows the short-term impacts of SEP based on the first (second) wave of the applicant and household level survey. (2) Column (3) and (4) report ITT and TOT estimates based on estimating equation 1 and 2, respectively. (3) Numbers in parenthesis are standard errors for the estimates in columns (3) and (4) and standard deviations elsewhere. (4) The cognitive skill index is based on the Abbreviated Raven?s test of progressive matrices and it is standardized to be mean zero and standard deviation 1 in the control group. (5) The noncognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale (6) The work skill index is based on self-reported indicators for the individual reading, writing, using math and using a pc in the last 12 months and an indicator for the individual speaking english. (7) The durables index is based on household self-reported ownership of durable goods among a list of 25 items. (8) The depression score is based on the Centre for Epidemiological Studies depression scale and it is standardized to be mean zero and standard deviation 1 in the control group. A higher score indicates a worse mental health. (9) An individual is defined to be clinically depressed if the rescaled depression score is larger than 16, as defined in Radloff (1977). (10) The noncognitive skill index, the work skill index and the durables index are computed following the methodology described in Anderson (2008).

A SEP beneficiaries and program modalities

A.1 Eligibility requirements for job seekers

Potential beneficiaries of the SEP program include the following categories of individuals: Social Financial Assistance (SFA) beneficiaries[2]; Permanent Financial Assistance (PFA) beneficiaries in the age range 18-26[3]; victims of domestic violence that live in a shelter or whose family monthly income per capita in the year prior to application was lower than 50% of the Macedonian average net salary (as published by the State Statistical Office); members of families that are beneficiaries of PFA or Child Allowance (CA)[4], and at the same time fulfill one of the following criteria: parents of children with disabilities who are beneficiaries of special allowance for at least three months before the SEP application; disabled person according to the Law for employment of disabled persons; persons with reduced work capacity according to the regulation of the Pension and Disability Insurance; unemployed parent of three or more children that has been unemployed for at least three months before the SEP application; a member of a one-parent family that has been unemployed for at least three months before the SEP application.

A.2 Modalities

The SEP program includes three different modalities:

- *Modality 1*. This modality provides a gross wage subsidy for the duration of 6 months. The employer is obliged to keep all program participants for an additional 6-month period after the end of the subsidized employment period. Under this modality, there are two types of subsidy level, one for the beneficiaries without qualifications (14,900 MKD per employee) and one for beneficiaries with higher educational degree or employed for performing more complex tasks (17,000 MKD). Companies shall receive an additional 5000 MKD per month per employee for the first 6 months to compensate the training and material costs.

- *Modality 2*. This modality provides a net wage subsidy for the duration of 6 months and an exemption from the payment of compulsory social insurance contributions for 60 months. Under this modality, there are two types of subsidy level, one for the beneficiaries without qualifications (10,500 MKD per employee) and one for beneficiaries with higher educational degree or employed for performing more complex tasks (11,900 MKD). The payment of

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[2]SFA is a means-tested monetary transfer to people who are fit for work, but who cannot support themselves. It is a minimum guaranteed income in which the benefit is equal to the difference between household income and the social assistance amount determined for the household, which depends on household size and time spent in SFA.

[3]PFA recipients are individuals that up to the age of 18 had the status of children without parent and parental care.

[4]CA is a social protection benefit provided to children enrolled in a regular education program and whose family income is below a pre-fixed amount.

personal income tax is an employers obligation. Personal income tax is equal to 10% of the gross salary minus the social security contributions[5].

- *Modality 3*. This modality provides a net wage subsidy for the duration of 6 months and an exemption from the payment of social insurance contributions and personal income tax for 36 months. The employer is obliged to keep all program participants for an additional 12-month period after the end of the period in which the employer is exempt from payment of contributions and personal income tax. Subsidy levels are in line with Modality 2. In addition to the general requirements for beneficiares (see appendix A.1), this modality is restricted to unemployed individuals below the age of 35 who have been unemployed in the three months previous to the SEP application.

