A job trial subsidy for youth:
cheap labour or a screening device?

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ABSTRACT

This paper evaluates a 90-day hiring subsidy designed for young jobseekers aged below 25, introduced in Hungary in 2015 as part of the Youth Guarantee programme. The subsidy covers the total wage cost with no obligation to retain the new hire when the subsidy expires. The analysis is based on linked administrative data taken from the unemployment register, cognitive skills measured at age 15, health and social security records. The causal impact of the subsidy on subsequent employment is identified in comparison to participants of a large-scale public works programme, using propensity score matching with exceptionally rich controls. The estimates indicate significant positive effects: participants spent 14-20 days more in employment within six months after the programme ended on the whole sample. The impact is weaker on the 12-month horizon. We find that the subsidy works well as a screening device: the programme has the highest impact on those workers who have very low levels of schooling (eight years of primary school or less), but demonstrated high skill levels on standardised competence tests. One potential explanation is that employers tend to retain those with better cognitive skills, irrespective of their formal qualifications. We also find some indication that the subsidy is (mis)used by some employers to hire short term, seasonal workers.

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90 napos munkatapasztalat-szerzési támogatás a fiataloknak: olcsó munkaerő vagy hatékony szűrő?

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ÖSSZEFOGLALÓ

A tanulmány a 25 év alatti álláskeresők számára kialakított 90 napos munkatapasztalat hatását vizsgálja, amelyet Magyarországon 2015-ben vezettek be az Ifjúsági Garancia program részeként. A program sajátossága, hogy az akár a bérköltség egészét fedező támogatás lejártával a vállalatnak nem kötelező tovább foglalkoztatni az újonnan felvett munkavállalót. Az elemzés az álláskeresők egyéni szintű nyilvántartásából, a 15 éves korban mért kognitív készségekből, az egészségügyi és társadalombiztosítási nyilvántartásból származó összekapcsolt adminisztratív adatokon alapul. A támogatásnak a későbbi foglalkoztatásra gyakorolt oksági hatását a nagyszabású közfoglalkoztatási program résztvevőivel összehasonlítva, részvételi valószínűség szerinti párosítás módszerét használva azonosítjuk. Az eredmények szerint a résztvevők a program befejezését követő hat hónapon belül 14-20 nappal több időt töltöttek foglalkoztatásban, mint a kontroll csoport tagjai. A hatás gyengébb a 12 hónapos horizonton. A támogatás hatékony szűrőként működik: a legnagyobb hatást azokra a munkavállalókra mértük, akiknek alacsony az iskolai végzettségük (nyolc év általános iskola vagy annál kevesebb), de a kompetencia felméréseken magas készségszintet értek el. Ennek egyik lehetséges magyarázata az, hogy a munkáltatók inkább a jobb kognitív készségekkel rendelkezőket tartják meg, függetlenül formális képzettségüktől. Ugyanakkor a támogatást egyes munkáltatók rövid távú, idénymunkások alkalmazására (ki)használják.

JEL: J08, J64, J68
Kulcsszavak: fiatalok munkanélkülisége, aktív munkaerőpiaci program, Ifjúsági Garancia, részvételi valószínűség szerinti párosítás

A job trial subsidy for youth: cheap labour or a screening device[1]?

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ABSTRACT

This paper evaluates a 90-day hiring subsidy designed for young jobseekers aged below 25, introduced in Hungary in 2015 as part of the Youth Guarantee programme. The subsidy covers the total wage cost with no obligation to retain the new hire when the subsidy expires. The analysis is based on linked administrative data taken from the unemployment register, cognitive skills measured at age 15, health and social security records. The causal impact of the subsidy on subsequent employment is identified in comparison to participants of a large-scale public works programme, using propensity score matching with exceptionally rich controls. The estimates indicate significant positive effects: participants spent 14-20 days more in employment within six months after the programme ended on the whole sample. The impact is weaker on the 12-month horizon. We find that the subsidy works well as a screening device: the programme has the highest impact on those workers who have very low levels of schooling (eight years of primary school or less), but demonstrated high skill levels on standardised competence tests. One potential explanation is that employers tend to retain those with better cognitive skills, irrespective of their formal qualifications. We also find some indication that the subsidy is (mis)used by some employers to hire short term, seasonal workers.

JEL Classification: J08, J64, J68

Keywords: youth unemployment, ALMP, Youth Guarantee, wage subsidy, public works, propensity score matching

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1 | INTRODUCTION

Young people entering the labour market are vulnerable: they have a higher risk of unemployment than older workers and their labour market position is more sensitive to demand-side fluctuations (e.g., Caliendo & Schmidl, 2016). Empirical evidence suggests that becoming unemployed right after leaving school has long-lasting labour market consequences: even 10-15 years later, these individuals tend to have lower wages, fewer hours worked, lower quality jobs, a higher risk of unemployment, and weaker labour market attachment. Thus, it appears that negative experiences during the transition from school to work can have long-lasting negative ("scarring") effects on a young person's career.[2] The fragility of the labour market position of young people increased throughout the 2008 recession, when the youth unemployment rate in the EU was persistently above 20%. Young jobseekers have also faced a higher risk of unemployment during the most recent crisis induced by the Covid19 pandemic (OECD 2021), which has yet again raised the stakes of improving the effectiveness of youth labour market policies.

The aim of this paper is to provide reliable estimates of the effects of one such policy measure, the sort of which, to our knowledge, has not been studied extensively. The 90-day job trial programme provides a maximum subsidy of 100% of the wage costs and entails no commitment for further employment. It was introduced as part of the EU funded Youth Guarantee (YG henceforth) programme in 2015 and became one of the most popular programme elements (in terms of participants) in Hungary.[3]

Subsidised short-term employment can provide a test of the worker's actual skills to the employer and also helps the young person decide if the job matches their skills and interests. The short duration lowers costs to the public budget and also lowers the risk that the programme may encourage early school leaving (Oskamp and Snower, 2006, O'Leary, 1998).

This paper aims to estimate the causal effects of participation in the 90-day job trial programme on the labour market outcomes of young jobseekers. We use propensity score matching to compare people in the subsidized job trial to participants of the large-scale public works programme.[4] This is a valid control group in the Hungarian case due to the specific features of the programme: despite its original aim as a last-resort measure, it was very broadly targeted and recruited a significant number of youths until 2016. There is also evidence that the use of public works funds was partly determined by political motivations (see Reizer et al. (2022)), introducing random variation into the selection into the control and treatment groups. Thus, although job trial participants have much better labour market prospects in terms of observable characteristics on average, the overlapping condition is satisfied, and young public works participants constitute a valid control group.

Using a large, linked dataset of the records of several public agencies allows us to use exceptionally rich controls. Exploiting the fact that we observe cognitive skills as well as qualifications for a subsample of participants, we can shed some light on the mechanism driving the estimated effects. To the best of our knowledge, our paper is the first impact analysis of the Hungarian 90-day job trial programme and a contribution to the limited literature on short-term wage subsidy programmes.

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[2] For examples of similar scarring effects see Nielsen and Reiso(2011), Kahn(2010) Gregg P.–Tominey E. (2005). For Hungary, Csillag(2019) showed that the scarring effects of entering the labour market in a recession resulted in permanently lower employment. For a survey of the literature, see, e.g., Caliendo(2016).

[3] Wage subsidies had been used in Hungary since 1990, but the previously existing schemes were of a longer duration (9-12 month) covered 70-100 percent of wage costs and tied to an obligation to retain the subsidised new hire for a period equivalent to 1,5 times the subsidy period.

[4] Public works had the highest number of participants prior to the introduction of the YG. It is often considered as a last resort by both jobseekers and PES staff.

Our estimates indicate that, compared to the public works programme, participation in the 90-day job trial improved labour market outcomes on the six-month horizon. Job trial participants spent 13-20 days more in employment within six months after completing the programme. The cumulative earnings of job trial participants on the primary labour market was higher by 0.61 times the monthly minimum wage over a six-month horizon.[5] The effect of participation in the job trial on employment and wage outcomes was lower on the horizon of 12 months compared to the impact on a six-month horizon, indicating that the impact weakens over time.

We also find some evidence that the programme works as a screening device. Using the high school literacy and numeracy competence test scores that are available for a large subsample, we calculate heterogenous effects by qualifications and skill level. We find that the programme had the highest impact on those workers who had very low levels of schooling (primary school or lower), but had achieved a relatively high test score in high school. A potential explanation is that firms tend to retain participants with higher test scores, even if they have lower education.

We checked if the impact varies by the availability of public works and found little difference across settlements with a high (above median) and low (below median) share of public works participants among the unemployed population aged 25 or over. This allays the concern that the estimated positive impact may be driven by micro-regions where public works is small-scale and highly selective.

Tracking the employment records of programme participants after the subsidised period we find that in some cases, the subsidy supports precarious jobs. First, some occupations where seasonal work is common are overrepresented among job trial participants. Second, close to 20 % of those retained by the employer 6 months after the programme are shifted into precarious forms of employment (public works, casual work or part-time), a few months after the programme ends.

We find that the most employable unemployed young people were selected into the programme – and into all programs of the Youth Guarantee – from the pool of eligible registered jobseekers. This finding suggests that in contrast to the declared goals of the Youth Guarantee, in practice the programme did not prioritize vulnerable and disadvantaged groups, who needed help the most. However, the estimated treatment effect (compared to public works participation) is similar for people with a higher and lower educational background. This implies that stronger incentives for the PES to enrol low-educated youth in the programme would not lead to a decrease in the average treatment effect, while it would likely reduce the deadweight loss of the programme.

The remainder of the paper is structured as follows. Section 2 summarises literature relevant to the analysis at hand. Section 3 provides a brief overview of the institutional setting: the Youth Guarantee and the 90-day job trial programme. Section 4 describes the administrative datasets used in the evaluation and explains the empirical strategies and the methodologies we used for the analysis of the job trial. Section 5 presents our main results. Section 6 uses descriptive analysis to highlight some potential risks to the programme. Finally, Section 7 concludes and summarises the main findings of this paper.

