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Can subsidized employment tackle long-term unemployment? Experimental evidence from North Macedonia[☆]

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ABSTRACT

This study experimentally assesses the effects of temporary wage subsidies on employment in North Macedonia. The target group consists of vulnerable unemployed individuals participating in an employment program that provides employers with a subsidy covering half of the wage payments during the first year of employment, as well as training expenses. Applicants are initially matched to job openings and then randomly selected for job interviews with employers, who decide whether to hire them under the program's conditions. Using administrative records, we find that being selected for an interview results in a 14-percentage-point increase in the probability of being employed in the formal economy 3.5 years after the start of the program, and an 85% increase in employment duration.

Subsidized private sector employment programs have substantial potential to promote employment, especially among the long-term unemployed. However, evidence about these programs comes primarily from high-income countries (Card et al., 2018).[1] For low- and middle-income countries (LMICs), evidence remains scarce and points to their ineffectiveness (Betcherman et al., 2004; Almeida et al., 2012; McKenzie, 2017). Limited effects of wage subsidy programs in LMICs

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[1] Although early studies suggest that wage subsidies are ineffective in tackling unemployment (Burtless, 1985; Dubin and Rivers, 1993; Cockx et al., 1998), more recent evidence shows the opposite both in the short and long run (Card and Hyslop, 2005; Jespersen et al., 2008; Heinesen et al., 2013). A related body of literature examines 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 revealing information about their abilities has a positive impact on their subsequent employment.

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have been associated with burdensome hiring regulations deterring firms from participating (Galasso et al., 2004), or with intervention designs incentivizing employment primarily in the short run (Groh et al., 2016). In addition, although matching or screening of job seekers has proven crucial for improving the employment prospects of disadvantaged groups in high-income countries (Katz, 1998; Algan et al., 2020), most wage subsidy programs in LMICs lack these features, and the evidence on the effectiveness of the few programs that do include them is mixed (Hardy et al., 2019; Alfonsi et al., 2020; Crépon and Premand, 2024).

In this paper we experimentally examine the impact of gaining access to subsidized employment in North Macedonia, a middle-income country. Launched by the Ministry of Labor and Social Policy in 2015, the Subsidized Employment Program (SEP) proposes to stimulate employment among long-term unemployed individuals by combining two components: (i) the matching of eligible program applicants to potential vacancies posted by participating employers; (ii) a subsidy provided to the employer, conditional on hiring a matched job applicant. The subsidy comprises a six-month transfer intended to fully cover the wage of the employee during this period, and an additional transfer to cover the training costs. In exchange, the employer commits to a minimum employment duration of 12 months.

For the purpose of the program evaluation, during the launch of the SEP, an initial cohort of applicants was first matched to available vacancies based on the specific skill requirements of each job and then randomly assigned to either a treatment or a control group. Matched candidates in the *treatment* group were invited to a job interview with the employer for the subsidized position. To secure the job, candidates needed to succeed in the interview and receive a job offer. Those who received and accepted an offer became employed under the program's conditions, allowing the employer to receive the program's benefits. The remaining subset of matched candidates (the *control* group) was not invited to attend any interview.

We use two data sources. First, we gather administrative records from the National Employment Agency (NEA). This dataset measures formal employment among study participants up to 3.5 years after the start of the program, beyond the time horizon of most programs in the literature. Second, we use two rounds of individual- and household-level surveys to assess the short- and medium-term impacts on job seekers' skills and other household outcomes.

The invitation to the job interview has a remarkably large effect on formal employment, both in the short and in the long run. As compared to job seekers in the control group, those who were invited to the interview experience an increase of 85%–94% in the duration of formal employment, and an increase of 16–21 percentage points in the probability of being employed in the formal economy in the first 6 months following the interview (although the confidence intervals also include more modest effects). This effect declines over time, mainly because job seekers in the control group find formal employment outside the program. Nevertheless, 42 months after the start of the program, job seekers in the treatment group exhibit a statistically significant 14-percentage-point higher probability of being formally employed and a 22-percentage-point higher probability of being employed with an open-ended (as opposed to a fixed-term) contract.

Our experimental design allows us to estimate not only the impact of being offered a job interview, but also the impact of receiving a subsidized employment offer. Under the assumption that the interview only affects subsequent outcomes through its effect on receiving subsidized employment, we estimate this effect by instrumenting the offer of employment with the random assignment of the applicant to the offer of an interview. We find that being selected for one of the subsidized jobs leads to an increase of 56 percentage points in the probability of being formally employed 42 months after the start of the intervention. A cost–benefit analysis further reveals that the increase in earnings among job seekers who received the job offer compensates the costs associated with the program in less than two years.

While our study is not designed to isolate the specific channels driving the long-term effects of the program on employment, we offer suggestive evidence on the likely mechanisms. First, we find that employment effects are particularly large for individuals with lower attachment to the labor market, such as women and those without prior formal employment experience. This finding suggests that the intervention may have mitigated information frictions, allowing employers to assess the quality of workers with limited ability to signal productivity. Second, we document positive effects on several measures of job-related tasks and noncognitive skills, suggesting that program beneficiaries may have acquired enough experience and productivity gains during the guaranteed employment period to retain their jobs after the subsidy ended. Finally, we explore whether the eligibility rules of the program, which require firms to keep the hired worker for at least 12 months and restrict the layoff of existing workers, might have induced firms with more stable labor demand to select into the program. Participating firms are more concentrated in manufacturing, but are otherwise similar in size and in the use of fixed-term contracts compared to the broader firm population, suggesting that firm selection may not be the main driver of the persistent employment effects.

Previous evidence on subsidized employment in LMICs, which relies primarily on experimental variation in the provision of wage vouchers, has found these programs to be mainly ineffective in the long run. Galasso et al. (2004) study Argentina's Proempleo Experiment, a program offering welfare recipients a wage subsidy voucher valid for up to 18 months, and show that the burdensome registration of any worker hired through the program and the large penalty for the firms for firing the worker after the end of the subsidy led to no effect on employment. Similar findings are observed by Levinsohn et al. (2014) in South Africa. In Jordan, Groh et al. (2016) study the effect of providing wage vouchers among recent college graduates and observe a large effect on employment, but limited to the duration of the voucher (6 months). The vanishing effect of these policies in the long run is also observed when the subsidy is provided to firms instead of workers (De Mel et al., 2010, 2019), or in programs supporting firms during temporary demand and/or liquidity shocks (Bruhn, 2020).

Our study contributes to this literature by showing that subsidized employment programs have the potential to boost long-term employment even in countries with high structural unemployment. To our knowledge, our research design is one of the first to experimentally vary access to job interviews in the context of subsidized employment, providing a novel and straightforward way to evaluate interventions that combine wage subsidies with matching or screening services. Because matching job seekers to existing openings ensures that individuals randomly invited to an interview have a comparable set of skills to those in the control group, our design provides a robust counterfactual to assess the labor market performance of job seekers in the absence of the intervention.

Our results also complement a growing body of research on active labor market policies in LMICs, both beyond and in conjunction with subsidized employment (see Kluve et al., 2019 for a review). These policies include interventions that address information frictions and search costs, such as lowering the cost of job search through transport subsidies (Abebe et al., 2021), or improving job seekers' ability to signal their skills (Bassi and Nansamba, 2022). Other studies focus on apprenticeship and training programs, which typically seek to improve youth employment by supporting skill development (Carranza and McKenzie, 2024). Among these, Hardy et al. (2019), Alfonsi et al. (2020) and Crépon and Premand (2024) examine programs that combine the matching of apprentices to firms with financial incentives for firms to hire and train them.[2] In these studies, apprentices are

[2] Training costs faced by firms can be substantial, especially in high-skill sectors (Caicedo et al., 2022). Brown et al. (2024) show that providing financial incentives to firms tied to apprentices' performance can improve the quality of training and increase apprentices' earnings.

typically randomly matched to a firm in the same local labor market or sector. In contrast, the intervention studied in this paper involves more customized matching based on firm requirements and job seekers' qualifications. While our estimated employment effects are larger than those reported in related studies, we also observe a significant mismatch between the skills of job seekers and the needs of employers, as many applicants cannot be matched to a job opening. Thus, our results highlight a potential policy trade-off between effectiveness and reach in the design of employment programs.

