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

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Abstract

This paper examines the impact of an experiment in North Macedonia in which vulnerable unemployed individuals applying to a subsidized employment program and being matched to a job opening are randomly selected to attend job interviews. Employers receive a subsidy which reduced by half the wage cost of a newly hired worker during the first year and compensated the firm for the training costs. We complement administrative employment records with survey data to study treatment impacts. Attending the job interview leads to an increase of 15 percentage points in the likelihood of being employed 3.5 years after the start of the intervention. Obtaining a job under the program results in a much larger effect of 50 percentage points. These long-term effects are larger for individuals with lower attachment to the labor market. Among these, we document positive treatment effects on both non-cognitive and job-related skills.

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In the presence of labor market frictions, firms might be reluctant to hire workers whose productivity is uncertain. As a result, individuals with low ability to signal productivity, such as the youth and disadvantaged groups, experience high unemployment rates. Subsidized private sector employment has become a popular policy option to stimulate employment in these situations (Kaldor, 1936; Layard and Nickell, 1980; Bell et al., 1999). Wage subsidies can be introduced for a limited amount of time to lower the cost of hiring, reducing learning costs for the firm and incentivizing training during the initial stages of employment. In advanced economies, empirical evidence shows that subsidized employment could generate large employment effects in both the medium and the long run (Card et al., 2018).[1]

In low- and middle-income countries, interest for wage subsidies has risen only recently. However, available evidence remains limited, and mainly suggests the ineffectiveness of these programs at tackling unemployment (Betcherman et al., 2004; Almeida et al., 2012; McKenzie, 2017). First, the presence of burdensome labor regulations for hiring through these programs disincentivize firms to participate (Galasso et al., 2004; Levinsohn and Pugatch, 2014). Second, the design of these programs is such that employment effects tend to fade off in the long run. For instance, Groh et al. (2016) documents large short-term employment effects of providing wage vouchers to recent college graduates in Jordan, but effects are limited to the duration of the voucher.[2] Third, while combining wage subsidies with job search assistance has proven effective in more developed countries (Katz, 1998; Card et al., 2018), in developing countries subsidized employment programs present very limited matching or screening components, which can be central for their success given the potential severity of labor market frictions in these settings.

This paper addresses these limitations by studying the Subsidized Employment Program (SEP) in the Republic of North Macedonia, a country with one of the largest unemployment rates

[1]Early studies suggest that wage subsidies are ineffective in tackling unemployment (Burtless, 1985; Dubin and Rivers, 1993; Cockx et al., 1998). However, more recent evidence shows that they can have positive effects on employment both in the short- (Card and Hyslop, 2005), and in the long-term (Jespersen et al., 2008; Heinesen et al., 2013). A related literature is the one on temporary work programs. In Germany and the US, Kvasnicka (2009) and Autor and Houseman (2010) find no impact of temporary jobs on long-term employment. Pallais (2014) finds that hiring inexperienced workers and providing them with a job rating generates a positive impact on subsequent employment.

[2]Similar conclusions are also observed when subsidies are provided to firms (De Mel et al., 2010, 2019), or in programs supporting firms during demand and/or liquidity shocks (Bruhn, 2020).

worldwide (IMF, 2016). The program aims at improving long-term employment among disadvantaged individuals by providing temporary wage subsidies to employers who hire eligible job seekers. By requiring both firms and job seekers to apply at the National Employment Agency (NEA), the program offers substantial screening and matching components. Interested employers have to file an application indicating the required qualifications for the job, and job seekers have to document their qualifications and work experience. The NEA matches job applicants to available vacancies based on both skill requirements of jobs and worker characteristics. This limits job search costs and the inability to signal skills, two important constraints of labor markets and of employment programs (Abebe et al., 2018; Kluve et al., 2019; Bassi and Nansamba, 2019; Belot et al., 2019). Removing screening burden from firms has also shown to be effective in increasing labor demand (Algan et al., 2020).

Upon matching, we implemented a unique experiment in conjunction with the Macedonian Ministry of Labor and Social Policy, and the NEA. Among all applicants, only the ones matched to a given vacancy were selected for the experiment. A subset of these matched candidates (*treatment group*) was randomly invited to a job interview with the employer. Following the interview, successful candidates were offered the job, and the employer received the program's benefits. These include a six-month wage subsidy meant to fully cover the wage of the employee, and an additional transfer to cover training costs for the new employee. The wage subsidy varied from 46% to 53% of the average wage at the national level depending on the employee's qualifications and the skills required for the vacancy. In return, firms guaranteed that the employee is hired for a minimum of 12 months. The remaining subset of matched candidates (*control group*) was not invited to attend any interview.

This research design provides the first attempt to exogenously shift the access to a job interview in the context of subsidized employment. Compared to previous studies, this presents two main advantages. Thanks to the ex-ante matching of job seekers to job vacancies, it favors significantly higher subsidy take-up as compared to studies that randomly assign subsidies to job seekers (Galasso et al., 2004; Groh et al., 2016). In addition, it allows constructing a robust counterfactual to identify how job seekers would have performed in the labor market had they not received the intervention, but conditional on having comparable characteristics for an existing job opening. This is generally not the case for programs studied in low- and middle-income countries (McKenzie, 2017).

To study treatment impacts we use two complementary data sources. First, we gathered administrative records from the NEA, measuring employment up to 3.5 years after the start of the program. This time-frame goes beyond the time horizon of most studied programs in the literature (Card et al., 2018), enabling us to estimate long-term employment effects. Second, we use two rounds of individual- and household-level surveys to assess the short- and medium-term impacts on individual skills. Going beyond the direct treatment effect on employment, this allows studying impacts on workers' employability in the long run.

The treatment intervention is remarkably successful at increasing employment in both the short and the long run. As compared to matched job seekers in the control group, the ones who were invited to the job interview experience a 80% increase in the duration of employment, and a 18 to 25 percentage points higher probability of being employed in the first 6 months. This effect declines over time, mainly because matched job seekers who were not invited to the interview found a job outside of the program. Nevertheless, 42 months after the start of the program, we still observe a statistically significant effect on employment of 15 percentage points. These effects are mainly driven by workers securing the job with the initial employer after the subsidy expired, suggesting the importance of the screening and matching component for the effectiveness of the program.

The experimental design allows us not only to compute intent-to-treat estimates–i.e. comparing outcomes of matched job applicants who were or were not randomly invited to the job interview– but also the effect of being offered the job under the program. We estimate treatment on the treated estimates by identifying the job seekers who receive the job offer under the SEP, and instrumenting this variable with the random assignment to the interview invitation.[3] The impact of being selected for one of the SEP jobs leads to a 50 percentage-point increase in the probability of being employed 42 months after the start of the intervention. The long-term program effects are particularly large for individuals with lower attachment to the labor market, such as female, lower-educated and less-experienced job seekers.

[3]Section 4 discusses the validity of this exclusion restriction.

To understand the mechanism behind these results, we focus on whether job seekers acquired skills in response to the treatment. We document positive and statistically significant treatment effects on both non-cognitive and work-related skills. As this effect is larger for the same group of individuals experiencing the largest increase in employment, the improvement in skills is the most likely mechanism behind the effect on long-term employment. Individuals in the treatment group might have acquired sufficient experience and productivity gains during the period of guaranteed employment to keep the job once the subsidy expired. This result corroborates the importance of human capital accumulation in explaining the effectiveness of subsidized employment, and is in line with the importance of non-cognitive skills in the labor market (Heckman and Kautz, 2012).

Contrary to previous evidence on the ineffectiveness of wage subsidies in developing countries, we show that when coupled with job search assistance, subsidized employment has the potential to address long-term unemployment in countries with high structural unemployment. In addition, it favors important gains in terms of skill accumulation among disadvantaged groups. This result complements recent evidence highlighting the importance of training in addressing high youth unemployment in developing countries (Alfonsi et al., 2020).

1 Background and intervention

The labor market in North Macedonia is characterized by very low levels of employment, especially among the youth, and by a high dependency on social assistance. Although employment rates have been increasing in the last decades, in 2014 only 47% of the working age population was formally employed. The employment rate of 15-29 years old individuals was only 27%. Women are less likely to be employed than men (37% versus 56%, respectively), and inactivity rates are much higher for women (47%, versus 32% for men). Individuals with primary or no education are 24 percentage points less likely to be employed than individuals with just secondary education, with employment rates equal to 25% and 49% respectively (Macedonian State Statistical Office, 2014).

Even more striking is the prevalence of long-term unemployment: more than 80% of the unemployed have been unemployed for more than a year. Because unemployment benefits have a maximum duration of 12 months and are granted only if the individual accumulated enough contributions to an employment fund while working, they cover only about 9% of the unemployed population (Petreski and Mojsoska-Blazevski, 2017). In contrast, most of the the long-term unemployed rely on Social Financial Assistance (SFA), a means-tested monetary transfer which represents the most significant income support in North Macedonia and accounts for approximately 0.5 percent of the country's GDP (Armand et al., 2020). In 2007, an estimated 220,000 individuals were SFA beneficiaries, which corresponded to 11% of the population (World Bank, 2009).

In this context, employers might find obstacles in hiring workers with little experience in the formal labor market or with obsolete skills. The lack of an adequately educated workforce and a poor work ethic are perceived by employers as two of the most problematic factors for doing business in North Macedonia (Schwab et al., 2014). The Subsidized Employment Program (SEP) was launched by the Ministry of Labor and Social Policy of the Republic of North Macedonia in the summer of 2015 to tackle this issue. The objective was to promote the employment of individuals at risk of social exclusion by providing a wage subsidy to eligible employers for hiring a new employee from eligible groups of individuals. The eligibility requirements for employers required that the company had not experienced a reduction in its total employment between the date the program was launched and the date the employer applied to the program. Eligible individuals include SFA recipients and other vulnerable groups who are registered as active employment seekers in the National Employment Agency (NEA). Appendix A.1 details the requirements to define potential beneficiaries.

To participate in the program, job seekers and employers had to file and submit an application to the NEA. Job seekers had to document their qualifications and skills, including the attained level of education and any previous work experience. Employers had to specify the number of vacancies they would like to fill through the program and the desired characteristics and skills of the workers for each job vacancy. In principle, when filing an application, employers could choose among three program modalities, which differed in the duration and extent of the wage subsidy as well as in the required employers' duties. After conducting a survey among participating employers, we saw that essentially all employers chose the same modality.[4] In what follows we only describe the prevailing modality, and discuss additional details about the program design in Appendix A.2.

Employers who hire a job seeker through the program receive a wage subsidy for the first six months of the employment relationship. After this period, transfers are discontinued but employers commit to maintaining the worker employed for an additional six-month unsubsidized period. This period of guaranteed employment is relatively long compared to similar programs, and might favor the employer's investment in the training of the newly hired worker and the accumulation of skills.

There are two subsidy levels depending on the qualifications that employers required for the job: for beneficiaries without qualifications, the subsidy amounts to 14,900 MKD (266 USD) per month per employee; for beneficiaries with a higher educational degree, or those who are going to perform more complex working tasks, the subsidy corresponds to 17,000 MKD (303 USD) per month per employee.[5] Both subsidies are slightly higher than the legal minimum wage in 2015, which was equal to 13,900 MKD, and represented respectively 46% and 53% of the average wage at the national level (approximately 32,000 MKD). In addition, employers receive a monthly subsidy of 5,000 MKD (89 USD) per employee for the first six months to compensate for the training and material costs of the newly hired worker.

A pilot survey conducted before the program started suggests that potential employers are willing to hire a new eligible job seeker through the program conditional on the worker having characteristics demanded for the job (Armand et al., 2014). Thus, a matching process between workers and firms was introduced. The matching of job seekers and employers was conducted by a centralized agency, the NEA, which assigned job seekers to job vacancies taking into account the characteristics of the worker required for the vacancy and the job seeker's qualifications.[6] If qualified candidates for a specific job opening could be identified, the NEA would

[4]The distribution of benefits over time reported by participating employers is compatible exclusively with the program modality described in the main text.

[5]The average nominal exchange rate with US dollars in July 2015 was 56.03 MKD/USD (source: National Bank of the Republic of North Macedonia).

[6]If the number of potential matches exceeded the program budget, subsidies would be distributed across local

present the profiles of selected job seekers to the employer, and possibly schedule job interviews. Of course, the selection of the candidate was at the discretion of the employer. An employer who decided to hire a candidate would sign a contract with the NEA which established the rights and obligations of each party. In addition, the employer would sign a contract with the selected employee, which was subject to the laws regulating industrial relations in the country. The employment contract is for a full-time position of at least 40 hours per week, and the salary paid to the employee during the first six months of subsidized employment can not be lower than the wage subsidy the employer receives from the program. There is no specific requirement about the wage to be paid during the additional six months of unsubsidized employment.

Employers participating in the program are not allowed to reduce their total employment for the whole duration of the stipulated contract with the NEA, to avoid substitution of unsubsidized for subsidized workers. If an employer terminates the contract before the end of the compulsory employment period, the employer is obligated to either hire another eligible job seeker, or to return the funds received (including interest).

The program does not include any requirement about the type of employment contract (i.e., permanent or fixed-term) between the employer and the hired worker. The costs of firing a worker on a permanent contract are rather low in North Macedonia: severance pay is granted only for dismissals related to economic reasons and amounts to one monthly net salary for a worker with up to 5 years of tenure. Severance pay in North Macedonia is in between that of other countries in which similar programs have been implemented (Groh et al., 2016; De Mel et al., 2019). For instance, in Jordan, it is granted for any dismissal and corresponds to one month of salary per year of tenure, while in Sri Lanka, only workers with at least 5 years of tenure are eligible to severance pay. Overall, North Macedonia ranks below the OECD average on a composite measure of the strength of employment protection of permanent workers against individual dismissals (OECD, 2015).

The program also creates incentives for participants to accept job offers and reduce their depen-

employment centers proportionally to the number of eligible beneficiaries. This criterion is not binding within the evaluation.

dency on financial assistance. Whereas SFA benefits represent an important source of income for the poor, the eligibility rules require individuals to be registered as unemployed, which might create disincentives for formal work, perpetuate long-term unemployment and depreciate individual skills. While individuals hired through the program would automatically loose their right to SFA, job seekers who refused a suitable employment offer or voluntarily left a job position would be excluded from receiving the SFA benefit for six months.[7]

2 Experimental design

Figure 1 shows the timeline of the intervention. The application process opened in June 2015, after the program was announced and advertised throughout national media channels. Applications from employers and job seekers that were received between mid June and mid August 2015 were used to conduct an evaluation of the program impact. The first step of the evaluation design required the NEA to match job seekers with available vacancies. This was carried out by using information on job seekers' qualifications and on the characteristics of the vacancy elicited during the application process. Note that this activity is not unique to the SEP program, since the NEA adopts screening procedures in all the vacancies managed under their activity.

Of the 510 employee's and the 100 employer's applications that were collected between June and August 2015, the matching process resulted in a total of 153 potential employees for 22 job vacancies.[8] Each candidate was matched to only one job vacancy. These vacancies were distributed among 16 employers: 69% of employers applied for one position, 25% applied for two positions and 6% requested three positions. Because all of the employers who applied for more than one position posted identical job openings (i.e., they required multiple workers with the same set of skills and qualifications), the NEA prepared a unique list of candidates for each

[7] The vast majority of targeted individuals are SFA recipients. SFA transfers typically represent more than a quarter of the total expenditure of households in the lowest income deciles (World Bank, 2009). SFA beneficiary households are entitled to a benefit increasing with household size and decreasing with time spent on SFA, with a maximum of 5,515 MKD (98 USD) for households with five or more members (Gotcheva et al., 2013).

[8] The apparently high mismatch between the total number of vacancies (100) and the number of vacancies for which qualified candidates could be identified (22) is in part the result of few large firms applying for a relatively large number of positions (57 positions in total) which required specific skills (e.g., experience with chemicals) that are hardly found within the targeted population of beneficiaries.

set of identical positions with the same employer.

For all of the employer-specific lists of candidates created by the NEA, there were at least four candidates for each vacancy, with a median number of 6 candidates. The experiment randomly assigned about half of job seekers in each employer-specific candidate list to a treatment group (80 individuals), and the remaining half of to a control group (73 individuals). The randomization was conducted at the employer level. It is worth remarking that, in this context, this would be de facto equivalent to a job level randomization given that all employers were hiring one or more workers for identical job positions.

Among all job seekers matched to job openings in the SEP, individuals in the treatment group were invited to a job interview with the employer, while individuals in the control group (who were also matched to the same job openings) were not invited to any job interview. Interviews were scheduled by NEA officials at a time mutually convenient for the firm and the job seeker, and took place between mid August and the beginning of September 2015. After meeting all candidates in the treatment group, the employer could decide to offer the job to any of the interviewees. Although in principle the employer could decide not to offer the job and keep searching on his own, the subsidy was strictly conditional on hiring a candidate from the treatment group, and all available vacancies in our experiment were indeed offered to one of such candidates. Upon signing the contract with the employer, successful applicants could start working immediately after that date depending on their mutual agreement with the employer, and the employer would then start receiving the wage subsidy and the other program's benefits.

Because the SEP application process closed in mid-August 2015, individuals in the experimental sample who did not obtain a job under the program by September 2015 could not have obtained a SEP job at a later stage. Note that only matched job seekers in the treatment group could access this possibility since the ones in the control group were not invited to any job interview. However, matched job seekers remained eligible to receive the standard job search assistance services provided by the NEA, and could be employed in other jobs outside the SEP.

3 Data and sample

We use two main sources of data. First, to study the impact of the SEP on various employment outcomes, we rely on administrative data from the NEA. This database reports all the formal employment spells of individuals registered with the NEA up to March 2019. This allows us to follow the working life of job seekers participating in the SEP up to 3.5 years after the program started. For each employment spell, the data reports additional information such as the type of contract governing the employment relationship (fixed-term or unlimited-term), the monthly salary (although only since 2013) and employer identifiers. The use of administrative records has several advantages. First, it is less prone to misreporting or measurement error as compared to self-reported data. Second, since employment is recorded throughout a relatively long post-program period, it enables us to study employment effects over the medium and long run. Third, it allows a detailed study of the employment dynamics over the entire post-program period as well as of the employment transitions.

The second source of data are two waves of job seeker- and household-level surveys. We rely on this data to explore potential mechanisms behind the effect on employment, and to study the impact of the SEP on other outcomes. The first survey was administered between October 2015 and February 2016, while the second took place between May and August 2017. Because the SEP started to be phased-in between August and September 2015, the first wave assesses the short-term effects of the program, when employers who hired a new worker were still receiving the wage subsidy. The second wave of survey data, which was collected more than one and a half years after the introduction of the SEP, provides information on the medium-term outcomes and characteristics of program participants after the last wage subsidy was paid to the employer and after the employer's contractual obligations were fulfilled. A baseline survey was not collected but baseline employment outcomes are available from the administrative NEA database.

The job seeker survey comprises extensive information about the applicant's education, labor supply as well as various measures of job-related and non-cognitive skills. The job-related skill survey is partly adapted from the World Bank's STEP survey (World Bank, 2016). We measure non-cognitive skills using two scales: the Big-5 questionnaire (Goldberg, 1992) and the 12-item grit scale (Duckworth and Quinn, 2009).[9] The household survey, which was administered to the head of the 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.

Experimental firms are mainly concentrated in services (57% of firms) and in manufacturing and construction (36% of firms). The average size is 20 employees, although the distribution is positively skewed: 69% of firms have less than 10 employees, 8% have between 20 and 49 employees and 23% have 50 or more employees. Medium and large firms are thus over-represented in the experiment, as 91% of enterprises in North Macedonia have less than 10 employees and only in 2% of them the total workforce exceeds 50 workers (Macedonian State Statistical Office, 2020). Interestingly, 99% of the total workforce within experimental firms is formed by permanent workers and 85% have more than one year of tenure at the firm. The job vacancies posted by participating employers include both relatively low-skill occupations in manufacturing or services (e.g., janitor, non-specialized factory worker), and positions requiring higher qualifications or technical skills (e.g., chemical technician, administrative clerk).

The estimation sample for the analysis of the program impact using administrative records consists of 128 job seekers.[10] Out of all experimental applicants, 107 individuals were surveyed in the short-term assessment, and 91 individuals in the medium-term survey assessment. Attrition rate from short- to medium-term assessment is 15 percent. Attrition rates are not statistically different across treatment status (Appendix B.1). Table 1 shows in columns (1)–(2) descriptive statistics for the experimental sample, separately for each treatment group. In column (3), we test for imbalances in individual characteristics by reporting the mean difference between the treatment and the control group, together with the corresponding standard error. Panel A reports demographic characteristics of the job seeker and employment outcomes from the administrative data, measured in the pre-program period (i.e. until August 2015). Panel B shows

[9] Borghans et al. (2008) discusses measurement error related to these tests.

[10] The estimation sample is the result of merging administrative records of the SEP applications, and administrative employment records of the NEA. Appendix C provides further details about this process and shows an extensive set of robustness checks relative to sample selection.

additional time-invariant demographic variables from the short-term survey, such as the job seeker's ethnicity and household composition.

