Iceland Liechtenstein Norway grants

Youth employment partnerSHIP

Can a short-term job trial programme kick-start young jobseekers' career?

Evaluation of the 90-day job trial in Hungary

JUDIT KREKÓ, MÁRTON CSILLAG, BALÁZS MUNKÁCSY, ÁGOTA SCHARLE

with the contribution of Tamás Molnár and Eszter Szedlacsek

BUDAPEST INSTITUTE FOR POLICY ANALYSIS, FEBRUARY 2021

Can a short-term job trial programme kick-start young jobseekers' careers?

ABSTRACT

In this paper, we perform a counterfactual evaluation of a publicly funded short-term hiring subsidy designed for young jobseekers in Hungary: namely, the 90-day job trial programme, which was introduced in 2015 a part of the Youth Guarantee scheme. The analysis is based on a linked administrative dataset of PES registers and social security records. We rely on propensity score matching for causal inference and compare job trial participants and participants in public works and classroom training programmes. Our estimates indicate that compared to participation in the public works programme, participation in the 90-day job trial improved employment prospects of young people: job trial participants spent 14-23 days more in employment within six months after the programme ended, the probability of being employed is higher by 4-7.7% six months after the programme on the whole sample. The difference is lower compared to the training participants: job trial participants spent 7.5-12 days more in employment status. The impact is weaker on the 12-month horizon compared to both control groups. It is the most employable young jobseekers who participate in the 90-day job trial subsidy: they have higher levels of education, live in more developed regions, spent less time in the NEET (not in employment, education, or training) status, and have more work experience than the pool of eligible jobseekers. These findings suggest that the programme did not reach disadvantaged and vulnerable groups, who needed help the most. We complement this analysis with a preliminary evaluation of the whole Youth Guarantee programme. We use an alternative identification strategy by exploiting the fact that in the Central Hungarian region, the programme started nine months later than in other parts of the country for administrative reasons. We apply a difference-in-differences framework to estimate the effect of the Youth Guarantee on eligible jobseekers' outcomes, and find a modest positive effect on employment in the 7-12 months after entering the register.

IMPLEMENTED BY:

The „Youth employment partnerSHIP" project is funded by Iceland, Liechtenstein and Norway through the EEA and Norway Grants Fund for Youth Employment.

Contents

1. Introduction[1]

Young people who are entering the labour market can be regarded as a vulnerable population: they have a higher risk of unemployment than older workers, and because of their low levels of labour market experience, their labour market position is more sensitive to demand-side fluctuations (e.g., Caliendo & Schmidl, 2016). There is extensive empirical evidence that young people who experience unemployment when starting their careers face long-lasting labour market consequences: even 10-15 years after entering the labour market, these individuals tend to have lower wages, fewer hours worked, lower quality jobs, a higher risk of unemployment, and a 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%. Since that time, the unemployment rate among young people in Europe has been consistently higher than that among older adults. Consequently, understanding the effectiveness of youth labour market policies is of utmost importance.

The aim of this report is to provide a counterfactual evaluation of the Hungarian Youth Guarantee (YG henceforth), primarily concentrating on one active labour market programme: namely, the 90-day job trial. The job trial is one of the most popular programme elements (in terms of participants) within the YG in Hungary, and although similar programmes also existed prior to the introduction of the YG, the number of

1 We are grateful for Asdrid Kunze, Jan Gromadzki and Ágnes Szabó-Morvai for their highly valuable detailed comments, and to the members of the Youth Employment Partnership for their continuous feedback and discussions throughout the research. We are also highly indebted to the owner of our administrative database, the Databank of the Centre for Economic and Regional Studies, for providing access to and help with the dataset. The would like to thank István Boza for his help in preparing and cleaning the dataset. The settlement level variables used in the analysis are derived from the settlement level unemployment databased, build up in the project "Mobility Research Centre"(KEP-4/2019) under the "Support for excellence grant programmes" of the Hungarian Academy of Science. We are grateful to György Molnár for providing access to these variables. We are grateful for József Tajti for his help with the data. We are thankful to Ildikó Tamási and Norbert Putz for their insights into the Youth Guarantee programmes. All remaining errors are of our own.
2 For example, Nielsen and Reiso(2011), Kahn(2010) Gregg P.–Tominey E. (2005). For Hungary, Csillag(2020) 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) and Tóth(2020).