2 | EARLIER EVIDENCE

Short-term wage subsidies may work especially well for youth as compared to other groups of jobseekers, as they may tackle the lack of labour market experience in a cost-effective way. The effect may emerge via two channels. First, the period of subsidized work can act as a screening device, allowing the employer to observe the productivity of the job candidate at a low cost. This may mitigate statistical discrimination on the grounds of experience, formal schooling, age, or other vulnerabilities

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[5] This is an indication of the lock-in effect of public works programs: public works participants spend more days in public works also after the first public works program, which compensates for their lower wages.

(Altonji and Pierret, 2001). Second, the subsidized employment can facilitate skill formation through "learning-by-doing", leading to increased productivity and improved employment prospects (Heckman et al., 2002).

The relative importance of these two mechanisms may depend on the target group of the subsidy: while lowering the risk of hiring an inexperienced job candidate may be relevant for all young persons, facilitating skill formation is of particular importance for low-skilled young jobseekers.

A short-term scheme may generate these effects by providing a subsidy for 2 or 3 months. Thus, if their effects are not much smaller (which is more likely if the effect is partly or mainly generated via the screening channel), such schemes are very cost-efficient compared to similar schemes of a longer duration. The relatively low cost of such schemes also allows the implementers to further reduce employers' risks by waiving the usual obligation of retaining the worker after the subsidy runs out, even at the expense of increasing deadweight costs. Moreover, substitution and displacement effects are less of a problem if the subsidy works well as a screening device: even if the subsidy does not generate additional jobs, it will generate a net impact by improving the longer-term employment prospects of the young jobseeker. Lastly, if the scheme is accessible to all young jobseekers, possible stigma effects are likely to be smaller than in other schemes narrowly targeted at disadvantaged jobseekers. It should be noted however that a short-term subsidy on its own may not be effective for youth facing multiple barriers to work.

Existing reviews find that the impact of wage subsidies targeting youth tends to be positive or zero, and the effects are typically smaller than those of similar measures targeting older workers (Caliendo and Schimdl 2016, Kluve, et al. 2019). Some reviews also point out that the take-up rate of subsidies depends on the ease of administering the subsidy and the way in which the subsidy is paid to the beneficiaries, and other hurdles in the implementation may further decrease the effectiveness of these programmes (Almeida et al. 2014, Bördős et al. 2016).

Our work is related to the debate on the stepping-stone effect of short-term jobs for young people in that the job trial subsidy may be (mis)used by employers as a form of temporary work. This literature covers various forms of potentially precarious employment (fixed-term contracts, temporary agency work etc.), and calls attention not only to the possible positive effects of having a short-term job (as a stepping stone to more stable jobs), but also to the risk of being locked into a series of low-paying jobs (which are used by employers as buffers). While this literature has been largely inconclusive, it appears that having a short-term job is more beneficial in non-dual labour markets (where the firing costs for open-ended contracts are not very high; see Bentolila et al. (2019)), and in periods of high unemployment (see Jahn & Rosholm (2018)).

The quantitative evidence for hiring or wage subsidies aimed at young people in Hungary is scarce. Svraka (2018) evaluates a capped payroll-cut applicable to all employees aged under 25, introduced in the Job Protection Act in 2013. Using a difference-in-differences methodology to compare change in employment outcomes around the eligibility cut-off, he shows that the subsidy raised the probability of employment by about two percentage points. He also provided some circumstantial evidence that substitution effects were negligible.

The Youth Guarantee Programme was evaluated in Hungary[6] by Czombos et al. (2018) at an early stage, using PES register and programme monitoring data. They used matching methods to measure the effect of YG participation relative to participation in a training programme for low-skilled youth and public works participants. They found that YG participants were 31.5% more likely to be working in the primary labour market six months after their participation in the programme ended. Comparing 24-

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[6] For another YG evaluation in the region see Madon et al(2021).

25-year-old YG participants with 25-26-year-old participants in a programme that was roughly similar but less readily available, they found a significant 2% effect. Both estimates applied to the programme as a whole, without distinguishing particular elements.

Evaluations of similar programmes in the CEE region (Hora & Sirovatka (2020); Madon et al. (2021); Stefánik & Lafférs (2021)) reach broadly consistent conclusions. Entry into short-term work insertion programmes (which are often combined with workplace-based training and last typically 6 months) at employers in the primary labour market are highly selective, with more educated (higher skilled) having much better chances; while lower skilled are relegated to municipality organised community services.[7] While the first type of programme enhances youths' later employment prospects, the latter often harms them.

3 | YOUTH GUARANTEE AND THE JOB TRIAL PROGRAMME

The EU Youth Employment Initiative aims to reduce youth unemployment by guaranteeing meaningful support to all youth aged below 25 who are not in employment, education, or training (NEETs) within a short time after they register with the public employment services.

In Hungary, the Youth Guarantee programme was introduced gradually, starting in 2015 with those registered for over 6 months. From 30 June 2016 onwards, the programme aimed to provide help within four months for all those who had been registered with the PES for at least four months. Once registered, young jobseekers can be referred to the programme where a dedicated counsellor assesses their needs and outlines options in an initial consultation. The two most popular measures are vocational training and wage subsidies (including job trials) and the 90-day job trial accounted for about 40% of all YG wage subsidy programmes and 25% of all YG programmes between 2015 and 2017. The number of participants by year are summarised in Table 1.[8]

While its measures are not novel, the Hungarian YG programme is an upgrade over preceding policies in that it guarantees jobseekers a good quality offer within a short period of time, it has a relatively large budget, and it is accompanied by relatively well-coordinated communication efforts.

[Table 1 here]

The 90-day subsidy offers favourable conditions: it covers 100 % of the wage costs (with a cap) and there is no obligation to retain new hires after the subsidy ends.[9] The contract must be for at least 4 hours a day. Participants can move on to a longer scheme (the 8+4-month programme) directly after the job trial. Both eligible jobseekers and firms might initiate a job trial contract, and the PES help both parties to find a suitable match, but this does not rule out cases where the employer sends a suitable applicant to register with the PES to be eligible for the subsidy.

In the Hungarian context, the job trial programme had the potential to tackle widespread negative discrimination against the Roma, which is a significant minority in the country. It could also encourage some employers to turn unreported or grey jobs into fully reported contracts (or generate deadweight by financing otherwise fully reported jobs), especially in highly seasonal activities, such as tourism or agriculture. However, as it was introduced during an economic boom, it is likely to have a fairly large

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[7] In contrast, Bratti et al. (2022) in Latvia , looking at a vocational training programme find that individuals participating in VT may be the least employable in terms of observable characteristics (past work experience, education, etc.) which consistent with VT programmes being primarily targeted to those lacking adequate vocational qualifications.

[8] Other measures include a start-up allowance and a rent subsidy that may accompany other programs.

[9] Hiring subsidies within the YG may last three, six, eight, or ten months and cover 70-100% of all wage costs. The longer subsidies prescribe that new-hires must be retained for some time after the subsidy ends (half the duration of the subsidy).

deadweight loss.

4 | DATA AND METHODOLOGY

Against the background presented above, we are aiming to estimate the causal impact of participation in the programme on future outcomes. A key assumption about the mechanism of the programme is that during the subsidised period, the young person develops skills and accumulates work experience that increases her chances of finding an unsubsidised job under regular conditions, either at the same or at another firm and allows accumulating more labour income after the subsidized period. Consequently, we expect an effective programme to improve the future employment prospects and earnings of the participants.

Data Our empirical analysis is based on an individual-level administrative panel database from Hungary, owned by the Databank of the Centre for Economic and Regional Studies. The data cover half of the population aged 0-74 in 2003, who were randomly selected and followed-up until 2017.[10] The database consists of linked data sets of the pension, tax, and health care authorities and the public employment services (hereafter PES) and contains detailed individual-level information on employment and earnings history, use of the health care system, pension, and other social benefits. The PES dataset (Jobseekers' registers) contains information on all registered jobseekers. Among the data it collects are records of ALMP participation, including the start date, the end date, and the type of the programme, details of education. Linking the PES database to the databases of the pension and health care authorities enables us to observe individuals' background characteristics and employment histories of jobseekers, and their employment and earnings outcomes after an arbitrary time span following the completion of the programme.

Empirical strategy The aim of the empirical strategy is to provide a causal estimate of the effect of the policy by applying a quasi-experimental framework. A challenge to the identification strategy comes from the interferences between the different programmes. The decision about which of the different programmes of the Youth Guarantee the young jobseeker will participate in is made by the young person together with a PES counsellor, based on the specific needs and qualifications of the young persons. However, there are no prescribed rules for how such choices are made, and the programmes cannot be differentiated by eligibility. As the eligibility criteria of the YG programme elements are not mutually exclusive, the effect of participation in the 90-day job trial cannot be identified separately from participation in other YG programmes based on exogenous variations in the eligibility criteria.

Against this background, we compare the outcomes of the participants in the 90-day job trial programme with outcomes of a control group of participants in public works applying propensity score matching method that relies on the conditional independence assumption; that is, given a set of observable covariates that are not affected by the treatment, the potential outcomes are independent of the treatment assignment. We believe that our rich set of data on the observable characteristics and the labour market histories of the participants ensures that the conditional independence (or unconfoundedness) assumption will hold.

Treatment group The main shortcoming of our data source is that it does not distinguish between the 90-day job trial programme and other YG wage subsidy variants – making it impossible to observe the 90-day job trial participants directly. Still, we can exploit the fact that the only three-month YG wage cost subsidy is the job trial and approximate the group of participants based on the length of the

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[10] For details, see Sebők (2021).

programme. To reduce the risk of including dropouts from longer programmes, we consider in the treatment group only those individuals who participated in a programme that was exactly 90 days long, as the probability of dropping out from another wage cost subsidy programme on exactly the 90th day is low. Furthermore, there is an outstanding peak in the distribution of programme length at 90 days, indicating that, we capture vast majority of the job trial participants if we fix the programme length at 90 days. The caveat of this approach is that we only measure the effect of a completed programme and lose participants who terminate the programme earlier. According to an interview with a[11] PES officer, drop-out rate from the 90-day job trial programme is low, around 6%[12].