Program participants are on average 43 years old and 66% of them are male. Macedonians are the most widely represented ethnic group (59% of the experimental applicants), while ethnic Albanians represent roughly 25% of program participants. On average, 37% of applicants have attended at most primary school, while 63% have attended secondary school or university. In terms of employment, 7% of job seekers have been employed in the last 12 months and they accumulated, on average, only 2.5 years of work experience in formal employment before participating in the SEP. The strikingly low duration of employment suggests that long unemployment spells, erratic participation in the labor market and possibly high levels of informality are extremely common in this population. Employment with an unlimited term contract is more frequent than employment with a fixed term contract (respectively 2.2 and 0.3 years). None of the demographic characteristics are statistically different across treatment groups. When comparing the pre-program employment outcomes, individuals in the treatment group accumulated, on average, slightly more work experience as compared to the control group. While this difference is not statistically significant, in the empirical analysis we control for the relevant pre-program outcome in the estimating framework.[11]

Appendix B.2 presents a comparison between the experimental sample, selected according to enrollment and to the matching process of the SEP, and a representative sample of individuals targeted by the program (SFA recipients in the corresponding age group). Individuals in the experimental sample are older, more likely to be ethnic Macedonian, and slightly more likely to have previously worked for a salary. However, they have, on average, comparable levels of education and job search skills than the general SFA population. Thus, selection into the experiment seems to be mostly determined by characteristics associated with the distribution of available vacancies at the time of the SEP application, and not by individuals being on average more educated or motivated.

[11] Appendix B.3 analyses pre-program employment in the experimental sample between January 2000 and July 2015. To check robustness to potential pre-program differences, in the empirical analysis we control for a set of yearly employment indicators for the 2000-2015 period, and conduct extensive robustness checks (Appendix B.3 and Appendix C.2.2).

4 Empirical strategy and results

4.1 The effect on employment

Our experimental design matched firms with potential workers, but randomly assigned the offer of an interview with a firm in the program only to individuals in 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 individuals in the control group. We estimate the following empirical specification:

$$Y_{i,1} = \alpha + \beta T_i + \lambda' X_i + \rho Y_{i,0} + \varepsilon_i, \tag{1}$$

where $Y_{i,1}$ is an outcome of interest for job seeker i in the post-program period; T_i is an indicator variable taking the value 1 if the applicant was assigned to the treatment group; X_i is a vector of individual control variables, which include age and gender; $Y_{i,0}$ is the pre-program outcome; and ε_i is an i.i.d. error term. As participation in the interview does not guarantee that the worker will be hired, the parameter β in equation (1) represents an intent-to-treat (ITT) estimate. Notice that this effect is conditional on having been matched to the job opening, which is the characteristic in common between job seekers in the treatment and the control groups.

Because the program aims to increase employment by providing subsidized jobs, we also focus on the effect of being offered the subsidized job. We focus on the following specification:

$$Y_{i,1} = \delta + \gamma D_i + \theta' X_i + \eta Y_{i,0} + v_i. \tag{2}$$

where D_i is an indicator variable equal to 1 if individual i was offered a subsidized job. Since D_i is correlated with unobserved individual or job characteristics, we follow an Instrumental Variable (IV) estimation strategy by instrumenting D_i with the random assignment to the interview, T_i. The coefficient γ is the impact of being offered a SEP job among those who were offered a SEP job interview, i.e., the effect of the treatment on the treated (TOT). 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 questionable if the interview increased the applicant's motivation to search for more jobs or interview skills. Given the duration of the interview, we assume these are not relevant in this setting. In addition, individuals in the treatment group that attended the interview and did not receive the job offer behave over time similarly to individuals in the control group, who were never invited to the interview (Section 4.2). This suggests that the interview alone had no major effect on employment.

Table 2 shows estimates of the impact of the SEP on different employment outcomes. Column (2) presents ITT estimates controlling for the pre-program outcome variable in each regression. Individuals who are offered the interview are 18 percentage points more likely to have worked for at least one day after applying to the SEP. This is a sizable effect, as it represents a 72% increase with respect to the control group's mean in the post-program period. The program also significantly increases employment on the intensive margin. On average, whereas individuals in the control group are employed for just 182 days after applying to the SEP, the overall employment duration for SEP interviewees during the same period is 146 days higher.

With respect to the type of contract, the point estimates for the impact of the interview on both the extensive and intensive margin of working in fixed-term (in contrast to permanent or unlimited-term) employment are negative, and large in magnitude. Although not statistically different from zero, they represent a 20% reduction in the probability of working and a 35% reduction in the duration of employment in a fixed-term job. In contrast, individuals who were offered the interview are significantly more likely to have been employed in an unlimited-term job. These job seekers have accumulated about 4 times more employment days in an unlimited-term job than individuals in the control group. These results suggest that the SEP not only increases overall employment, but also improves the quality of employment by allowing participants to substitute fixed-term with unlimited-term employment.

As a result, labor earnings in the treatment group are about 64,000MKD larger than in the control group, a 91% increase.[12] This result is confirmed by survey data. In the treatment

[12]Labor earnings are computed by multiplying the daily wage by the number of days within each employment spell, and then adding labor earnings across all spells. The daily wage is constructed by multiplying the monthly wage from the administrative data by 12/365. Because wages are not available for employment spells before

group, both the probability of receiving SFA and the value of the SFA subsidy received are significantly reduced. However, we do not observe an increase in household's ownership of durables (Appendix B5).

In column (4), TOT estimates suggest that the estimated effect of being offered a SEP job is even larger. For example, job seekers who were offered the subsidized job are 66 percentage points more likely to work at least one day in the follow-up period, and 71 percentage points more likely to work in a permanent job. We observe again a strong switch from fixed-term to unlimited-term jobs, as job seekers who are offered the SEP job have been employed about two years more with an unlimited-term contract (and about half a year less with a fixed-term contract) than job seekers in the control group. Successful applicants' accumulated labor earnings are 3.5 times larger than the counterfactual mean in the control group.

For both ITT and TOT, estimates are robust to controlling more flexibly for the pre-program employment dynamics. In columns (3) and (5) of Table 2 we replace the control variable for the baseline outcome with a set of yearly employment dummies for the pre-program period (2000-2014, and January–July 2015). Each variable indicates whether the individual was employed in the corresponding year. Estimates are also robust to the inclusion of firm fixed effects in the estimating equation (Appendix Table C10). Finally, to alleviate concerns about multiple hypothesis testing, we build an aggregate measure of employment summarizing the independent information contained in the employment outcomes analyzed in the table. The measure is constructed as in Anderson (2008). The last row of Table 2 shows that the program has a strong and significant effect on this aggregate measure.

4.2 Employment dynamics

To analyse the evolution of employment over time, we convert the employment spells from administrative records into monthly employment status indicators. We define an individual to be employed in a given month if he/she worked for at least one day in that month. Results are

2013, we construct the baseline outcome in the labor earning regression by imputing the average daily wage to each pre-program employment spell without a valid wage, and then multiplying the imputed wage by the number of employment days within the spell.

robust to alternative definitions of employment (Appendix C.2.3). Since the SEP started to be phased-in in September 2015, we should observe the employment trajectories of treated and untreated matched job seekers to diverge around this date, with no significant difference in the months preceding the start of the program.

ITT estimates Panel A of Figure 2 compares the percentage of employed individuals among those who were offered a job interview (the solid line) and those who were not (the dashed line). Between September 2014 and August 2015, the employment rates of the two groups were extremely similar and very close to zero. Since September 2015, a marked spike in the employment rates of treated individuals is recorded, with an initial increase of about 25 percentage points. Employment rates in the treatment arm remain high in the subsequent months, and increase to almost 40%. Individuals in the treatment group are still employed even after employers stopped receiving the wage subsidy (6 months after the hired worker started working), and were freed from the contractual obligations of the program (12 months after the worker started working).

Employment rates in the control group also start to increase around the beginning of the program. This is due to matched individuals who were not offered the interview finding employment outside the program, either applying directly to non-participating firms or through the job search assistance services typically provided by the NEA to the long-term unemployed. Nevertheless, the increase in employment occurs at a much slower rate, eventually reaching an average slightly below 20% two years after the start of the SEP.

To compute ITT estimates, we estimate equation (1) using the monthly employment indicators as dependent variables and without controlling for their baseline value or for individual characteristics. The estimated coefficient on the treatment dummy for each month in the period of analysis are reported in columns (1)–(3) of Table 3 and summarized in Panel A of Figure 3. Results are robust to alternative sets of control variables, such as including individual demographic characteristics (Figure C9 and Table C11), employer fixed effects (Figure C11 and Table C13), and yearly pre-program employment indicator variables (Figure C12).

Given the random assignment, differences in the employment rates of the treatment and control groups are not statistically different from zero before the SEP started. The ITT estimates show statistically significant increases in the employment of interviewees of 20 to 25 percentage points in the first six months. The program effects remain large and statistically different from zero throughout most of the period. Indeed, in March 2019, about 42 months after the start of the SEP, and about 30 months after the end of the employer's contractual obligations, we can still detect an effect on the probability to be employed of 15 percentage points.[13] Conclusions are similar when considering the probability of being employed in an unlimited-term or in a fixed-term job as outcome of interest (Panels A and C of Appendix Figure B5). The ITT estimates on the unlimited-term employment are even larger and more precisely estimated than the estimates in Figure 3. Estimates using self-reported employment from our survey data are in line with the estimates on administrative data in the corresponding period, especially in the short-term (Table B5).

Due to the relatively small sample size of the study, we supplement individual t-test statistics based on heteroskedasticity-robust inference with inference based on permutation tests (Fisher, 1937; Pitman, 1937; Imbens and Rubin, 2015). This method allows constructing test statistics with exact finite sample size. We present the p-values of the permutation tests using 1000 replications in column (3) of Table 3. Results are very similar to those based on heteroskedasticity-robust inference. Following the same procedure and using permutations of the outcome variables instead of the treatment assignment leads to the same conclusion.

TOT estimates Panel B of Figure 2 plots employment rates over time for three groups of participants in the experiment: those in the treatment group who were offered a SEP job after the interview (labeled as *Treatment Job*); those in the treatment group who were offered the interview but did not get the job (labeled as *Treatment No Job*); and those in the control group who were not offered the interview (labeled as *Control*). Within the first two months since the SEP started, the employment rates of those offered a job jump to about 65%. Employment

[13] ITT estimates are not statistically different from zero only between January and August 2017. Given the small sample size, we are underpowered to detect such effects but these remain economically large and relatively constant throughout this period at about 10 percentage points. Moreover, 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 (Figure 2).

subsequently declines slightly to about 50%, and remains approximately at this level until the end of the post-program period, more than three years after the program started. Although the slight decline in employment occurred roughly 6 months after the start of the SEP, it is unlikely to be related to 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 about the reason for the job termination in the employer survey, all employers reported that job terminations were due to workers voluntarily quitting the job. The employment rates of the other two groups are much lower than those of individuals who were offered the job. By the end of the period of analysis, they are equal to 20-25% and almost indistinguishable between each other.

We estimate TOT impacts from equation (2) using the monthly employment indicators as dependent variables, without controlling for their baseline value or for individual characteristics. Similar to the ITT estimates, results are robust to alternative specifications (Appendix C.2). Monthly TOT estimates are reported in columns (4)–(5) in Table 3 and summarized in Panel B of Figure 3. In September 2015, we estimate that a SEP job offer leads to an increase in employment of 33 percentage points, and by April 2016 this increase is of 71 percentage points. Large and statistically significant impacts are found throughout most of the period of analysis. In the first quarter of 2019, the TOT estimates suggest that the impact of being offered a subsidized job on employment is still above 50 percentage points, with even larger effects on unlimited-term employment (Panel B of Appendix Figure B5).

Employment transitions The large and persistent employment effects documented above can be driven either by individuals managing to secure the initial job offer after the expiration of the subsidized employment period, or by individuals finding a new job, possibly using the accumulated work experience during the SEP to signal their productivity to a new employer. In order to better understand these mechanisms, we turn to an analysis of the program effect on employment transitions. Let t be any month between September 2014 and March 2019. Using the administrative data, we construct the following monthly employment transition indicators: (i) *stay employed*, equal to one if an individual who was employed in $t-1$ is employed in t, and zero otherwise; (ii) *stay unemployed*, equal to one if an individual who was unemployed in $t-1$ is unemployed in t, and zero otherwise; (iii) *job entry*, equal to one if an unemployed individual in $t-1$ is employed in t, and zero otherwise; (iv) *job exit*, equal to one if an employed individual in $t-1$ is unemployed in t, and zero otherwise. For those individuals who remain employed throughout two consecutive periods, we also look at whether they continue their employment relationship with the same employer, or if they switch to a different employer. We create a longitudinal monthly dataset and estimate the following panel model, controlling for job seeker fixed effects:

$$Y_{it} = \beta T_i \times Post_t + \delta Post_t + \theta_i + \varepsilon_{it}. \tag{3}$$

Y_{it} is one of the employment transition dummies defined above for individual i in month t; T_i is an indicator variable taking value 1 if individual i was assigned to the treatment group; $Post_t$ is an indicator for the post-program period (September 2015–March 2019); and θ_i is an individual fixed effect. To uncover the effect of being offered a subsidized job on the outcome of interest, we also estimate the following IV specification:

$$Y_{it} = \gamma D_i \times Post_t + \lambda Post_t + \eta_i + v_{it}, \tag{4}$$

in which we instrument the interaction between the dummy for being offered a SEP job, D_i, and the post-program dummy $Post_t$ with $T_i \times Post_t$.

Column (1) of Table 4 reports the estimates of β from equation (3), whereas column (2) shows the estimates of γ from equation (4). Being offered the SEP interview increases the probability of remaining employed by 13 percentage points, and decreases the probability of remaining unemployment by the same amount. The IV estimates suggest even larger effects of being offered a SEP job on these employment transitions, of 47 percentage points. In contrast, there is basically no effect on the probability of finding or losing a job. Moreover, the positive employment effects are entirely driven by individuals continuing their employment relationship with the same employer, rather than by individuals remaining employed but moving to a different employer. The program has a large effect on job entry, of about 10 percentage points, in the first two months after it was launched (Panel C of Appendix Figure B6). After this initial period, individuals in the treatment group are not more likely to find a new job, nor to switch to a new job. Instead, the impact of the program on employment mainly operates through a higher probability of remaining employed with the same employer after starting a new job (Panel E of Appendix Figure B6).

4.3 The effect on skills

We look at whether the SEP had any significant impact on several measures of individual skills from the short-term and medium-term surveys. We classify skills into two categories: job-related and non-cognitive. Job-related skills refer to several self-reported indicators for the individual reading, writing, using math and using a pc. Measures of non-cognitive skills include the Big-5 personality trait test and the 12-item grit scale test. We construct an index for each group of skills following the methodology in Anderson (2008), and we standardize all measures within each index to have mean zero in the control group.

We obtain ITT estimates by regressing each skill measure on a dummy for the individual being offered the job interview. The TOT estimates correspond to a regression of a skill outcome on a dummy for being offered the subsidized job, which is instrumented with the random assignment to the job interview. Columns (1)–(5) of Table 5 show the short-term effect of the SEP, whereas columns (6)–(10) report the estimated medium-term impacts. Columns (1)–(2) report the average value of each skill measure in the control and in the treatment group, respectively. Columns (3) and (8) report the ITT estimates, while columns (4) and (9) report the TOT estimates.

Job-related skills are limited within the population of job seekers. For example, 54% reported to have performed simple arithmetic operations, 27% read a book in the last 12 months, and only 33% used a computer in the 3 months prior to the interview. Both the ITT and TOT estimates suggest that the program has a short-term positive effect on the aggregate measures of job-related and non-cognitive skills. Compared to matched participants in the control group, participants who are offered the SEP interview score 0.3 standard deviations more on the job-related skill index and 0.24 standard deviations more on the non-cognitive skill test. The estimates for being offered a job correspond, respectively, to a 1.3 and 1 standard deviation increase over the mean in the control group. Medium-term effects are quantitatively very similar to the short-term effects. Treated individuals are significantly more likely to read, write or use a pc in the last 12 months. These results confirm that matched job seekers who are offered the interview are not only more likely to work than matched individuals who are not offered the interview, but are also significantly more likely to be employed in occupations which require and enhance the use of work-specific skills.

The program also has a short-term positive effect on the non-cognitive skill index. This is mainly driven by a reduction in neuroticism, which is related to the experience of anxiety, worry, fear, and frustration. This is in line with Gottschalk (2005), which show that working at a job can improve neuroticism by increasing the extent to which individuals believe that they have control over their lives through self-motivation or self-determination. In the medium-term, we notice effects of similar size to those in the the short-term for most measures, and larger impacts on the extraversion score. These results are robust to the inclusion as control variables of a standard measure for abstract reasoning (the Abbreviated Raven's test of progressive matrices, Bilker et al., 2012), as well as of other demographic characteristics (Appendix Table C14). These results are in line with evidence suggesting that major shifts in social roles (e.g., getting a job after long spells of unemployment) can lead to changes in personality traits also in adulthood (Almlund et al., 2011).

In addition to the job-related skills, the positive effect of the program on non-cognitive skills helps explaining the persistent effect on employment. These skills are well-rewarded in the labor market (Heckman and Kautz, 2012).[14] Before the SEP started, individuals in the experiment experienced a prolonged period of unemployment, which might have depreciated their human capital. By increasing the probability of finding a job, the program also improved some personality traits and enhanced the job-related skills of matched individuals who were offered the job interview. This might have increased their productivity, allowing them to maintain the job once the subsidy expired.

[14] In the survey among participating employers, we asked them to rank several types of skills based on their relevance for the posted vacancy. Employers particularly value non-cognitive skills.

4.4 Impact heterogeneity

Figure 4 explores the heterogeneity of the program impact across a variety of dimensions. Panel A compares the monthly employment rate of the treatment and control group for male and female job seekers. Employment rates in the control group suggest that male job seekers are more likely to be employed than female job seekers in the absence of the program. While the initial increase in employment of those who were offered the interview is above 20 percentage points for both males and females, the employment of female interviewees increases thereafter and remains substantially larger than that of females who were not offered the job interview. Instead, for males the employment rates in the treatment and in the control group are almost the same two years after the start of the SEP. In panel B of Figure 4 we split the sample among individuals younger and older than 45, the average age of job seekers in the sample. In the first year, the program effect is mostly concentrated on relatively older individuals. For younger job seekers, the program has a large initial effect on employment but the control group catches up relatively quickly and, after one year, the employment gap reduces to 8 percentage points. The impact of the SEP operates mainly by increasing the employment rates of less educated (panel C) and less experienced workers (panel D). The most notable effect is among individuals with no previous experience, whose employment rates three years after the start of the program are more than 30 percentage points larger than workers with similar characteristics in the control group. As for more educated or more experienced workers, in the long run the employment rates of individuals in the treatment group converge to those of more educated or experienced individuals who did not receive the interview.

The main beneficiaries of the SEP are individuals with lower attachment to the labor market, such as women, the inexperienced and those with low education levels. In the absence of the intervention, the employment rates of these groups would have been equal to or below 10%. With the intervention, employment rates rose by 15 to 30 percentage points. To understand if these findings are related to the accumulation of human capital, we investigate if the program effect on the measures of individual skills varies across different job seekers in a way which is consistent with the heterogeneous employment effects. Figure 5 shows the effect on the job-related skill index (Panel A) and on the non-cognitive skill index (Panel B) for the same groups analyzed in Figure 4.[15]

The program has similar positive effects on skills for both males and females, and a larger effect for young as compared to old individuals. We find a large, positive and statistically significant effect on both the job-related skill index and the non-cognitive skill index among individuals with primary education or less, whereas the effects for individuals with secondary education are small and not statistically different from zero. When we condition on the individual previous work experience, we see large and significant effects only for individuals without previous experience, and null effects for the experienced individuals. Consistently with the results shown in Figure 4, the largest program effects are found exactly on less educated and inexperienced individuals. The most likely explanation behind the persistent employment effect is a combination of acquired work experience and skills among individuals who would not have easily found employment, and that the improvement of such skills allows them to secure a job even after the subsidy expired.

5 Conclusion

This paper studies the impact of a subsidized employment program in North 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. After matching job seekers with the available vacancies posted by the employers, we evaluate the effectiveness of the program on short-term and long-term employment by randomly varying the access to a job interview with the potential employer.

In the short run, the SEP program increases the employment rates of individuals who were offered the job interview by more than 20 percentage points. The effect persists even after the end of the subsidized period and of the employer's contractual obligations, at about 15 percentage

[15]We pool together the short-term and medium-term assessments, and estimate such effects jointly on both waves. Conclusions are robust when using only the short-term or the medium-term surveys (Appendix Figures B7–B8).

points three and a half 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 50 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, inexperienced and unskilled individuals. Among these groups, the program has a large impact on several measures of job-related and non-cognitive skills, suggesting that human capital accumulation is an important factor in explaining the persistence of the employment effects in the long run.

Our results offer noteworthy policy implications. When coupled with job search assistance and training, wage subsidies can be effective in increasing long-term employment in low- and middle-income countries. Matching services to the unemployed can significantly increase the take-up of employment offers compared to voucher-based programs. Moreover, training subsidies to the employer can contribute to the investment in human capital and the accumulation of skills. Both factors have been shown to be important in explaining long-term effects of subsidized employment programs in advanced economies (Card et al., 2018). The lack of these features provides a plausible explanation for the ineffectiveness of previously studied programs in developing countries in reducing unemployment in the long run.