A.3   Comparison of benefits

Each modality provides benefits that are different in terms of components of amount and duration. To compare costs and benefits associated with the different modalities, we indicate w as the net salary, t as the employee's personal income tax paid by the employer, s as the social security contribution, and k as the training cost. The net salary for an employee is determined as the difference between the gross salary and the sum of social security contributions and the personal income tax. Therefore, for low-skilled workers, the net wage equals 10,500 MKD, personal income tax equals 1,050 MKD and social contributions amount to 3,350 MKD. For high-skilled workers, the net wage equals 11,900 MKD, personal income tax equals 1,190 MKD and social contributions amount to 3,910 MKD.

Table A1: Labour cost and SEP benefits

	Monthly costs and benefits since employment			5-year totals ('000 MKD)	
	1-6 months	7-36 months	37-60 months	Low-skilled (%)	High-skilled (%)
Labour Cost	w + t + s + k	w + t + s	w + t + s	924.0 (100.0)	1050.0 (100.0)
Modality 1					
SEP benefit	w + t + s + k	0	0	119.4 (12.9)	132.0 (12.6)
Net cost for employer	0	w + t + s	w + t + s	804.6 (87.1)	918.0 (87.4)
Modality 2					
SEP benefit	w + s	s	s	264.0 (28.6)	306.0 (29.1)
Net cost for employer	t + k	w + t	w + t	660.0 (71.4)	744.0 (70.9)
Modality 3					
SEP benefit	w + t + s	t + s	0	221.4 (24.0)	255.0 (24.3)
Net cost for employer	k	w	w + t + s	702.6 (76.0)	795.0 (75.7)

Note. The table reports costs and SEP benefits assuming the employer hires a worker for 5 years, without any change in salary and with a training program during the first 6 months of the work contract (the monthly cost is assumed at 5,000 MKD). w is the net salary, t is income tax paid by the employer, s is the social security contribution, and k is the training cost.

Table A1 presents costs and benefits for the employer associated with the hiring of an employee for 5 years, without any change in salary and with a monthly training program during the first 6 months of the employment period. We assume that the training program has a monthly cost in line with the benefit provided by modality 1, i.e. 5,000 MKD. We compute total costs and benefits

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[5]Source: http://www.ujp.gov.mk/en/vodic/category/708

during the 5-year period. We observe that the lowest net cost for the employer is achieved in the modality 2, with a net cost of hiring a low-skilled (high-skilled) worker for 5 years of 660,000 MKD (744,000 MKD). Since benefits are distributed with different timelines, we compute under the same scenario, the net present value at the beginning of employment of the net cost for the employer under different discount rates. Figure A1 presents the results. We observe that modality 2 is preferred for very low discount rates. With more present-biased employers, first modality 3 and then modality 1 are preferred.

Figure A1: Present value of the net cost for the employee across different modalities
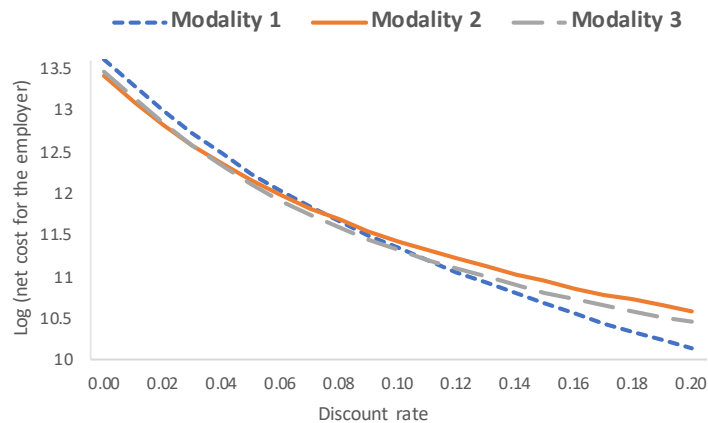

Note. The figure shows the present value of the net cost for the employer assuming the employer hires a worker for 5 years, without any change in salary and with a training program during the first 6 months of the work contract (the monthly cost is assumed at 5,000 MKD). Discount rates ranges from 0 to 0.2.

B Additional analysis

B.1 Attrition

As we discussed in the paper (Section 3.1), employment records were not available for about a third of the sample of SEP participants. In this section we present some additional results on attrition. Table B2 shows attrition rates for the treatment and control groups, as well as baseline balance tests for the subsample of job seekers with valid employment records from the administrative data. The first row of the table suggests that the difference in the attrition rates of the treatment and control groups was not statistically different from zero. In fact, for both groups employment data are missing for about a third of the sample. The rest of the table replicates Table 1 from the paper for the 72 individuals with non-missing employment data. As can be seen, this sample is essentially balanced across treatment groups in terms of basic demographic characteristics, education, ethnicity and previous work experience.