We organize the remainder of the paper as follows. Section 1 describes the institutional background and the intervention. Section 2 outlines the experimental design. Section 3 describes the data, while Section 4 presents the empirical strategy. Section 5 presents the quantitative results, and finally Section 6 concludes.

1. Background and intervention

The SEP was launched by the Ministry of Labor and Social Policy of North Macedonia in the summer of 2015 to tackle systemic unemployment among individuals at risk of social exclusion. When the program was launched, the labor market was characterized by extremely low levels of formal employment, the prevalence of long-term unemployment, and limited protection for the unemployed (IMF, 2016). In 2014, the total unemployment rate was 28%, raising to 54% for the age group 15–24 and 40% for individuals with less than upper secondary education (World Bank, 2023). More than 80% of the unemployed had been out of work for over a year (Macedonian State Statistical Office, 2014). Despite these levels, only 9% of the unemployed population was covered by unemployment benefits, while the remainder was included in a long-term financial assistance scheme known as Social Financial Assistance (SFA)—a means-tested minimum guaranteed income targeted at individuals who are fit for work, but cannot support themselves (Petreski and Mojsoska-Blazevski, 2017).[3]

Eligible job seekers are SFA recipients and other vulnerable groups who are registered as active employment seekers in the NEA (see Appendix Section A.1 for the detailed list of eligibility criteria). Eligible firms are all firms that have not experienced a reduction in total employment between the date the program was launched and the date the employer applied to the program. These eligibility rules are somehow more restrictive than those of other firm-side wage subsidy programs in LMICs.[4] To participate in the program, both job seekers and potential employers have to file and submit an application. Job seekers have to document their qualifications and skills, including their level of education and any previous work experience. Firms have to specify the number of available vacancies and the desired skills for each job vacancy.

The first component of the program is the screening of job seekers and their matching to available job openings. This activity is carried out by the NEA, which assigns job seekers to vacancies taking into account the requirements specified by employers in their applications (e.g., education level, previous experience in the occupation or sector, or any skill required for the job), as well as the geographic proximity of job seekers and firms. For each job opening, if one or more qualified candidates could be identified, the NEA would present the profiles of the selected job seekers to the employer, and schedule job interviews. Note that this activity is not unique to the program, since the NEA adopts screening procedures for all vacancies they manage. The mismatch between the demand and supply of skills is an important constraint in the country (Schwab et al., 2014).

The employer has full discretion over which interviewed candidates receive a job offer. An employer who decides to hire a candidate under the program signs a contract with the NEA, establishing the rights and obligations of each party. A provision in this contract does not allow the firm to reduce its total employment for the whole duration of the contract, to avoid substitution of unsubsidized for subsidized workers. In addition, the selected employee signs an employment contract with the employer, governed by the country's industrial relations laws.

The program requires the firm to hire a worker for a full-time position of at least 40 h per week, and does not impose any requirement on whether the employment contract should be open-ended or fixed-term.[5] In return, the employer receives a wage subsidy and training support for the first six months of the employment relationship. The amount of the subsidy depends on the qualifications required in the job opening: for beneficiaries with a higher educational degree, or being able to perform more complex tasks, the subsidy corresponds to 17,000 MKD (303 US$) per month per employee; for the other workers, the subsidy amounts to 14,900 MKD (266 US$) per month per employee.[6] Subsidies correspond to 54% and 47% of the average wage in formal employment (31,644 MKD; Ministry of Finance, 2023), respectively. Training support consists of a monthly subsidy of 5,000 MKD (89 US$) per worker.[7]

After the first six months of employment, transfers are discontinued, but employers must keep the worker employed for an additional period of six months. If an employer terminates the contract before the end of the compulsory employment period, they are required either to hire another eligible job seeker or to return the funds received (plus interest). The NEA conducts periodic monitoring visits to ensure that employers fulfil their contractual obligations, including providing training to the hired worker and verifying the beneficiary's attendance at the workplace. While the salary paid to the employee during the first six months cannot be lower than the wage subsidy, no requirement is set for the six months of unsubsidized employment.

Individuals hired through the program lose their right to social assistance. Although job seekers who receive a job offer are not obligated to accept it, the program incentivizes acceptance by stipulating that those who refuse suitable employment offers or voluntarily leave their job are excluded from receiving social assistance for six months.

2. Experimental design

Fig. 1 summarizes the timeline of the experiment. The application process opened in June 2015, after the program was announced and

[3] The scheme represents the most significant income support in the country, supporting 11% of the population (World Bank, 2009; Armand et al., 2020). Beneficiary households are entitled to a benefit increasing with household size and decreasing with time spent on SFA, with a maximum of 5515 MKD (98 US$) for households with five or more members (Gotcheva et al., 2013). Among the recipients, the transfer typically represents more than 25% of the household expenditure (World Bank, 2009).

[4] For instance, a wage subsidy program in Mexico supporting firms during an economic crisis required firms to tie layoffs to the percentage drop in sales they experienced to qualify for the subsidy (Bruhn, 2020). On the other hand, firing restrictions were not included in wage subsidy program in Sri Lanka studied by De Mel et al. (2010, 2019), as the program targeted microenterprises with at most one employee.

[5] The cost of firing a worker on an open-ended contract is low (OECD, 2015). Severance pay, equivalent to one month's net salary for a worker with up to five years of tenure, is granted only in cases of dismissal for economic reasons. This is comparable to severance rules in other countries where similar programs have been implemented (Groh et al., 2016; De Mel et al., 2019).

[6] The legal minimum wage in 2015 was 13,900 MKD (248 US$). The average nominal exchange rate with US dollars in July 2015 was 56.03 MKD/US$ (National Bank of the Republic of North Macedonia, 2015).

[7] The program offers three modalities that differ in the duration and extent of the wage subsidy, as well as in the employers' obligations. Although employers could choose their preferred modality when applying to the program, all employers selected the modality discussed in the main text (NEA, 2016). Appendix Section A.2 provides further details about the modalities and discusses why all firms made the same choice.

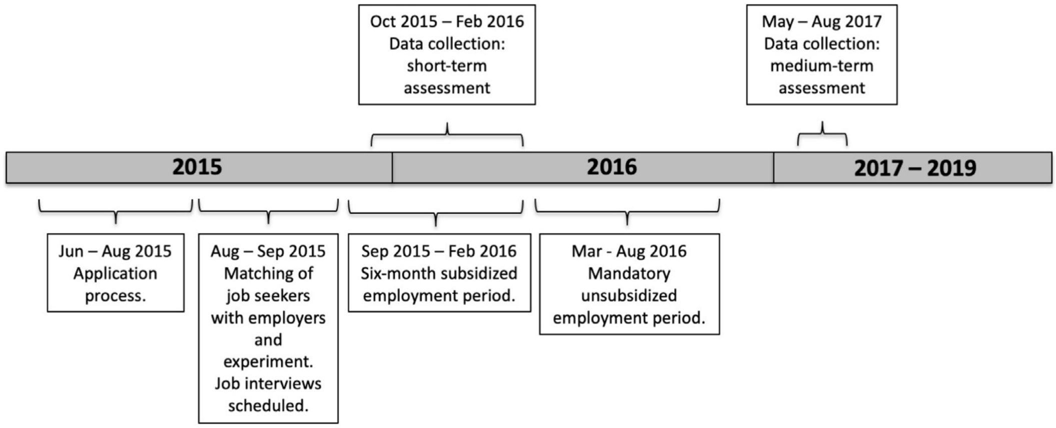

Fig. 1. Timeline of the intervention and of the data collection.
Notes. The figure shows the timeline of the intervention and data collection. The actual starting date is job-specific and typically occurred between September and October 2015.

advertised through national media. For the evaluation, we use applications to the program received between June and August 2015. In this period, the NEA collected applications from 510 job seekers for 43 job openings, which included both low-skilled occupations (e.g., non-specialized factory worker) and positions requiring higher qualifications or technical skills (e.g., chemical technician).[8]

In a first step, the NEA proceeded to match job seekers with available vacancies following two main experimental guidelines. First, each candidate could be matched to only one job vacancy. Second, a minimum of 4 candidates should be identified for each job opening.[9] This process resulted in a total of 153 potential employees for a total of 22 job vacancies from 16 firms. The gap between the initial number of vacancies and the number of vacancies for which sufficiently qualified candidates could be identified reflects the lack of specific skills among job seekers. Applicants who were not matched to any job were excluded from the study.