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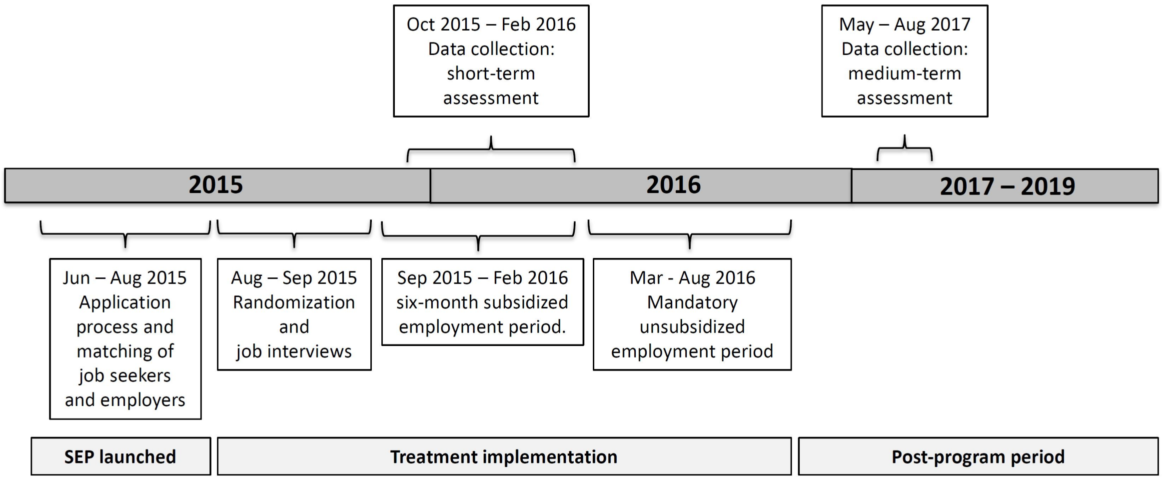
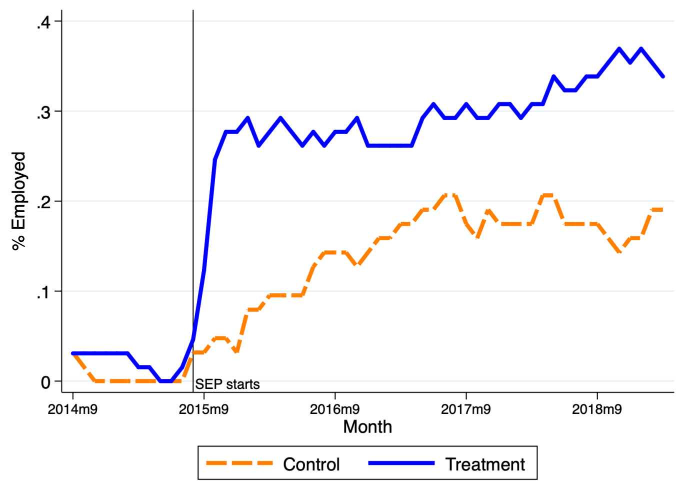
Figure 1: Timeline of the intervention and of the data collection



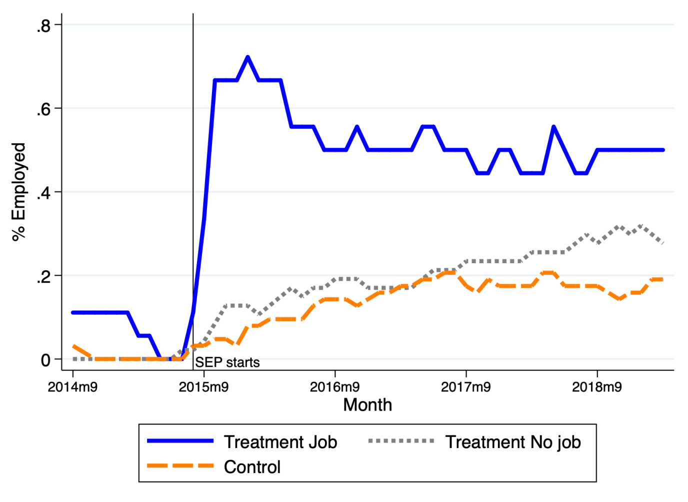
Notes: The figure shows the timeline of the intervention for a subsidized employment job starting in September 2015. The actual starting date is job-specific and typically occurred between September and October 2015.

Figure 2: Employment rate dynamics

A. Treatment group versus control group



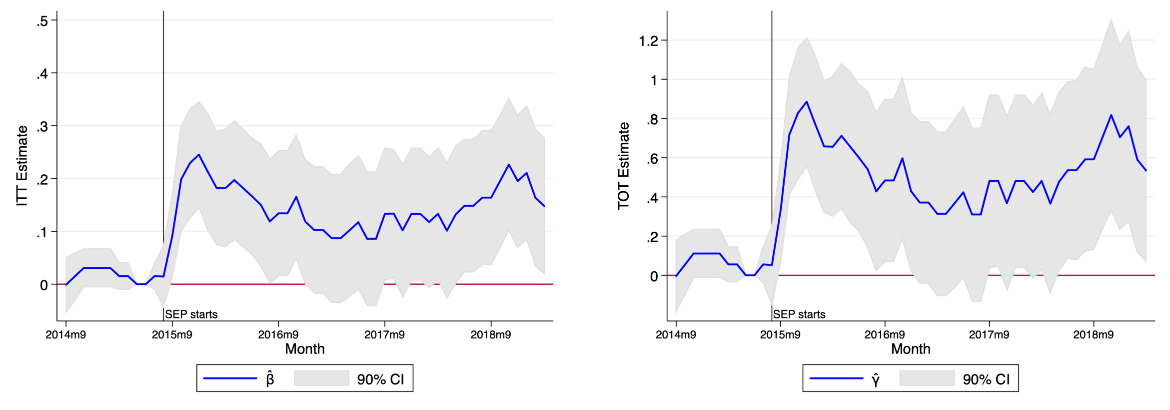
B. Treatment group split according to job offer versus control group



Notes: Panel A shows the employment dynamics of individuals in the treatment (solid line) and control (dashed line) group. Panel B shows the employment dynamics of individuals in the treatment group who were offered a SEP job (*Treatment Job*), those in the treatment group who were not offered a SEP job (*Treatment No job*), and those in the control group (dashed line). The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). Employment rates at the monthly frequency are computed by converting employment spells from National Employment Agency's administrative data. The estimation sample includes 128 individuals.

Figure 3: The impact of the SEP on employment

A. ITT estimates **B. TOT estimates**

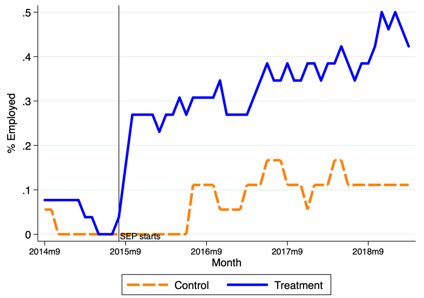


Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid line in panel A shows estimates of β from estimation of equation (1) without control variables. The solid line in panel B shows estimates of γ from estimation of equation (2) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

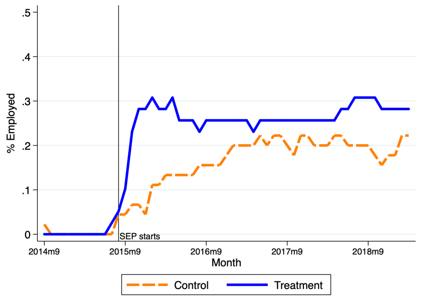
Figure 4: Heterogeneous effects of the SEP on employment

A. Gender

Female

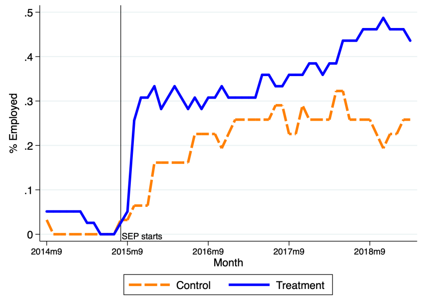


Male

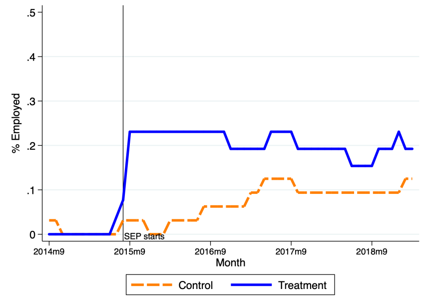


B. Age

45 or younger

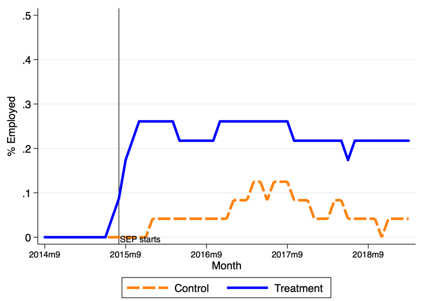


Older than 45

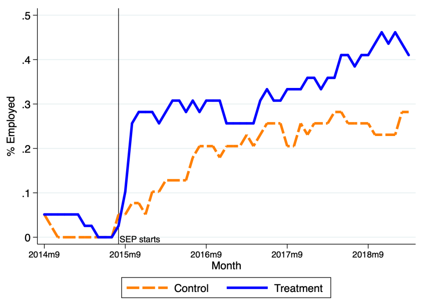


C. Education

Primary or no education

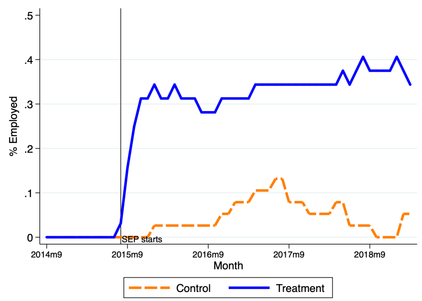


Secondary education or above

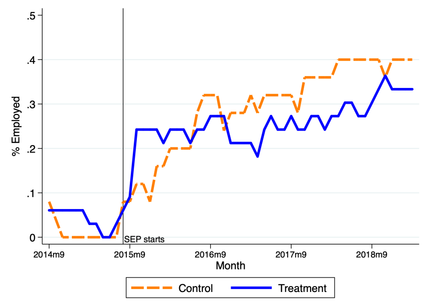


D. Work experience

No experience

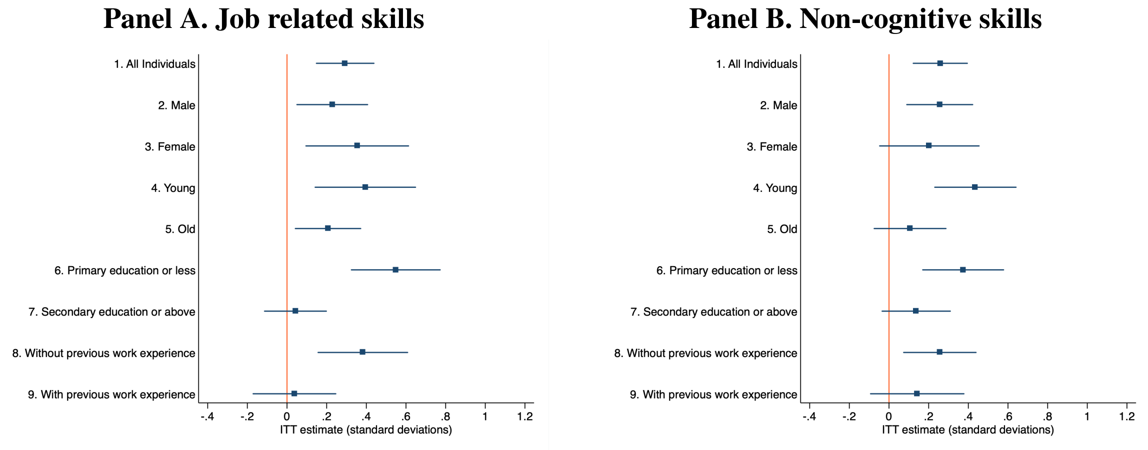


Experienced



Notes: Employment dynamics of individuals in the treatment (control) group are represented with the solid (dashed) line. Vertical lines indicate the month in which the SEP started (September 2015). In Panel A the sample is split among male (N=84) and female (N=44). In Panel B the sample is split among individuals who are 45 years old or younger (N=70) and individuals older than 45 (N=58). In Panel C the sample is split among individuals with primary education or less (N=47) and secondary education or more (N=78). In panel D the sample is split among individuals without previous work experience (N=70) and with previous experience (N=58). 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. Appendix Tables B6–B9 report ITT and TOT estimates of the impact of the SEP on employment for the subgroups represented in the four panels of this figure.

Figure 5: Heterogeneous effects of the SEP on individual skills



Notes: Panel A shows the point estimates and 90% confidence intervals of the estimated effect of the SEP on the job-related skill index. Panel B shows the same for the non-cognitive skill index. Estimated effects are ITT estimates based on the estimation of equation (1) without control variables, and obtained by pooling together the short-term and medium-term surveys. The job-related skill index is based on self-reported indicators for the individual reading, writing, using math and using a pc in the last 12 months. The non-cognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale. The job-related skill index and the non-cognitive skill index are computed following the methodology described in Anderson (2008). Confidence intervals are calculated based on robust standard errors.

Table 1: Individual descriptive characteristics, by treatment group

	Control (C) (1)	Treatment (T) (2)	Difference (T-C) (3)	Obs (4)
Panel A. Administrative data				
Age	44.30	41.71	-2.59	128
	(10.84)	(11.53)	(1.98)	
Male	0.71	0.60	-0.11	128
	(0.46)	(0.49)	(0.08)	
Primary or no education	0.38	0.37	-0.01	125
	(0.49)	(0.49)	(0.09)	
Secondary education or above	0.62	0.63	0.01	125
	(0.49)	(0.49)	(0.09)	
Employed last 12 months	0.06	0.08	0.01	128
	(0.25)	(0.27)	(0.05)	
Years employed	2.24	2.79	0.55	128
	(4.48)	(4.43)	(0.79)	
Years employed, fixed term	0.26	0.35	0.09	128
	(1.28)	(0.95)	(0.20)	
Years employed, unlimited term	1.98	2.44	0.46	128
	(4.23)	(4.39)	(0.76)	
Panel B. Survey data				
Macedonian	0.55	0.61	0.06	106
	(0.50)	(0.49)	(0.10)	
Albanian	0.31	0.25	-0.05	106
	(0.47)	(0.44)	(0.09)	
Other ethnic group	0.15	0.14	-0.01	106
	(0.36)	(0.35)	(0.07)	
Number of household members	3.52	3.61	0.09	105
	(1.55)	(1.72)	(0.32)	
Has child 0-6 years old	0.19	0.16	-0.03	105
	(0.39)	(0.37)	(0.07)	

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. The education dummies measure the highest education level attended by the job seeker, and are constructed from the administrative information in the job seeker's application to the SEP. When this information is missing, we use the survey data to impute the corresponding education level.

Table 2: The impacts of the SEP on employment outcomes

	Control mean at follow-up	ITT		TOT	
	(1)	(2)	(3)	(4)	(5)
Ever employed	0.25	0.18**	0.19**	0.66**	0.69**
		(0.08)	(0.09)	(0.29)	(0.28)
Days employed	182.10	145.73*	156.71*	557.22**	558.51**
		(76.45)	(83.05)	(282.63)	(261.24)
Ever employed fixed term	0.24	-0.05	-0.06	-0.19	-0.22
		(0.07)	(0.08)	(0.25)	(0.26)
Ever employed unlimited term	0.13	0.19**	0.17**	0.71**	0.59**
		(0.07)	(0.08)	(0.26)	(0.23)
Days employed fixed term	131.16	-44.96	-55.02	-165.50	-196.08
		(47.73)	(43.99)	(174.20)	(143.23)
Days employed unlimited term	50.94	201.26***	211.73**	773.95***	754.59***
		(59.76)	(73.35)	(220.19)	(221.69)
Labor earnings (1,000 MKD)	70.65	64.16*	64.15*	245.32**	228.62**
		(32.74)	(34.64)	(124.87)	(111.94)
Employment index	-0.00	0.40**	0.44**	1.52**	1.57**
		(0.18)	(0.19)	(0.64)	(0.60)
Baseline outcome		Yes	No	Yes	No
Employment history controls		No	Yes	No	Yes
Observations		128	128	128	128

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors are presented in parentheses. Each row shows the results of a regression with a different dependent variable. All dependent variables are measured throughout the post-program period (September 2015-March 2019). *Ever employed* is a dummy equal to 1 if the individual has worked for at least one day. *Days employed* is the total number of days the individual has been employed. *Ever employed fixed term* is a dummy equal to 1 if the individual has worked for at least one day in a fixed-term job. *Ever employed unlimited term* is a dummy equal to 1 if the individual has worked for at least one day in an unlimited-term job. *Days employed fixed term* is the total number of days the individual has been employed in a fixed-term job. *Days employed unlimited term* is the total number of days the individual has been employed in an unlimited-term job. *Labor earnings* is the cumulative labor income (product of the daily wage and the number of days employed in a given occupation). The employment index is constructed following the methodology described in Anderson (2008) and includes the following four variables: ever employed, days employed, ever employed unlimited term, labor earnings (we exclude the remaining outcomes as they would be collinear with the variables included). Column (1) shows the average of the dependent variable in the control group in the follow-up period. Columns (2)–(3) present ITT estimates of the program impact (equation (1)). Coefficients correspond to an indicator variable equal to 1 if the individual is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)–(4) present TOT estimates (equation (2)). Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Control variables include the age of the applicant and a gender dummy. Results in columns (2) and (4) control for the corresponding baseline outcome (measured throughout the pre-program period until July 2015) in each regression; results in columns (3) and (5) control for a set of yearly employment dummies for the pre-program period (2000-2014, and January-July 2015).

Table 3: Monthly estimates of the impact of the SEP on employment

	ITT			TOT	
	Coeff.	Std. error	Perm. test (p-value)	Coeff.	Std. error
	(1)	(2)	(3)	(4)	(5)
Employed in 2015/7	0.015	(0.015)	0.563	0.056	(0.057)
Employed in 2015/8	0.014	(0.034)	0.681	0.052	(0.122)
Employed in 2015/9	0.091*	(0.047)	0.049	0.330**	(0.157)
Employed in 2015/10	0.199***	(0.060)	0.002	0.717***	(0.184)
Employed in 2015/11	0.229***	(0.062)	0.000	0.828***	(0.203)
Employed in 2015/12	0.245***	(0.060)	0.001	0.885***	(0.199)
Employed in 2016/1	0.213***	(0.066)	0.001	0.769***	(0.209)
Employed in 2016/2	0.182***	(0.065)	0.006	0.658***	(0.204)
Employed in 2016/3	0.182***	(0.067)	0.008	0.656***	(0.217)
Employed in 2016/4	0.197***	(0.068)	0.003	0.712***	(0.224)
Employed in 2016/5	0.182***	(0.067)	0.002	0.656***	(0.234)
Employed in 2016/6	0.166**	(0.066)	0.019	0.601***	(0.227)
Employed in 2016/7	0.150**	(0.070)	0.038	0.541**	(0.241)
Employed in 2016/8	0.119*	(0.071)	0.101	0.429*	(0.245)
Employed in 2016/9	0.134*	(0.071)	0.050	0.484*	(0.251)
Employed in 2016/10	0.134*	(0.071)	0.082	0.484*	(0.251)
Employed in 2016/11	0.165**	(0.071)	0.025	0.597**	(0.248)
Employed in 2016/12	0.119*	(0.071)	0.109	0.429*	(0.245)
Employed in 2017/1	0.103	(0.072)	0.169	0.371	(0.249)
Employed in 2017/2	0.103	(0.072)	0.165	0.371	(0.249)
Employed in 2017/3	0.087	(0.073)	0.229	0.314	(0.253)
Employed in 2017/4	0.087	(0.073)	0.253	0.314	(0.253)
Employed in 2017/5	0.102	(0.076)	0.192	0.368	(0.261)
Employed in 2017/6	0.117	(0.076)	0.129	0.423	(0.265)
Employed in 2017/7	0.086	(0.077)	0.282	0.310	(0.269)
Employed in 2017/8	0.086	(0.077)	0.246	0.310	(0.269)
Employed in 2017/9	0.133*	(0.075)	0.104	0.481*	(0.268)
Employed in 2017/10	0.134*	(0.073)	0.078	0.482*	(0.265)
Employed in 2017/11	0.102	(0.076)	0.186	0.368	(0.270)
Employed in 2017/12	0.133*	(0.075)	0.098	0.481*	(0.268)
Employed in 2018/1	0.133*	(0.075)	0.089	0.481*	(0.268)
Employed in 2018/2	0.118	(0.075)	0.127	0.425	(0.267)
Employed in 2018/3	0.133*	(0.075)	0.084	0.481*	(0.273)
Employed in 2018/4	0.101	(0.077)	0.220	0.366	(0.277)
Employed in 2018/5	0.132*	(0.078)	0.107	0.477*	(0.277)
Employed in 2018/6	0.148*	(0.076)	0.064	0.536**	(0.273)
Employed in 2018/7	0.148*	(0.076)	0.069	0.536*	(0.279)
Employed in 2018/8	0.164**	(0.076)	0.046	0.592**	(0.286)
Employed in 2018/9	0.164**	(0.076)	0.043	0.592**	(0.280)
Employed in 2018/10	0.195**	(0.076)	0.014	0.705**	(0.286)
Employed in 2018/11	0.226***	(0.075)	0.001	0.817***	(0.295)
Employed in 2018/12	0.195**	(0.076)	0.019	0.705**	(0.286)
Employed in 2019/1	0.211***	(0.076)	0.006	0.760***	(0.294)
Employed in 2019/2	0.163**	(0.078)	0.040	0.590**	(0.287)
Employed in 2019/3	0.148*	(0.077)	0.063	0.534*	(0.281)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors are presented in parentheses. The dependent variables are indicators equal to 1 if the individual is employed in the correspondent period and 0 otherwise. Columns (1)–(2) present ITT estimates of the program impact based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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. Columns (4)–(5) present TOT estimates based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data for 128 individuals.