Table B2: Attrition rates and baseline characteristics of non-attrited sample, by treatment group

	Control (C)	Treatment (T)	Difference (T-C)	N
	(1)	(2)	(3)	(4)
Attrition Rate	0.33	0.32	-0.01	107
	(0.48)	(0.47)	(0.09)	
Age	42.28	45.17	2.89	72
	(12.33)	(11.20)	(2.78)	
Male	0.66	0.56	-0.10	71
	(0.48)	(0.50)	(0.12)	
Macedonian	0.58	0.64	0.06	72
	(0.50)	(0.49)	(0.12)	
Albanian	0.22	0.19	-0.03	72
	(0.42)	(0.40)	(0.10)	
Other	0.19	0.17	-0.03	72
	(0.40)	(0.38)	(0.09)	
Primary Education	0.27	0.26	-0.02	68
	(0.45)	(0.44)	(0.11)	
Secondary Education or Above	0.73	0.74	0.02	68
	(0.45)	(0.44)	(0.11)	
Has Worked in the Past	0.47	0.58	0.11	72
	(0.51)	(0.50)	(0.12)	
Years of Past Work Experience	3.61	5.41	1.80	72
	(1.21)	(1.40)	(1.85)	

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Numbers in parenthesis are standard errors for the differences in column (3) and standard deviation elsewhere. Column (4) reports the number of observations. The sample in row 2 to 10 is restricted to individuals with non-missing employment records from the administrative data.

In Table B3 we test whether attrition lead to changes in the sample composition by comparing the mean of several demographic characteristics for attrited and non-attrited individuals. While the two groups are balanced in terms of gender, age and education, individuals who are ethnic Albanians are more likely to be attrited and individuals who are neither Albanians not Macedonians are less likely to be attrited. The remaining differences are not statistically different from zero.

Table B3: Differences between attrited and non-attrited

	Non-attrited (N)	Attrited (A)	Difference (A-N)	N
	(1)	(2)	(3)	(4)
Age	43.72	47.15	3.42	106
	(11.79)	(11.54)	(2.39)	
Male	0.61	0.62	0.01	105
	(0.49)	(0.49)	(0.10)	
Macedonian	0.61	0.46	-0.15	107
	(0.50)	(0.49)	(0.10)	
Albanian	0.21	0.49	0.28***	107
	(0.41)	(0.51)	(0.09)	
Other	0.18	0.06	-0.12*	107
	(0.39)	(0.24)	(0.07)	
Primary Education	0.26	0.41	0.15	102
	(0.44)	(0.5)	(0.1)	
Secondary Education or Above	0.74	0.59	-0.14	102
	(0.44)	(0.50)	(0.10)	

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Non-attrited (column 1) are SEP participants with non-missing administrative employment data. Attrited (column 2) are SEP participants with missing employment data. The total number of attrited individuals is equal to 35. Numbers in parenthesis are standard errors for the differences in column (3) and standard deviation elsewhere. Column (4) reports the combined number of observations.

B.2 Robustness checks for SEP impacts on employment

In this section we present additional analysis testing the robustness of our main results to alternative specifications and alternative sample definitions. Table B4 replicates the results of Table 2 by further adding the following control variables to the estimating models in equations (1) and (2): the age and gender of the SEP participant, as well as indicators for the ethnicity and the level of education. As can be seen, both the ITT and the TOT estimates are extremely similar to those reported in the paper and, if anything, they are slightly larger in magnitude and slightly more precise. Overall, the estimates in Table B4 suggest that our results are robust to the inclusion of demographic controls.

Next, we check whether our results are robust to the exclusion of the 19 individuals for which information on the firm to which they were matched could not be retrieved. Table B5 shows the estimated impact of SEP for the sample of program participants with a valid firm identifier. Once again both the ITT and TOT estimates are very similar to those reported in Table 2 in the paper. The ITT estimates are slightly larger although less precise; the TOT estimates are slightly larger

in some months and slightly smaller in other months but they are very similar to those shown in the paper both in terms of magnitude and statistical significance.