Participants in the 90-day job trial programme were allowed to participate in the 8+4-month wage subsidy programme after the completion of the job trial. As the employment rate of this group 6-12 months after the completion of the job trial was automatically high because of their subsequent programme participation, we excluded those who were enrolled in any wage subsidy programme in addition to the job trial in our baseline estimation. However, the exclusion of participants with programme combinations might introduce a selection problem, as the firms may have chosen the most promising job trial participants to participate in a subsequent wage subsidy. We ignore this selection problem in our baseline estimations but address it later by exploiting county-level "house rules" in enrolment into YG programmes.

Control group Our control group consists of participants of another labour market programme, the public works programme[13] and this choice requires an explanation. The municipal public works scheme was launched in Hungary in 2000 and upscaled dramatically after 2009 as an answer to the high unemployment rate following the financial crisis. The programme offers unskilled jobs created by local governments, and pay a salary equal to about 70% (in 2015, cc 170 EUR a month) of the minimum wage to low-skilled workers.

Participants of the Hungarian public works programme may constitute a suitable control group for three reasons: as similar programmes in other countries, it has little impact on re-employment probabilities, while it is less selective than in most other countries due to the large size of the programme and to exogenous regional variation in the selection process.

First, international evidence shows that public works programmes usually have zero or negative effects on reemployment (Caliendo et al, 2016 or Card et al, 2010). The Hungarian scheme is not especially well-designed to reduce the typical pitfalls of such programmes: in most cases, the jobs are created by local governments and are expected to be „additional" activities beyond the core function of the municipality and not provided by the market, which limits the value of the experience acquired in such jobs. The programme does not include any mentoring or individualised training in basic skills. Most participants work full time, which leaves little time for job search, increasing the risk of lock-in. The existing evaluations (though not fully eliminating endogeneity and selection bias) indeed show that the programme does not improve the likelihood of reemployment and has failed to reduce long-term unemployment at the settlement level (e.g. Köllő and Scharle 2012, Cseres-Gergely and Molnár 2015).

Second, the Hungarian public works scheme is exceptionally large, which increases variation among participants allowing us to find suitable matches for those in the job trial programme. In 2015 and

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[11] Interview with the manager of the PES of Nograd county on 2019. November. Nevertheless, we likely only use observations where there was a decent match between the firm and the youngster, hence we slightly over-estimate the effect of ENROLLING into a 90-day wage subsidy.

[12] Nevertheless, we likely only use observations where there was a decent match between the firm and the youngster, hence we slightly over-estimate the effect of enrolling into a 90-day wage subsidy.

[13] Restricted to those who spent at least 30 days continuously in public works during the observation period.

2016, public works participants constituted around 40% of registered jobseekers and ALMP participants.[14] In these two years, more young people were enrolled in the public works scheme than in the YG programme (see Table 2)[15]. The scheme is not reserved for the long-term unemployed: in 2016, for example, nearly 20 percent of newly registered 16-19-year-olds entered the public works programme within 90 days (Molnár, 2019). Although their number declined somewhat in 2017, participation in the public works scheme was relatively widespread until 2018.[16]

Third, all registered unemployed (except those receiving insured unemployment benefit) are eligible to participate in public works, and selection is delegated to municipalities, in a highly decentralised municipal system. Disadvantaged regions can receive additional funding for implementing public works, otherwise there are no detailed guidelines or supervision to ensure that the scheme is targeted to those in need. Survey-based evidence suggests that mayors follow varying strategies in allocating work opportunities in the scheme (Koltai 2015). Further, Reizer et al. (2022) demonstrates that the programme was used by the governing party to build client networks, which introduces external (non-needs-based) variation into the local availability of public works. Some municipalities received more funding for public works on the basis of election performance, exogenously increasing the chance that more employable workers enrol into the programme.

In summary, the size of the public works programme and the role of mayors' motivations in the allocation of public works funding creates random variation in the selection process of participants to the Youth Guarantee, including the job trial, and makes the participants of public works programme a valid control group. In Section 4 we show that the overlapping condition between our treatment and control groups holds.

We also argue that registered jobseekers who have not participated in any programme, nor in the public works scheme, do not constitute a valid pool for a control group. In the period of our analysis, roughly half of the young unemployed found a job within six months without entering an active measure, they are likely positively selected. By contrast, the small group of young persons (around 15% of new entrants) who had not been enrolled in either an ALMP or the public works scheme within six months of registration nor could they find a job by themselves, are likely to have markedly different unobserved characteristics, which would bias the analysis. For example, they may be more likely to participate in illegal work, have a disability, or have other family care obligations, and hence are not ready to take up a job on the primary labour market.[17]

We excluded those individuals from the control group who were 25 or older, enrolled outside the sample period or had also participated in any of the YG programmes or participated less than 30 days. In the remaining group of public works participants, the average length of employment was somewhat longer than that of the 90-day job trial: the median length was 136 and the mean duration was 158 days (while 99% of the participants were enrolled in a programme that was shorter than one year).

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[14] See Table 5.14. https://kti.krtk.hu/wp-content/uploads/2018/06/stat.pdf

[15] Roughly 1.8% of women and 3.2% of men aged 20-24 were employed in a PW programme in 2017. At the same time, the NEET rate was 14% and 20%, for young men and young women, respectively.

[16] Although from 2017 onwards, young people under age 25 could be enrolled in the public works scheme only at their own request, Molnár (2019) shows that initially this had little impact on enrolment practices.

[17] In fact, in interviews with LLO staff Bördős et al. (2018) found that those who could not (or did not want to) enrol even in public works were considered not employable (due to motivation or substance abuse issues).

A person can participate in a public works programme several times, but we considered only the first appearance of each person starting after 1 January 2015 and before 1 April 2017[18]. The latter restriction is necessary because outcome variables are available until the end of 2017. However, they could have been public works participants in the past.

[Table 2 here]

Outcome variables To draw a picture of how successfully the job trial improved the labour market prospects of participants, we look at three outcome variables[19]:

1. Number of days the participant is employed in a position with earnings above 80% of the minimum wage[20] in the six months after completion of the programme.
2. Number of days employed as an employee during the six months after completion of the programme. This outcome gives us a glimpse of the stability of work.
3. Cumulative earnings from the primary labour market. As public works is a labour market programme, which uses public funds and our primary interest is in job prospects of young participants on the open labour market., we excluded public works wages.

Control variables The propensity score is estimated with a logit model where the probability of employment is explained an exceptionally rich set of variables listed below. (For a more detailed look at the set of variables used in this paper, see Appendix B.)

Demographics

Age (at the start of the programme); level of education (ISCED 1-9); health indicators (based on medical records of the past 12 months).

Labour market and parental history

History of PES registry spells (number and time since last spell); long-term and short-term employment history (following Lechner and Wunsch, 2013); long-term and short-term NEET and parental benefit history.

Job-related variables

Type of job (ISCO-1 code; for job trials and public works); and type of relevant jobs (ISCO-1 code, claimed by the individual at initial PES interviews).

Geographical variables

We include a bunch of geographical variables to capture transportation costs and labour market environment of the young persons. Being faced with costly and cumbersome transportation options might induce a young jobseeker to choose a labour market programme in her home village rather than spending a lot of time and money finding and securing a job with a wage subsidy in a distant town or even regularly visit the PES. To address this problem, in addition to NUTS2 region of PES office we

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[18] There are some exceptions to this rule. As the duration of public works programs varies between 30-428 days, the control group includes workers who reported participating in a public works program in May 2017.
[19] To enable comparability with other studies, we also report probability of employment 6 months after the programme in the Appendix.
[20] It is worth noting that the public works wage at the time was fixed at 80% of the minimum wage.

include the type of settlement where the jobseeker lives; travelling distance between settlement and PES office (shortest travelling time by car); an indicator variable showing whether the PES office is located in the main city of the county.

To capture labour market environment and segregation, we include ratio of public works participants in the settlement; ratio of Roma people in the administrative district (LAU1); a development index for the PES's district used by authorities[21]; and the ratio of public works participants among the registered unemployed in the settlement among those aged 25 or lower.

We believe that our outstandingly rich set of data on the observable characteristics and the labour market histories of the participants ensures that the conditional independence assumption will hold.

Descriptive statistics and selection into the programme According to the principles of the YG, priority should be given to the long-term unemployed, the vulnerable, and the socially excluded groups. While some countries such as Latvia managed to keep to these goals (Bratti et al., 2018) we find that the Hungarian YG definitely has room for improvement in this regard.

As Table 3 and Table A 1 suggest, Hungarian YG job trial participants are in a much more favourable position based on most of the characteristics measured. For most of the variables, the differences between the treatment group and public works participants were significant. For example, there are considerable differences in the level of education: 61% of public works participants, compared to 30% of job trial participants, had elementary education or less. Accordingly, program participants are less likely to look for jobs with elementary work responsibilities, they have longer work experience and spent less time as a public works participant or in NEET status, they are less likely to have received any child-related benefits in the past, and the average number of months they received these benefits was also lower. They are less likely to come from a less developed region, and from a settlement with a high incidence of public work. Job trial participants also tend to spend less on healthcare and visit medical practitioners less often.

We come to a similar conclusion if we compare 90-day job trial participants or all Youth Guarantee programme participants with the whole eligible population, the pool of registered jobseekers under the age of 25 years (see Figure A 1 and Figure A 2). The comparison reveals that jobseekers with better labour market prospects participate in the programmes of the Youth Guarantee, and the most employable jobseekers are enrolled into the job trial.

[Table 3 here]

Matching method Kernel matching is applied based on a propensity score[22] with the restriction that everyone should be matched to people from the same gender, with similar level of education (ISCED 1-2, 3-5, or 6-9), and same semester of entry into the respective programme (job trial or public works). Our chosen bandwidth is based on the method proposed by Huber et al. (2015). As the results are usually robust to changes in the shape of the weight function (see Caliendo & Kopeinig (2008)), we simply opt for the widely used parabolic (aka. Epanechnikov) kernel with the common support restriction.

The standard errors are computed based on estimated influence functions, as proposed by Jann (2019, 2020). These errors are robust to heteroskedasticity; however, they assume fixed matching weights, which is an oversimplification. Monte Carlo simulations suggest that this bias usually leads to estimates

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[21] Based on gov. decree 290/2014. (XI. 26.)
[22] Estimated via logistic regression using the variables listed in Appendix B.

with a relatively small bias if multiple matches are used (Jann, 2020) and this bias tends to be conservative (i.e. loo large standard errors) when using propensity score.