Table 4: SEP impact on employment transitions

	Received interview (1)	Received subsidized job (2)	Obs.
Stay employed	0.131**	0.472**	6528
	(0.056)	(0.200)	
Stay unemployed	-0.129**	-0.467**	6528
	(0.058)	(0.206)	
Job entry	0.002	0.007	6528
	(0.005)	(0.019)	
Job exit	-0.003	-0.012	6528
	(0.003)	(0.012)	
Stay employed, same employer	0.131**	0.474**	6528
	(0.056)	(0.197)	
Stay employed, switch employer	-0.000	-0.002	6528
	(0.002)	(0.006)	

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the individual level are presented in parentheses. Each row shows the results of a regression with a different dependent variable. *Stay employed* is a dummy equal to one if the individual was employed at time $t-1$ and remains employed at time t. *Stay unemployed* is a dummy equal to one if the individual was unemployed at time $t-1$ and remains unemployed at time t. *Job entry* is a dummy equal to 1 if the individual was unemployed at time $t-1$ and is employed at time t. *Job exit* is a dummy equal to one if the individual was employed at time $t-1$ and is unemployed at time t. *Stay employed, same employer* is a dummy equal to one if the individual was employed at time $t-1$ and remains employed at time t with the same employer. *Stay employed, switch employer* is a dummy equal to one if the individual was employed at time $t-1$ and remains employed at time t with a new employer. Column (1) presents estimates of the program impact from a fixed effect panel model (equation (3)). Columns (2) presents IV estimates in which being offered a job in the post program period is instrumented using the random assignment into the interview in the post-program period (equation (4)).

Table 5: Short- and medium-term impact of the SEP on individual skills

	Short-term impact					Medium-term impact				
	Control (C) (1)	Treatment (T) (2)	ITT (T-C) (3)	TOT (4)	Obs. (5)	Control (C) (6)	Treatment (T) (7)	ITT (T-C) (8)	TOT (9)	Obs (10)
Job related skills										
Job-relatd skill index	-0.00	0.30	0.30**	1.27**	106	-0.00	0.29	0.29**	1.15**	93
	(0.73)	(0.59)	(0.13)	(0.60)		(0.61)	(0.54)	(0.12)	(0.53)	
Read in the last 12 months	0.00	0.47	0.47***	1.98**	106	-0.00	0.54	0.54***	2.16**	93
	(1.00)	(0.72)	(0.17)	(0.84)		(1.00)	(0.74)	(0.18)	(0.88)	
Wrote in the last 12 months	-0.00	0.29	0.29*	1.23	106	0.00	0.45	0.45**	1.80**	93
	(1.00)	(0.72)	(0.17)	(0.79)		(1.00)	(0.68)	(0.18)	(0.83)	
Used math in the last 12 months	0.00	0.08	0.08	0.33	105	-0.00	0.23	0.23	0.90	87
	(1.00)	(0.92)	(0.19)	(0.80)		(1.00)	(0.87)	(0.20)	(0.81)	
Used pc in the last 12 months	0.00	0.39	0.39*	1.64*	106	-0.00	0.22	0.22	0.88	93
	(1.00)	(1.07)	(0.20)	(0.84)		(1.00)	(0.94)	(0.20)	(0.80)	
Noncognitive skills										
Noncognitive skill index	0.00	0.24	0.24**	1.01**	105	0.00	0.28	0.28**	1.13**	93
	(0.59)	(0.51)	(0.11)	(0.48)		(0.61)	(0.63)	(0.13)	(0.55)	
Extraversion	0.00	0.19	0.19	0.81	105	0.00	0.35	0.35*	1.40*	92
	(1.00)	(0.93)	(0.19)	(0.81)		(1.00)	(0.93)	(0.20)	(0.82)	
Agreeableness	-0.00	0.17	0.17	0.72	105	-0.00	0.25	0.25	1.01	92
	(1.00)	(0.91)	(0.19)	(0.78)		(1.00)	(1.07)	(0.22)	(0.90)	
Conscientiousness	0.00	0.30	0.30	1.25	105	0.00	0.33	0.33	1.33	92
	(1.00)	(0.94)	(0.19)	(0.79)		(1.00)	(1.14)	(0.22)	(0.92)	
Neuroticism (inverted scale)	0.00	0.47	0.47**	2.01**	105	0.00	0.41	0.41*	1.66	92
	(1.00)	(0.88)	(0.18)	(0.93)		(1.00)	(1.20)	(0.23)	(1.02)	
Openness	0.00	0.31	0.31	1.33	105	-0.00	0.26	0.26	1.05	92
	(1.00)	(1.11)	(0.21)	(0.91)		(1.00)	(0.84)	(0.19)	(0.79)	
Grit	-0.00	0.05	0.05	0.25	103	0.00	0.08	0.08	0.31	93
	(1.00)	(1.01)	(0.20)	(0.88)		(1.00)	(0.93)	(0.20)	(0.77)	

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Columns (1)–(5) show the short term impact of the SEP based on the first wave of the applicant survey. Columns (6)–(10) show the medium term impact of the SEP based on the second wave of the applicant survey. Columns (3) and (8) report ITT estimates based on the estimation of equation (1) without control variables. Columns (4) and (9) report TOT estimates based on the estimation of equation (2) without control variables. Numbers in parenthesis are standard errors for the estimates in columns (3), (4), (8) and (9), and standard deviations elsewhere. The job-related skill index is based on self-reported indicators for the individual reading, writing, using math and using a PC in the last 12 months. The non-cognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale. The job-related skill index and non-cognitive skill index are computed following the methodology described in Anderson (2008).

ONLINE APPENDIX

A SEP beneficiaries and program modalities

A.1 Eligibility requirements for job seekers and employers

Potential beneficiaries of the SEP program must be registered as active employment seekers in the NEA, and include the following categories of vulnerable individuals:

- beneficiaries of Social Financial Assistance (SFA);[1]
- beneficiaries of Permanent Financial Assistance (PFA) between 18 and 26 years old;[2]
- individuals who used to be beneficiaries of the CCT for secondary education (see Armand et al., 2020 for details);
- members of families that are beneficiaries of PFA or Child Allowance (CA);[3]
- individuals whose household monthly income per capita in the year prior to the SEP application was lower than 50% of the average net salary (as published by the State Statistical Office) and who fulfill any of the following criteria: (i) are victims of domestic violence who live in a shelter; (ii) have been given a state scholarship in the last 15 years; (iii) are 29 years old or younger.[4]

The following eligibility criteria apply to potential employers:

- Employers must not have laid off any of their employees, or have decreased the total number of employees, in the period between the publication of the public call and the

[1] 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.

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

[3] 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.

[4] Recipients in the 16–29 age group were deemed as a priority group. In practice, this criterion was not applied. In fact, individuals in the experimental sample are on average older (Table 1).

date of submission of the SEP application, excluding those cases related to the retirement or death of an employee.

- Employer who were enrolled in similar programs and did not adhere to the clauses of the contract signed with the NEA are automatically excluded.

- Employers who apply for the exemption from payment of social contributions for compulsory social insurance and/or personal income tax (see modalities 2 and 3 in the next subsection) should also adhere to the following conditions: (i) the hired worker can not have been employed by the same employer in the last two year; (ii) the employer can not have decreased its employment base in the last 3 months; (iii) the employer should not have unpaid salaries or contributions for more than two consecutive months.

A.2 Modalities

Employers applying to the SEP program could choose among the following three modalities, whose benefits and obligations are also summarized in Figure A1:

- *Modality 1*. This modality provides a gross wage subsidy for the duration of 6 months. The employer is obliged to keep the worker for an additional 6-month after the end of the subsidized employment period. Therefore, the employer is under the obligation of keeping the worker for a total of 12 months. There are two subsidy levels: one for job seekers without qualifications (14,900 MKD per employee); and one for job seekers with higher educational degree or employed for performing more complex tasks (17,000 MKD). 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 employee.

- *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. There is no obligation to keep the worker after the end of the 60-month period. However, an employer terminating the contract before the end of the agreement is obliged to either replace the worker with another suitable candidate from the group of eligible job seekers, or to return the wage and tax benefits received (plus interests).[5] There are two subsidy levels: 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). Employers are subject to the payment of the personal income tax for the hired worker, which is an employer's obligation in North Macedonia. Personal income tax is equal to 10% of the gross salary minus the social security contributions.[6]

- *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 the worker for an additional 12-month after the end of the 36-month period. An employer terminating the employment relationship with the employee before this period is subject to the same obligations of the previous modalities (i.e., replacement of the worker with another suitable candidate or repayment of the wage and tax benefits already received). The subsidy levels coincide with those in modality 2. In addition to the general requirements for the eligibility of job seekers (Appendix A.1), this modality is restricted to individuals younger than 35 who have been unemployed in the three months prior to the SEP application.

Thus, the duration and extent of the program's benefits, as well as the employer's obligations, vary substantially across modalities. In order to get some insight about the distribution of preferred modalities across employers, we conducted a firm-level survey in which we elicited information about the type of benefits the employers received and about the duration of such benefits. All employers reported that the maximum duration of the exemption from payment of social insurance contributions and personal income tax was equal to six months. Therefore, the net benefits of the first modality were perceived to dominate those of the other two modalities by all employers.

[5]Moreover, the employer can not reduce the total number of employees for the whole period for which the tax exemption is in place. The same requirement applies to the third modality.

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

Figure A1: 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 60-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

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

A.3 Comparison of benefits

Each modality provides benefits that are different in terms 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. 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.

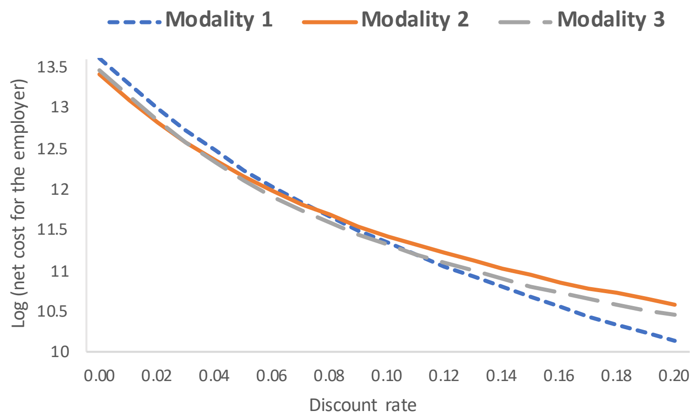
Table A1: Labor cost and SEP benefits

	Monthly costs and benefits since employment			5-year totals ('000 MKD)	
	1-6 months	**7-36 months**	**37-60 months**	**Low-skilled (%)**	**High-skilled (%)**
Labor 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)

Notes: 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. The lowest net cost for the employer is achieved with 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 the net present value at the beginning of employment of the net cost for the employer under different discount rates. Figure A2 presents the results. Modality 2 is preferred for very low discount rates. With more present-biased employers, first modality 3 and then modality 1 are preferred. It should be noted that this analysis does not take into account the differences in the employer's cost of keeping the worker employed for the compulsory period specified in each program modality, nor the obligation of not reducing the total number of employees throughout the tax-emption period with the second and third modalities. As a result, the calculations in Table A1 and Figure A2 probably overstate the net benefits of the second and third modalities with respect to those of the first modality.

Figure A2: Present value of the net cost for the employee across different modalities



Notes: 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 range from 0 to 0.2.

B Additional analysis

B.1 Attrition

Table B2 shows the attrition rate in the administrative and survey data across treatment groups. In the administrative data, attrition arises in two cases: (i) when the individual can not be linked to the database of the NEA because of an incorrect individual identifier; (ii) when the individual has never had any affiliation to the NEA.[7] In the survey, the reported attrition rates correspond to individuals in the short-term assessment survey who could not be re-interviewed in the medium-term survey. As can been, attrition rates are between 13% and 14% in both the administrative and survey data. Although attrition is slightly higher in the treatment group, differences are not statistically significant.

Table B2: Attrition rates in administrative and survey data by treatment group

	Control (C) (1)	Treatment (T) (2)	Difference (T-C) (3)	Obs. (4)
Panel A. Administrative data				
Empty employment records	0.11	0.16	0.05	153
	(0.31)	(0.37)	(0.06)	
Panel B. Survey data				
Attrition, medium-term survey	0.12	0.14	0.01	107
	(0.33)	(0.35)	(0.07)	

Notes: * p<0.10, ** p<0.05, *** p<0.01. Numbers in parenthesis are robust standard errors for the differences in column (3), and standard deviations elsewhere.

B.2 Sample representativeness

This section compares the characteristics of job seekers in our experimental sample with those of the general SFA population in North Macedonia. We use data from a pilot survey conducted with the objective of understanding the combination of program benefits which would maximize participation of potential beneficiaries into the program (refer to Armand et al., 2014 for further details). The survey was implemented between April and May 2014, more than a year

[7]In the database, individuals with no affiliation to the NEA have an unknown current status and no employment spell. We exclude these individuals from the administrative sample. Another group of individuals have an unknown current status but at least one employment spell in the past. We consider these individuals as not employed (inactive) for all the periods outside the corresponding employment spells, and keep them within the analysis.

before the launch of the SEP. The sample includes 274 SFA recipients between 18 and 49 years of age, and it is constructed to be representative of the 18-49 years old SFA population in North Macedonia.[8] For comparability, we restrict the experimental sample to job seekers who are 18-49 years old (approximately two thirds of the sample).

Table B3 presents a comparison across the two samples using information on demographics, education, previous work experience and job search skills. This information was collected with the same instrument for all but one indicator: for work experience, we use survey data for the SFA sample, while for the experimental sample, we use the NEA administrative records up to August 2015 since survey data were collected after the program started. Column (3) presents t-tests for the differences in the sample means being equal to zero allowing for the variance to be different across samples.

The experimental sample is on average older and over-represents males and ethnic Macedonians. However, average years of education are remarkably similar across samples. Individuals in the experimental sample are slightly more likely to have been employed for a salary than the general SFA population (43% versus 32%, on average). Finally, differences in several measures of job search skills between the two samples are not statistically different from zero.

[8] The sample was restricted to 18-49 years old individuals because the preliminary design of the SEP restricted eligibility to this age group. However, it was later established that any individual from the eligible disadvantaged groups in Section A.1 could participate into the program, irrespective of age.

Table B3: Comparison between the experimental sample and the corresponding SFA population

	Experimental sample 18-49	SFA population 18-49	Difference	Obs.
	(1)	(2)	(3)	(4)
Demographic characteristics				
Age	36.78	32.72	4.06***	377
	(8.71)	(9.47)	(1.07)	
Male	0.65	0.55	0.10*	377
	(0.48)	(0.50)	(0.06)	
Years of education	9.43	9.53	-0.10	373
	(3.78)	(3.68)	(0.43)	
Macedonian	0.47	0.22	0.25***	346
	(0.50)	(0.41)	(0.06)	
Albanian	0.31	0.39	-0.09	346
	(0.46)	(0.49)	(0.06)	
Other ethnic group	0.22	0.39	-0.17***	346
	(0.42)	(0.49)	(0.06)	
Work experience				
Ever worked for salary	0.43	0.32	0.12**	364
	(0.50)	(0.47)	(0.06)	
Job search skills (self-assessed)				
Find job vacancies	0.77	0.76	0.01	347
	(0.43)	(0.43)	(0.06)	
Prepare resume	0.50	0.49	0.01	346
	(0.50)	(0.50)	(0.07)	
Fill our job applications	0.73	0.65	0.07	347
	(0.45)	(0.48)	(0.06)	
Perform adequately in interview	0.74	0.74	-0.00	344
	(0.44)	(0.44)	(0.06)	

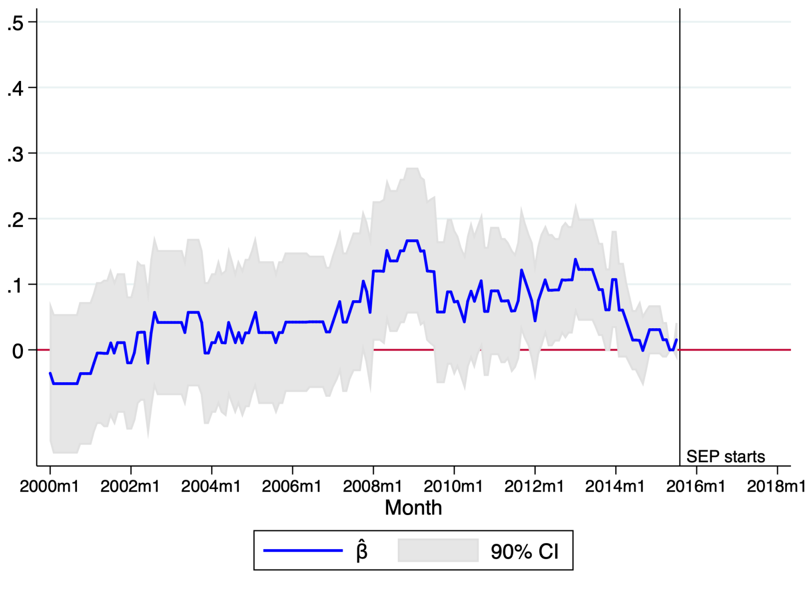
Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Column (1) reports means for the 18-49 individuals in the experimental sample. Column (2) reports the same for a sample which is representative of the 18-49 years old SFA population. *Ever worked for salary* is a binary variable constructed from administrative data in the experimental sample and from survey data in the SFA sample. Numbers in parenthesis are standard errors for the differences in column (3), and standard deviations elsewhere. Standard errors for the differences in column 3 assume that the variance is different across samples. The number of observations in column 4 is the total number of observations in the two samples.

B.3 Pre-program employment dynamics

In this section, we discuss the pre-program employment dynamics for the treatment and control group in the period between January 2000 and July 2015. We start with an imbalance test. We regress each monthly employment indicator on a dummy for the individual belonging to the treatment group, and plot the estimated coefficients in each time period. Note that these estimates are analogous to the ITT estimates shown in Figure 3 in the paper. We present the results in Figure B3. There are two periods in which the employment rates are imbalanced across treatment arms: between January 2008 and July 2009; and between March 2012 and September 2013. In Section C.2.2 we show that the main results reported in the paper are robust to the inclusion of pre-program employment dummies.

To get further insights on these dynamics, Panel A of Figure B4 reports the monthly employment rates in the treatment (solid line) and in the control group (dashed line). Panel B further splits the treatment group into those individuals who would eventually receive the job offer (solid line, labeled *Treatment Job*) and those individuals who would not receive the offer (dotted line, labeled *Treatment No Job*). The employment rates of both the treatment and the control group are between 15% and 20% in the 2000-2007 period. However, the employment rate of the treatment group increases to about 25% in 2008 and 2009, whereas that of the control group slightly declines to about 10%. Thereafter employment declines sharply for both groups. Employment dynamics in Panel B suggest that the diverging patterns across treatment groups in the 2008-2009 period are caused by both individuals who would later get a subsidized job and individuals who would not.

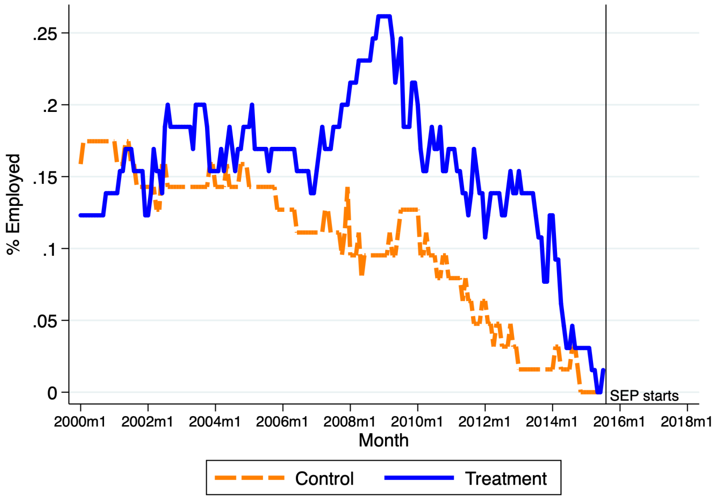
Figure B3: Employment dynamics, pre-program imbalance test



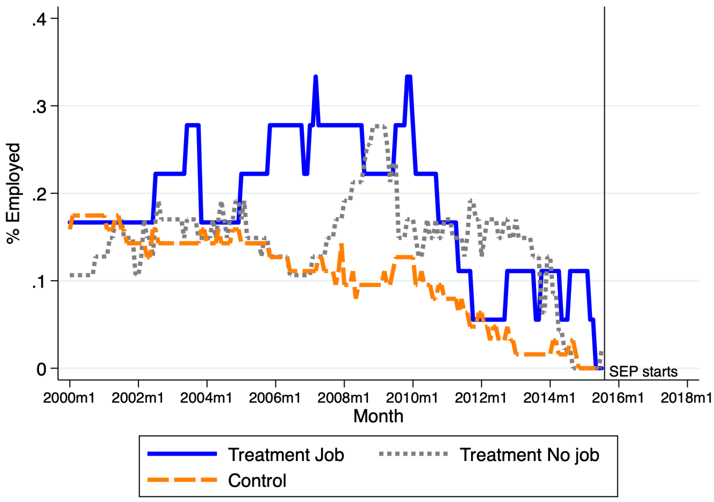
Notes: The solid line in panel A shows estimates of β from estimation of equation (1) without control variables. Bands around the solid lines are 90 % confidence intervals. The vertical line indicates the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

Figure B4: Pre-program employment rate dynamics, by treatment group

A. Treatment versus control



B. Treatment group split according to job offer versus control group



Notes: Panel A shows the employment dynamics of individuals in the treatment (solid line) and control (dashed line) group. Panel B shows the employment dynamics of individuals in the treatment group who were offered a SEP job (*Treatment Job*), those in the treatment group who were not offered a SEP job (*Treatment No job*) and those in the control group (dashed line). The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). Employment rates at the monthly frequency are computed by converting employment spells from National Employment Agency's administrative data. The estimation sample includes 128 individuals.