Table B4: Impact of SEP on employment, robustness check including individual demographic characteristics

Dependent variable	ITT		TOT	
	Coeff. (1)	Std.err. (2)	Coeff. (3)	Std.err. (4)
Employed in 2015/7	0.000	(0.000)	-	-
Employed in 2015/8	0.022	(0.023)	0.063	(0.061)
Employed in 2015/9	0.152*	(0.059)	0.435**	(0.155)
Employed in 2015/10	0.346***	(0.085)	0.989***	(0.161)
Employed in 2015/11	0.385***	(0.085)	1.102***	(0.205)
Employed in 2015/12	0.385***	(0.085)	1.102***	(0.205)
Employed in 2016/1	0.335***	(0.093)	0.957***	(0.211)
Employed in 2016/2	0.335***	(0.093)	0.957***	(0.211)
Employed in 2016/3	0.335***	(0.096)	0.959***	(0.225)
Employed in 2016/4	0.340***	(0.091)	0.974***	(0.260)
Employed in 2016/5	0.305**	(0.092)	0.874**	(0.272)
Employed in 2016/6	0.299**	(0.095)	0.857**	(0.275)
Employed in 2016/7	0.259**	(0.101)	0.741**	(0.291)
Employed in 2016/8	0.230**	(0.104)	0.658**	(0.291)
Employed in 2016/9	0.230**	(0.104)	0.658**	(0.291)
Employed in 2016/10	0.230**	(0.104)	0.658**	(0.291)
Employed in 2016/11	0.273**	(0.101)	0.679**	(0.250)
Employed in 2016/12	0.230*	(0.100)	0.623**	(0.249)
Employed in 2017/1	0.205*	(0.100)	0.554*	(0.249)
Employed in 2017/2	0.205*	(0.100)	0.554*	(0.249)
Employed in 2017/3	0.188*	(0.105)	0.537*	(0.283)
Employed in 2017/4	0.187*	(0.109)	0.545*	(0.294)
Employed in 2017/5	0.158	(0.110)	0.453	(0.294)
Employed in 2017/6	0.181	(0.110)	0.519*	(0.296)
Employed in 2017/7	0.158	(0.110)	0.453	(0.294)
Employed in 2017/8	0.158	(0.110)	0.453	(0.294)
Employed in 2017/9	0.183*	(0.108)	0.524*	(0.293)
Employed in 2017/10	0.202*	(0.102)	0.579*	(0.283)
Employed in 2017/11	0.230**	(0.101)	0.658**	(0.284)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Control variables included in the estimation are age, gender, an indicator for the individual having completed primary school and indicators for the individual being ethnic Macedonians or ethnic Albanian. Estimates based on administrative data of the entire employment history for 67 individuals.

Table B5: Impact of SEP on employment, robustness check excluding individuals without firm identifier

Dependent variable	ITT		TOT	
	Coeff.	Std.err.	Coeff.	Std.err.
	(1)	(2)	(3)	(4)
Employed in 2015/7	0.000	(0.000)	-	-
Employed in 2015/8	0.038	(0.038)	0.083	(0.080)
Employed in 2015/9	0.192**	(0.079)	0.417**	(0.142)
Employed in 2015/10	0.423***	(0.099)	0.917***	(0.138)
Employed in 2015/11	0.462***	(0.100)	1.000***	(0.167)
Employed in 2015/12	0.462***	(0.100)	1.000***	(0.167)
Employed in 2016/1	0.387***	(0.112)	0.840***	(0.191)
Employed in 2016/2	0.387***	(0.112)	0.840***	(0.191)
Employed in 2016/3	0.389***	(0.117)	0.843***	(0.218)
Employed in 2016/4	0.389***	(0.117)	0.843***	(0.218)
Employed in 2016/5	0.350***	(0.117)	0.759***	(0.249)
Employed in 2016/6	0.350***	(0.117)	0.759***	(0.249)
Employed in 2016/7	0.315**	(0.126)	0.682**	(0.271)
Employed in 2016/8	0.278**	(0.129)	0.602**	(0.273)
Employed in 2016/9	0.278**	(0.129)	0.602**	(0.273)
Employed in 2016/10	0.278**	(0.129)	0.602**	(0.273)
Employed in 2016/11	0.316**	(0.129)	0.685**	(0.268)
Employed in 2016/12	0.278**	(0.129)	0.602**	(0.273)
Employed in 2017/1	0.241*	(0.132)	0.522*	(0.276)
Employed in 2017/2	0.241*	(0.132)	0.522*	(0.276)
Employed in 2017/3	0.204	(0.134)	0.441	(0.279)
Employed in 2017/4	0.204	(0.134)	0.441	(0.279)
Employed in 2017/5	0.167	(0.136)	0.361	(0.282)
Employed in 2017/6	0.204	(0.134)	0.441	(0.279)
Employed in 2017/7	0.167	(0.136)	0.361	(0.282)
Employed in 2017/8	0.167	(0.136)	0.361	(0.282)
Employed in 2017/9	0.204	(0.134)	0.441	(0.279)
Employed in 2017/10	0.202	(0.132)	0.438	(0.279)
Employed in 2017/11	0.239*	(0.129)	0.519*	(0.277)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history for 53 individuals with a valid firm identifier in the matching process.