We prefer kernel matching over the commonly used one-to-one or nth neighbour matching because it is more efficient, allowing us to exploit more variation from the control sample. Keep in mind that kernel matching has lower variance and higher bias than one-to-one matching (see e.g. Caliendo & Kopeinig, 2008 or Blundell & Costa-Diaz, 2009), which may decrease the overall error. When we compared our results to the one-to-one and 5-neares-neighbor estimates as a test of robustness we found similar results[23].

As a result of significant differences in observable characteristics, the propensity score distributions before matching sharply differ in the control and the treatment groups. One way to test if the matching pool is varied enough is to look at the distribution of the probability of participating in the job trial programme (the propensity score, as described later in this section) in the control group. At first glance it might seem alarming that most public works participants have a propensity score of less than 10%. However, as (young) public works participants are quite numerous there are enough people on the higher end of the distribution to form a counterfactual control group, so the overlap condition holds. The probability of participation is higher than 60% for 1102 public workers, and is over 80% for 848 among them[24]. As a result, the two distributions overlap well so this condition holds.

The distribution of propensity scores after matching are in line and no significant difference remains for the main covariates in the matched sample (see Figure 1). The two graphs indicate that the matching was successful.

[Figure 1 here]

5 | RESULTS

Baseline results The baseline matching results are summarised in Table 4. Comparison of the averages of the outcomes on the 6 months horizon for the treatment and the control group show that on average, job trial participants perform much better than public works participants. The difference is sizeable in case of all outcomes, however, our matching results reveal that the differences in observable characteristics explain 1/2-3/4 of the raw mean differences in the outcomes between job trial and public works participants.

Compared to the control group of public works participants, the job trial participants were employed more by 14 days in a job that pays at least 80% of the minimum wage more during the six months following the programme. As the baseline value for the public works participants is 24 days, the ATT in relative terms is close to 58%. When we consider employee contracts, the effect is somewhat stronger: job trial participants work almost 20 days more as an employee during the six months following the programme, indicating than the programme facilitates placement in standard employment statuses. Program participants accumulated more earnings by 65% of the statutory minimum wage than the control group during the six months after completing the programme.[25]

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[23] Tables of this test of robustness are provided upon request.
[24] The same numbers for job trial programme participants are higher but comparable in magnitude: 1737 over 60% and 1152 over 80%.
[25] We also estimated the impact of the programme on the probability of being employed for our outcome variables, see Table A 2 in the Appendix.

[Table 4 here]

Taking into account that according to both international (Caliendo et al, 2016; Card et al, 2010) and Hungarian (Cseres-Gergely and Molnár, 2015, Köllő and Scharle, 2012) evidence, participation in the public works programme might have a negative impact on employment prospects on the primary labour market, the relative efficiency of the job trial compared to the public works programme might encompass both the positive impact of the job trial and the negative effect of the public works programme.

Robustness checks: sub-sample with competence test scores As a robustness check, we estimated the model with a variable that is presumably correlated with abilities, results of mathematics and literacy competence tests[26] written in all schools of the country by pupils in 8th and 10th grade (roughly ages 12 and 16) (Hermann,2020). As tests scores are available for only about 85% of the sample, and missing test scores cannot be regarded as random[27], we estimated the propensity score with and without the 10th class standardized competence test score on the same subsample with competence scores available. The results indicate that the competence test score does not alter the results significantly, indicating that unobserved abilities do not threaten our results (see Table A 3).

Robustness check: program combinations As we discussed earlier, job trial participants who were enrolled in a longer-term wage subsidy after completing the job trial are excluded from our treatment group, as the probability of employment in the six or 12 months after completing the job trial is automatically high when participants are in another wage subsidy programme. However, this restriction may pose a selection problem, as the selection into the programme combination is presumably not random.

We addressed the problem of eliminating participants from the treatment group by exploiting the fact that some of the public employment services did not apply this programme combination for financial motives. The job centres have an incentive to enrol a high number of young jobseekers into the YG programmes, but because their financial resources are limited, the offices may try to restrict the subsidy per young person. Therefore, in some cases, the offices may fail to support programme combinations. We found four counties[28] that had not applied programme combinations at all during our sample period, and estimated the propensity score matching model with the control group of public works participants by restricting the sample to only these counties.

The estimated ATT based on the restricted sample is higher for all outcomes, but considering the wide standard errors, the results are broadly in line with the baseline results (see Table A 4). Nonetheless, our finding that almost 30% of the job trial participants received another wage subsidy implies that participation in the job trial programme prepared the ground for establishing stable working relationships.

Robustness check: heterogeneity by share of public work participants to unemployed population aged 25 or over

A potential concern with our results is that there are settlements where the probability of getting to public works is relatively low, and in these areas, only those with particularly low skills are selected into public works. Hence, the overlap between the skill distribution of job trial and public works participants is weak, and as a consequence the estimated impact of the job trial might be exaggerated. To address this concern, we estimated the ATT on two subgroups: young persons living in settlements

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[26] These are largely similar to the PISA tests.

[27] Table A 7 shows the comparison of those with and without competence test score in the sample of job trial and public work programme participants

[28] These were: Győr-Moson-Sopron, Tolna, Vas and Zala

with high (above median) and low (below median) share of public works participants among the unemployed population aged 25 or over[29]. The share of the public workers in the unemployed population can be regarded as exogenous to the selection young participants into the programme. We find that the treatment effect is similar in the two groups, indicating that - after controlling for other observable characteristics and regional covariates - the coverage of the local public works programme does exert a strong influence the impact of the job trial.

[Table 5 here]

Heterogeneity by the level of education We estimate the treatment effect separately on participants with elementary education or less and secondary or higher education. Compared to the public works participants, comparison of higher and lower educated participants reveals no major difference in the estimated treatment effect (see Table 6). While young jobseekers with low education had a lower chance of being enrolled in the programme, the impact of participation on their employment probability on the six months horizon is comparable to participants with secondary or higher education and even higher in relative terms, as the baseline employment rate is lower for those with basic education. This implies that giving lower educated young unemployed a higher priority in the job trial programme would not decrease the average treatment effect, while likely reducing the deadweight loss of the programme.

Heterogeneity by level of competence and education We took the sub-sample of participants for whom we have available PISA competence test scores (from 8th and 10th grades), and divided them based on how well they performed on these tests[30]. We further divided the sample by whether the participant finished secondary school or not. We estimated the ATT for each group separately and the counterfactual (post-matching) control average as well (see Table 7).

The estimates suggest that schooling is an important signal of ability for employers as those who finished secondary school will work more and have higher earnings from the primary labour market than those who did not, even if their competence levels are low. However, the results also indicate that the 90-day job trial helps those with higher competence levels but lower level of education to overcome this signalling-barrier, as the effect of the programme is by far the highest for this group.

[Table 6 here]

[Table 7 here]

Gender differences in the selection We investigate the gender dimension of the policy from two aspects: gender differences in selection into the programme and in the impact of the programme on the participants' employment and earning outcomes and focusing on the question of whether participation in YG programmes can help young mothers return to or enter the labour market.

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[29] Formally, as long-term programme participants are not counted as registered unemployed, we calculate the share of programme participants among young people below age 25 if they are either registered unemployed or public works or other ALMP participants.

[30] The two groups were divided among the following line: did the student reach level 7 out of 14 if we sum up the achieved levels on the literacy and numeracy tests (originally measured on a scale of 1-7)?

The gender composition of the job trial participants was close to 50-50%. Nevertheless, considering that the NEET rate of young women was higher, the gender balance of participation implies that the outreach to women is weaker (Csillag et al, 2021).

[Table 8 here]

The comparison between male and female job trial participants indicates that the female participants are, on average, in a more favourable labour market position and had a higher level of education than the male participants. Though female participants received parental benefits for a longer period (1,87 months in average, compared to the 0,1 months for males), they have a shorter NEET history even including parental leave (see Table 8). Comparison of participation in job trial and public works participants separately between for women and men reveals that female job trial participants constitute an even more strongly selected group, with even better labour market prospects compared to the control than that male participants, that is a young woman jobseeker has to show better labour market prospects than male peers to have a chance to be selected into the programme (see Table A 5 and Table A 6).

Gender differences in the effect of the job trial Our results indicate that compared to participation in public works programmes, participation in the 90-day job trial had a somewhat stronger impact on the employment prospects of the male than of the female participants: i.e., participation in the job trial increased both the number of days spent in employment in the six months after the programme, and the cumulative earnings and employment within the six months following the programme more for men than for women (see Table 9).

[Table 9 here]

This is in contrast with the conclusion of majority of the existing studies on the gender differences in the effects of active labour market policies have found that women benefit more than men from labour market programmes. (e.g., see a survey by Bergemann and van den Berg (2008) for Europe).

We argue that the main explanation for these diverging results is that the factors mentioned in the literature as explaining the stronger impact of ALMPs on women are missing in the case of the 90-day job trial. Specifically, Bergemann and van den Berg (2008) argues women have more options than men to split their time between paid employment and housework and childcare which implies that the female labour supply is more responsive to wage changes than the male labour supply[31]. Unemployed women – even if they are registered jobseekers – presumably have higher reservation wages, as having more outside options makes the option of not working more attractive. Participation in a programme that increases labour market opportunities also increases the probability of receiving attractive job offers, and results in a greater average programme effect for women.

We showed that addition, that female participants were, on average, in a more favourable labour market position and had a higher level of education than the male participants, which could mean that they were even more engaged in the labour market than the male participants (see Table A5 and A6 in the Appendix). The weaker impact found for women is also in line with the results on impact heterogeneity by education, which indicate that the effects of the programme are slightly weaker on better educated participants.

Outcomes 6 vs 12 months after the program In this section, we compare the results for the outcome variables 6 vs. 12 months after completing the programme. To ensure that the estimations on the two horizons were compatible with each other, we restrict our sample here to young people whose outcomes could be observed 12 months after completing the programme. This implies that the job

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[31] This is the so-called Chatelier principle, which states that individuals with more options have more elastic supply functions, This principle has largely been confirmed by empirical studies (see, e.g., Evers, M., De Mooij, R., & Van Vuuren, D., 2008).

trial participants who entered between 1 January 2015 and 30 September 2016 are included in the treatment group.