Firms matched to job seekers operates in the manufacturing (50%), service (42%) and construction (8%) sectors. The average size of a firm is 22 employees, and roughly 97% of the total workforce in these firms consists of workers on open-ended contracts.[10] Most participating firms offered only one job vacancy. A few firms requested more than one worker for the same position; in such cases, identical vacancies posted by the same firm were pooled for matching purposes. Thus, in our context, matching job seekers to job openings is equivalent to matching job seekers to firms. All applicants matched to a particular firm were randomized into either treatment group or control group (i.e., each firm is a randomization stratum). The treatment and control groups consist of 80 and 73 job seekers, respectively.

Job seekers in the treatment group were invited to a job interview with the employer, which was scheduled by the NEA between August and September 2015. After the interview, the employer could choose the interviewee for the position. The employer could also decide not to offer the job to any of the interviewees, but the subsidy was strictly conditional on hiring a candidate from the treatment group. In fact, all available vacancies in the experiment were offered to one of such candidates.

The selected candidate could start working immediately after the interview, depending on the agreement with the employer, and the employer would begin receiving program benefits. Job seekers in the control group were not invited to any job interview, and therefore were excluded from the program. Both job seekers in the control group and unsuccessful job seekers in the treatment group remained eligible to receive the standard job search assistance services provided by the NEA, and could be employed in other jobs outside of the SEP.

The aim of the study is to detect large impacts of the intervention on employment. Assuming long-term employment rates of 10%–20% and 153 individuals in the experiment, power calculations indicate that it would be possible to detect impacts of being offered an interview on employment rates of at least 15–18 percentage points. Such effects are in line with the expectation that one in four interviewees obtains a job under the program, and imply even larger impacts of obtaining a job (as opposed to an interview) on employment probabilities.

3. Data

Information on employment comes from administrative data from the NEA, which tracks all formal employment spells of study participants between September 2014 (one year before the program began) and February 2019 (3.5 years after the program started).[11] For each spell, we observe the type of contract (fixed-term or open-ended), the monthly salary, and employer identifiers. This administrative dataset has two key advantages. First, it is less prone to misreporting and measurement error, which are common in self-reported surveys. Second, it covers an extended post-program period, allowing us to study employment impacts and transitions over the short, medium, and long term.

We complement administrative data with two waves of survey data collected from study participants. The surveys provide information on demographics, self-reported labor supply, noncognitive skills, and various measures of the frequency with which job seekers perform specific job-related tasks. The job-related task module is adapted from the World Bank's STEP survey (World Bank, 2016). We measure noncognitive skills using the Big-5 questionnaire (Goldberg, 1992), and the 12-item grit scale (Duckworth and Quinn, 2009).[12] The surveys also include modules directed at the household head, who can coincide with the job seeker. These modules cover household demographics, education and employment of all members, participation in social

[8] The total number of job openings received under the program were 100, of which 57 were excluded for not meeting the eligibility requirements.

[9] Because our empirical strategy requires variation in job offer outcomes among individuals assigned to the treatment group (see Section 4), we require a minimum of 4 candidates per vacancy (2 treated, 2 control). The median number of candidates per job opening was 6.

[10] Section 5.2 and Appendix Section B.2 discuss potential issues related to the selection of firms participating in the program and compare participating firms with the broader firm population in North Macedonia.

[11] For a small number of candidates, administrative data includes spells before August 2014, but with a limited amount of information (e.g., salary is not available). Due to quality concerns, we discard these observations.

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

assistance programs, expenditures, ownership of durables, and housing conditions.

The first survey (*short-term survey*) was administered between October 2015 and February 2016, while the second (*medium-term survey*) took place between May and August 2017. Since the program was phased-in during August and September 2015, the first wave assesses the short-term effects of the program, when the employers who hired a new worker were still receiving the wage subsidy. The second wave captures medium-term outcomes after employers had fulfilled their contractual obligations under the program. A baseline survey was not collected because employment outcomes before the launch of the program are available from the administrative data.

We merge the administrative employment records and survey data with additional administrative data related to the experiment from the NEA, which contain information on each job seeker's education level, the firm to which they were matched, their treatment status, and whether they received a SEP job offer. Of the 153 job seekers in the experiment, we identify 143 job seekers with valid records in the NEA database, while 107 and 93 job seekers were surveyed in the first and second survey waves, respectively.[13] There are no significant differences in attrition rates between treatment and control individuals in the administrative data (first row of Table 1). Individuals in the treatment group are less likely to be interviewed in the short-term survey. However, job seekers who did not participate in the short-term survey are similar to those who did in terms of demographic characteristics and pre-program employment. The attrition rate from the short- to the medium-term survey is 15%, but again it is not statistically different across treatment groups (Appendix Section B.1). Appendix Section C.4 provides estimates of the impact of the program using Lee (2009) bounds to correct for attrition.

Columns (1)–(2) in Table 1 show descriptive statistics for the sample of job seekers participating in the experiment, distinguishing by control group and treatment group. In column (3), we test for imbalances in individual characteristics by reporting the mean difference between the treatment and the control group for each variable, together with the corresponding standard error. Panel A reports demographic characteristics and employment outcomes from the administrative data, measured in the 12 months prior to the start of the program. Panel B shows additional time-invariant demographic variables from the short-term survey.

Participants in the experiment are, on average, 43 years old, and 64% are male. Macedonians are the largest represented ethnic group (57%), followed by Albanians (29%). Regarding education, 41% of applicants have completed at most primary school, while 59% have attended secondary school or university. In terms of employment, only 6% of job seekers were employed in the 12 months prior to the start of the program. Moreover, just 43% of job seekers had ever been registered as employed in the NEA database before September 2015, indicating that informality is widespread in this population. None of the demographic characteristics we examine differ statistically between treatment and control groups. Nevertheless, for robustness, we control for pre-program outcomes in the empirical analysis when estimating the impacts of the program (Section 4).

Finally, to alleviate concerns about self-selection into the program, Appendix Section B.2 presents a comparison between the experimental sample and a representative sample of unemployed individuals targeted by the program. Individuals in the experimental sample are older and more likely to be ethnic Macedonian. However, they are not more likely to have previously worked for a salary and have, on average, comparable levels of education and job search skills. Thus, selection into the experiment appears to be primarily driven by characteristics associated with the distribution of available vacancies at the time of program application, rather than by individuals being inherently more educated or more motivated.

4. Empirical strategy

Our empirical strategy aims at measuring the impact of being invited to the job interview as part of the program, and the effect of receiving the job offer. We capture the effect of being invited to the job interview by estimating the following specification:

$$Y_{ij,t} = \alpha + \beta T_i + \rho Y_{ij,t_0} + \lambda' X_{i,t_0} + \nu_j + \varepsilon_{ij,t} \tag{1}$$

where $Y_{ij,t}$ is an outcome at time t in the post-program period for job seeker i matched to firm j, T_i is an indicator variable equal to 1 if the applicant was assigned to the treatment group, Y_{i,t_0} is the pre-program outcome (when available), X_{i,t_0} is a vector of individual demographic characteristics (age, gender and education dummies), ν_j are firm fixed effects, and $\varepsilon_{ij,t}$ is an idiosyncratic error term.

Because of the random assignment of T_i, the parameter β in Eq. (1) can be estimated by OLS and captures the causal effect of being invited to a job interview, conditional on having been matched to the job opening. The experimental design exogenously increases the probability of obtaining a job in the treatment group, but being randomly invited to the interview does not guarantee that the job seeker obtains a job as part of the program. For this reason, β is closer in interpretation to an intention-to-treat impact of the program.

To measure the impact of being offered a subsidized job, we supplement the approach of Eq. (1) with the following specification:

$$Y_{ij,t} = \delta + \gamma D_i + \eta Y_{ij,t_0} + \theta' X_{i,t_0} + \nu_j + \epsilon_{ij,t} \tag{2}$$

where D_i is an indicator variable equal to 1 if individual i was offered a subsidized job, and 0 otherwise. Because D_i is correlated with unobserved individual or job characteristics, we follow an Instrumental Variable (IV) approach and instrument D_i with the random assignment to the job interview, T_i. Appendix Section B.5 complements the empirical strategy of Eqs. (1) and (2) by estimating the panel version of these equations.