B.3 Additional results for the heterogeneity analysis

In this section, we complement the results presented in section 5.2 and present ITT and TOT estimates after splitting the sample of SEP participants according to the four criteria already discussed in Figure 4. In particular, Table B6 shows results for male and female program participants; Table B7 shows results for "young" (45 years old or younger) and "old" (older than 45) beneficiaries; Table B8 splits the sample into those with and without previous work experience (as constructed from employment spells prior to September 2015); and Table B9 shows results for unskilled versus skilled beneficiaries (as defined by the listed occupations they could be able to do at the time of the application to the program).

Table B6: Heterogeneous effects of SEP on employment, by gender

	Male				Female			
	ITT		TOT		ITT		TOT	
Dependent variable	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed in 2015/7	0.000	(0.000)	-	-	0.000	(0.000)	-	-
Employed in 2015/8	0.000	(0.000)	-	-	0.063	(0.063)	0.167	(0.152)
Employed in 2015/9	0.100	(0.069)	0.333	(0.236)	0.250**	(0.112)	0.667***	(0.192)
Employed in 2015/10	0.300***	(0.105)	1.000***	(0.236)	0.375***	(0.126)	1.000***	(0.272)
Employed in 2015/11	0.350***	(0.109)	1.167***	(0.312)	0.375***	(0.126)	1.000***	(0.272)
Employed in 2015/12	0.350***	(0.109)	1.167***	(0.312)	0.375***	(0.126)	1.000***	(0.272)
Employed in 2016/1	0.263**	(0.125)	0.877***	(0.336)	0.375***	(0.126)	1.000***	(0.272)
Employed in 2016/2	0.263**	(0.125)	0.877***	(0.336)	0.375***	(0.126)	1.000***	(0.272)
Employed in 2016/3	0.220*	(0.131)	0.732**	(0.355)	0.438***	(0.129)	1.167***	(0.312)
Employed in 2016/4	0.220*	(0.131)	0.732*	(0.408)	0.438***	(0.129)	1.167***	(0.312)
Employed in 2016/5	0.170	(0.127)	0.565	(0.414)	0.438***	(0.129)	1.167***	(0.312)
Employed in 2016/6	0.170	(0.127)	0.565	(0.414)	0.438***	(0.129)	1.167***	(0.312)
Employed in 2016/7	0.170	(0.127)	0.565	(0.414)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2016/8	0.126	(0.133)	0.420	(0.423)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2016/9	0.126	(0.133)	0.420	(0.423)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2016/10	0.126	(0.133)	0.420	(0.423)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2016/11	0.083	(0.137)	0.275	(0.435)	0.479**	(0.153)	1.278***	(0.372)
Employed in 2016/12	0.083	(0.137)	0.275	(0.435)	0.417**	(0.154)	1.111**	(0.402)
Employed in 2017/1	0.039	(0.141)	0.130	(0.451)	0.417**	(0.154)	1.111**	(0.402)
Employed in 2017/2	0.039	(0.141)	0.130	(0.451)	0.417**	(0.154)	1.111**	(0.402)
Employed in 2017/3	0.039	(0.141)	0.130	(0.451)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/4	0.039	(0.141)	0.130	(0.451)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/5	−0.004	(0.144)	−0.014	(0.469)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/6	0.039	(0.141)	0.130	(0.451)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/7	−0.004	(0.144)	−0.014	(0.469)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/8	−0.004	(0.144)	−0.014	(0.469)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/9	0.039	(0.141)	0.130	(0.451)	0.333*	(0.171)	0.889**	(0.421)
Employed in 2017/10	0.039	(0.141)	0.130	(0.451)	0.354**	(0.153)	0.944**	(0.419)
Employed in 2017/11	0.083	(0.137)	0.275	(0.435)	0.354**	(0.153)	0.944**	(0.419)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) and (5)-(6) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) and (7)-(8) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history. In columns (1)-(4), the sample is restricted to male workers (N = 43), and in columns (5)-(8), the sample is restricted to female workers (N = 28).