B.4 Employment effects and employment dynamics

Employment effects: panel estimates In this section, we estimate the effect of the SEP program by using a panel estimation strategy. We convert the data on individual employment spells into a monthly panel dataset, and estimate the impact of the program on several employment outcomes using equation (3) from the paper. To uncover the effect of being offered a subsidized job on the outcome of interest, we also present instrumental variable estimates based on equation (4).

In Table B4 we report the results for the same employment outcomes already studied in Table 2. Results are in line with those in Section 4.1, as we find positive and statistically significant effects on both the extensive margin (probability of being employed) and the intensive margin (number of working days) of employment. Moreover, this effect is entirely driven by a larger probability of finding an unlimited-term job, whereas estimates for fixed-term employment outcomes are not significant. Finally, we also observe that the program significantly increases labor earnings.

The magnitude of the estimated effects in Table B4 varies with respect to Table 2 because Table B4 estimates the program effect at the monthly level, whereas estimates in Table 2 refer to a total effect throughout the entire post-program period (September 2015-March 2019). However, if we multiply the estimates in Table B4 by the number of post-program periods (43 months), we obtain figures which are similar to those in Table 2. For example, an individual randomly receiving the interview works 4.6 days more per month than individuals who did not receive the interview. This corresponds to 198 working days throughout the entire post-program period, an estimate very close to the 201 extra days in an unlimited-term job reported in Table 2.

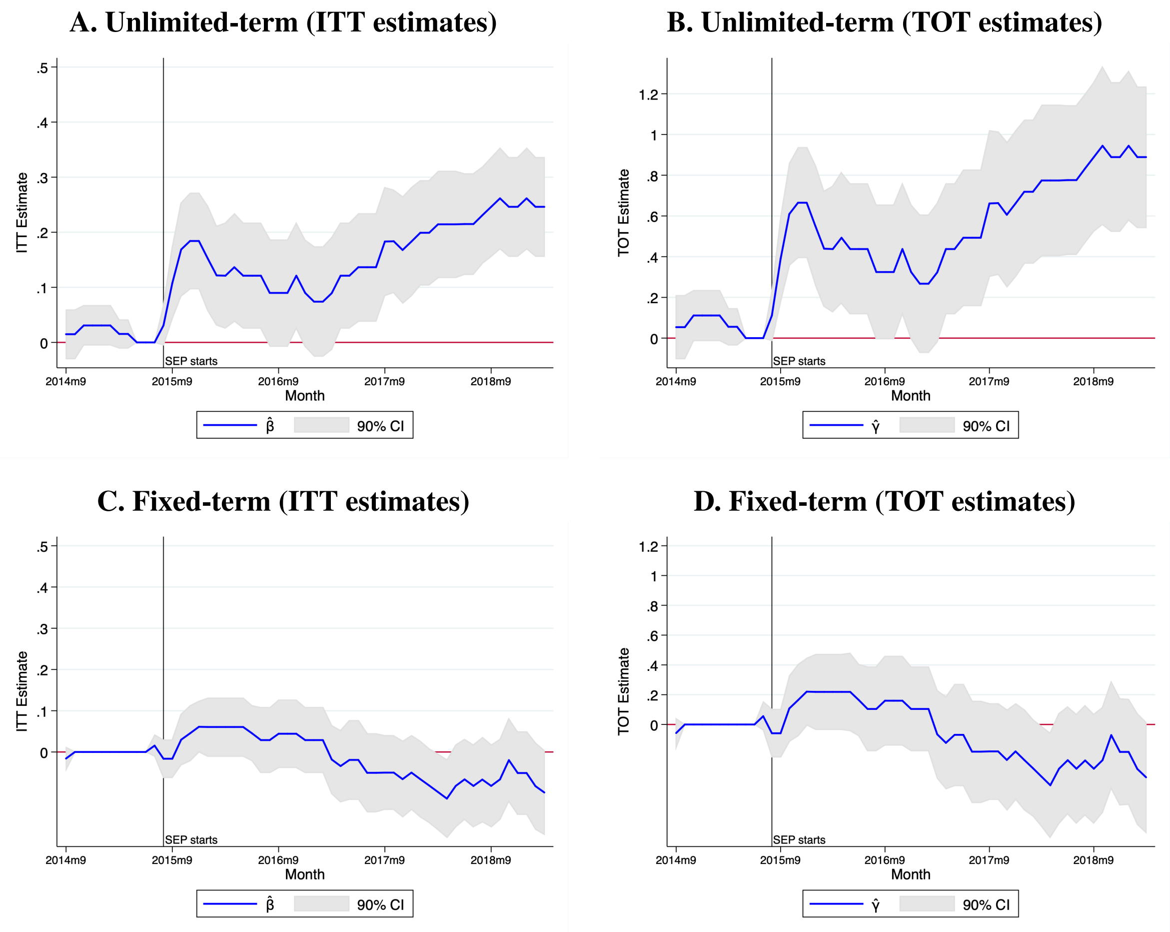
Table B4: SEP impact on employment, panel estimates

	Received interview (1)	Received subsidized job (2)	Obs.
Employed	0.133**	0.479**	6528
	(0.057)	(0.202)	
Employed unlimited term	0.149***	0.539***	6528
	(0.044)	(0.159)	
Employed limited term	-0.018	-0.064	6528
	(0.038)	(0.137)	
Days work	4.033**	14.563**	6528
	(1.724)	(6.125)	
Days work unlimited term	4.578***	16.531***	6528
	(1.320)	(4.722)	
Days work limited term	-0.575	-2.076	6528
	(1.147)	(4.138)	
Labor earnings (1,000MKD)	1.786**	6.507**	6526
	(0.731)	(2.699)	

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the individual level are presented in parentheses. Each row shows the results of a regression with a different dependent variable. *Employed* is a dummy equal to 1 if the individual worked for at least one day at time t. *Days employed* is the total number of days the individual has been employed at time t. *Employed fixed term* is a dummy equal to 1 if the individual has worked for at least one day in a fixed-term job at time t. *Employed unlimited term* is a dummy equal to 1 if the individual has worked for at least one day in an unlimited-term job at time t. *Days employed fixed term* is the total number of days the individual has been employed in a fixed-term job at time t. *Days employed unlimited term* is the total number of days the individual has been employed in an unlimited-term job at time t. *Labor earnings* is the monthly labor income at time t. Column (1) shows the estimated impact of being offered a job interview from a panel model with individual fixed effects (equation (3)). Column (2) shows IV estimates in which being offered a job in the post program period is instrumented using the random assignment into the interview in the post-program period (equation (4)).

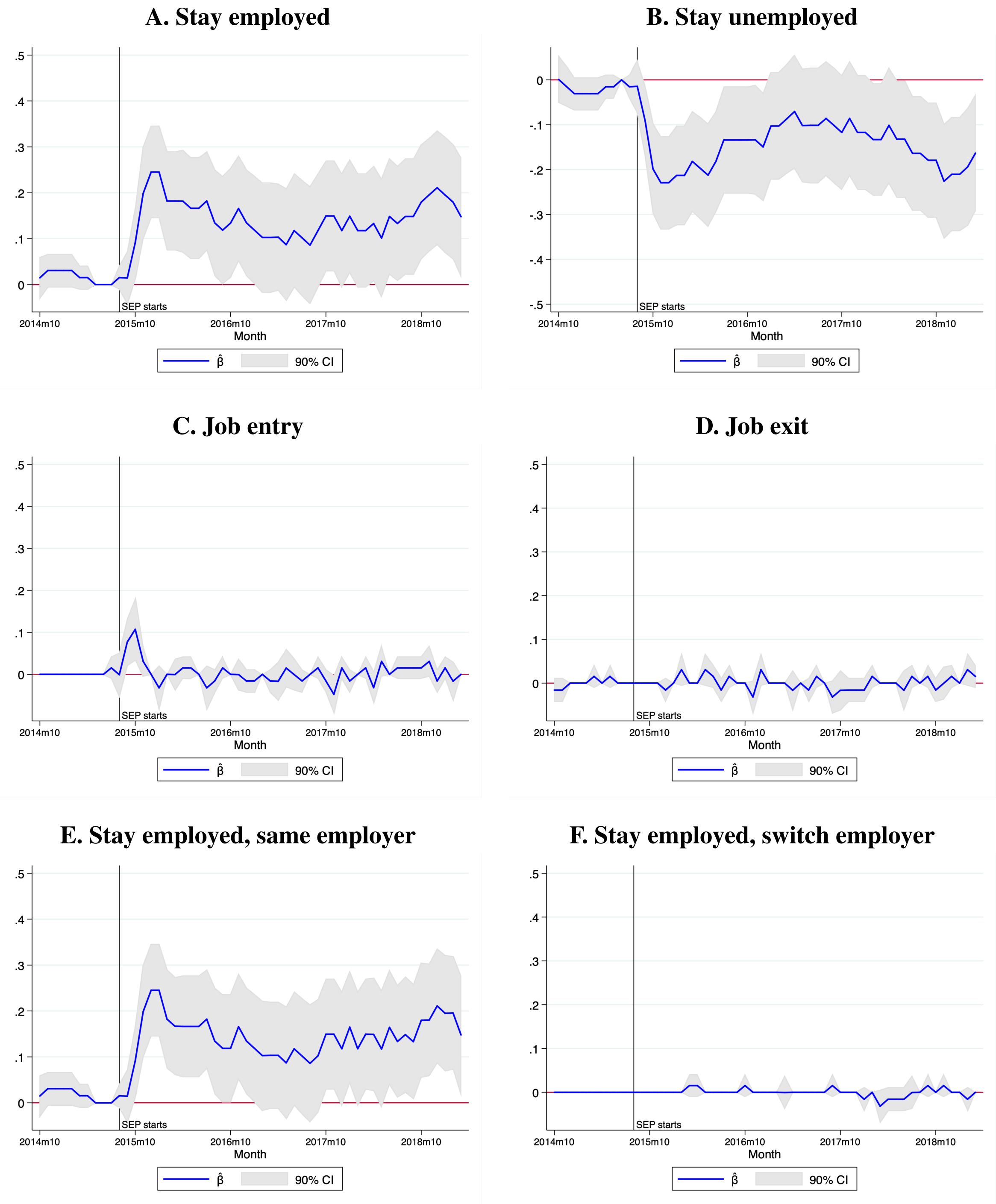
Employment dynamics Next, we present additional results about the effect of the program on the type of employment and on employment transitions. In Figure B5 we present monthly ITT and TOT estimates of the program effect on unlimited-term and fixed-term employment. In Figure B6 we show monthly ITT and TOT estimates on the following employment transitions: (i) stay employed (Panel A); (ii) stay unemployed (Panel B); (iii) job entry (Panel C); (iv) job exit (Panel D); (v) stay employed, same employer (Panel E); stay employed, switch employer (Panel F). See Section 4.2 for a definition of these employment transitions.

Figure B5: ITT and TOT estimates for unlimited- and fixed-term employment



Notes: In Panels A and B, the dependent variable is a dummy equal to 1 if the individual is employed in an unlimited-term job in the corresponding month. In Panels C and D, the dependent variable is a dummy equal to 1 if the individual is employed in a fixed-term job in the corresponding month. The solid line in Panels A and C shows estimates of β from estimation of equation (1) without control variables. The solid line in Panels B and D shows estimates of γ from estimation of equation (2) without control variables. In all figures, bands around the solid lines are 90% confidence intervals. The vertical lines indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

Figure B6: ITT estimates on employment transitions



Notes: The dependent variable *Stay employed* in Panel A is a dummy equal to 1 if an individual employed in the previous month remains employed in the current month. The dependent variable *Stay unemployed* in Panel B is a dummy equal to 1 if an individual unemployed in the previous month remains unemployed in the current month. The dependent variable *Job entry* in Panel C is a dummy equal to 1 if an individual unemployed in the previous month is employed in the current month. The dependent variable *Job exit* in Panel D is a dummy equal to 1 if an individual employed in the previous month is unemployed in the current month. The dependent variable *Stay employed, same employer* in the Panel E is a dummy equal to 1 if an individual employed in the previous month remains employed in the current month with the same employer. The dependent variable *Stay employed, switch employer* in Panel F is a dummy equal to 1 if an individual employed in the previous month remains employed in the current month with a different employer.The solid lines in all six panels show estimates of β from estimation of equation (1) without control variables. In all figures, bands around the solid lines are 90% confidence intervals. The vertical lines indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

B.5 Household income and self-reported employment

Table B5 looks at the program impact on outcomes related to household income, following the same empirical strategy discussed in Section 4.3. We rely on data on household's ownership of durable goods to proxy for household income. To reduce the dimensionality of the 25 durable measures elicited in the survey, we construct an index following Anderson (2008). The program effect on household income is not statistically significant, neither in the short nor in the medium run. However, in the second and third rows of the table, we also observe large reductions in both the probability of receiving SFA and in the value of the SFA subsidy received in the last month, especially in the short run. This is expected since such benefits are automatically removed when recipients find employment in a formal job. Individuals in the treatment group are 24 percentage points less likely to receive SFA benefits in the short-term, and 16 percentage points less likely in the medium-term. It is worth remarking that the magnitude of both estimates is very similar to the magnitude of the estimated short-term and medium-term employment effects in Table 3. This suggests that SEP beneficiaries substitute social benefits with labor earnings.

We also use our survey data to look at the probability that the SEP applicant was working for a salary in the last seven days. In the short-term, the ITT estimate suggests that individuals who participated in the interview are 18 percentage points more likely to work than individuals in the control group. The TOT estimate indicates an even larger effect (78 percentage points) of being offered a subsidized job. Both estimates are very similar to the estimates using administrative data in the corresponding period (October 2015-February 2016). In the medium-term (May-August 2017), individuals in the treatment group are still 5 percentage points more likely to be employed, although the estimate is insignificant. The effect size is slightly smaller than the impacts estimated on administrative data in the corresponding period (Table 3). This is driven by a larger probability of being employed in the control group in the survey data than in the administrative data (24% versus 20%, respectively), which could suggest that individuals in the control group are on average more likely to be employed in the informal economy.

Table B5: Short- and medium-term impact of the SEP on income and self-reported employment

	Control (C) (1)	Treatment (T) (2)	ITT (T-C) (3)	TOT (4)	Obs (5)
Panel A. Short-term impact					
Durables index	-0.00	0.06	0.06	0.23	104
	(0.35)	(0.37)	(0.07)	(0.30)	
Received SFA last month	0.92	0.69	-0.24***	-1.01***	103
	(0.27)	(0.47)	(0.08)	(0.28)	
SFA received (1,000 MKD)	2.77	2.22	-0.55	-2.36	103
	(1.48)	(2.21)	(0.37)	(1.44)	
Works for salary (self-reported)	0.09	0.27	0.18**	0.78***	106
	(0.29)	(0.45)	(0.07)	(0.29)	
Panel B. Medium-term impact					
Durables index	0.00	-0.03	-0.03	-0.13	90
	(0.29)	(0.28)	(0.06)	(0.26)	
Received SFA last month	0.70	0.53	-0.16	-0.70*	86
	(0.46)	(0.50)	(0.10)	(0.40)	
SFA received (1,000 MKD)	1.99	1.49	-0.50	-2.14	86
	(1.79)	(1.78)	(0.38)	(1.54)	
Works for salary (self-reported)	0.24	0.30	0.05	0.20	93
	(0.43)	(0.46)	(0.09)	(0.36)	

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Panel A (B) shows the short (medium) term impact of the SEP based on the first (second) wave of the applicant and household level survey. Columns (3) and (4) report ITT and TOT estimates based on the estimation of, respectively, equation (1) and equation (2) without control variables. Numbers in parenthesis are standard errors for the estimates in columns (3) and (4), and standard deviations elsewhere. The durables index is based on household self-reported ownership of durable goods among a list of 25 items and it is computed following the methodology described in Anderson (2008).

B.6 Other outcomes and heterogeneity

In this section, we complement the results presented in Section 4.4 and present ITT and TOT estimates on employment for several groups of individuals. 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 compares the employment effects of individuals with at most primary education and individuals with secondary education or more; Table B9 splits the sample into those with and without previous work experience, as constructed from employment spells prior to September 2015.

Figure B7 reports estimates of the short-term impact of the program on non-cognitive and job-related skills for different groups of individuals, whereas Figure B8 shows the medium-term impacts.

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

	Female				**Male**			
	ITT		TOT		ITT		TOT	
	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)	0.026	(0.026)	0.026	(0.026)
Employed in 2015/8	0.038	(0.039)	0.143	(0.132)	0.007	(0.047)	0.024	(0.166)
Employed in 2015/9	0.154**	(0.072)	0.571**	(0.225)	0.058	(0.058)	0.206	(0.203)
Employed in 2015/10	0.269***	(0.089)	1.000***	(0.343)	0.164**	(0.078)	0.582**	(0.232)
Employed in 2015/11	0.269***	(0.089)	1.000***	(0.343)	0.215**	(0.082)	0.764***	(0.267)
Employed in 2015/12	0.269***	(0.089)	1.000***	(0.343)	0.238***	(0.079)	0.842***	(0.263)
Employed in 2016/1	0.269***	(0.089)	1.000***	(0.343)	0.197**	(0.089)	0.697**	(0.273)
Employed in 2016/2	0.231***	(0.085)	0.857***	(0.275)	0.171*	(0.087)	0.606**	(0.279)
Employed in 2016/3	0.269***	(0.089)	1.000***	(0.343)	0.149*	(0.089)	0.527*	(0.286)
Employed in 2016/4	0.269***	(0.089)	1.000***	(0.343)	0.174*	(0.091)	0.618**	(0.298)
Employed in 2016/5	0.308***	(0.093)	1.143***	(0.410)	0.123	(0.087)	0.436	(0.301)
Employed in 2016/6	0.269***	(0.089)	1.000***	(0.343)	0.123	(0.087)	0.436	(0.301)
Employed in 2016/7	0.197	(0.120)	0.730*	(0.399)	0.123	(0.087)	0.436	(0.301)
Employed in 2016/8	0.197	(0.120)	0.730*	(0.399)	0.075	(0.087)	0.267	(0.303)
Employed in 2016/9	0.197	(0.120)	0.730*	(0.399)	0.101	(0.089)	0.358	(0.315)
Employed in 2016/10	0.197	(0.120)	0.730*	(0.399)	0.101	(0.089)	0.358	(0.315)
Employed in 2016/11	0.291**	(0.110)	1.079***	(0.353)	0.101	(0.089)	0.358	(0.315)
Employed in 2016/12	0.214**	(0.105)	0.794**	(0.331)	0.079	(0.091)	0.279	(0.319)
Employed in 2017/1	0.214**	(0.105)	0.794**	(0.331)	0.056	(0.093)	0.200	(0.324)
Employed in 2017/2	0.214**	(0.105)	0.794**	(0.331)	0.056	(0.093)	0.200	(0.324)
Employed in 2017/3	0.158	(0.117)	0.587	(0.376)	0.056	(0.093)	0.200	(0.324)
Employed in 2017/4	0.197	(0.120)	0.730*	(0.399)	0.031	(0.091)	0.109	(0.316)
Employed in 2017/5	0.235*	(0.122)	0.873**	(0.385)	0.034	(0.095)	0.121	(0.329)
Employed in 2017/6	0.218	(0.133)	0.810*	(0.437)	0.056	(0.093)	0.200	(0.324)
Employed in 2017/7	0.179	(0.131)	0.667	(0.450)	0.034	(0.095)	0.121	(0.329)
Employed in 2017/8	0.179	(0.131)	0.667	(0.450)	0.034	(0.095)	0.121	(0.329)
Employed in 2017/9	0.274**	(0.124)	1.016**	(0.462)	0.056	(0.093)	0.200	(0.324)
Employed in 2017/10	0.235*	(0.122)	0.873*	(0.468)	0.079	(0.091)	0.279	(0.319)
Employed in 2017/11	0.235*	(0.122)	0.873*	(0.468)	0.034	(0.095)	0.121	(0.329)
Employed in 2017/12	0.329***	(0.112)	1.222***	(0.455)	0.034	(0.095)	0.121	(0.329)
Employed in 2018/1	0.274**	(0.124)	1.016**	(0.462)	0.056	(0.093)	0.200	(0.324)
Employed in 2018/2	0.235*	(0.122)	0.873*	(0.468)	0.056	(0.093)	0.200	(0.324)
Employed in 2018/3	0.274**	(0.124)	1.016**	(0.505)	0.056	(0.093)	0.200	(0.324)
Employed in 2018/4	0.218	(0.133)	0.810	(0.507)	0.034	(0.095)	0.121	(0.329)
Employed in 2018/5	0.256*	(0.134)	0.952*	(0.501)	0.060	(0.096)	0.212	(0.331)
Employed in 2018/6	0.274**	(0.124)	1.016**	(0.505)	0.082	(0.095)	0.291	(0.326)
Employed in 2018/7	0.235*	(0.122)	0.873*	(0.505)	0.108	(0.096)	0.382	(0.336)
Employed in 2018/8	0.274**	(0.124)	1.016*	(0.544)	0.108	(0.096)	0.382	(0.336)
Employed in 2018/9	0.274**	(0.124)	1.016**	(0.505)	0.108	(0.096)	0.382	(0.336)
Employed in 2018/10	0.312**	(0.125)	1.159**	(0.544)	0.130	(0.094)	0.461	(0.333)
Employed in 2018/11	0.389***	(0.126)	1.444**	(0.630)	0.126	(0.091)	0.448	(0.318)
Employed in 2018/12	0.350***	(0.126)	1.302**	(0.586)	0.104	(0.093)	0.370	(0.322)
Employed in 2019/1	0.389***	(0.126)	1.444**	(0.630)	0.104	(0.093)	0.370	(0.322)
Employed in 2019/2	0.350***	(0.126)	1.302**	(0.586)	0.060	(0.096)	0.212	(0.331)
Employed in 2019/3	0.312**	(0.125)	1.159**	(0.544)	0.060	(0.096)	0.212	(0.331)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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 based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data. In columns (1)–(4), the sample is restricted to male workers (N=84), and in columns (5)–(8) to female workers (N=44).