The results (Table 10) indicate that the effect of participation in the job trial on employment and wage outcomes was lower on the horizon 12 months after the programme compared to the impact on the six-month horizons. The coefficients for cumulative days are lower than the double of the corresponding coefficients within six months after the programme, indicating that the difference weakens in the second six months.

[Table 10 here]

6 | DESCRIPTIVE ANALYSIS OF LABOUR MARKET TRAJECTORIES AFTER THE PROGRAMME

This section outlines some risks that may reduce the impact of short-term wage cost subsidy programs like the 90-day job trial. We analyse the labour market trajectories of those enrolled in the programme and look at the raw differences between those who manage to stay at the same company (stayers) and those who lose their job after the job trial program (leavers).

[Table 11 here]

Overall, 4444 people enter the job trial programme in our sample[32], and 47% of these people (2098 persons) leave the company after the 90 days. Of those who stay, about 29% (1284 persons) receive some kind of wage subsidy from the government after the 90-day trial. The remaining 1062 people are the ones we call stayers: they spend at least one more month at the company after the job trial ends (with no additional cost to the state). As Table 11 suggests, stayers are (on average) a better-qualified, more experienced group than leavers. Those who leave the company after the job trial tend to be lower educated, have less work experience, and but only slightly lower competence test scores from school. They are more likely to have children, and to come from a less developed settlement where the ratio of public workers is higher in the working age population.

To be more precise, stayers are 10 percentage points more likely to have completed secondary education than leavers, while their level of competence is only slightly better (a fraction of a competence level, on average). We also estimate that job trial participants who finished secondary school have higher levels of competence (around one level higher for both numeracy and literacy) on average. These two statements can only be true at the same time if our finding from Section 5 stands, and the job trial programme works as an effective screening device for employers, increasing the probability to become a stayer for those with lower education but higher skills.

To get a glimpse of what happened to participants after the programme, we compared the trajectories of several labour market outcomes for stayers and leavers over time to assess the quality of their labour market attachment. We looked at the probability of being employed with earning at least the equivalent of 80% of the monthly minimum wage during the given month, wages (conditional on being employed) earned during a given month (measured as percentage of the median monthly wage of 20–25-year-olds).

As Figure 2 depicts, the share of those employed and earning above 80% of the minimum wage during a given month (not in public works) declined in both groups after the end of the 90-day job trial. There are two conclusions we draw from this finding. First, around 40% of those who manage to stay at the

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[32] Which is restricted to those who are observed for at least 12 months after the job trial, in this analysis.

firm after the programme will end up in low-paying jobs, public works, or out of a job within 4 months after the programme, indicating that when the subsidy and therefore the period of rent-maximizing is over, some companies try to cut costs by decreasing the wages or working hours of the labourer, or that firms shared the subsidy during the first 3 months with the employee. Second, if the participant ends up in the group of leavers, their labour market prospects are doomed for a long time while the main alternative, public works at least offers stable income for a longer period (public works spells often last around 9 months or even more)[33]. The decrease is smaller if we consider all types of work (excluding public works). Figure A 3 in the Appendix shows that less than 50% of those who leave the company after the job trial will have any kind of job 6 months after the programme.

[Figure 2 here]

We find some indication that the job trial is used in short-term, seasonal jobs, as commercial and catering occupations are overrepresented among job trial participants (23%) compared to their share among 20-25-year-old population (15%). The industry breakdown of the firms participating in the job trial shows a similar pattern: the share of commerce, hotels and catering is 37% among firms who employ job trial participants, compared to 26% among the employers of all 20-25 year olds.

Another indication of misuse of the subsidy is that close to 20% of those who are retained by the employer 6 months after the programme are shifted into precarious forms of employment: public works, casual work or part-time position. This means that companies cut costs radically on around one fifth of those who stay at the company after the subsidy runs out, leaving them with more precarious working conditions.

The total monthly earnings of programme participants paint a similar picture about their labour market trajectories. This includes all types of wages (excluding earnings from public works). As Figure 3 indicates, during the job trial the wages of both stayers and leavers are around the median wage of 20–25-year-olds. After the 90 days are over, salaries of both employed stayers and leavers fall. Those who work in the leaver group start earning significantly less (less than 80% after 6 months) and stayers are fare only slightly better (around 85-90% after 6 months).

Overall, the analysis suggests that staying with the first employer receiving the subsidy is an important channel in generating the impact of the job trial programme. However, these data draw attention to one of the risks of short-term wage subsidies. Even if a firm does not (mis)use the programme to finance short-term seasonal work, it might still try to cut costs by lowering wages or working hours. Without further incentives from the state, there is no guarantee that participating youth will remain in relatively high-quality jobs with a stable contract, insurance, and good compensation.

[Figure 3 here]

7 | CONCLUSIONS

The 90-day job trial programme represents a type of subsidy that is often overlooked by policy makers and researchers alike. For the government, it is a short-term and consequently relatively low-cost wage subsidy. For the employer, it works as a cheap screening device testing potential employees' skills in

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[33] Of course, these results might be coupled with selection bias, as those who need a high-paying job only for a short period of time (those who plan to study or raise children for example) probably prefer the 90-day programme over public works.

practice. For the employee, it provides an opportunity to evaluate whether the job offered matches their skills and interests.

We used an outstandingly rich set of administrative data from a variety of authorities to estimate the impact of participation on labour market outcomes in the 2015-2017 period in Hungary. Our identification strategy relies on matching programme participants to public workers with similar propensity scores – calculated based on work history, education, skills measures by high school competence tests, geographical and demographic variables, etc. Public works participants constitute a more credible control group than non-participants, basically all young jobseekers, who were not enrolled into any ALMP, were sent to public works programmes.

We find that the programme is effective, as it significantly increases the number of days worked as an employee (by ~+20 days), the number of days working above 80% of the minimum wage (~+13 days), and cumulative earnings on the 6-month horizon (~+61% of the statutory minimum wage). Unfortunately, these beneficial impacts seem to diminish over time, as 1-year effects are significantly lower than twice the 6-month effects on employment and wages. Interestingly, we find that this active labour market policy benefits men rather than women (in terms of improved labour market prospects).

We estimate that the programme has a similar impact on days worked and cumulative earnings regardless of level of education, and even stronger impact in relative terms. Additionally, it seems that lower educated people with higher competence test results from school benefit the most from the programme (in terms of labour market outcomes). One potential explanation for this finding is that employers retain those participants with higher competence level, even if they have lower education. This implies that giving lower educated young unemployed a higher priority in the enrolment process would not decrease the average treatment effect, while reduce the deadweight loss of the programme. Our findings also indicate that people who did not complete secondary school and have lower skill levels will not benefit much from the job trial programme alone, without further training, which suggests that the 90-day job trial programme achieves its goal as an effective screening device for employers (it enables them to discover young people among the lower educated. We also show that in practice those with lower education are less likely to be enrolled in the job trial programme which highlights how crucial targeting and implementation for such ALMPs.

We also present some risks to this programme design, policy makers should account for, and incentivise companies against. These are all related to the short duration of the subsidy. First, we show that the 90-day job trial could be exploited by employers to cover the costs of short-term or seasonal work (and we present some evidence indicating that this probably happened in Hungary during the implementation of the job trial programme). Second, we highlight that even among those who stay at the company at least one months after the subsidised period, a quarter of them end up in public works, casual work or working part-time, a couple of months after the programme. Finally, we also present evidence that some companies cut costs after the job trial by lowering wages or decreasing working hours.

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TABLES AND FIGURES

TABLE 1 NUMBER OF PARTICIPANTS IN YOUTH GUARANTEE PROGRAMMES BY GENDER AND YEAR ON THE DATABASE*

	2015		2016		2017	
	Male	**Female**	**Male**	**Female**	**Male**	**Female**
All YG programmes	5711	4973	5267	5096	7355	7015
YG Wage subsidies	3170	2975	3586	3360	4887	4337
YG 90-day job trial**	1249	1253	1617	1595	1939	1890

Source: Admin3 database: a 50% random sample of the Hungarian population.
*The expected value of participation the population is twice the numbers shown in the table.
**Estimation based on the duration, the details see in Section 3.

TABLE 2 NUMBER OF PEOPLE UNDER AGE 25 ENROLLED IN YG AND PUBLIC WORKS

	2015	**2016**	**2017**
Youth Guarantee	10684	10373	14370
Public works	21223	19470	11830

Source: Admin3 database: a 50% random sample of the Hungarian population.
*The expected value of participation the population is twice the numbers shown in the table.
**Estimation based on the duration, the details see in Section 3.

.

Variable	Mean	
	Job trial	Public works participants
Gender (male)	51.0%	57.4%
Elementary education (or lower)	28.2%	61.6%
Employment in the last 2 years	8.2 months	4.1 months
NEET (last 2 years, excl. par. leave)	6.8 months	9.4 months
Received par. benefit ever	1.0 months	3.7 months
Time spent in hospital last year	0.36 days	0.64 days
Underdeveloped district[34]	22.5%	31.1%
Ratio of public workers (among unempl.)	22.5%	37.1%
Small settlement (< 1K cap)	11.8%	19.8%
Competence level (mathematics)*	3.27	2.62
Competence level (literacy)*	3.59	2.97
Number of persons	**3760***	**24753***

Means presented in this table are significantly different from each other with 99% confidence.

For a more detailed view on the descriptives see Table A 1 of the Appendix.

*Competence levels (tested in 8th and 10th grade of school, around age 14 and 16) are only available for a sub-sample of participants (17,783 out of 28,513). They are measured on a scale of 1-7 where 7 represents the highest level of competence.