Table B7: Heterogeneous effects of SEP on employment, by age groups

	Young				Old			
	ITT		TOT		ITT		TOT	
Dependent variable	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed in 2015/7	0.000	(0.000)	-	-	0.000	(0.000)	-	-
Employed in 2015/8	0.000	(0.000)	-	-	0.053	(0.053)	0.200	(0.195)
Employed in 2015/9	0.059	(0.059)	0.143	(0.132)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2015/10	0.412***	(0.123)	1.000***	(0.202)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2015/11	0.471***	(0.124)	1.143***	(0.244)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2015/12	0.471***	(0.124)	1.143***	(0.244)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2016/1	0.371***	(0.142)	0.900***	(0.286)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2016/2	0.371***	(0.142)	0.900***	(0.286)	0.263**	(0.104)	1.000***	(0.283)
Employed in 2016/3	0.429***	(0.142)	1.043***	(0.327)	0.201*	(0.121)	0.762**	(0.350)
Employed in 2016/4	0.488***	(0.141)	1.186***	(0.371)	0.148	(0.115)	0.563	(0.372)
Employed in 2016/5	0.429***	(0.142)	1.043***	(0.386)	0.148	(0.115)	0.563	(0.372)
Employed in 2016/6	0.371***	(0.142)	0.900***	(0.344)	0.201*	(0.121)	0.762*	(0.428)
Employed in 2016/7	0.329**	(0.155)	0.800**	(0.381)	0.201*	(0.121)	0.762*	(0.428)
Employed in 2016/8	0.329**	(0.155)	0.800**	(0.381)	0.138	(0.134)	0.525	(0.465)
Employed in 2016/9	0.329**	(0.155)	0.800**	(0.381)	0.138	(0.134)	0.525	(0.465)
Employed in 2016/10	0.329**	(0.155)	0.800**	(0.381)	0.138	(0.134)	0.525	(0.465)
Employed in 2016/11	0.388**	(0.153)	0.943***	(0.365)	0.138	(0.134)	0.525	(0.465)
Employed in 2016/12	0.388**	(0.153)	0.943***	(0.365)	0.086	(0.129)	0.325	(0.465)
Employed in 2017/1	0.338**	(0.158)	0.821**	(0.365)	0.086	(0.129)	0.325	(0.465)
Employed in 2017/2	0.338**	(0.158)	0.821**	(0.365)	0.086	(0.129)	0.325	(0.465)
Employed in 2017/3	0.338**	(0.158)	0.821**	(0.365)	0.023	(0.139)	0.087	(0.508)
Employed in 2017/4	0.338**	(0.158)	0.821**	(0.365)	0.023	(0.139)	0.087	(0.508)
Employed in 2017/5	0.338**	(0.158)	0.821**	(0.365)	−0.039	(0.147)	−0.150	(0.557)
Employed in 2017/6	0.388**	(0.153)	0.943***	(0.365)	−0.039	(0.147)	−0.150	(0.557)
Employed in 2017/7	0.338**	(0.158)	0.821**	(0.365)	−0.039	(0.147)	−0.150	(0.557)
Employed in 2017/8	0.338**	(0.158)	0.821**	(0.365)	−0.039	(0.147)	−0.150	(0.557)
Employed in 2017/9	0.388**	(0.153)	0.943***	(0.365)	−0.039	(0.147)	−0.150	(0.557)
Employed in 2017/10	0.388**	(0.153)	0.943***	(0.365)	−0.030	(0.132)	−0.113	(0.492)
Employed in 2017/11	0.388**	(0.153)	0.943***	(0.365)	0.033	(0.121)	0.125	(0.447)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) and (5)-(6) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) and (7)-(8) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history. In columns (1)-(4), the sample is restricted to workers younger than 45 years old (N = 37), and in columns (5)-(8), the sample is restricted to workers older than 45 years old (N = 35).