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

	Young				Old			
	ITT		TOT		ITT		TOT	
	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)	0.038	(0.038)	0.038	(0.038)
Employed in 2015/8	-0.007	(0.041)	-0.020	(0.122)	0.046	(0.062)	0.237	(0.311)
Employed in 2015/9	0.019	(0.048)	0.057	(0.140)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2015/10	0.192**	(0.084)	0.576***	(0.216)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2015/11	0.243***	(0.087)	0.730***	(0.244)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2015/12	0.243***	(0.087)	0.730***	(0.244)	0.231***	(0.084)	1.200***	(0.342)
Employed in 2016/1	0.172*	(0.102)	0.516*	(0.275)	0.231***	(0.084)	1.200***	(0.342)
Employed in 2016/2	0.121	(0.099)	0.362	(0.272)	0.231***	(0.084)	1.200***	(0.342)
Employed in 2016/3	0.146	(0.101)	0.439	(0.278)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2016/4	0.172*	(0.102)	0.516*	(0.286)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2016/5	0.146	(0.101)	0.439	(0.297)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2016/6	0.121	(0.099)	0.362	(0.288)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2016/7	0.082	(0.107)	0.246	(0.312)	0.200**	(0.090)	1.037***	(0.385)
Employed in 2016/8	0.056	(0.106)	0.169	(0.310)	0.168*	(0.095)	0.875**	(0.398)
Employed in 2016/9	0.082	(0.107)	0.246	(0.316)	0.168*	(0.095)	0.875**	(0.398)
Employed in 2016/10	0.082	(0.107)	0.246	(0.316)	0.168*	(0.095)	0.875**	(0.398)
Employed in 2016/11	0.140	(0.105)	0.419	(0.312)	0.168*	(0.095)	0.875**	(0.398)
Employed in 2016/12	0.082	(0.107)	0.246	(0.312)	0.130	(0.090)	0.675	(0.418)
Employed in 2017/1	0.050	(0.109)	0.149	(0.320)	0.130	(0.090)	0.675	(0.418)
Employed in 2017/2	0.050	(0.109)	0.149	(0.320)	0.130	(0.090)	0.675	(0.418)
Employed in 2017/3	0.050	(0.109)	0.149	(0.320)	0.099	(0.095)	0.512	(0.438)
Employed in 2017/4	0.050	(0.109)	0.149	(0.320)	0.099	(0.095)	0.512	(0.438)
Employed in 2017/5	0.101	(0.111)	0.303	(0.324)	0.067	(0.099)	0.350	(0.465)
Employed in 2017/6	0.101	(0.111)	0.303	(0.324)	0.106	(0.103)	0.550	(0.495)
Employed in 2017/7	0.043	(0.113)	0.129	(0.331)	0.106	(0.103)	0.550	(0.495)
Employed in 2017/8	0.043	(0.113)	0.129	(0.331)	0.106	(0.103)	0.550	(0.495)
Employed in 2017/9	0.133	(0.109)	0.400	(0.325)	0.106	(0.103)	0.550	(0.495)
Employed in 2017/10	0.133	(0.109)	0.400	(0.325)	0.099	(0.095)	0.512	(0.483)
Employed in 2017/11	0.069	(0.114)	0.206	(0.334)	0.099	(0.095)	0.512	(0.483)
Employed in 2017/12	0.127	(0.112)	0.380	(0.330)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/1	0.127	(0.112)	0.380	(0.330)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/2	0.101	(0.111)	0.303	(0.330)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/3	0.127	(0.112)	0.380	(0.336)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/4	0.062	(0.116)	0.186	(0.343)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/5	0.113	(0.117)	0.340	(0.342)	0.099	(0.095)	0.512	(0.483)
Employed in 2018/6	0.178	(0.113)	0.533	(0.335)	0.060	(0.089)	0.312	(0.466)
Employed in 2018/7	0.178	(0.113)	0.533	(0.344)	0.060	(0.089)	0.312	(0.466)
Employed in 2018/8	0.203*	(0.114)	0.610*	(0.353)	0.060	(0.089)	0.312	(0.466)
Employed in 2018/9	0.203*	(0.114)	0.610*	(0.343)	0.060	(0.089)	0.312	(0.466)
Employed in 2018/10	0.236**	(0.111)	0.707**	(0.342)	0.099	(0.095)	0.512	(0.524)
Employed in 2018/11	0.294***	(0.109)	0.881**	(0.354)	0.099	(0.095)	0.512	(0.524)
Employed in 2018/12	0.236**	(0.111)	0.707**	(0.342)	0.099	(0.095)	0.512	(0.524)
Employed in 2019/1	0.236**	(0.111)	0.707**	(0.342)	0.137	(0.099)	0.712	(0.589)
Employed in 2019/2	0.203*	(0.114)	0.610*	(0.343)	0.067	(0.099)	0.350	(0.522)
Employed in 2019/3	0.178	(0.113)	0.533	(0.335)	0.067	(0.099)	0.350	(0.522)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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 based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data. In columns (1)–(4), the sample is restricted to workers younger than 45 years old (N=70), and in columns (5)–(8) to workers older than 45 years old (N=58).

Table B8: Heterogeneous effects of the SEP on employment, by education level

	Primary or less				**Secondary or more**			
	ITT		TOT		ITT		TOT	
	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed in 2015/7	0.043	(0.043)	0.043	(0.043)	0.000	(0.000)	0.000	(0.000)
Employed in 2015/8	0.087	(0.060)	0.333	(0.236)	-0.026	(0.044)	-0.111	(0.197)
Employed in 2015/9	0.174**	(0.081)	0.667**	(0.272)	0.051	(0.061)	0.222	(0.246)
Employed in 2015/10	0.217**	(0.088)	0.833***	(0.264)	0.179**	(0.083)	0.778***	(0.296)
Employed in 2015/11	0.261***	(0.094)	1.000***	(0.333)	0.205**	(0.085)	0.889***	(0.318)
Employed in 2015/12	0.261***	(0.094)	1.000***	(0.333)	0.231***	(0.081)	1.000***	(0.310)
Employed in 2016/1	0.219**	(0.102)	0.840**	(0.348)	0.179**	(0.088)	0.778**	(0.328)
Employed in 2016/2	0.219**	(0.102)	0.840**	(0.348)	0.154*	(0.086)	0.667**	(0.311)
Employed in 2016/3	0.219**	(0.102)	0.840**	(0.348)	0.154*	(0.091)	0.667*	(0.340)
Employed in 2016/4	0.219**	(0.102)	0.840**	(0.348)	0.179*	(0.092)	0.778**	(0.356)
Employed in 2016/5	0.176*	(0.097)	0.674*	(0.354)	0.179*	(0.092)	0.778**	(0.382)
Employed in 2016/6	0.176*	(0.097)	0.674*	(0.354)	0.154*	(0.091)	0.667*	(0.364)
Employed in 2016/7	0.176*	(0.097)	0.674*	(0.354)	0.128	(0.097)	0.556	(0.394)
Employed in 2016/8	0.176*	(0.097)	0.674*	(0.354)	0.077	(0.098)	0.333	(0.406)
Employed in 2016/9	0.176*	(0.097)	0.674*	(0.354)	0.103	(0.099)	0.444	(0.417)
Employed in 2016/10	0.176*	(0.097)	0.674*	(0.354)	0.103	(0.099)	0.444	(0.417)
Employed in 2016/11	0.219**	(0.102)	0.840**	(0.348)	0.128	(0.097)	0.556	(0.411)
Employed in 2016/12	0.219**	(0.102)	0.840**	(0.348)	0.051	(0.096)	0.222	(0.406)
Employed in 2017/1	0.178	(0.110)	0.681*	(0.367)	0.051	(0.096)	0.222	(0.406)
Employed in 2017/2	0.178	(0.110)	0.681*	(0.367)	0.051	(0.096)	0.222	(0.406)
Employed in 2017/3	0.178	(0.110)	0.681*	(0.367)	0.026	(0.098)	0.111	(0.417)
Employed in 2017/4	0.136	(0.116)	0.521	(0.391)	0.051	(0.096)	0.222	(0.406)
Employed in 2017/5	0.136	(0.116)	0.521	(0.391)	0.077	(0.101)	0.333	(0.423)
Employed in 2017/6	0.178	(0.110)	0.681*	(0.367)	0.077	(0.104)	0.333	(0.438)
Employed in 2017/7	0.136	(0.116)	0.521	(0.391)	0.051	(0.103)	0.222	(0.438)
Employed in 2017/8	0.136	(0.116)	0.521	(0.391)	0.051	(0.103)	0.222	(0.438)
Employed in 2017/9	0.136	(0.116)	0.521	(0.391)	0.128	(0.101)	0.556	(0.445)
Employed in 2017/10	0.134	(0.105)	0.514	(0.374)	0.128	(0.101)	0.556	(0.445)
Employed in 2017/11	0.134	(0.105)	0.514	(0.374)	0.077	(0.104)	0.333	(0.447)
Employed in 2017/12	0.134	(0.105)	0.514	(0.374)	0.128	(0.104)	0.556	(0.445)
Employed in 2018/1	0.176*	(0.097)	0.674*	(0.354)	0.103	(0.105)	0.444	(0.447)
Employed in 2018/2	0.176*	(0.097)	0.674*	(0.354)	0.077	(0.104)	0.333	(0.447)
Employed in 2018/3	0.176*	(0.097)	0.674*	(0.354)	0.103	(0.105)	0.444	(0.459)
Employed in 2018/4	0.134	(0.105)	0.514	(0.374)	0.077	(0.107)	0.333	(0.460)
Employed in 2018/5	0.134	(0.105)	0.514	(0.374)	0.128	(0.108)	0.556	(0.457)
Employed in 2018/6	0.132	(0.091)	0.507	(0.348)	0.154	(0.107)	0.667	(0.456)
Employed in 2018/7	0.176*	(0.097)	0.674*	(0.404)	0.128	(0.106)	0.556	(0.459)
Employed in 2018/8	0.176*	(0.097)	0.674*	(0.404)	0.154	(0.107)	0.667	(0.474)
Employed in 2018/9	0.176*	(0.097)	0.674*	(0.404)	0.154	(0.107)	0.667	(0.456)
Employed in 2018/10	0.176*	(0.097)	0.674*	(0.404)	0.205*	(0.106)	0.889*	(0.475)
Employed in 2018/11	0.217**	(0.088)	0.833**	(0.403)	0.231**	(0.106)	1.000**	(0.495)
Employed in 2018/12	0.176*	(0.097)	0.674*	(0.404)	0.205*	(0.106)	0.889*	(0.475)
Employed in 2019/1	0.176*	(0.097)	0.674*	(0.404)	0.231**	(0.106)	1.000**	(0.495)
Employed in 2019/2	0.176*	(0.097)	0.674*	(0.404)	0.154	(0.109)	0.667	(0.469)
Employed in 2019/3	0.176*	(0.097)	0.674*	(0.404)	0.128	(0.108)	0.556	(0.457)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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 based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data. In columns (1)–(4), the sample is restricted to workers with primary education or less (N=47); in columns (5)–(8), to workers with at least secondary education (N=78).

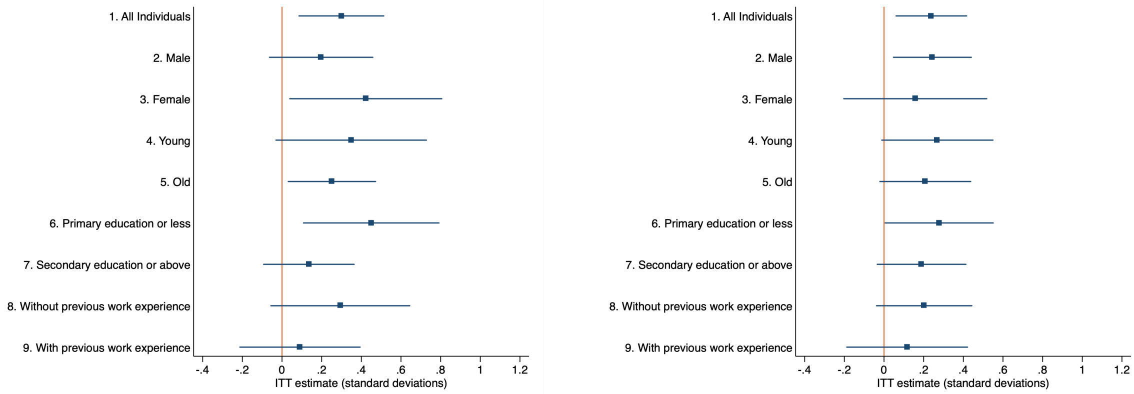
Table B9: Heterogeneous effects of the SEP on employment, by previous work experience

	Inexperienced				Experienced			
	ITT		TOT		ITT		TOT	
	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)	0.030	(0.030)	0.030	(0.030)
Employed in 2015/8	0.031	(0.031)	0.091	(0.080)	-0.019	(0.070)	-0.091	(0.327)
Employed in 2015/9	0.156**	(0.065)	0.455***	(0.159)	0.011	(0.075)	0.051	(0.344)
Employed in 2015/10	0.250***	(0.078)	0.727***	(0.173)	0.122	(0.101)	0.577	(0.421)
Employed in 2015/11	0.312***	(0.083)	0.909***	(0.229)	0.122	(0.101)	0.577	(0.421)
Employed in 2015/12	0.312***	(0.083)	0.909***	(0.229)	0.162*	(0.094)	0.766**	(0.390)
Employed in 2016/1	0.317***	(0.089)	0.923***	(0.228)	0.082	(0.106)	0.389	(0.456)
Employed in 2016/2	0.286***	(0.087)	0.833***	(0.234)	0.052	(0.104)	0.246	(0.451)
Employed in 2016/3	0.286***	(0.087)	0.833***	(0.234)	0.042	(0.111)	0.200	(0.493)
Employed in 2016/4	0.317***	(0.089)	0.923***	(0.259)	0.042	(0.111)	0.200	(0.493)
Employed in 2016/5	0.286***	(0.087)	0.833***	(0.262)	0.042	(0.111)	0.200	(0.502)
Employed in 2016/6	0.286***	(0.087)	0.833***	(0.262)	0.012	(0.109)	0.057	(0.499)
Employed in 2016/7	0.286***	(0.087)	0.833***	(0.262)	-0.038	(0.119)	-0.177	(0.568)
Employed in 2016/8	0.255***	(0.085)	0.742***	(0.262)	-0.078	(0.122)	-0.366	(0.605)
Employed in 2016/9	0.255***	(0.085)	0.742***	(0.262)	-0.047	(0.123)	-0.223	(0.593)
Employed in 2016/10	0.255***	(0.085)	0.742***	(0.262)	-0.047	(0.123)	-0.223	(0.593)
Employed in 2016/11	0.260***	(0.091)	0.756***	(0.265)	0.033	(0.117)	0.154	(0.534)
Employed in 2016/12	0.260***	(0.091)	0.756***	(0.265)	-0.068	(0.117)	-0.320	(0.568)
Employed in 2017/1	0.234**	(0.094)	0.679**	(0.268)	-0.068	(0.117)	-0.320	(0.568)
Employed in 2017/2	0.234**	(0.094)	0.679**	(0.268)	-0.068	(0.117)	-0.320	(0.568)
Employed in 2017/3	0.234**	(0.094)	0.679**	(0.268)	-0.108	(0.119)	-0.509	(0.605)
Employed in 2017/4	0.238**	(0.099)	0.694**	(0.290)	-0.098	(0.114)	-0.463	(0.580)
Employed in 2017/5	0.238**	(0.099)	0.694**	(0.290)	-0.078	(0.122)	-0.366	(0.605)
Employed in 2017/6	0.238**	(0.099)	0.694**	(0.290)	-0.047	(0.123)	-0.223	(0.593)
Employed in 2017/7	0.212**	(0.102)	0.617**	(0.292)	-0.078	(0.122)	-0.366	(0.593)
Employed in 2017/8	0.212**	(0.102)	0.617**	(0.292)	-0.078	(0.122)	-0.366	(0.593)
Employed in 2017/9	0.265***	(0.096)	0.770***	(0.288)	-0.047	(0.123)	-0.223	(0.585)
Employed in 2017/10	0.265***	(0.096)	0.770***	(0.288)	-0.038	(0.119)	-0.177	(0.556)
Employed in 2017/11	0.265***	(0.096)	0.770***	(0.288)	-0.118	(0.124)	-0.554	(0.608)
Employed in 2017/12	0.291***	(0.093)	0.847***	(0.287)	-0.087	(0.126)	-0.411	(0.613)
Employed in 2018/1	0.291***	(0.093)	0.847***	(0.287)	-0.087	(0.126)	-0.411	(0.613)
Employed in 2018/2	0.291***	(0.093)	0.847***	(0.287)	-0.118	(0.124)	-0.554	(0.608)
Employed in 2018/3	0.291***	(0.093)	0.847***	(0.287)	-0.087	(0.126)	-0.411	(0.599)
Employed in 2018/4	0.265***	(0.096)	0.770***	(0.288)	-0.127	(0.127)	-0.600	(0.624)
Employed in 2018/5	0.296***	(0.098)	0.861***	(0.285)	-0.097	(0.129)	-0.457	(0.629)
Employed in 2018/6	0.317***	(0.089)	0.923***	(0.287)	-0.097	(0.129)	-0.457	(0.629)
Employed in 2018/7	0.349***	(0.091)	1.014***	(0.312)	-0.127	(0.127)	-0.600	(0.624)
Employed in 2018/8	0.380***	(0.092)	1.105***	(0.338)	-0.127	(0.127)	-0.600	(0.624)
Employed in 2018/9	0.349***	(0.091)	1.014***	(0.312)	-0.097	(0.129)	-0.457	(0.629)
Employed in 2018/10	0.375***	(0.087)	1.091***	(0.315)	-0.067	(0.130)	-0.314	(0.619)
Employed in 2018/11	0.375***	(0.087)	1.091***	(0.315)	0.004	(0.130)	0.017	(0.600)
Employed in 2018/12	0.375***	(0.087)	1.091***	(0.315)	-0.067	(0.130)	-0.314	(0.619)
Employed in 2019/1	0.406***	(0.088)	1.182***	(0.314)	-0.067	(0.130)	-0.314	(0.619)
Employed in 2019/2	0.322***	(0.094)	0.938***	(0.311)	-0.067	(0.130)	-0.314	(0.619)
Employed in 2019/3	0.291***	(0.093)	0.847***	(0.287)	-0.067	(0.130)	-0.314	(0.619)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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 based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative data. In columns (1)–(4), the sample is restricted to workers with no work experience (N=70); in columns (5)–(8), to workers with some work experiences (N=58). 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.