participants in similar programmes was not as large.[3] The job trial is a short-term programme with a maximum subsidy of 100% of the wage costs, and does not include a commitment for further employment. Despite its short duration, participating in the job trial might be ideal for young jobseekers for several reasons. First, it can function as a "stepping stone": it is useful in mitigating informational asymmetries between the young person and the employer, which can be a significant barrier to finding an appropriate job for an unexperienced jobseeker without a solid work history. The short duration and the generous subsidy mean that the risk of participating in the programme is relatively low for both the employer and the participant. In addition, short-term employment can provide a test of whether the work matches the young person's skills and interests, increase the value of the young person's CV, and alleviate negative stereotypes on both sides. Another potential advantage of the job having a short duration is that it does not give the young person an incentive to leave school before graduating, which is a possible perverse effect of longer-term youth wage subsidy programmes (Oskamp and Snower, 2006, O'Leary et al., 2011).

Although there have been many evaluations of longer-term wage subsidy programmes, there is much less research on short-term programmes. In Hungary, two previous papers have evaluated the Youth Guarantee in Hungary (Czombos et al, 2018, Szabó-Morvai et al., 2015). Moreover, to the best of our knowledge, our paper is the first attempt to analyse the impact of the 90-day job trial. The aim of this study is to estimate the causal effects of participation in the 90-day job trial programme on the labour market outcomes of young jobseekers: i.e., the probability of the participants being employed, and their cumulative earnings six months after they completed the programme, for the 2015-2017 period. The causal inference relies on propensity score matching, in which job trial participants and participants in public works and classroom training programmes are compared.[4]

Our propensity score matching estimations indicate that compared participation in public works programmes, participation in the 90-day job trial improved labour market outcomes on the six-month horizon: the probability of employment six months after completing the programme is higher by 4-7.7% point on the whole sample. Job trial participants spent 14-22 days more in an employment status within six months after completing the programme than public works participants with similar characteristics.

[3] In the previous two decades, longer-lasting (9-12 month) wage subsidies covering 70-100 percent of wage costs were used. The obligation no to fire the person for a period equivalent to 1,5 times the subsidy period was tied to these hiring subsidies.

[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.

Compared to public work participants, the cumulative labour income of participants in the job trial, excluding public works wages, were higher by 0.67 times the monthly minimum wage. However, the higher probability of employment six months after completing the programme was not reflected in the participants' total labour income, including wages from public works programmes, as public work participants spent more days in public work also after the first public works programme that compensated for the lower public works wages. This is a clear indication of the locking-in effect of public work programmes.

Better employment outcomes compared to public work programme participants possibly capture the combined effects of the impact of the public works programme and the job trial. As both international and Hungarian evidence points to the negative employment effect of public work programmes, the causal impact of job trial participation compared to non-participation might be lower.

We found only moderate positive employment effect of participation in the job trial compared to participation in classroom training. In contrast with case when we compared the job trial participants with the public works participants, the average treatment effect on the treated on the cumulated earnings was found to be significantly positive only when including public works wages, indicating that job trial participants end up in public works programme more often than training participants after the programme.

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 the six-month horizons, indicating that the impact weakens over time.

There are marked differences in the observable characteristics of the treatment group and the public work participants. There is evidence that compared to their non-participating peers, those who were selected into the programme were in a better labour market position, had a higher level of education, were living in a more developed region, had more work experience, and spent less time in the NEET (not in employment, education, or training) status. Therefore, it appears that the most employable unemployed young people were selected into the programme. This finding suggests that the programme was not reaching vulnerable and disadvantaged groups, who needed help the most.