The parameter of interest, γ, identifies a Local Average Treatment Effect (LATE), i.e., the effect of receiving a SEP job offer among job seekers whose job offer status was affected by the invitation to the job interview (i.e., compliers). To help interpretation, Appendix Table B8 shows the average characteristics of individuals in the treatment group, distinguishing by whether they received a SEP job offer. Individuals who received an offer tend to be younger, more likely to have been employed in the 12 months prior to the start of the program, and more likely to be ethnic Macedonians.

Identification of the impact of a subsidized job on future outcomes relies on the assumption that the job interview affects an individual's labor market outcomes only through the subsidized employment offer. This assumption might be violated if the interview increases the applicant's motivation to search for other jobs, or improves interview-related skills. In addition, being invited to a job interview without getting a job offer could decrease job seekers' motivation and expectations about their employment prospects, especially if the job seeker is rarely invited to job interviews (see, e.g., Abebe et al., 2025; Bandiera et al., 2025). While we cannot rule out this possibility due to the lack of data on the frequency at which job seekers participate in job interviews outside the program, we suggest three reasons why belief recalibration is unlikely. First, job seekers in the treatment group were invited to only one job interview, which was of short duration. Second, as we discuss in Section 5.1, there is suggestive evidence that the interview alone has little impact on employment, as individuals in the treatment group who did not receive a job offer behave over time similarly to individuals in the control group, who were matched to the same job but were never

[13] Ten job seekers could not be identified in the administrative data due to an incorrect individual identifier in the system or in the application to the program. Appendix Section C provides further details on this point and shows an extensive set of robustness checks relative to sample selection.

Table 1
Individual descriptive characteristics, by treatment group.

	Control (C) (1)	Treatment (T) (2)	Difference (T-C) (3)	Obs. (4)
Panel A. Administrative data				
Missing employment records	0.05	0.07	0.02	153
	(0.23)	(0.27)	(0.04)	
Age 19–34	0.28	0.27	−0.01	143
	(0.45)	(0.45)	(0.08)	
Age 35–44	0.22	0.27	0.05	143
	(0.42)	(0.45)	(0.07)	
Age 45–54	0.26	0.27	0.01	143
	(0.44)	(0.45)	(0.07)	
Age 55–64	0.25	0.19	−0.06	143
	(0.43)	(0.39)	(0.07)	
Male	0.70	0.59	−0.10	143
	(0.46)	(0.49)	(0.08)	
Primary or no education	0.41	0.41	−0.00	143
	(0.49)	(0.49)	(0.08)	
Secondary education or above	0.59	0.59	0.00	143
	(0.49)	(0.49)	(0.08)	
Any formal employment	0.39	0.47	0.08	143
	(0.49)	(0.50)	(0.08)	
Employed in the last 12 months	0.06	0.07	0.01	143
	(0.24)	(0.25)	(0.04)	
Days employed in the last 12 months	1.04	6.59	5.55	143
	(5.47)	(32.53)	(3.84)	
Employed in the last 12 months, fixed-term	0.04	0.01	−0.03	143
	(0.21)	(0.12)	(0.03)	
Employed in the last 12 months, open-ended	0.01	0.05	0.04	143
	(0.12)	(0.23)	(0.03)	
Panel B. Survey data				
Age 19–34	0.25	0.18	−0.07	107
	(0.44)	(0.39)	(0.08)	
Age 35–44	0.23	0.25	0.02	107
	(0.43)	(0.44)	(0.08)	
Age 45–54	0.29	0.35	0.07	107
	(0.46)	(0.48)	(0.09)	
Age 55–64	0.23	0.22	−0.02	107
	(0.43)	(0.42)	(0.08)	
Male	0.68	0.53	−0.15	107
	(0.47)	(0.50)	(0.09)	
Primary or no education	0.43	0.37	−0.06	107
	(0.50)	(0.49)	(0.10)	
Secondary education or above	0.57	0.63	0.06	107
	(0.50)	(0.49)	(0.10)	
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)	

Notes. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. 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. *Any formal employment* is a dummy equal to 1 if the job seeker has ever been registered as employed in the NEA database before September 2015. Numbers in parentheses are robust standard errors for the differences in column (3), and standard deviations elsewhere.

invited to the interview. Finally, data from the short- and medium-term surveys suggest that job seekers in the treatment group who did not get an offer are, on average, similar to job seekers in the control group in terms of motivation, job search behavior, and expectations about the probability of being employed in the next 12 months (Appendix Table B6).

5. Results

5.1. Employment

Long-term employment. Table 2 shows estimates of treatment impacts on employment outcomes measured over the entire post-program period (from the start of the program up to 3.5 years after the start). Column (1) shows the average value of the outcome variable in the control group, while columns (2)–(4) present estimates of the effect of being invited to the job interview using Eq. (1) and including alternative sets of control variables. Column (2) includes only firm fixed effects, column (3) adds demographic characteristics, and column (4) further adds the pre-program value of the outcome variable. Due to the small sample size of the study, we supplement individual t-test statistics based on heteroskedasticity-robust inference with test statistics with exact finite sample size based on permutation tests (Fisher, 1937; Pitman, 1937; Imbens and Rubin, 2015). We report the p-values of the permutation tests in squared brackets. Results are similar to those based on heteroskedasticity-robust inference.

Job seekers who were offered the interview are 20–22 percentage points more likely to have worked for at least one day in formal

Table 2
The impacts on employment.

	Control mean	Invited to interview			Received subsidized job offer		
	at follow-up	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employed	0.23	0.22***	0.21***	0.20***	0.87***	0.85***	0.84***
		(0.07)	(0.07)	(0.06)	(0.27)	(0.26)	(0.26)
		[0.00]	[0.01]	[0.00]			
Days employed	159.96	148.43**	149.93**	135.32**	601.66**	619.93**	613.21**
		(64.01)	(62.83)	(62.62)	(240.72)	(241.36)	(264.58)
		[0.04]	[0.04]	[0.05]			
Employed fixed-term	0.22	−0.01	−0.02	−0.00	−0.05	−0.07	−0.00
		(0.06)	(0.06)	(0.06)	(0.24)	(0.23)	(0.23)
		[0.84]	[0.80]	[0.99]			
Employed open-ended	0.12	0.20***	0.19***	0.17***	0.81***	0.79***	0.81***
		(0.06)	(0.06)	(0.06)	(0.23)	(0.23)	(0.26)
		[0.00]	[0.01]	[0.01]			
Days employed fixed-term	113.45	−29.01	−27.12	−25.71	−117.58	−112.13	−106.49
		(42.28)	(41.88)	(38.19)	(160.61)	(158.09)	(144.41)
		[0.55]	[0.55]	[0.52]			
Days employed open-ended	46.51	177.43***	177.05***	176.04***	719.24***	732.06***	804.86***
		(49.21)	(50.27)	(51.41)	(185.58)	(192.39)	(215.94)
		[0.00]	[0.01]	[0.00]			
Labor earnings (1000 MKD)	61.98	62.84**	63.30**	57.93**	254.73**	261.71**	263.96**
		(26.77)	(26.45)	(26.06)	(103.98)	(104.58)	(114.14)
		[0.04]	[0.05]	[0.05]			
Employment index	0.00	0.52***	0.51***	0.52***	2.12***	2.09***	2.14***
		(0.17)	(0.17)	(0.16)	(0.63)	(0.62)	(0.60)
		[0.01]	[0.01]	[0.00]			
Firm FE		Yes	Yes	Yes	Yes	Yes	Yes
Individual controls		No	Yes	Yes	No	Yes	Yes
Baseline outcome		No	No	Yes	No	No	Yes
Observations		143	143	143	143	143	143