TABLE 4 | MATCHING RESULTS: EMPLOYMENT AND CUMULATED EARNINGS DURING THE SIX MONTHS AFTER THE PROGRAMME

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative earnings, excl. public works
ATT	13.30***	19.17***	0.611***
	(2.644)	(2.682)	(0.148)
Raw diff.	33.90	40.55	1.67
Raw baselines			
Job trial participants	58.47	67.77	2.97
Public works participants	24.57	27.22	1.30
# treated	3183	3183	3183
# control	19209	19209	19209

Standard errors in parentheses. Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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[34] „To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)

TABLE 5 | AVERAGE TREATMENT EFFECT ON THE TREATED BY SHARE OF PUBLIC WORKERS AMONG UNEMPLOYED AGED 25 OR ABOVE

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative wage, excl. public works	# treated	# control
Primary school or lower, ATT	15.04*** (3.448)	21.71*** (3.512)	0.752*** (0.168)	1804	5511
Counterfactual average.					
Secondary or higher, ATT	14.93*** (3.796)	19.21*** (3.890)	0.637*** (0.184)	893	5573
Counterfactual average.					

Standard errors in parentheses. Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Propensity score was estimated on the restricted samples. Control averages presented in the table are the post-matching, weighted (counterfactual) control means.
$^{*} p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01$

TABLE 6 | AVERAGE TREATMENT EFFECT ON THE TREATED BY LEVEL OF EDUCATION

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative wage, excl. public works	# treated	# control
Primary school or lower, ATT	15.41*** (2.614)	19.24*** (2.704)	0.706*** (0.122)	925	11553
Counterfactual average.	21.29	24.35	1.18		
Secondary or higher, ATT	14.76*** (3.369)	22.24*** (3.385)	0.733*** (0.171)	2344	6533
Counterfactual average.	54.61	57.46	2.82		

Standard errors in parentheses. Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Propensity score was estimated on the restricted samples. Control averages presented in the table are the post-matching, weighted (counterfactual) control means.
$^{*} p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01$

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative earnings, excl. public works	# treated	# control
Low comp low educ	11.67*** (4.351)	15.87*** (4.718)	0.490*** (0.185)	367	3541
Cont. avg.	23.50	28.06	1.33		
Low comp high educ	10.10 (8.576)	16.91** (8.447)	0.448 (0.382)	552	1563
Cont. avg.	54.63	58.44	2.75		
High comp low educ	23.82*** (8.941)	25.23*** (9.021)	1.092*** (0.383)	146	601
Cont. avg.	28.80	32.20	1.48		
High comp high educ	12.22** (4.847)	19.89*** (4.917)	0.593** (0.238)	763	1507
Cont. avg.	54.07	56.067	2.82		

Standard errors in parentheses

Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Control averages presented in the table are the post-matching, weighted (counterfactual) control means. Competence test scores are only available for a younger subsample (as 8th and 10th grade standardized test scores are only available from 2006).

$^{*} p < 0.1$, $^{**} p < 0.05$, $^{***} p < 0.01$

TABLE 8 | DESCRIPTIVE STATISTICS OF SOME KEY VARIABLES FOR MEN AND WOMEN

Variable	Mean		
	Men	Women	p-value
Elementary education (or lower)	34.8%	21.3%	0.000
Employment in the last 2 years	8.6 months	7.9 months	0.011
NEET (last 2 years, excl. par. leave)	7.2 months	6.4 months	0.001
Received par. benefit ever	0.1 months	1.9 months	0.000
Time spent in hospital last year	0.359 days	0.364 days	0.934
Underdeveloped district[35]	24.2%	20.8%	0.013
Ratio of public workers (among unempl.)	25.2%	24.9%	0.579
Small settlement (< 1K cap)	13%	10.6%	0.021
Competence level (mathematics)*	3.33	3.22	0.028
Competence level (literacy)*	3.26	3.90	0.000
Number of persons	**12397 ***	**16116***	

Means presented in this table are significantly different from each other with 99% confidence.
*Competence levels (tested in 8th and 10th grade of school, around age 14 and 16) are only available for a sub-sample of participants (17,783 out of 28,513; as 8th and 10th grade standardized test scores are only available from 2006). They are measured on a scale of 1-7 where 7 represents the highest level of competence.

TABLE 9 | AVERAGE TREATMENT EFFECT ON THE TREATED BY GENDER

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative wage, excl. public works	# treated	# control
Women, ATT	11.54*** (3.775)	17.92*** (3.801)	0.652*** (0.169)	1579	7296
Men, ATT	15.71*** (3.171)	22.00*** (3.240)	0.608*** (0.160)	1645	10143

Standard errors in parentheses. Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Propensity score was estimated on the restricted sample – this is why some pairs of estimates are not comparable to the baseline estimates in Table 4.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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[35] To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)

TABLE 10 | COMPARISON OF OUTCOMES SIX AND 12 MONTHS AFTER THE PROGRAMME

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative wage, excl. public works
6-months ATT	11.84***	18.33***	0.529***
	(3.158)	(3.185)	(0.177)
1-year ATT	14.96**	28.95***	0.698**
	(6.284)	(6.203)	(0.350)
# treated	2718	2718	2718
# control	15291	15291	15291

Standard errors in parentheses

Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 11 | DESCRIPTIVE STATISTICS OF STAYERS AND LEAVERS

Variable	Mean		
	Stayers	**Leavers**	**p-value**
Gender (male)	50.8%	51.2%	0.836
Elementary education (or lower)	20.6%	31.7%	0.000
Employment in the last 2 years	10.2 months	7.3 months	0.000
NEET (last 2 years, excl. par. leave)	5.1 months	7.6 months	0.000
Received par. benefit ever	0.5 months	1.2 months	0.002
Time spent in hospital last year	0.28	0.40	0.121
Underdeveloped district[36]	15.3%	25.9%	0.000
Ratio of public workers (among unempl.)	23.2%	25.9%	0.000
Small settlement (< 1K people)	13.6%	11.0%	0.020
Competence level (mathematics)*	3.4	3.21	0.001
Competence level (literacy)*	3.68	3.54	0.009

Programme participants are categorized as stayers if they stay the at firm for at least one month after the job trial (without any further incentive from the government). Sample is restricted to those who are observed for at least 12 months after the job trial.

*Competence levels (tested in 8th or 10th grade of school, around age 14-15 or 16-17) are only available for a younger sub-sample of participants (3227 out of 3760; as 8th an d 10th grade standardized test scores are only available from 2006).

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[36] „To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)

FIGURE 1 | COMPARISON OF THE RAW AND THE MATCHED SAMPLE
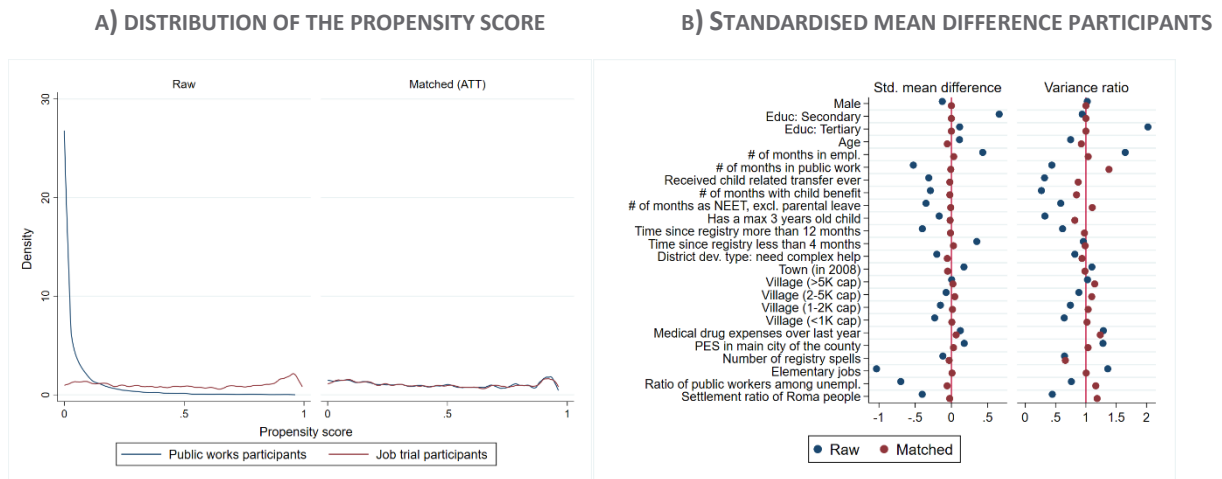
A) DISTRIBUTION OF THE PROPENSITY SCORE   **B) STANDARDISED MEAN DIFFERENCE PARTICIPANTS**


Notes: The standardised difference is calculated as the difference between the mean of the two groups over the standard deviation of the treatment group. The variance ratio reflects the variances of the given covariate in the treatment group over the variance of the control group. The blue dots represent raw sample statistics, while the red dots indicate post-matching statistics. Matching is more balanced if the post-matching statistics are closer to the red lines.