Although better educated job trial participants have better labour market prospects after the programme than less educated participants, the treatment effect, compared to public works participation is comparable for the two groups. Giving young unemployed people with less education a higher priority in the job trial would not decrease the average treatment effect, while reducing the deadweight loss of the programme.

The results show that mothers were less likely to participate in job trial than in public works (and training programmes) even after controlling for differences in other characteristics, which implies that being a mother

decreased the probability of participating in the YG programmes, and particularly in the job trials. In addition, 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. We found that job trial participation has a weaker impact on women compared to participation in public works, but women profit more from job trial than from classroom trainings, which probably can be explained by the better education background and labour market prospects of female participants.

We found that firms, which hired young persons with YG wage subsidy increased their size more than similar firms without YG subsidized hires. However, the increase in the workforce due to the subsidized young employee lags behind the number of subsidized hires at the firm, suggesting the presence of deadweight losses.

We also provide a preliminary assessment of the early phase of the Youth Guarantee scheme as a whole to complement the analysis above, as such an assessment can provide us with a better understanding of the policy context. For this analysis, we exploit the fact that in the Central Hungarian region, the programme started nine months later, and apply a difference-in-differences framework to estimate the effect of the Youth Guarantee on the outcomes of eligible jobseekers. We find small positive, albeit non-significant effects for young men, and no effects for young women. Thus, we conclude that participation in the Youth Guarantee scheme only affected whether the jobseekers had access to decent work, and then only for a very limited period after they registered as unemployed.

The remainder of the paper is structured as follows. Section 2 describes the situation of the NEETs in Hungary, the Youth Guarantee initiative, and the institutional background of the programme. Section 3 summarises the related literature. Section 4 describes the administrative datasets used in the evaluation. Section 5 explains the empirical strategies and the methodologies we used for the analysis of the job trial, and presents our results. Section 7 presents the analysis of the whole Youth Guarantee scheme. Section 8 concludes.