Table B8: Heterogeneous effects of SEP on employment, by previous work experience

	No Experience				Experienced			
	ITT		TOT		ITT		TOT	
Dependent variable	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed in 2015/7	0.000	(0.000)	-	-	0.000	(0.000)	-	-
Employed in 2015/8	0.000	(0.000)	-	-	0.000	(0.000)	-	-
Employed in 2015/9	0.200*	(0.106)	0.429**	(0.187)	0.000	(0.000)	-	-
Employed in 2015/10	0.400**	(0.130)	0.857***	(0.136)	0.286**	(0.101)	1.200***	(0.390)
Employed in 2015/11	0.467***	(0.133)	1.000***	(0.208)	0.286**	(0.101)	1.200***	(0.390)
Employed in 2015/12	0.467***	(0.133)	1.000***	(0.208)	0.286**	(0.101)	1.200***	(0.390)
Employed in 2016/1	0.417***	(0.142)	0.893***	(0.220)	0.223*	(0.119)	0.937**	(0.421)
Employed in 2016/2	0.417***	(0.142)	0.893***	(0.291)	0.223*	(0.119)	0.937**	(0.421)
Employed in 2016/3	0.417***	(0.142)	0.893***	(0.291)	0.208	(0.136)	0.875*	(0.510)
Employed in 2016/4	0.417***	(0.142)	0.893***	(0.291)	0.208	(0.136)	0.875*	(0.510)
Employed in 2016/5	0.417***	(0.142)	0.893***	(0.291)	0.161	(0.132)	0.675	(0.526)
Employed in 2016/6	0.483***	(0.142)	1.036***	(0.340)	0.113	(0.128)	0.475	(0.488)
Employed in 2016/7	0.483***	(0.142)	1.036***	(0.340)	0.036	(0.151)	0.150	(0.602)
Employed in 2016/8	0.483***	(0.142)	1.036***	(0.340)	−0.027	(0.156)	−0.112	(0.651)
Employed in 2016/9	0.483***	(0.142)	1.036***	(0.340)	−0.027	(0.156)	−0.112	(0.651)
Employed in 2016/10	0.483***	(0.142)	1.036***	(0.340)	−0.027	(0.156)	−0.112	(0.651)
Employed in 2016/11	0.500***	(0.147)	1.071***	(0.321)	0.036	(0.151)	0.150	(0.602)
Employed in 2016/12	0.500***	(0.147)	1.071***	(0.321)	−0.012	(0.147)	−0.050	(0.602)
Employed in 2017/1	0.450***	(0.154)	0.964***	(0.321)	−0.012	(0.147)	−0.050	(0.602)
Employed in 2017/2	0.450***	(0.154)	0.964***	(0.321)	−0.012	(0.147)	−0.050	(0.602)
Employed in 2017/3	0.450***	(0.154)	0.964***	(0.321)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/4	0.400**	(0.160)	0.857**	(0.321)	−0.012	(0.147)	−0.050	(0.602)
Employed in 2017/5	0.400**	(0.160)	0.857**	(0.321)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/6	0.450***	(0.154)	0.964***	(0.321)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/7	0.400**	(0.160)	0.857**	(0.32)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/8	0.400**	(0.160)	0.857**	(0.32)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/9	0.450***	(0.154)	0.964***	(0.321)	−0.074	(0.153)	−0.312	(0.651)
Employed in 2017/10	0.450***	(0.154)	0.964***	(0.321)	−0.060	(0.142)	−0.250	(0.580)
Employed in 2017/11	0.500***	(0.147)	1.071***	(0.321)	−0.060	(0.142)	−0.250	(0.580)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) and (5)-(6) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) and (7)-(8) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history. In columns (1)-(4), the sample is restricted to workers with no work experience (N = 35); in columns (5)-(8), the sample is restricted to workers with some work experiences (N = 37). Previous work experience is an indicator equal to 1 if the individual had at least one employment spell before September 2015 within the administrative data.