Notes. * p<0.10, ** p<0.05, *** p<0.01. Each row shows the results of a regression with a different dependent variable measured in the whole follow-up period (September 2015–February 2019). *Employed* is a dummy equal to 1 if the individual has been employed for at least one day, and 0 otherwise. *Days employed* is the total number of days of employment. *Employed fixed-term (open-ended)* are dummies equal to 1 if the individual has been employed for at least one day with a fixed-term (open-ended) contract, and 0 otherwise. *Days employed fixed-term (open-ended)* is the total number of days of employment with a fixed-term (open-ended) contract. *Labor earnings* is the cumulative labor income, computed by multiplying the daily wage by the number of days in which the individual is employed in the corresponding employment spell, and summing across all spells. *Employment index* is constructed using the (Anderson, 2008) methodology, including the following variables: *employed, days employed, employed open-ended, labor earnings*. Column (1) shows the average of the dependent variable in the control group. Columns (2)–(4) and (5)–(7) present estimates using equations (1) and (2), respectively. *Firm FE* are dummy variables identifying the firm to which a job seeker is matched. Because for some job seekers this information is missing in the data, we include a dummy variable equal to 1 for those observations in which the firm identifier is missing. Appendix Section C.1 shows robustness to alternative approaches. *Individual controls* include indicator variables for gender, for the individual having attained primary education or less, and for the individual being in the 19–34, 35–44 or 45–54 age groups. Robust standard errors are presented in parentheses. Squared brackets in columns (2)–(4) report p-values of a permutation test using 1000 replications. Further details about measurement are provided in Section 3.

employment during the post-program period. As compared to the control group, this effect corresponds to an increase in employment of 87%–96%. The program also significantly increases employment on the intensive margin. On average, individuals in the control group are employed for 160 days since the launch of the program, while being invited to the job interview leads to an additional 135–150 days of employment. While estimates are robust to changes in the set of control variables, it is important to note that, due to the relatively small sample size of our study, the 95% confidence intervals are wide, ranging from 7 to 36 percentage points for the extensive margin estimates and from 13 to 273 days for the intensive margin estimates. Thus, while the point estimates are large compared to those in the existing literature, confidence intervals encompass both modest and very large effects.

The program affects not only employment but also the type of employment contract under which one is hired. The job interview leads to a small insignificant decrease in fixed-term employment on the extensive margin, and an insignificant 23%–26% reduction in the duration of fixed-term employment. In contrast, individuals who were invited to the interview are significantly more likely to have been employed with an open-ended contract. Job seekers in the treatment group accumulate about 4 times more employment days in an open-ended job as compared to individuals in the control group. This result is also reflected in labor earnings: over the full post-program period, individuals in the control group earn a total of 61,980 MKD (1,106 US$), while individuals in the treatment group earn an additional 57,930–63,300 MKD (1,034–1,130 US$), a 93%–102% increase relative to the control group.

Next, we present IV estimates of the effect of being offered a subsidized job using Eq. (2) and including alternative sets of control variables. The invitation to the job interview increases the probability of receiving a SEP job offer by 24–25 percentage points, with a first-stage F-statistic above 20 in all specifications (Appendix Table B.4). Columns (5)–(7) of Table 2 show that receiving the job offer increases the probability of having been employed for at least one day in the follow-up period by 84–87 percentage points, extends employment duration by 1.65–1.70 years, and increases the probability of being employed in an open-ended contract by 79–81 percentage points.[14] In the 42 months following the launch of the program, labor earnings of job seekers who were offered a job were 4.1–4.3 times larger than those of job seekers who were not. We relate this estimate to a cost–benefit analysis in Section 5.3.

[14] Appendix Table C20 shows the results of a robustness check in which we exclude from the estimating sample the few job seekers without information on the firm they were matched to. As none of these job seekers received a SEP job offer, the IV estimates are slightly smaller than those presented in Table 2. Nevertheless, estimates for all employment outcomes remain large and statistically significant.

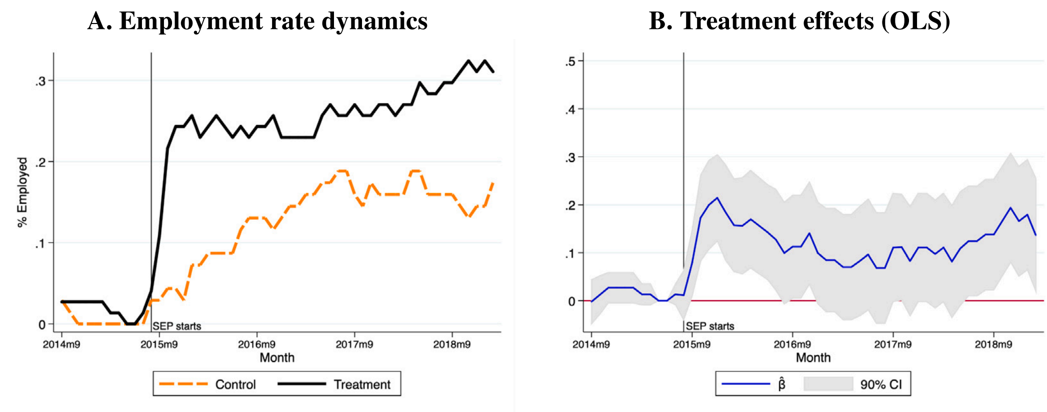

Fig. 2. Employment dynamics.
Notes. Panel A shows the share of participants in the treatment group (solid line) and in the control group (dashed line) that are employed in formal jobs. Employment rates at the monthly frequency are computed by converting employment spells from the NEA's administrative data. Panel B shows the effect of being invited to the job interview on employment using administrative data. The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month, and 0 otherwise. The solid line represents the point estimates, estimated using Eq. (1) without control variables separately for each month. The band around the point estimates is the 90% confidence interval. The vertical lines indicate the month in which the program started. The estimation sample includes 143 individuals. Further details about measurement are provided in Section 3.

Finally, to alleviate concerns about multiple hypothesis testing, we combine the employment outcomes into an index following Anderson (2008), normalizing the index in terms of standard deviations in the control group. Estimates of both Eqs. (1) and (2) confirm the significant effect on employment of the program. Similar to the discussion of the effects on employments, the confidence intervals are wide. At the 95% confidence level, the intervals cover 0.18–0.85 standard deviations for the OLS specification, and 0.87–3.35 standard deviations for the IV specification.

Employment dynamics. To analyze the evolution of employment over time, we convert the employment spells from administrative records into monthly employment indicators. We define an individual to be employed in a given month if he/she worked for at least one day in that month. Panel A in Fig. 2 compares the percentage of employed individuals in the treatment group (the solid line) and the control group (the dashed line). Consistent with targeted participants being long-term unemployed, the employment rates of the two groups in the year before the start of the program are close to zero. When the program begins, we observe an increase of about 25 percentage points in the employment rate of individuals in the treatment group. The employment rate in the treatment group remains high in the subsequent months, and increases to 30% by the end of the follow-up period.

The employment rate in the control group also increases after the program begins. It is important to note that, although job seekers in the control group were excluded from the program, they had been matched to available vacancies, indicating that they had skills in demand in the labor market at the time the program was phased in. These job seekers were able to secure employment outside the program, either by applying directly to non-participating firms or through standard job search services provided by the NEA. However, the growth in their employment rate occurs at a slower pace compared to the treatment group, remaining below 20% throughout the post-program period. Employment in the treatment group remains higher than in the control group even after employers stopped receiving the wage subsidy (6 months after the start of SEP employment), and after they were freed from the contractual obligations of the program (12 months after the start of SEP employment).

Panel B plots treatment effects on monthly-level employment, obtained by estimating Eq. (1) separately for each month and without any control variable. In the year prior to the start of the program, the difference in employment between the treatment and control groups is never statistically different from zero. In the first six months of the program, the invitation to the job interview increases employment by a statistically significant 16 to 21 percentage points. Even though employment in the control group also increases over time, program effects remain large and statistically significant throughout most of the period. Estimates are not statistically significant between 1.3 and 2 years since the start of the program. However, by the end of the follow-up period we detect a statistically significant effect equal to 14 percentage points. These results are robust to different definitions of employment, such as changing the minimum number of days above which we consider an individual to be employed in each month (Appendix Figure C11), and to estimating Eq. (1) adding demographic characteristics and firm fixed effects (Panel B and Panel C of Appendix Figure C10).