2. NEETs in Hungary and the Youth Guarantee

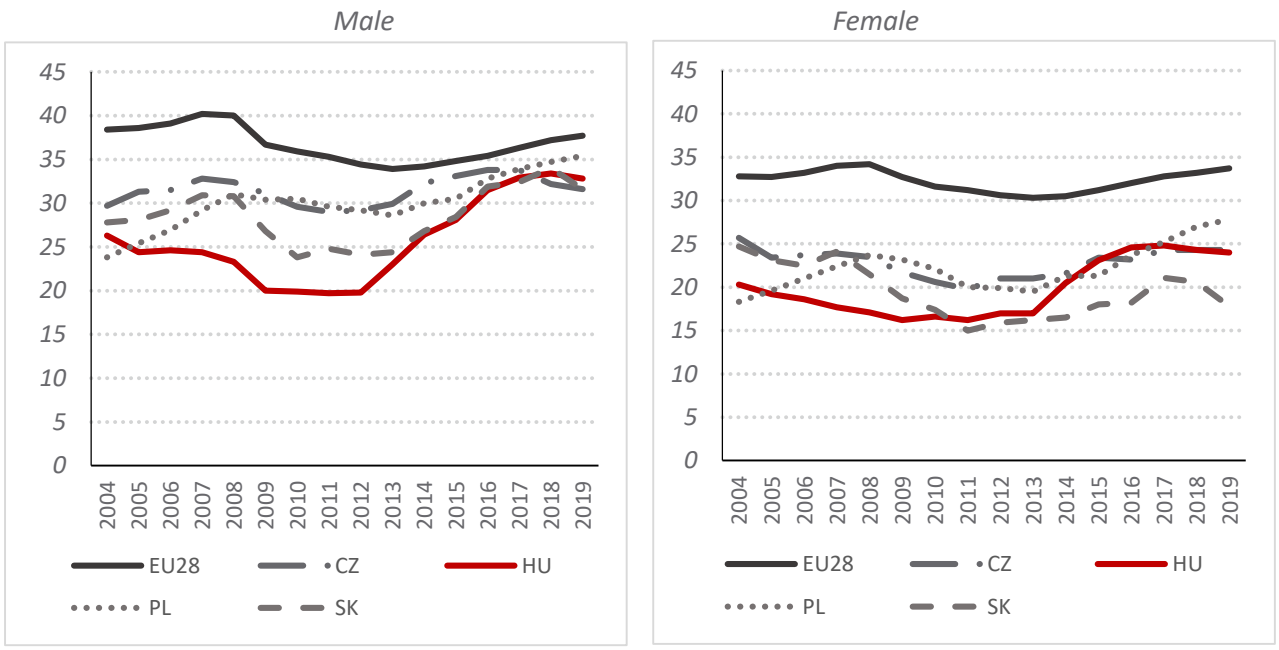
2.1. The situation of NEETs in Hungary

A young person is considered have NEET status if she is not employed, not enrolled in any educational

institution, and is not participating in any active labour market programme. NEET is a concept that embraces a broad array of vulnerabilities among young people, including the risk of unemployment, discouragement, and leaving school early. As the main reason for inactivity among young people is participation in education, the concept of NEET highlights that the aim of labour market policies is to reduce forms of young unemployment and inactivity that do not contribute to the human capital of the young person.

While the financial crisis and economic downturn of 2008 affected all labour market groups, it hit the younger generation particularly hard. In Hungary, the employment rate of 15-25-year-olds fell significantly from a level that was already lower than the EU average (see *Figure 2-1*). Until recent years, the employment rate of young people was lower in Hungary than in the other countries of the region, mostly because of a high inactivity rate. However, the employment of young people in Hungary started to increase sharply after 2013, and was close the EU average for males by 2018.

Figure 2-1 Youth employment rate 2004-2019, 15-24 age group

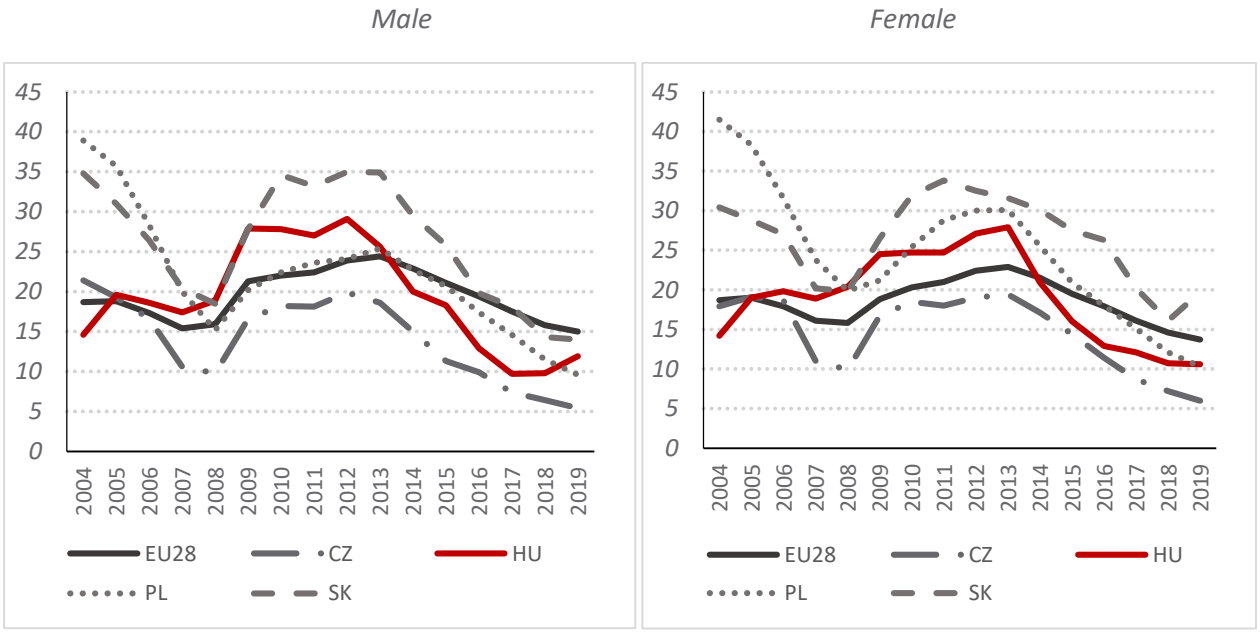


Source: Eurostat

After increasing during the financial crisis, the unemployment rate in Hungary fell at a rapid pace in the following years, and is currently below 10% for both males and females. In 2015, when the Hungarian YG was actually launched, the unemployment rate of young people aged 15-24 was already below the EU average (see *Figure 2-2*).