Figure B7: Heterogenous effects of the SEP on individual skills, short-term estimates

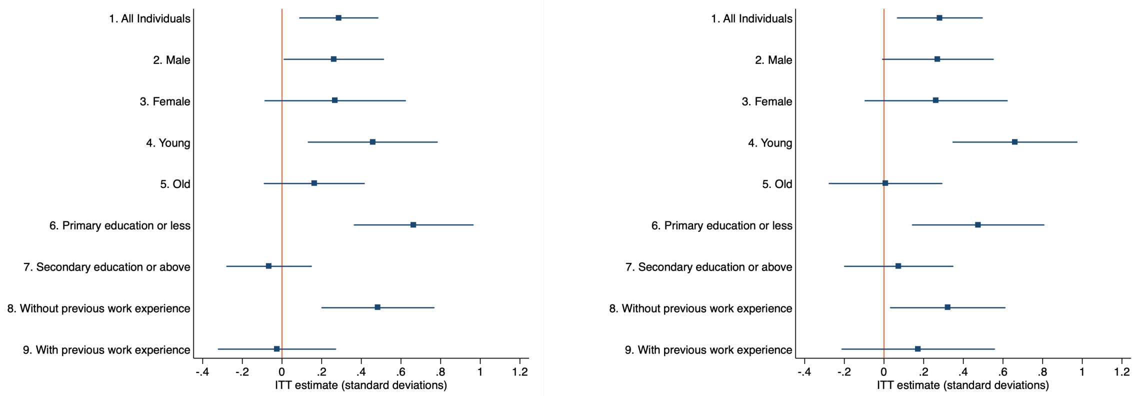
Panel A. Job related skills **Panel B. Noncognitive skills**



Notes: Panel A shows the point estimates and 90% confidence intervals of the estimated effect of the SEP on the job-related skill index. Panel B shows the same for the non-cognitive skill index. Estimated effects are ITT estimates based on the estimation of equation (1) without control variables. The sample includes individuals in the short-term survey. The job-related skill index is based on self-reported indicators for the individual reading, writing, using math and using a pc in the last 12 months. The non-cognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale. The job-related skill index and the non-cognitive skill index are computed following the methodology described in Anderson (2008). Confidence intervals are calculated based on robust standard errors.

Figure B8: Heterogenous effects of the SEP on individual skills, medium-term estimates

Panel A. Job related skills **Panel B. Noncognitive skills**



Notes: Panel A shows the point estimates and 90% confidence intervals of the estimated effect of the SEP on the job-related skill index. Panel B shows the same for the non-cognitive skill index. Estimated effects are ITT estimates based on the estimation of equation (1) without control variables. The sample includes individuals in the medium-term survey. The job-related skill index is based on self-reported indicators for the individual reading, writing, using math and using a pc in the last 12 months. The non-cognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale. The job-related skill index and the non-cognitive skill index are computed following the methodology described in Anderson (2008). Confidence intervals are calculated based on robust standard errors.

C Robustness checks

C.1 Robustness checks: sample selection and employment effects

The estimation sample is the result of merging the administrative data on job seekers' SEP applications and the administrative database of the NEA. About 16 percent of the SEP applicants could not be matched with the NEA employment record database, either because an invalid individual identifier was provided at the time of the application, or because an individual is no longer registered with the NEA. Although applicants should be registered at the NEA, they need to check-in periodically in order to confirm their employment status and they are automatically deleted from the list if they do not comply. We exclude these individuals from the analysis, but keep individuals no longer registered with at least one employment spell in the past. In addition, 25 job seekers have no information on the matching firm. None of these job seekers obtained a SEP job, but half participated in an interview with the employer.

A plausible explanation for the lack of this information is that the matching process proceeded in stages, and the firm identifier might have been lost in one of the subsequent stages. In the first stage of the matching process, a group of job seekers with adequate qualifications for the job opening 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 vacancy but from other municipalities entered a second randomisation round and lottery winners were sent to an interview with the employer. To avoid high commuting costs in geographically larger municipalities, this second randomization stage involved only the 17 municipalities of the capital city of Skopje. 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.

Table C10 replicates the results in Section 4.1 adding firm fixed effects to equations (1) and (2). When doing this, we have to exclude 25 individuals without a valid firm identifier from the estimation sample. The first column reports the mean of the relevant variable in the control group in the post-program period. Columns (2)–(3) show the ITT estimates, while columns

(4)–(5) report the TOT estimates. Estimates in columns (2) and (4) control for the pre-program outcome, where the pre-program period goes from January 2000 to July 2015. For example, the pre-program outcome for the outcome *Ever employed* is a dummy equal to one if the individual had at least one employment spell between January 2000 and July 2015. Columns (3) and (5) control for yearly employment dummies for the 2000-2014 period. Firm fixed effects are included in all regressions. Standard errors are clustered at the firm level.

Table C10: SEP impact on employment outcomes: controlling for firm fixed effects

	Control mean at follow-up	ITT		TOT	
	(1)	(2)	(3)	(4)	(5)
Ever employed	0.31	0.19*	0.19**	0.53**	0.50**
		(0.10)	(0.09)	(0.22)	(0.20)
Days employed	224.94	150.83**	157.36**	445.41**	421.35**
		(71.57)	(79.06)	(183.52)	(172.72)
Ever employed fixed term	0.29	-0.08	-0.10	-0.22	-0.26
		(0.08)	(0.10)	(0.23)	(0.23)
Ever employed unlimited term	0.16	0.19**	0.23**	0.55**	0.61***
		(0.08)	(0.08)	(0.19)	(0.17)
Days employed fixed term	162.02	-65.14	-90.30	-187.85	-241.78*
		(56.87)	(62.68)	(150.41)	(136.95)
Days employed unlimited term	62.92	219.17***	247.66**	648.20***	663.13***
		(64.75)	(87.48)	(168.45)	(200.77)
Labor earnings (1,000 MKD)	87.27	69.07**	68.79**	203.95**	184.20**
		(26.73)	(34.00)	(72.52)	(76.61)
Employment index	0.12	0.41**	0.45**	1.21**	1.20**
		(0.20)	(0.21)	(0.47)	(0.44)
Baseline outcome		Yes	No	Yes	No
Employment history controls		No	Yes	No	Yes
Firm fixed effects		Yes	Yes	Yes	Yes
Observations		103	103	103	103

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors are presented in parentheses and are clustered at the firm level. Each row shows the results of a regression with a different dependent variable. All dependent variables are measured throughout the post-program period (September 2015-March 2019). *Ever employed* is a dummy equal to 1 if the individual has worked for at least one day. *Days employed* is the total number of days the individual has been employed. *Ever employed fixed term* is a dummy equal to 1 if the individual has worked for at least one day in a fixed-term job. *Ever employed unlimited term* is a dummy equal to 1 if the individual has worked for at least one day in an unlimited-term job. *Days employed fixed term* is the total number of days the individual has been employed in a fixed-term job. *Days employed unlimited term* is the total number of days the individual has been employed in an unlimited-term job. *Labor earnings* is the cumulative labor income (product of the daily wage and the number of days employed in a given occupation). The employment index is constructed following the methodology described in Anderson (2008) and includes the following four variables: ever employed, days employed, ever employed unlimited term, labor earnings (we exclude the remaining outcomes as they would be collinear with the variables included). Column (1) shows the average of the dependent variable in the control group in the follow-up period. Columns (2)–(3) present ITT estimates of the program impact (equation (1)). Coefficients correspond to an indicator variable equal to 1 if the individual is part of the treatment group (i.e. is given the opportunity to participate in a job interview). Columns (3)–(4) present TOT estimates (equation (2)). Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Control variables include the age of the applicant and a gender dummy. Results in columns (2) and (4) control for the corresponding baseline outcome (measured throughout the pre-program period until July 2015) in each regression; results in columns (3) and (5) control for a set of yearly employment dummies for the pre-program period (2000-2014, and January-July 2015). Firm fixed effects are included in all regressions.

C.2 Robustness checks: employment dynamics

C.2.1 Additional control variables and alternative estimation samples

In this section, we test the robustness of the results on the employment dynamics to alternative specifications and alternative sample definitions. Figure C9 and Table C11 replicate the results of Figure 3 and Table 3 from the paper by adding the age of the individual and a gender dummy as control variables. As can be seen, both the ITT and the TOT estimates are extremely similar to those reported in the paper.

Next, we check whether the results are robust to the exclusion of individuals for which information on the matching firm could not be retrieved. Figure C10 and Table C12 show that the estimated impacts of the SEP for the sample of program participants with a valid firm identifier are very similar to the main estimates in the paper both in terms of magnitude and statistical significance.

Finally, we test whether the results are robust to the inclusion of firm fixed effects. It should be noted that the estimation sample for this exercise is smaller than the original estimation sample since, as said above, firm identifiers are missing for 25 program participants. Nevertheless, Figure C11 shows that the results are robust to the inclusion of firm fixed effects, as the estimated effect of the SEP on employment over time follows a very similar pattern to the one observed in Figure 3. Estimated effects with firm fixed effects are also reported in Table C13.

Table C11: SEP impact on employment: including demographic characteristics

	ITT			TOT	
	Coeff.	Std. error	Perm. test (p-value)	Coeff.	Std. error
	(1)	(2)	(3)	(4)	(5)
Employed in 2015/7	0.018	(0.018)	0.092	0.068	(0.069)
Employed in 2015/8	0.018	(0.034)	0.567	0.069	(0.124)
Employed in 2015/9	0.099**	(0.047)	0.035	0.368**	(0.165)
Employed in 2015/10	0.196***	(0.060)	0.001	0.731***	(0.187)
Employed in 2015/11	0.226***	(0.062)	0.000	0.842***	(0.207)
Employed in 2015/12	0.240***	(0.060)	0.000	0.895***	(0.204)
Employed in 2016/1	0.204***	(0.066)	0.002	0.758***	(0.215)
Employed in 2016/2	0.179***	(0.065)	0.005	0.666***	(0.213)
Employed in 2016/3	0.175***	(0.067)	0.009	0.653***	(0.221)
Employed in 2016/4	0.191***	(0.067)	0.003	0.712***	(0.228)
Employed in 2016/5	0.170**	(0.066)	0.006	0.631***	(0.238)
Employed in 2016/6	0.158**	(0.066)	0.025	0.587**	(0.233)
Employed in 2016/7	0.127*	(0.071)	0.072	0.473*	(0.250)
Employed in 2016/8	0.098	(0.071)	0.179	0.366	(0.254)
Employed in 2016/9	0.113	(0.072)	0.114	0.420	(0.258)
Employed in 2016/10	0.113	(0.072)	0.159	0.420	(0.258)
Employed in 2016/11	0.141**	(0.070)	0.047	0.527**	(0.253)
Employed in 2016/12	0.098	(0.069)	0.197	0.366	(0.250)
Employed in 2017/1	0.081	(0.070)	0.272	0.302	(0.253)
Employed in 2017/2	0.081	(0.070)	0.263	0.302	(0.253)
Employed in 2017/3	0.064	(0.072)	0.367	0.239	(0.260)
Employed in 2017/4	0.060	(0.072)	0.396	0.224	(0.260)
Employed in 2017/5	0.073	(0.074)	0.324	0.271	(0.266)
Employed in 2017/6	0.084	(0.076)	0.271	0.312	(0.272)
Employed in 2017/7	0.055	(0.076)	0.510	0.204	(0.275)
Employed in 2017/8	0.055	(0.076)	0.448	0.204	(0.275)
Employed in 2017/9	0.106	(0.074)	0.191	0.395	(0.272)
Employed in 2017/10	0.104	(0.072)	0.169	0.388	(0.267)
Employed in 2017/11	0.070	(0.074)	0.337	0.262	(0.270)
Employed in 2017/12	0.104	(0.073)	0.176	0.386	(0.268)
Employed in 2018/1	0.100	(0.074)	0.205	0.371	(0.270)
Employed in 2018/2	0.087	(0.073)	0.238	0.326	(0.269)
Employed in 2018/3	0.098	(0.073)	0.188	0.365	(0.272)
Employed in 2018/4	0.060	(0.075)	0.442	0.224	(0.275)
Employed in 2018/5	0.087	(0.075)	0.239	0.324	(0.274)
Employed in 2018/6	0.112	(0.073)	0.154	0.415	(0.271)
Employed in 2018/7	0.114	(0.073)	0.147	0.423	(0.274)
Employed in 2018/8	0.123*	(0.073)	0.101	0.458*	(0.277)
Employed in 2018/9	0.126*	(0.073)	0.109	0.469*	(0.275)
Employed in 2018/10	0.158**	(0.073)	0.034	0.587**	(0.283)
Employed in 2018/11	0.182**	(0.072)	0.016	0.679**	(0.283)
Employed in 2018/12	0.153**	(0.072)	0.049	0.571**	(0.277)
Employed in 2019/1	0.168**	(0.073)	0.025	0.625**	(0.285)
Employed in 2019/2	0.123*	(0.074)	0.101	0.459*	(0.278)
Employed in 2019/3	0.110	(0.074)	0.165	0.411	(0.274)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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. Columns (4)–(5) present TOT estimates based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Control variables included in the estimation are the age of the individual and a gender dummy. Estimates based on administrative employment data for 128 individuals.

Table C12: SEP impact on employment: excluding individuals without firm identifier

	ITT			TOT	
	Coeff.	Std. error	Perm. test (p-value)	Coeff.	Std. error
	(1)	(2)	(3)	(4)	(5)
Employed in 2015/7	0.019	(0.019)	0.501	0.056	(0.000)
Employed in 2015/8	0.018	(0.043)	0.671	0.053	(0.121)
Employed in 2015/9	0.095*	(0.055)	0.099	0.276*	(0.147)
Employed in 2015/10	0.230***	(0.072)	0.002	0.663***	(0.174)
Employed in 2015/11	0.268***	(0.074)	0.000	0.775***	(0.192)
Employed in 2015/12	0.288***	(0.071)	0.000	0.831***	(0.188)
Employed in 2016/1	0.248***	(0.079)	0.000	0.717***	(0.199)
Employed in 2016/2	0.210***	(0.077)	0.007	0.606***	(0.194)
Employed in 2016/3	0.209**	(0.080)	0.010	0.605***	(0.207)
Employed in 2016/4	0.229***	(0.081)	0.004	0.660***	(0.214)
Employed in 2016/5	0.209**	(0.080)	0.010	0.605***	(0.224)
Employed in 2016/6	0.190**	(0.079)	0.024	0.549**	(0.217)
Employed in 2016/7	0.170**	(0.083)	0.040	0.491**	(0.231)
Employed in 2016/8	0.131	(0.084)	0.125	0.379	(0.235)
Employed in 2016/9	0.150*	(0.085)	0.105	0.435*	(0.240)
Employed in 2016/10	0.150*	(0.085)	0.076	0.435*	(0.240)
Employed in 2016/11	0.189**	(0.084)	0.030	0.547**	(0.237)
Employed in 2016/12	0.131	(0.084)	0.116	0.379	(0.235)
Employed in 2017/1	0.112	(0.086)	0.174	0.322	(0.238)
Employed in 2017/2	0.112	(0.086)	0.202	0.322	(0.238)
Employed in 2017/3	0.092	(0.087)	0.312	0.266	(0.242)
Employed in 2017/4	0.092	(0.087)	0.308	0.266	(0.242)
Employed in 2017/5	0.111	(0.090)	0.223	0.320	(0.249)
Employed in 2017/6	0.130	(0.090)	0.149	0.376	(0.253)
Employed in 2017/7	0.091	(0.091)	0.312	0.264	(0.256)
Employed in 2017/8	0.091	(0.091)	0.304	0.264	(0.256)
Employed in 2017/9	0.130	(0.088)	0.140	0.377	(0.250)
Employed in 2017/10	0.131	(0.086)	0.142	0.378	(0.248)
Employed in 2017/11	0.092	(0.089)	0.298	0.265	(0.253)
Employed in 2017/12	0.130	(0.088)	0.155	0.377	(0.250)
Employed in 2018/1	0.130	(0.088)	0.144	0.377	(0.250)
Employed in 2018/2	0.111	(0.088)	0.216	0.321	(0.250)
Employed in 2018/3	0.130	(0.088)	0.148	0.377	(0.255)
Employed in 2018/4	0.091	(0.091)	0.326	0.264	(0.259)
Employed in 2018/5	0.130	(0.092)	0.158	0.375	(0.259)
Employed in 2018/6	0.150*	(0.089)	0.101	0.432*	(0.255)
Employed in 2018/7	0.150*	(0.089)	0.093	0.432*	(0.261)
Employed in 2018/8	0.169*	(0.090)	0.076	0.488*	(0.267)
Employed in 2018/9	0.169*	(0.090)	0.053	0.488*	(0.261)
Employed in 2018/10	0.208**	(0.089)	0.020	0.600**	(0.267)
Employed in 2018/11	0.247***	(0.088)	0.007	0.712***	(0.274)
Employed in 2018/12	0.208**	(0.089)	0.014	0.600**	(0.267)
Employed in 2019/1	0.227**	(0.089)	0.012	0.656**	(0.274)
Employed in 2019/2	0.169*	(0.091)	0.082	0.487*	(0.267)
Employed in 2019/3	0.149	(0.091)	0.106	0.431*	(0.262)

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 based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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. Columns (4)–(5) present TOT estimates based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Estimates based on administrative employment data for 103 individuals with a valid firm identifier.

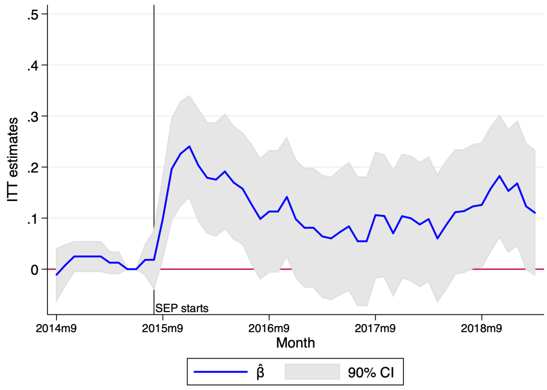
Table C13: SEP impact on employment: controlling for firm fixed effects

	ITT			TOT	
	Coeff.	Std. error	Perm. test (p-value)	Coeff.	Std. error
	(1)	(2)	(3)	(4)	(5)
Employed in 2015/7	0.016	(0.017)	0.722	0.046	(0.046)
Employed in 2015/8	0.014	(0.044)	0.773	0.039	(0.115)
Employed in 2015/9	0.097	(0.059)	0.136	0.278**	(0.141)
Employed in 2015/10	0.213***	(0.072)	0.011	0.612***	(0.157)
Employed in 2015/11	0.252***	(0.074)	0.003	0.726***	(0.174)
Employed in 2015/12	0.274***	(0.072)	0.003	0.789***	(0.170)
Employed in 2016/1	0.231***	(0.079)	0.013	0.664***	(0.179)
Employed in 2016/2	0.199**	(0.077)	0.029	0.573***	(0.175)
Employed in 2016/3	0.200**	(0.082)	0.023	0.575***	(0.192)
Employed in 2016/4	0.222***	(0.083)	0.015	0.640***	(0.201)
Employed in 2016/5	0.203**	(0.081)	0.027	0.583***	(0.206)
Employed in 2016/6	0.185**	(0.080)	0.037	0.532***	(0.201)
Employed in 2016/7	0.165**	(0.083)	0.085	0.475**	(0.207)
Employed in 2016/8	0.123	(0.083)	0.176	0.355*	(0.209)
Employed in 2016/9	0.139	(0.084)	0.157	0.401*	(0.215)
Employed in 2016/10	0.139	(0.084)	0.142	0.401*	(0.215)
Employed in 2016/11	0.179**	(0.083)	0.068	0.514**	(0.213)
Employed in 2016/12	0.116	(0.084)	0.212	0.332	(0.213)
Employed in 2017/1	0.096	(0.083)	0.309	0.275	(0.212)
Employed in 2017/2	0.096	(0.083)	0.323	0.275	(0.212)
Employed in 2017/3	0.076	(0.085)	0.407	0.219	(0.217)
Employed in 2017/4	0.080	(0.084)	0.390	0.230	(0.216)
Employed in 2017/5	0.096	(0.086)	0.339	0.277	(0.218)
Employed in 2017/6	0.118	(0.087)	0.217	0.340	(0.222)
Employed in 2017/7	0.074	(0.088)	0.455	0.212	(0.227)
Employed in 2017/8	0.074	(0.088)	0.434	0.212	(0.227)
Employed in 2017/9	0.109	(0.086)	0.277	0.315	(0.224)
Employed in 2017/10	0.116	(0.085)	0.221	0.332	(0.224)
Employed in 2017/11	0.072	(0.089)	0.475	0.207	(0.231)
Employed in 2017/12	0.110	(0.088)	0.258	0.318	(0.227)
Employed in 2018/1	0.114	(0.089)	0.241	0.329	(0.230)
Employed in 2018/2	0.092	(0.089)	0.353	0.264	(0.232)
Employed in 2018/3	0.108	(0.089)	0.263	0.310	(0.234)
Employed in 2018/4	0.068	(0.089)	0.512	0.196	(0.233)
Employed in 2018/5	0.110	(0.091)	0.271	0.318	(0.233)
Employed in 2018/6	0.122	(0.088)	0.226	0.352	(0.230)
Employed in 2018/7	0.117	(0.088)	0.237	0.337	(0.235)
Employed in 2018/8	0.137	(0.087)	0.170	0.394*	(0.237)
Employed in 2018/9	0.140	(0.088)	0.160	0.402*	(0.233)
Employed in 2018/10	0.180**	(0.087)	0.061	0.516**	(0.238)
Employed in 2018/11	0.219**	(0.087)	0.019	0.630**	(0.246)
Employed in 2018/12	0.182**	(0.088)	0.072	0.523**	(0.241)
Employed in 2019/1	0.199**	(0.088)	0.040	0.573**	(0.246)
Employed in 2019/2	0.144	(0.089)	0.139	0.414*	(0.238)
Employed in 2019/3	0.124	(0.088)	0.214	0.357	(0.232)

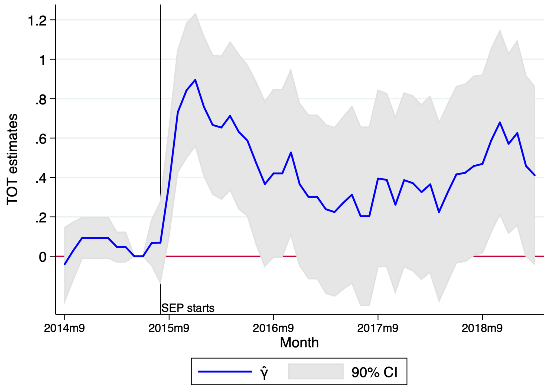
Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors are presented in parentheses. The dependent variables are indicator variables equal to 1 if the worker is employed in the correspondent period and 0 otherwise. Columns (1)–(2) present ITT estimates of the program impact based on the estimation of equation (1) without control variables. Coefficients correspond to an indicator variable equal to 1 if the individual 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. Columns (4)–(5) present TOT estimates based on the estimation of equation (2) without control variables. Coefficients correspond to a dummy variable equal to 1 if the individual is offered a job as part of the program, and is instrumented using the random assignment into the job interview. Both regressions include firm fixed effects. Estimates based on administrative employment data for 103 individuals with a valid firm identifier.