Panel A of Appendix Figure B3 shows the same employment dynamics as Panel A of Fig. 2, but separating treated job seekers into two groups: those who were invited to the interview and received the job offer, and those who were invited to the interview but did not receive the offer. Panel B plots estimated treatment effects on monthly-level employment using Eq. (2). In the first two months of the program, 70% of job seekers who received a job offer are employed. This proportion then declines slightly, dropping to just above 50%, and remaining roughly at this level until the end of the post-program period. Because the firm is contractually obligated to keep the worker for 12 months, these statistics indicate that not all job seekers accept the job offer or they voluntarily quit their job. In an interview with participating firms in 2017, all employers report that job terminations are due to workers voluntarily quitting the job. The role of the interview on its own (beyond its effect through the offer of employment under the program) in driving employment results appears to be limited. In fact, the employment rates of both the job seekers in the treatment group that did not receive the job offer and the control group increase over time in a similar manner. By the end of the follow-up period, they are equal to 20% and almost indistinguishable between each other. In the first month of the program, receiving a job offer leads to an increase in employment of 33 percentage points, peaks 4 months into the program at 88 percentage points, and decreases to 56 percentage points at the end of the follow-up period.

We compare administrative data to self-reported survey data on employment (Appendix Table B11). In the short-term, job seekers in the treatment group are 18 percentage points more likely to work for a salary, and earn approximately 2,190 MKD (39 US$) more than job seekers in the control group. These estimates are in line with those from administrative data in the corresponding period. In the medium-term, the employment effect reduces to 5 percentage points (not statistically significant), whereas estimates on administrative data in the corresponding period are equal to 10 percentage points. The

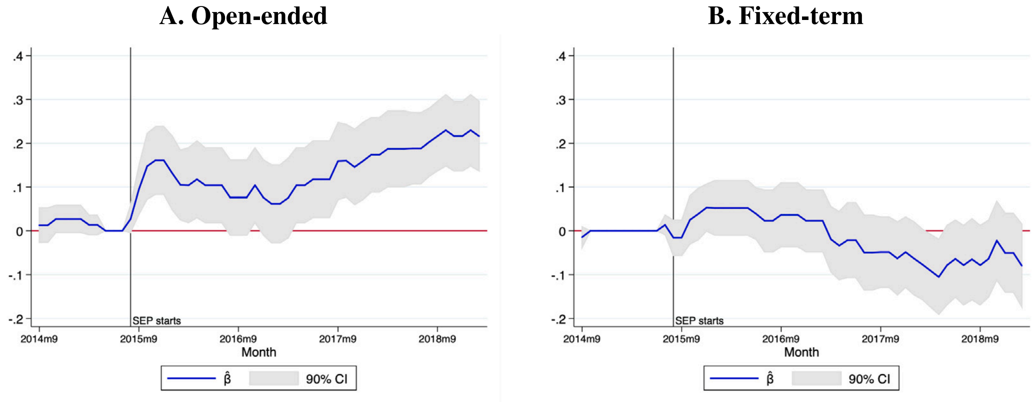

Fig. 3. Employment dynamics, by contractual arrangement.
Notes. The figures show the effect of being invited to the job interview on employment with an open-ended contract (Panel A) or a fixed-term contract (Panel B). The dependent variables are dummies equal to 1 if the individual is employed with one of the contracts in the corresponding month, and 0 otherwise. The solid lines represent the point estimates, estimated using Eq. (1) separately for each month (without control variables. The band around the point estimates is the 90% confidence interval. The vertical lines indicate the month in which the program started. The estimation sample includes 143 individuals. Further details about measurement are provided in Section 3.

difference in employment effects between administrative and survey data remains unchanged if we restrict the sample to job seekers who are observed in both data sources (Panel E of Appendix Figure C10). Similarly, the medium-term impact on earnings reduces to 1,130 MKD (20 US$), no longer statistically significant.

The difference in the medium-term employment effects across data sources can be explained by individuals in the control group being more likely to be employed in the informal economy. The percentage of control group individuals who report to work for a salary in the medium-term survey is 24%, higher than the formal employment rate in the corresponding period using administrative data (17%), whereas for the treatment group the employment rate measured with survey data is only slightly higher than the employment rate measured with administrative data (29% versus 27%, respectively). In line with these results, we find that in the medium-term being invited to the job interview decreases the probability of working in the informal economy (defined as an indicator variable equal to 1 if an individual reports to be employed in the survey data but has no employment record in the administrative data at the time the survey took place) by 6 percentage points, although the estimate is not statistically significant. While a response of the control group in terms of informal employment is not surprising due to the prevalence of the informal economy in North Macedonia (35% of GDP in 2015; Elgin et al., 2021), this result suggests that the estimated differences in formal employment between treatment and control group could be partially offset by informal employment, and that the SEP intervention could have moved workers from informal into formal employment.

Employment dynamics by type of employment. Fig. 3 plots impact estimates on the monthly probability of being employed with an open-ended (panel A) or a fixed-term contract (panel B). We find that job seekers who have been initially employed with a fixed-term contract are eventually able to secure a more stable employment offer in the long-term. Being invited to the interview increases significantly employment with an open-ended contract already in the first months of the program, and the effect increases over time. At the end of the follow-up period, treatment effects on this type of employment are equal to 22 percentage points. On the contrary, estimates for fixed-term contracts are not statistically different from zero in the first part of the follow-up period and becomes negative towards the end.

We check whether this increase in contractual stability reduces dependency on social assistance (Appendix Table B11). Using the survey data, we find that the invitation to the job interview reduces the probability of receiving a transfer from the SFA program by 24 percentage points in the short-term (significant at the 1% level) and by 16 percentage points in the medium-term (not statistically significant), as compared to the control group.

Employment transitions. We look at employment transitions to understand whether the employment effects are due to job seekers getting employed at the beginning of the program and extending the employment beyond the program's timeline with the same employer, or whether the job seekers use the experience gained in the subsidized job to secure employment offers from other employers. In Appendix Section B.7, we construct six transition indicator variables based on employment status in two consecutive months and estimate the average effect of the program on these variables over the entire post-program period (Appendix Table B10). Being offered the job interview increases the probability of remaining employed in two consecutive months by 11 percentage points, and decreases the probability of remaining unemployed by the same magnitude. IV estimates suggest even larger effects of being offered a job at 44–45 percentage points. We find no effect on the probability of job entry or exit. Estimating the effect of the job interview invitation on employment transitions separately for each month (Appendix Figure B5), we find positive effects on the probability of finding a new job only in the first two months of the program. After this period, employment effects are entirely driven by individuals continuing their employment relationship with the same employer, potentially indicating that the original job matches were of high quality.

Labor earnings. Appendix Figure B4 shows average monthly labor earnings for the treatment and control groups (Panel A), as well as the estimated effect of being invited to the SEP interview (Panel B). Participants who were invited to the interview experience a significant increase of approximately 2,000 MKD (36 US$) in monthly earnings immediately after the program begins. This effect persists over time even after the labor earnings of control group participants started increasing due to the increase in their employment rate. Overall, the dynamics of labor earnings follow closely the employment dynamics shown in Fig. 2, suggesting that program effects on labor earnings are driven by differences in employment probability between the treatment and the control groups, rather than differences in wages (conditional on employment).

5.2. Mechanisms

In this section, we discuss several channels through which the program may have generated employment effects that persisted even after the subsidy expired. Because our experiment was not designed to test specific hypotheses about underlying mechanisms, we provide suggestive evidence on these channels and interpret the discussion as speculative.

A first channel relates to information frictions in the hiring process. When employers lack reliable signals of a worker's productivity, as is often the case for long-term unemployed individuals, they face costs associated with screening and learning over time (Lange, 2007; Kahn and Lange, 2014). Both the temporary wage subsidy and the matching component of the program could mitigate these frictions, allowing employers to assess the quality of the worker.[15] Because information frictions are expected to be more severe for job seekers with lower attachment to the labor market, in Fig. 4, we examine heterogeneity in employment effects.