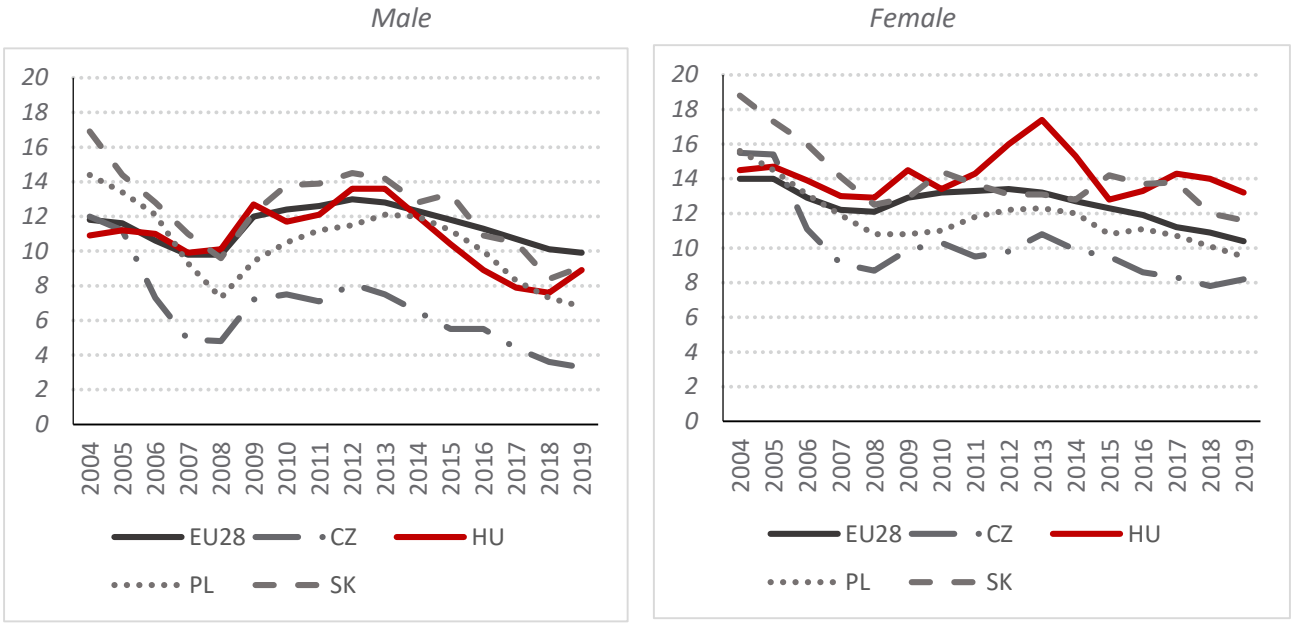
Over the same period, due to adverse reforms in the educational system, the dropout rate from formal education was rising (12.4% in 2015 compared to an average of 11% in the previous years). Accordingly, the proportion of young NEETs (11.68%) in Hungary is close to the EU average (12%), based on 2015 Eurostat data. Compared to the EU average, there are large gender-based differences in NEET rates in Hungary: while the NEET rate of men lags behind the EU average, the NEET rate of women is much higher (*Figure 2-3*) due to a high inactivity rate.