FIGURE 2 | PROBABILITY OF BEING EMPLOYED IN A POSITION WITH EARNINGS ABOVE 80% OF THE MINIMUM WAGE EMPLOYEES WAGE BY MONTHS SINCE THE START OF THE TREATMENT
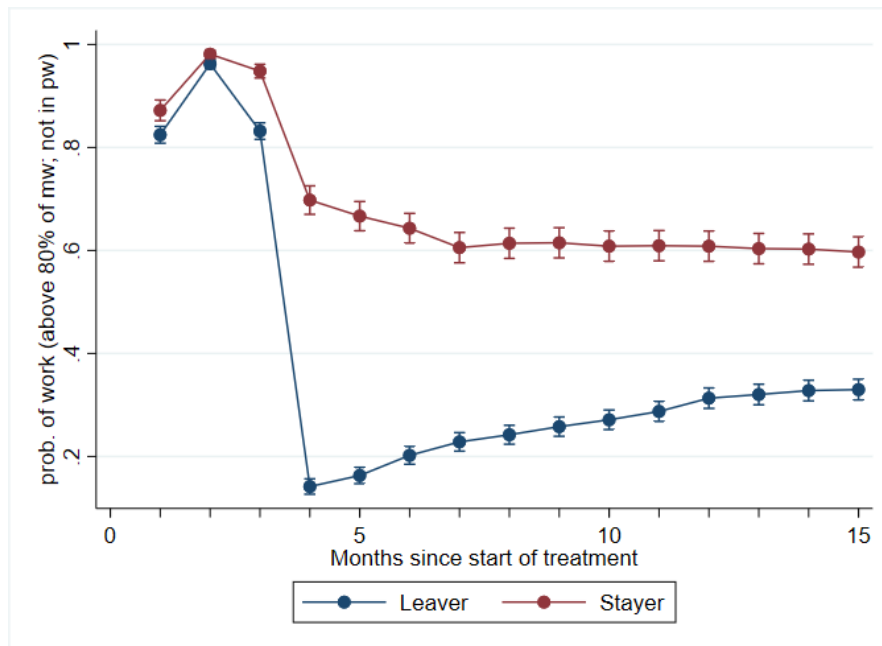
Notes: 90-day job trial programme participants are divided into the groups of stayers and leavers based on whether they stayed at the firm for at least one month after the subsidised period or not. Dots represent the probability of having a job and earning above 80% of the minimum wage for a given month counting from the beginning of the job trial.
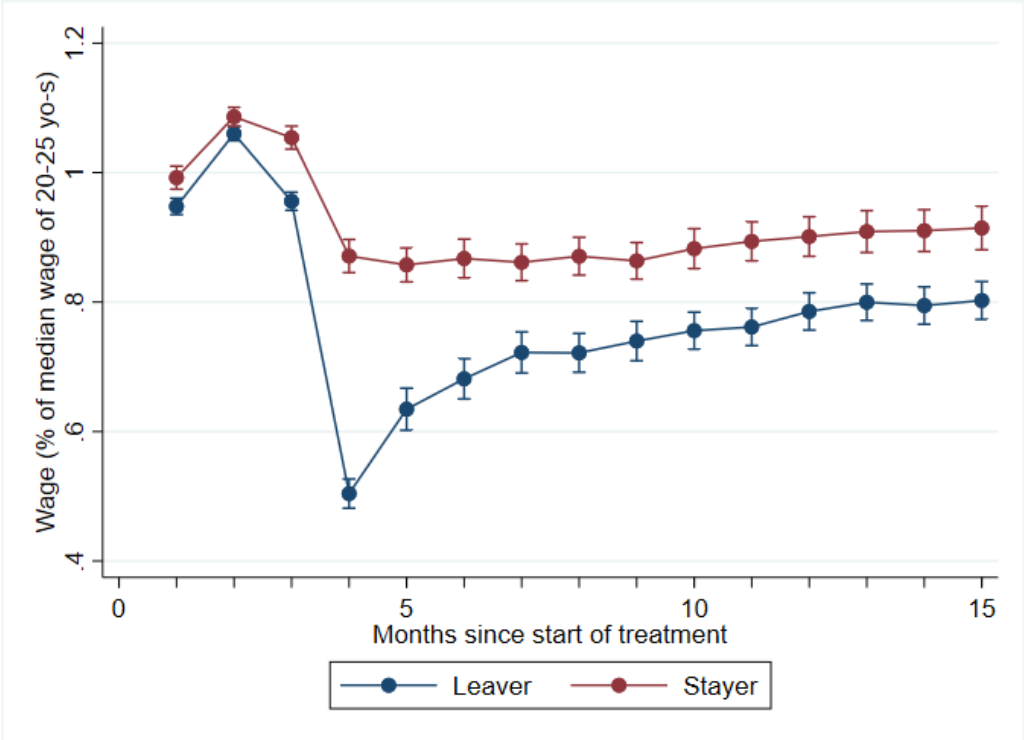
Notes: 90-day job trial programme participants are divided into the groups of stayers and leavers based on whether they stayed at the firm for at least one month after the subsidised period or not. Dots represent the mean wage of those who work (compared to the median wage of all 20–25-year-olds for a given month counting from the beginning of the job trial.

APPENDIX

Variable	Mean		T-test		
	Treated	Control	%bias	t	p>|t|
Male	0.510	0.573	-12.60	-7.230	0.000
Age	21.26	21.04	10.40	5.660	0.000
Education: elementary	0.282	0.615	-71.10	-39.46	0.000
Education: secondary	0.680	0.367	65.90	37.21	0.000
Education: tertiary	0.038	0.0178	12.30	8.180	0.000
medical history					
Medical drug expenses in last year (HUF)	2416	1909	11.70	6.970	0.000
Inpatient expenses in last year (HUF)	7406	10672	-6.300	-3.100	0.002
Days spent in hospital last year	0.361	0.639	-6.800	-3.160	0.002
Outpatient expenses last year (HUF)	7352	6454	6	3.330	0.001
Visits to the General Practitioner	4.186	4.605	-8.100	-4.390	0.000
working history					0.000
# of months in empl.	14.46	8.702	43.40	27.47	0.000
# of months in empl. in last 2 years	8.235	4.126	59.40	38.38	0.000
# of months in public work	1.835	5.407	-53.90	-27.03	0.000
# of months in public work in last 2 years	1.317	4.348	-59.50	-29.27	0.000
# of months as NEET, excl. parental leave	12.90	19.98	-36.30	-18.95	0.000
# of months as NEET in last 2 years, excl. parental leave	6.341	7.987	-24	-13.50	0.000
# of months with child benefit	0.997	3.725	-28.90	-13.81	0.000
# of months with child benefit in last 2 years	0.497	1.511	-23.70	-11.63	0.000
Received child related transfer ever	0.033	0.113	-31.20	-15.17	0.000
Has a max 3-year-old child	0.012	0.038	-16.70	-8.140	0.000
Time since registry more than12 months	0.159	0.336	-41.70	-21.92	0.000
Time since registry less than 4 months	0.625	0.444	36.90	20.90	0.000
Number of registry spells	1.815	2.009	-13.90	-7.350	0.000
geographic characteristics					
District, cat.2 (preferential)	0.275	0.294	-4.200	-2.370	0.018
District, cat.3 (need development)	0.095	0.105	-3.200	-1.820	0.069
District, cat.4 (need complex dev.)	0.225	0.317	-20.80	-11.43	0.000
PES in county capital	0.270	0.196	17.60	10.48	0.000

Ratio of public workers	0.250	0.371	-67.30	-33.74	0.000
Travelling distance from PES (min.)	542.5	737.7	-31.40	-17.40	0.000

settlement type					
County capital	0.162	0.082	24.40	15.54	0.000
Town (in 2008)	0.409	0.326	17.30	9.930	0.000
Village (>10K cap)	0.001	0.000	2.700	2.020	0.043
Village (5-10K cap)	0.024	0.024	0	0	0.997
Village (2-5K cap)	0.162	0.190	-7.500	-4.120	0.000
Village (1-2K cap)	0.125	0.179	-15	-8.060	0.000
Village (<1K cap)	0.118	0.198	-22	-11.59	0.000

TABLE A 2 | BASELINE RESULTS FOR 6-MONTHS SPOT OUTCOME

	Being employed in primary labour market, 6 mo. After the programme
ATT	0.0726***
	(0.0184)
# treated	3183
# control	19209

TABLE A 3 | MATCHING RESULTS FOR SUB-SAMPLE WITH COMPETENCE TEST SCORES

	Cum. days worked above 80% of mw	**Cum days worked as employee**	**Cumulative wage, excl. public works**
ATT with comp. test. scores	13.05*** (3.711)	20.00*** (3.754)	0.554*** (0.185)
# treated	1988	1988	1988
# control	7386	7386	7386

Standard errors in parentheses
Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Competence test scores are only available for a younger subsample (as 8th and 10th grade standardized test scores are only available from 2006). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A 4 | AVERAGE TREATMENT EFFECT ON THE TREATED FOR COUNTIES WHERE NO SUBSEQUENT WAGE SUBSIDY IS PRESENT APPLIED AFTER THE JOB TRIAL

	Cum. days worked above 80% of mw	Cum days worked as employee	Cumulative wage, excl. public works
ATT	17.70	27.90***	0.390
	(11.09)	(9.952)	(0.570)
# treated	261	261	261
# control	737	737	737

Standard errors in parentheses
Table shows estimates of average treatment effect on the treated. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender, semester of entry, and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Estimation is restricted to the following counties: Győr-Moson-Sopron, Tolna, Vas, and Zala. Propensity score is estimated on the sample of all counties. Robustness of results was tested and confirmed by the use of propensity score estimated on the restricted sample.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A 5 | DESCRIPTIVE STATISTICS FROM MALE JOB TRIAL AND PUBLIC WORKS PARTICIPANTS

Variable	Mean		
	Job trial	Public works	p-value
Elementary education (or lower)	34.8%	69.2%	0.000
Employment in the last 2 years	8.6 months	4.4 months	0.000
NEET (last 2 years, excl. par. leave)	7.2 months	9 months	0.000
Received par. benefit ever	0.1 months	0.6 months	0.000
Time spent in hospital last year	0.36 days	0.56 days	0.148
Underdeveloped district[37]	24.2%	33.9%	0.000
Ratio of public workers to unempl. in the settlement	25%	37.1%	0.000
Small settlement (< 1K cap)	13%	20.5%	0.000

TABLE A 6 | DESCRIPTIVE STATISTICS FEMALE JOB TRIAL AND PUBLIC WORKS PARTICIPANTS

Variable	Mean		
	Job trial	Public works	p-value
Elementary education (or lower)	21.4%	51.3%	0.000
Employment in the last 2 years	7.9 months	3.8 months	0.000
NEET (last 2 years, excl. par. leave)	6.4 months	9.9 months	0.000
Received par. benefit ever	1.9 months	7.9 months	0.000
Time spent in hospital last year	0.36 days	0.74 days	0.000
Underdeveloped district[38]	20.8%	28.6%	0.000
Ratio of public workers among unempl. in the settlement	24.8%	36.3%	0.000
Small settlement (< 1K cap)	10.6%	18.9%	0.000