Table B9: Heterogeneous effects of SEP on employment, by qualification of the worker

	Low				High			
	ITT		TOT		ITT		TOT	
Dependent variable	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed in 2015/7	0.000	(0.000)	-	-	0.000	(0.000)	-	-
Employed in 2015/8	0.038	(0.038)	0.111	(0.109)	0.000	(0.000)	-	-
Employed in 2015/9	0.231**	(0.084)	0.667***	(0.211)	0.000	(0.000)	-	-
Employed in 2015/10	0.423***	(0.099)	1.222***	(0.181)	0.100	(0.099)	0.333	(0.272)
Employed in 2015/11	0.462***	(0.100)	1.333***	(0.222)	0.100	(0.099)	0.333	(0.272)
Employed in 2015/12	0.462***	(0.100)	1.333***	(0.222)	0.100	(0.099)	0.333	(0.272)
Employed in 2016/1	0.420***	(0.108)	1.213***	(0.235)	0.017	(0.130)	0.056	(0.403)
Employed in 2016/2	0.420***	(0.108)	1.213***	(0.235)	0.017	(0.130)	0.056	(0.403)
Employed in 2016/3	0.378***	(0.115)	1.093***	(0.250)	0.117	(0.157)	0.389	(0.499)
Employed in 2016/4	0.340***	(0.114)	0.981***	(0.271)	0.217	(0.174)	0.722	(0.640)
Employed in 2016/5	0.340***	(0.114)	0.981***	(0.271)	0.117	(0.157)	0.389	(0.579)
Employed in 2016/6	0.340***	(0.114)	0.981***	(0.271)	0.117	(0.157)	0.389	(0.579)
Employed in 2016/7	0.337***	(0.121)	0.972***	(0.307)	0.033	(0.174)	0.111	(0.569)
Employed in 2016/8	0.295**	(0.126)	0.852***	(0.316)	0.033	(0.174)	0.111	(0.569)
Employed in 2016/9	0.295**	(0.126)	0.852***	(0.316)	0.033	(0.174)	0.111	(0.569)
Employed in 2016/10	0.295**	(0.126)	0.852***	(0.316)	0.033	(0.174)	0.111	(0.569)
Employed in 2016/11	0.295**	(0.126)	0.852***	(0.316)	0.133	(0.189)	0.444	(0.631)
Employed in 2016/12	0.256**	(0.126)	0.741**	(0.329)	0.133	(0.189)	0.444	(0.631)
Employed in 2017/1	0.256**	(0.126)	0.741**	(0.329)	0.050	(0.201)	0.167	(0.640)
Employed in 2017/2	0.256**	(0.126)	0.741**	(0.329)	0.050	(0.201)	0.167	(0.640)
Employed in 2017/3	0.256**	(0.126)	0.741**	(0.329)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/4	0.256**	(0.126)	0.741**	(0.329)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/5	0.215*	(0.130)	0.620*	(0.339)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/6	0.256**	(0.126)	0.741**	(0.329)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/7	0.215*	(0.130)	0.620*	(0.339)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/8	0.215*	(0.130)	0.620*	(0.339)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/9	0.256**	(0.126)	0.741**	(0.329)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/10	0.260**	(0.119)	0.750**	(0.328)	−0.033	(0.208)	−0.111	(0.677)
Employed in 2017/11	0.301**	(0.113)	0.870**	(0.321)	−0.033	(0.208)	−0.111	(0.677)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)-(2) and (5)-(6) present ITT estimates of the program impact (see equation 1). Coefficients correspond to an indicator variable equal to 1 if the worker is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)-(4) and (7)-(8) present TOT estimates (see equation 1). Coefficients correspond to a dummy variable equal to 1 if the worker is hired as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data of the entire employment history. In columns (1)-(4), the sample is restricted to workers with low skill jobs (N = 50), and in columns (5)-(8), the sample is restricted to workers with high skill jobs (N = 22), as determined by the reported type of occupation they would be qualified to cover at the the time of the application.