Panel A shows that employment effects are concentrated among female participants, while for males we observe a positive and significant impact only in the first months of the program. We do not observe different patterns in the effects on employment with respect to the age of the participant (Panel B), whereas treatment effects appear to be more persistent over time for individuals with lower education levels (Panel C). The most notable effect is among individuals not previously employed in the formal sector (Panel D). Individuals in this group who were invited to the job interview experience an increase of almost 30 percentage points in the probability of being employed 3.5 years after the start of the program, as compared to those with similar characteristics in the control group. Employment rates in the control group for women, individuals with primary education and those without previous experience in formal employment indicate that for these groups it is hard to find a job outside the program (Appendix Figure B6), presumably because firms might be reluctant to hire workers with more uncertain productivity. These results suggest that the intervention could have mitigated information frictions by providing employment opportunities to individuals with limited ability to signal their productivity.

A second channel concerns skill acquisition during the subsidized employment period, which could have increased workers' productivity and long-term employability. We provide evidence on this mechanism by looking at impacts on several task and skill measures from the surveys. We classify these measures into two categories: job-related tasks and noncognitive skills. Job-related tasks refer to self-reported indicators for whether the individual reads, writes, uses math and uses a computer. Measures of noncognitive skills include the Big-5 personality trait and the 12-item grit scale tests. We standardize all measures to have mean zero and unitary standard deviation in the control group. In addition, we construct an index for job-related tasks and an index for noncognitive skills following Anderson (2008).

Table 3 shows impact estimates for these measures estimated using Eqs. (1) and (2).[16] Columns (1)–(4) and (5)–(8) show estimates for the short-term and medium-term surveys, respectively. The intervention has a short-term positive effect on both job-related tasks and noncognitive skills. Compared to the control group, participants who were offered the job interview score 0.42 standard deviations more on the job-related task index, and 0.40 standard deviations more on the noncognitive skill index. The estimates for the effect of being offered a job are 1.77 and 1.72 standard deviations, respectively for job-related tasks and noncognitive skills. Medium-term effects are quantitatively similar to the short-term effects. Because survey data have larger rates of attrition, Appendix Table C23 provides treatment effect bounds for these measures as in Lee (2009). In both the short and the medium term, the bounds for the job-related task and noncognitive skills indices, and for three out of ten individual measures are positive. For the remaining measures, bounds include both negative and positive effects, and therefore we cannot conclude that the program significantly improved these outcomes.

The positive impact on the job-related task index indicates that the intervention provided job opportunities in occupations that require a more frequent engagement in work-specific tasks. This could be particularly relevant for individuals experiencing long-term unemployment, who may have suffered human capital depreciation (see, e.g., Heckman and Kautz, 2012). The findings on noncognitive skills also align with evidence that major life events (e.g., getting a job after long spells of unemployment) can lead to changes in personality traits in adulthood (Almlund et al., 2011). In our context, the improvement in noncognitive skills is primarily driven by a reduction in neuroticism, a trait associated with anxiety, worry, and frustration. This result is consistent with Gottschalk (2005), which shows that employment can reduce neuroticism by strengthening individuals' sense of control over their lives through self-motivation and self-determination. In the medium-term, we also observe gains in extraversion.

Since employment effects are concentrated among individuals with lower attachment to the labor market (Fig. 4), we also investigate if the effects on skills vary across the same dimensions. Appendix Figure B7 shows that the effect on job-related tasks is driven primarily by participants with lower education and with no prior formal employment experience. The impact on noncognitive skills is strongest among younger participants, as well as those with primary education and no formal work experience. Consistent with the results shown in Fig. 4, these findings suggest that persistent employment effects could be linked to the acquisition of experience and skills through employment.

Finally, we discuss to what extent our estimated employment effect may be related to (self) selection of firms into the program. The eligibility criteria for firms (see Section 1) may have attracted employers with relatively stable labor demand, which may not be entirely representative of firms in North Macedonia. Compared to non-participating firms, these firms may have been more likely to post durable vacancies and retain new hires even after the subsidy expired. This would not affect the internal validity of our results, but could affect their interpretation, as well as their use for policy. Therefore, in Appendix Table B5, we compare the characteristics of participating firms with those of a representative sample of firms in North Macedonia. While there are some significant sectoral differences, participating firms closely resemble the broader population of North Macedonian firms in terms of average size. Moreover, participating firms employ, on average, 3% of their workforce on a fixed-term contract. This statistic aligns with the incidence of fixed-term contracts in the broader population of firms, and the difference is not statistically significant. Overall, participating firms do not appear to be highly selected in terms of size and employment stability, suggesting that firm selection is unlikely to be the main driver of the persistent employment effects of the program.

5.3. Cost-effectiveness and scalability

In Section 5.1, we estimate that in the 42 months following the launch of the program, the labor earnings of job seekers who were offered a job were 4.1–4.3 times larger than those of job seekers who were not offered a job. We use this estimate to compute the cost-effectiveness of the program in this period. On average, job seekers who were offered a job saw their total earnings increase by 263,960 MKD (4,711 US$), corresponding to a monthly increase of 6,285 MKD (112.17 US$). The cost of the program in the prevalent modality varied slightly between low-skilled and high-skilled workers, with a total subsidy equal to 119,400 MKD (2,131 US$) and 132,000 MKD (2,356 US$), respectively (see Appendix Section A.2). In addition, we estimate administrative costs associated with the management of the program

[15] Without subsidies, risk-averse employers may only hire at wages below the expected marginal productivity of labor, leading to high unemployment rates. Job search assistance can help workers better signal their quality to firms (Belot et al., 2019). Wage subsidies may also raise reservation wages (Levinsohn and Pugatch, 2014), and therefore labor costs might not decrease by the full amount of the subsidy.

[16] Results are robust when controlling for a standard measure of abstract reasoning (the Abbreviated Raven's test of progressive matrices; Bilker et al., 2012) and other demographic characteristics (Appendix Table C21).

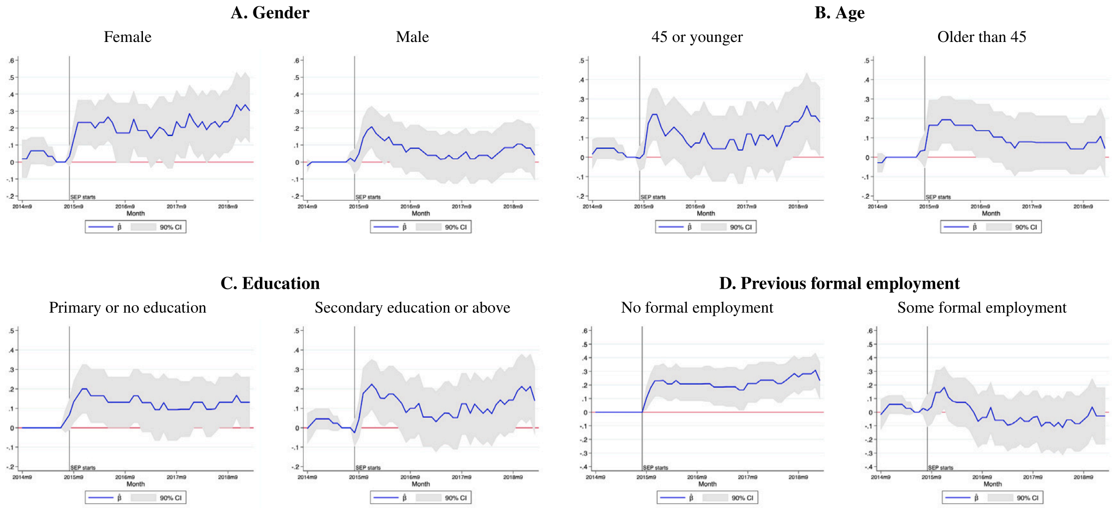

Fig. 4. Heterogeneous effects on employment.
Notes. The figures show, for different groups, the effect of being invited to the job interview on employment using administrative data. The dependent variable is a dummy equal to 1 if the individual is employed in the corresponding month, and 0 otherwise. In each panel, the solid line represents the point estimate, estimated restricting the sample to the corresponding group and using Eq. (1) without control variables separately for each month. The band around the point estimates is the 90% confidence interval. The vertical lines indicate the month in which the program started. The sample size in each group is the following: female ($N = 51$), male ($N = 92$), 45 years old or younger ($N = 77$), older than 45 ($N = 66$), primary education or less ($N = 58$), secondary education or more ($N = 85$), not previously employed in the formal sector ($N = 81$), previously employed in the formal sector ($N = 62$). Previous formal employment is an indicator equal to 1 if before September 2015 the individual has ever been registered as employed in the administrative data. Further details about measurement are provided in Section 3. Appendix Figure B6 shows the share of employed participants in each group, distinguishing by treatment and control group.