Figure C9: SEP impact on employment dynamics: including demographic characteristics

A. ITT estimates



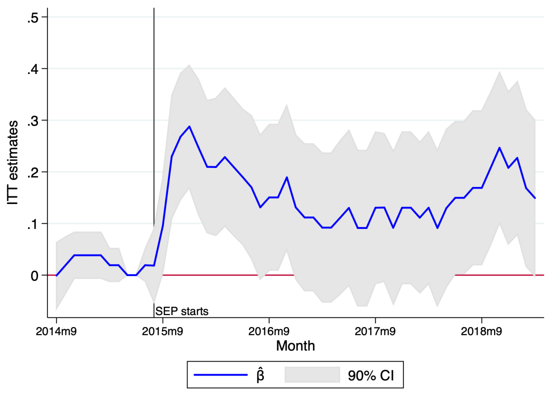
B. TOT estimates



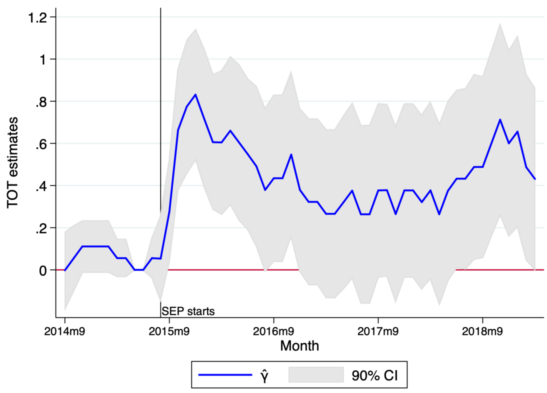
Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid line in panel A shows estimates of β from estimation of equation (1) without control variables. The solid line in panel B shows estimates of γ from estimation of equation (2) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals. Individual controls include the age of the individual and a gender dummy.

Figure C10: SEP impact on employment dynamics: excluding individuals without firm identifier

A. ITT estimates

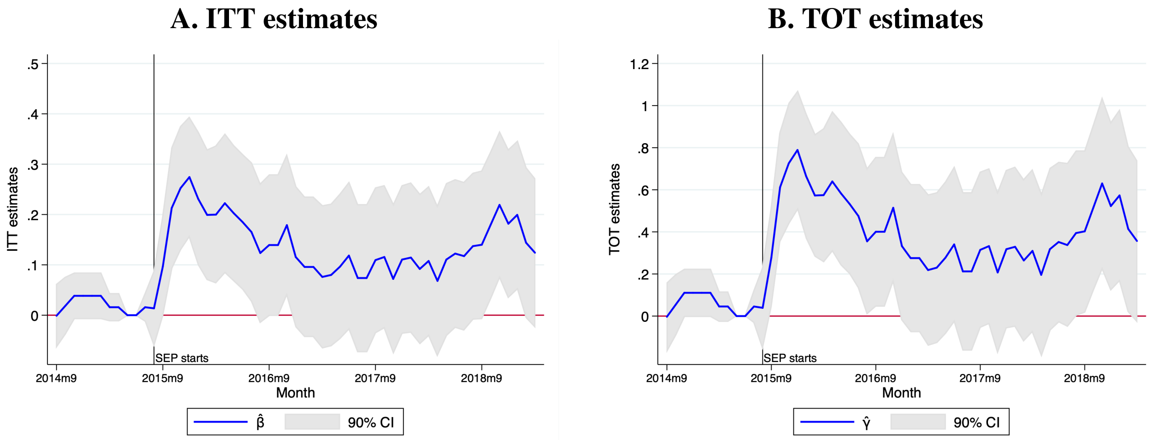


B. TOT estimates



Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid line in panel A shows estimates of β from estimation of equation (1) without control variables. The solid line in panel B shows estimates of γ from estimation of equation (2) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 103 individuals with a valid firm identifier.

Figure C11: SEP impact on employment dynamics: controlling for firm fixed effects



Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid line in panel A shows estimates of β from estimation of equation (1) without control variables. The solid line in panel B shows estimates of γ from estimation of equation (2) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 103 individuals with a valid firm identifier. Firm fixed effects are included in all regressions.

C.2.2 Controlling for individual's pre-program employment history

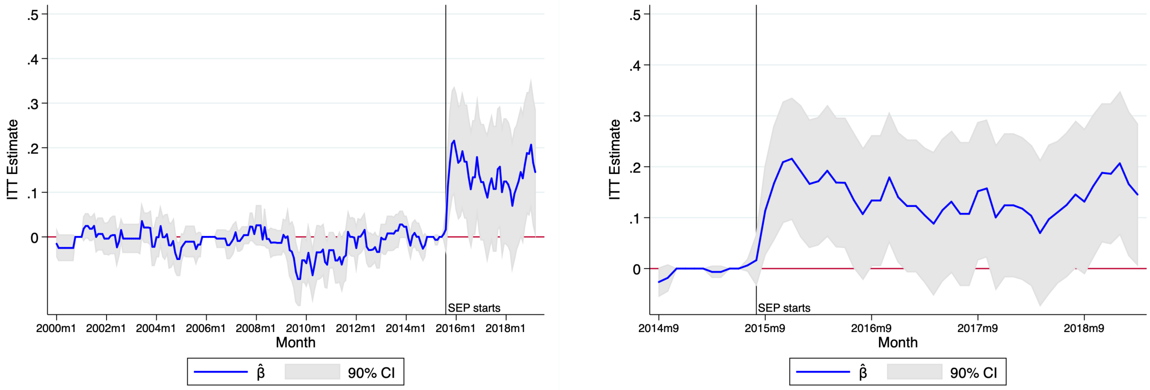
In Section B.3 of the Appendix, we showed the existence of some imbalance in the pre-program employment rates of the treatment and control groups. In this section, we estimate the effect of the SEP program on monthly employment by controlling for the individual employment history in the pre-program period. Specifically, we construct 15 employment dummies, one for each year within the 2000-2015 pre-program period, which are equal to one if the individual was employed for at least one day in the corresponding year.[9] We then add the full set of pre-program employment dummies into equation (1) and (2), and plot the estimated program effects on each month in the post-program period.

Figure C12 shows the ITT estimates, whereas Figure C13 shows the TOT estimates. In order to show that the inclusion of the pre-program employment dummies effectively eliminates the imbalance observed in Figure B3, we include two panels in each figure. Panel A shows the full individual employment history at the monthly level between January 2000 and March 2019. Panel B zooms into the post-program period in order to visualize more clearly the estimated program effect. We observe that the inclusion of the employment dummies eliminates the imbalance between the treatment and the control group. Moreover, estimated program effects in the post-program periods remain large in magnitude and statistically significant throughout most of the period of analysis.

[9]The yearly employment dummy for 2015 refers to the pre-program period between January and July 2015.

Figure C12: ITT estimates: controlling for the individual's pre-program employment history

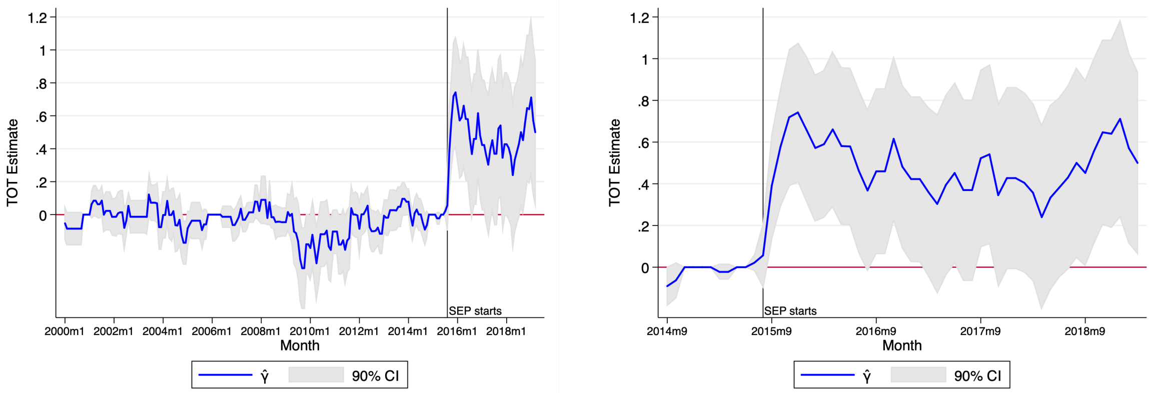
A. Jan. 2000-Mar. 2019 **B. Sep. 2014-Mar. 2019**



Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid lines in panel A and B show estimates of β from estimation of equation (1) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals. Controls included in the regressions are 15 year-level employment dummies. An employment dummy for a given year is defined as an indicator equal to 1 if the individual has at least one employment spell in that year.

Figure C13: TOT estimates: controlling for the individual's pre-program employment history

A. Jan. 2000-Mar. 2019 **B. Sep. 2014-Mar. 2019**



Notes: The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month. The solid lines in panel A and B show estimates of γ from estimation of equation (2) without control variables. In both figures, bands around the solid lines are 90% confidence intervals. The vertical lines in both panel A and panel B indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals. Controls included in the regressions are 15 year-level employment dummies. An employment dummy for a given year is defined as an indicator equal to 1 if the individual has at least one employment spell in that year.

C.2.3 Alternative definitions of employment

In this section, we test the robustness of the results in Section 4.2 to alternative definitions of employment. In the paper, we considered an individual to be employed in a given month if he/she worked for at least one day in that month. Here, we define an individual to be employed if he/she worked for at least 5, 10 or 15 days in a month. We report ITT and TOT estimates in Figure C14 and Figure C15, respectively. For comparison, Panel A in each figure corresponds to the benchmark definition from the paper (and therefore Panel A of Figures C14 and C15 are identical to Figure 3).

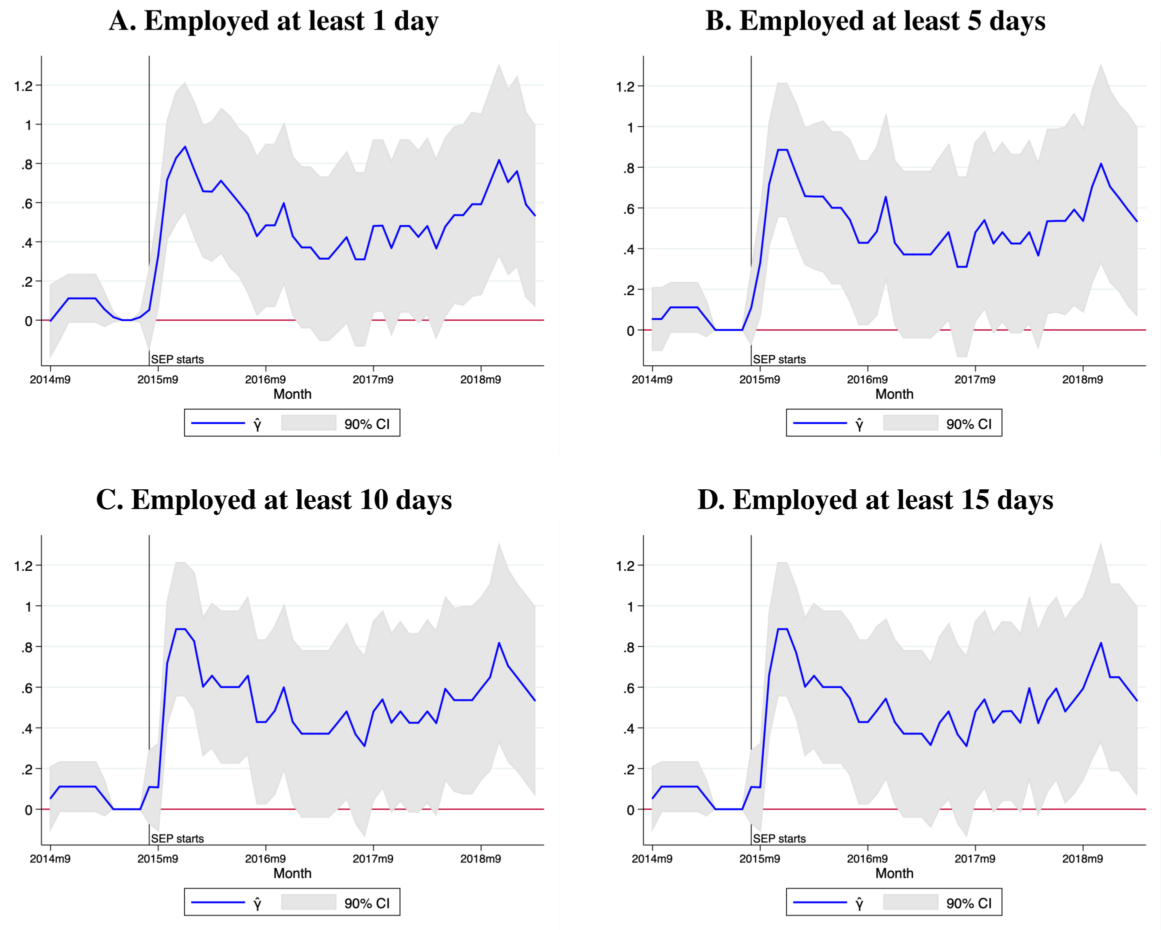
Figure C14: ITT estimates on employment dynamics: alternative employment definitions

A. Employed at least 1 day **B. Employed at least 5 days**

C. Employed at least 10 days **D. Employed at least 15 days**

Notes: The dependent variable is a dummy equal to 1 if the individual is employed for at least 1 (Panel A), 5 (Panel B), 10 (Panel C) or 15 (Panel D) days in the corresponding month. The solid lines in show estimates of β from estimation of equation (1) without control variables. Bands around the solid lines are 90% confidence intervals. The vertical lines indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

Figure C15: TOT estimates on employment dynamics: alternative employment definitions



Notes: The dependent variable is a dummy equal to 1 if the individual is employed for at least 1 (Panel A), 5 (Panel B), 10 (Panel C) or 15 (Panel D) days in the corresponding month. The solid lines in show estimates of γ from estimation of equation (2) without control variables. Bands around the solid lines are 90% confidence intervals. The vertical lines indicate the month in which the SEP started (September 2015). The estimation sample includes 128 individuals.

C.3 Robustness checks: other outcomes

Table C14 replicates the estimates in Table 5 adding individual control variables and a measure of individual's abstract reasoning. Controls include the age of the individual, a gender dummy and an indicator for the individual having attained primary education or no education. The abstract reasoning measure is based on the Abbreviated Raven's test of progressive matrices.

Table C14: Short- and medium-term impact of the SEP on individual skills: controlling for individual characteristics

	Short-term impact							Medium-term impact						
	ITT			TOT			Obs	ITT			TOT			Obs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Job related skills														
Job-relatd skill index	0.36***	0.33***	0.30**	1.43**	1.33**	1.23**	94	0.26**	0.22*	0.22*	1.01**	0.88*	0.86*	87
	(0.13)	(0.12)	(0.12)	(0.59)	(0.55)	(0.57)		(0.12)	(0.12)	(0.12)	(0.51)	(0.51)	(0.51)	
Read in the last 12 months	0.47***	0.42**	0.41**	1.87**	1.70**	1.72**	94	0.56***	0.45**	0.44**	2.12**	1.81**	1.73**	87
	(0.17)	(0.17)	(0.17)	(0.79)	(0.77)	(0.81)		(0.19)	(0.18)	(0.17)	(0.87)	(0.83)	(0.79)	
Wrote in the last 12 months	0.36**	0.34**	0.32*	1.46*	1.37*	1.31	94	0.36**	0.33*	0.32*	1.37*	1.32*	1.27*	87
	(0.17)	(0.17)	(0.18)	(0.78)	(0.78)	(0.83)		(0.18)	(0.17)	(0.17)	(0.75)	(0.77)	(0.75)	
Used math in the last 12 months	0.13	0.13	0.09	0.53	0.51	0.36	93	0.17	0.18	0.17	0.64	0.70	0.65	81
	(0.19)	(0.18)	(0.20)	(0.77)	(0.72)	(0.80)		(0.21)	(0.21)	(0.20)	(0.78)	(0.83)	(0.80)	
Used pc in the last 12 months	0.48**	0.44**	0.39**	1.90**	1.78**	1.61**	94	0.27	0.21	0.21	1.03	0.82	0.85	87
	(0.21)	(0.19)	(0.19)	(0.87)	(0.75)	(0.78)		(0.21)	(0.21)	(0.21)	(0.80)	(0.83)	(0.82)	
Noncognitive skills														
Noncognitive skill index	0.28***	0.23**	0.20*	1.13**	0.93**	0.84*	94	0.26*	0.22*	0.22*	1.01*	0.87*	0.88*	87
	(0.11)	(0.10)	(0.11)	(0.45)	(0.44)	(0.44)		(0.13)	(0.13)	(0.13)	(0.54)	(0.51)	(0.51)	
Extraversion	0.28	0.21	0.20	1.13	0.84	0.82	94	0.32	0.26	0.26	1.21	1.05	1.03	86
	(0.18)	(0.18)	(0.19)	(0.76)	(0.74)	(0.78)		(0.21)	(0.21)	(0.22)	(0.81)	(0.81)	(0.82)	
Agreeableness	0.29	0.27	0.26	1.18	1.10	1.06	94	0.25	0.19	0.21	0.97	0.76	0.84	86
	(0.19)	(0.19)	(0.19)	(0.75)	(0.75)	(0.80)		(0.22)	(0.22)	(0.22)	(0.89)	(0.88)	(0.87)	
Conscientiousness	0.38*	0.33*	0.23	1.53*	1.33*	0.96	94	0.34	0.29	0.30	1.29	1.15	1.19	86
	(0.20)	(0.19)	(0.20)	(0.81)	(0.76)	(0.79)		(0.23)	(0.24)	(0.23)	(0.89)	(0.93)	(0.91)	
Neuroticism (inverted scale)	0.38*	0.36*	0.36*	1.51*	1.44*	1.49	94	0.42*	0.47**	0.48**	1.60*	1.87**	1.92**	86
	(0.19)	(0.20)	(0.21)	(0.84)	(0.87)	(0.94)		(0.22)	(0.22)	(0.21)	(0.96)	(0.95)	(0.94)	
Openness	0.42*	0.32	0.27	1.68*	1.30	1.12	94	0.23	0.17	0.17	0.89	0.68	0.66	86
	(0.21)	(0.22)	(0.22)	(0.92)	(0.91)	(0.93)		(0.20)	(0.20)	(0.20)	(0.77)	(0.76)	(0.77)	
Grit	0.00	-0.06	-0.14	0.01	-0.26	-0.59	93	0.05	-0.00	0.00	0.19	-0.02	0.01	87
	(0.21)	(0.19)	(0.19)	(0.88)	(0.81)	(0.85)		(0.21)	(0.21)	(0.21)	(0.76)	(0.81)	(0.79)	
Individual controls	NO	YES	YES	NO	YES	YES		NO	YES	YES	NO	YES	YES	
Raven test	NO	NO	YES	NO	NO	YES		NO	NO	YES	NO	NO	YES	

Notes: * p<0.10, ** p<0.05, *** p<0.01. Columns (1)–(7) show the short term impact of the SEP based on the first wave of the applicant survey. Columns (8)–(14) show the medium term impact of the SEP based on the second wave of the applicant survey. Columns (1)–(3) and (8)–(10) report ITT estimates based on the estimation of equation (1). Columns (4)–(6) and (11)–(13) report TOT estimates based on the estimation of equation (2). Numbers in parenthesis are robust standard errors. The job-related skill index is based on self-reported indicators for the individual reading, writing, using math and using a PC in the last 12 months. The non-cognitive skill index is based on the Big-5 questionnaire and the 12-item grit scale. The job-related skill index and non-cognitive skill index are computed following the methodology described in Anderson (2008). Individual controls include the age of the applicant, a gender dummy and a dummy for the individual having attained primary education or less. The Raven test is the Abbreviated Raven's test of progressive matrices (standardized to be mean zero and standard deviation 1 in the control group).

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