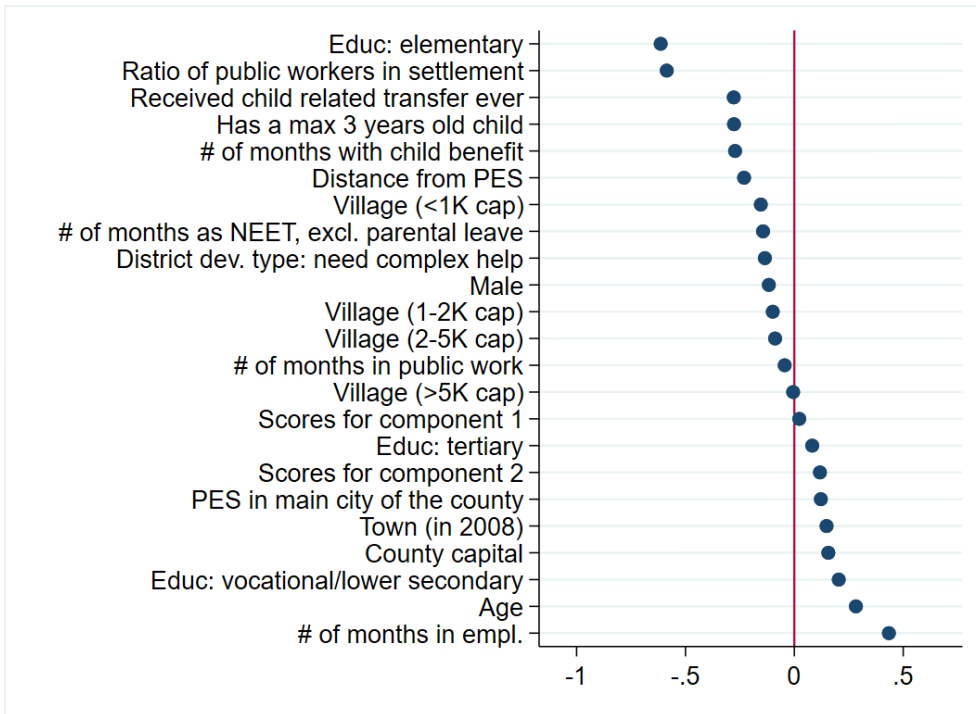[37] „To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)
[38] „To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)

Variable	Mean		
	With	**Without comp.**	**p-value**
Gender (male)	51.5%	64.8%	0.000
Elementary education (or lower)	44.5%	78.2%	0.000
Employment in the last 2 years	5.5 months	3.3 months	0.000
NEET (last 2 years, excl. par. leave)	7.3 months	11.8 months	0.000
Received par. benefit ever	2.2 months	5.3 months	0.000
Time spent in hospital last year	0.57 days	0.64 days	0.265
Underdeveloped district[39]	29.3%	32.3%	0.000
Ratio of public workers among unempl. in the settlement	35.1%	36.1%	0.000
Small settlement (< 1K cap)	18.6%	19%	0.412

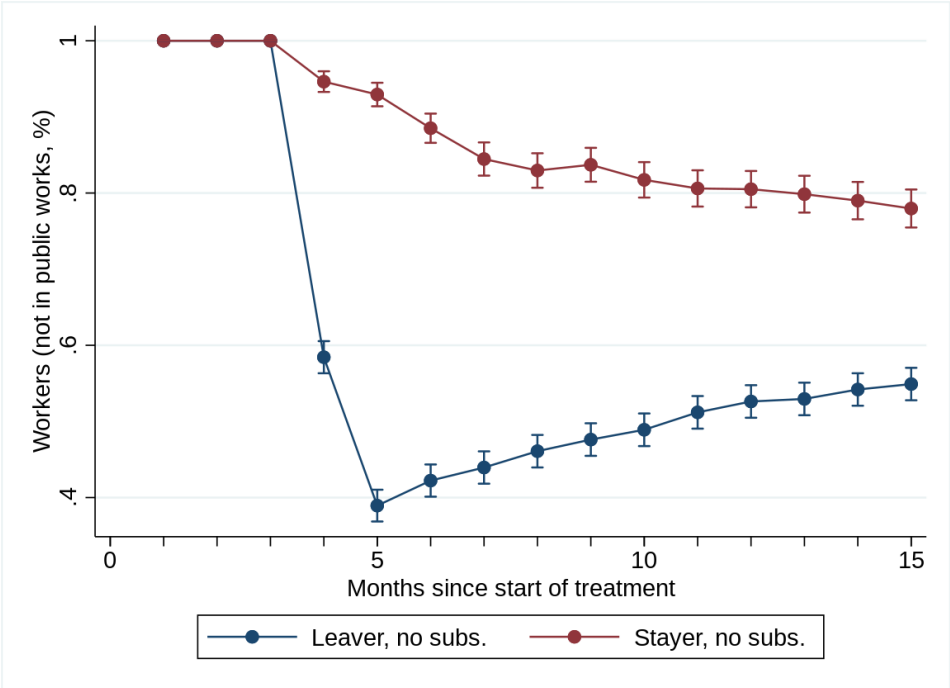[39] „To be developed with a complex programme" based on gov. decree 290/2014. (XI. 26.)

Notes: The standardised difference is calculated as the difference between the mean of the group of participants and registered jobseekers over the standard deviation of the treatment group.

FIGURE A 2 | COVARIATE BALANCE FOR YG PROGRAMME PARTICIPANTS VS REGISTERED JOBSEEKERS UNDER AGE 25

Notes: The standardised difference is calculated as the difference between the mean of the group of participants and registered jobseekers over the standard deviation of the treatment group.

Notes: 90-day job trial programme participants are divided into the groups of stayers and leavers based on whether they stayed at the firm for at least one month after the subsidised period or not. Dots represent the probability of having a job on the primary labour market during a given month counting from the beginning of the job trial.

APPENDIX B - OBSERVABLE CHARACTERISTICS

The selection into the programme is not random, and it may be assumed that the participants differ not only in their observable characteristics, but also in their unobservable characteristics such as motivations and abilities. These characteristics can influence their chances of participating in the programme, and can directly influence their future labour market outcomes. Matching methods rely on the key identifying assumption that conditional on the available observable variables, selection into treatment can be regarded as random. Our strategy is based on the idea that the employment and education histories, detailed geographical and health variables as competence test scores in our administrative dataset allow us to find a rich enough set of personal characteristics that will eliminate the bulk of the selection bias.

The following covariates are used in the analysis. The *age* of the participant shows the age at the time of the start of the programme (see *Figure A2).* We observe the month and the year of the birth date.

The *level of education* is observed on ISCED levels, ISCED1-ISCED9. We categorised the variable as follows: elementary education: ISCED 1-2; secondary education: ISCED 3, 4, 5; and tertiary education:

ISCED 6-9.

Health status is measured as the first two principal components of five health indicators (all measured for the past 12 months): medical drug expenses, outpatient care expenses, inpatient care expenses, days spend in hospital, and number of visits at a General Practitioner. The first principal component captures some kind of overall health effect (as it is positively correlated with all five indicators) while the second captures the variation on the inpatient-outpatient scale (negatively correlated with inpatient care expenses and days spend in hospital and positively correlated with the others).

The *number of registry spells* shows the number of PES register spells in the labour market history of a person. A higher number indicates that the person entered the registry more frequently. We also include the time spent between the last registration with the public employment service and the start of the programme.

Labour market history

The administrative data from the social security administration allow us to construct variables that capture the employment history of a given person. Our employment history variables are expressed in the number of months spent in the given status after age of 16. Based on Lechner and Wunsch (2013), we add both long-term and short-term history variables as follows. We add the number of months spent in employment excluding in public works, in public works since age 16, and in the last two years. We also use a variable that captures the person's NEET history: the number of months spent in the NEET status, excluding any periods when the person was receiving child-related transfers after age 16 and in the last two years preceding participation in the programme. The broad concept of NEET does not distinguish parenthood from other inactive statuses outside of education. Our decision to exclude periods when the person was receiving child-related transfers was based on the assumption that time spent caring for a child at home differs from other inactive NEET periods in terms of both the causes and consequences, and thus needs to be treated separately.[40]

Child-related variables

To investigate the role of having children in the selection and the effects of the programmes, we include variables that capture the participants' parental status. The numbers and the ages of the participants' children are not directly observable in the database. However, data on parental benefits are available, which allows us to create parenthood variables. Moreover, as we can distinguish between different benefits related to the age of the child, we can estimate the birth date of the child.

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[40] However, the decision to have a child might itself be a consequence of bad labour market prospects.

We use the following parenthood-related variables: the number of months receiving any child-related transfers throughout the individual's life and the during the last two years. A parental status is also added as a dummy variable that equals one if the person received any child-related transfer in her life. The variable *child max three years old* equals one if the person received any child-related transfer given to mothers for less than three years in at least one of the two months preceding the programme.

Geographical variables

We also use regional dummy variables for the seven regions of the country in order to capture regional heterogeneity in the selection process and the labour market environment in the propensity score matching model. (As the region of the home address variable is missing in many cases, we use the region of the public employment office where the programme is administered.) In addition, we apply a variable that shows the development of the district of the public employment service. (A district is a smaller geographical unit – LAU1; there are 175 districts in Hungary). All Hungarian districts are officially divided into four groups according to the general level of development, starting from category 1 (most developed) to category 4 (needs complex development).

Differences in transportation costs and time constitute an important barrier to both build up a regular contact with the PES and take a subsidized job. Hence, we add two additional variables that capture these costs. The type of the settlement[41] based on the assumption that places differ strongly in terms of the proximity of available jobs. The other variable is the distance of the home settlement from the settlement of the PES to which the young persons is assigned (measured as the shortest legally possible travelling time by car). Being faced with costly and cumbersome transportation options might induce a young jobseeker to choose a labour market programme in her home village rather than spending a lot of time and money finding and securing a job with a wage subsidy in a distant town or even regularly visit the PES.

We also add the ratio of public works participants in the settlement to account for geographical differences in the incidence of public works programmes.

The last variable is an indicator that equals one if the public employment office is located in the main city of the county. There are two reasons why we added this variable. First, the public employment offices in the county seat are usually better equipped and have more labour market experts, which might have an effect on both the selection process and the success of the programme. The other reason is that county seats usually offer better labour market opportunities than smaller towns.

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[41] Capital, county seat, town, village above 10,000, between 5000 and 10,000, between 2000 and 5000, between 1000 and 2000, and below 1000 inhabitants.

Type of job

We use the type of the job – based on the single-digit ISCO code – the young person was working in at the time of the job trial or the public works programme, based on the assumption that public works programmes have higher proportions of elementary unskilled jobs than semi-skilled or skilled work, and that unskilled jobs do not develop human capital of the participant. We also include the types of jobs that the young person selected at the public employment office as relevant for her job search.