Table 3
Short- and medium-term impacts on job-related tasks and noncognitive skills.

	Short-term impact				Medium-term impact			
	Control mean	Invited to interview (OLS)	Received subsidized job offer (IV)	Obs.	Control mean	Invited to interview (OLS)	Received subsidized job offer (IV)	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Job-related tasks								
Job-related task index	−0.00	0.42**	1.77**	106	−0.00	0.49***	1.98**	93
	(1.00)	(0.18)	(0.83)		(1.00)	(0.18)	(0.85)	
Read in the last 12 months	0.00	0.47***	1.98**	106	−0.00	0.54***	2.16**	93
	(1.00)	(0.17)	(0.84)		(1.00)	(0.18)	(0.88)	
Wrote in the last 12 months	−0.00	0.29*	1.23	106	0.00	0.45**	1.80**	93
	(1.00)	(0.17)	(0.79)		(1.00)	(0.18)	(0.83)	
Used math in the last 12 months	0.00	0.11	0.49	106	0.00	0.33*	1.30	93
	(1.00)	(0.18)	(0.79)		(1.00)	(0.19)	(0.82)	
Used pc in the last 12 months	0.00	0.39*	1.64*	106	−0.00	0.22	0.88	93
	(1.00)	(0.20)	(0.84)		(1.00)	(0.20)	(0.80)	
Noncognitive skills								
Noncognitive skill index	0.00	0.40**	1.72**	105	0.00	0.46**	1.85**	93
	(1.00)	(0.18)	(0.82)		(1.00)	(0.21)	(0.90)	
Extraversion	0.00	0.19	0.81	105	0.00	0.35*	1.40*	92
	(1.00)	(0.19)	(0.81)		(1.00)	(0.20)	(0.82)	
Agreeableness	−0.00	0.17	0.72	105	−0.00	0.25	1.01	92
	(1.00)	(0.19)	(0.78)		(1.00)	(0.22)	(0.90)	
Conscientiousness	0.00	0.30	1.25	105	0.00	0.33	1.33	92
	(1.00)	(0.19)	(0.79)		(1.00)	(0.22)	(0.92)	
Neuroticism (inverted scale)	0.00	0.47**	2.01**	105	0.00	0.41*	1.66	92
	(1.00)	(0.18)	(0.93)		(1.00)	(0.23)	(1.02)	
Openness	0.00	0.31	1.33	105	−0.00	0.26	1.05	92
	(1.00)	(0.21)	(0.91)		(1.00)	(0.19)	(0.79)	
Grit	−0.00	0.05	0.25	103	0.00	0.08	0.31	93
	(1.00)	(0.20)	(0.88)		(1.00)	(0.20)	(0.77)	

Notes. * p<0.10, ** p<0.05, *** p<0.01. Columns (1)–(4) show the impact of the intervention using the short-term survey. Columns (5)–(8) show the impact of the intervention using the medium-term survey. Columns (2) and (6) present estimates using equation (1) without control variables. Columns (3) and (7) present estimates using equation (2) without control variables. In columns (1) and (5), standard deviations are presented in parenthesis, while in columns (2)–(3), and (6)–(7), standard errors are presented in parenthesis. The *job-related task index* is based on self-reported indicators for the individual reading, writing, using math and using a PC in the last 12 months. The *noncognitive skill index* is based on the Big-5 questionnaire and the 12-item grit scale. Both indices are computed following the methodology described in Anderson (2008), and are normalized by the standard deviation of the index in the control group. Robust standard errors are reported in parenthesis. Further details about measurement are provided in Section 3.

and the matching of applicants to job openings at 4,551 MKD (81 US$) per employment contract.[17]

The resulting cost per employed worker is approximately 123,951 MKD (2,212 US$) for low-skilled workers and 136,551 MKD (2,437 US$) for high-skilled workers. These figures imply a return of 113% and 93%, respectively, indicating that the earnings increase approximately doubles the program expenditure in the 42–month period following the launch. Workers recover the cost of their subsidy through increased earnings in roughly 20 to 22 months.

While these results indicate high cost-effectiveness, they must be interpreted in light of the specific features of the SEP's design. Many subsidized employment programs rely on general voucher schemes and suffer from low take-up rates (Galasso et al., 2004; De Mel et al., 2010; Groh et al., 2016; Alfonsi et al., 2020), while the SEP relied on careful recruitment of both firms and workers, matching applications with job openings, and providing incentives to encourage participation and stimulate employment. These design features likely contributed to the high take-up of subsidized jobs, as also observed in programs demanding firms to apply to participate (see, e.g., Hardy et al., 2019). However, these same elements could potentially limit the scalability of the program. The NEA initially collected applications for 43 eligible job openings but deemed only 22 appropriate for the pool of applicants, as only 30% of applicants could be matched to a vacancy (see Section 2). This pattern highlights a significant mismatch between labor supply and demand, revealing a key policy trade-off: while screening and matching may substantially improve employment outcomes, they also limit the number of viable matches and increase the administrative complexity of scaling up.

6. Conclusion

This paper studies whether subsidized employment can produce long-term changes in the employment prospects of unemployed individuals in North Macedonia, a middle-income country. For the program we study, job seekers are first matched to potential employers. Employers then conduct interviews and choose their preferred candidates. The wages of the hired candidates are subsidized for 6 months, giving the firms a strong incentive to try out the candidates. Firms also receive training subsidies. In return, firms commit to keep these workers employed not only during the subsidized employment period, but also for an additional 6 months after the subsidy expires. To evaluate the program, we design a unique experiment in which we randomly vary access to the interview with the employer.

We find that the program significantly increases formal employment and earnings. Estimated impacts are large and persist well beyond the duration of the subsidy. These effects are primarily driven by individuals maintaining employment with the initial employer, and are concentrated among job seekers with lower counterfactual participation rates in the labor market, such as women and participants with no previous formal employment experience.

Our results offer noteworthy implications. In contrast to most existing evidence from LMICs, our results suggest that subsidized employment programs can effectively promote long-term employment in contexts with high structural unemployment. In our setting, the increase in earnings among job seekers who receive a subsidized employment offer offsets the cost of the subsidy and related administrative expenses within two years. Careful recruitment and customized matching can substantially increase the take-up of employment offers as compared to voucher-based employment programs. At the same time, we document an important mismatch between job seekers' qualifications and firms' labor needs which subsidized employment programs alone can only partially address. Programs that combine skill development in high-demand areas with targeted job matching may be better positioned to close persistent mismatch between labor supply and demand.

[17] We do not have access to administrative costs specific to the SEP because the program is run by the existing staff of the NEA. We approximate these costs using the general budget of the NEA in the year of the program's launch (NEA, 2015). In 2015, the agency spent a total of 244,218,951 MKD (4,358,718 US$) on personnel, the purchase of goods and services, and on capital expenditures. In the same period, the NEA registered a total of 53,663 new employment contracts among those individuals who were registered as unemployed, taking into account that individuals could sign multiple short-term contracts in the same year.

CRediT authorship contribution statement

Alex Armand: Writing – review & editing, Visualization, Supervision, Resources, Methodology, Funding acquisition, Data curation, Writing – original draft, Validation, Software, Project administration, Investigation, Formal analysis, Conceptualization. **Pedro Carneiro:** Writing – review & editing, Visualization, Supervision, Resources, Methodology, Funding acquisition, Data curation, Writing – original draft, Validation, Software, Project administration, Investigation, Formal analysis, Conceptualization. **Federico Tagliati:** Writing – review & editing, Visualization, Supervision, Resources, Methodology, Funding acquisition, Data curation, Writing – original draft, Validation, Software, Project administration, Investigation, Formal analysis, Conceptualization. **Yiming Xia:** Writing – review & editing, Visualization, Supervision, Resources, Methodology, Funding acquisition, Data curation, Writing – original draft, Validation, Software, Project administration, Investigation, Formal analysis, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2025.103598. Replication package is available at https://www.openicpsr.org/openicpsr/project/237190.

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