Figure 2-2 Unemployment rate 2004-2019, 15-24 age group



Source: Eurostat

Figure 2-3 NEET rate 2004-2019, 15-24 age group



Source: Eurostat

2.2. The Youth Guarantee and the 90-day job trial

THE YOUTH GUARANTEE IN HUNGARY

The Youth Guarantee programme in Hungary, as in other states of the European Union, aims to ensure that all young people under the age of 25 who are not in employment, education, or training (NEETs) receive support to find a job or training, or to return to school within a short time after they register with the public employment services. This support is typically provided by individual action plans, individual mentors, and youth advisors. The Hungarian government made a commitment to implement measures as part of the YG programme in 2013, and the programme was actually implemented in Hungary starting in 2015. The programme was financed mainly by Youth Employment Initiative funds in the eligible convergence regions, and by matched European Social Fund sources in the remaining regions. The Youth Guarantee programme is implemented as part of the Youth Guarantee system, but this system also includes programme elements that are not implemented by the PES, such as the apprenticeship programme. An outline of the YG system in

Hungary is provided in the country's Youth Guarantee Implementation Plan[5], while a more detailed description of the programme elements is available in the documents related to the relevant Operative Programmes[6].

Hungary implemented the YG gradually. Starting in January 2015, the programme guaranteed that jobseekers would be given an offer within six months of registering with the public employment service[7]. From 30 June 2016 onwards, the programme had to provide help within four months for those who had been registered with the PES for at least four months. Finally, from January 2018 onwards, the programme guaranteed that all NEETs would be given an offer within four months of registering with the PES.

The YG in Hungary was originally intended to reach and engage about 170,000 NEETs during the project period (Hungary's National YG Implementation Plan, 2014). All NEETs between the ages of 15 and 24 who are officially registered as jobseekers are eligible to participate in these measures. Until 2018, long-term unemployed young people were the key target group of the measures. Since then, however, this priority no longer applies. Initially, the total budget for actions under the YG was HUF 200 billion (about 625 million euros) for the 2015-2020 period. The budget was co-financed by the Youth Employment Initiative and the European

Structural and Investment Funds. According to the official online monitoring database established by the government, this budget was increased by 25% in 2016.

One of the main innovations in the design of the Hungarian YG initiative is that it recommends regular and close cooperation between the PES and the educational and (vocational) training institutes, the municipalities, and the representatives of local and national youth organisations familiar with the target group. The coordination of stakeholders at the local level can make the programme more effective. However, in practice, this close cooperation is often implemented rather weakly.

5 URL: https://ngmszakmaiteruletek.kormany.hu/download/9/4c/c0000/Youth%20Guarantee%20Implementation%20Plan.pdf Last accessed on 30 January 2020

6 For convergence regions, see EDIOP 5.2.1. (wage subsidy and training, core elements of YG, URL: https://nfsz.munka.hu/Lapok/programok/ginop/ginop_g521.aspx), EDIOP 5.2.2. (training part of the entrepreneurship programme, URL: https://www.palyazat.gov.hu/doc/4386), EDIOP 5.2.3. (subsidy part of the entrepreneurship programme, URL: https://www.palyazat.gov.hu/ginop-523-16-fiatalok-vllalkozv-vlsa-vllalkozs-indtsi-kltsgeinek-tmogatsa-1), EDIOP 5.2.4. (traineeship programme, URL: https://www.palyazat.gov.hu/ginop-524-16-gyakornoki-program-plyakezdk-tmogatsra), for Central Hungary see CCHOP 8.2.1 (wage subsidy and training, core elements of YG, URL: https://nfsz.munka.hu/Lapok/programok/vekop/vekop_v821.aspx)

7 In the initial implementation phase, the objective was to provide measures for those who had already been registered as a jobseeker for six months.

Within the framework of the Hungarian Youth Guarantee plan, individual jobseekers receive individualised support from the labour departments of the government offices (i.e., the local offices of the public employment services, PES) after they have registered to participate in the YG. The Hungarian YG assigns interested young people to two main and one minor programme based on individual consultations and individual action plans. The two main types of measures are vocational training and wage subsidies (including job trials). Entrepreneurship programme is a small-scale measure. The numbers of participants by year are summarised in *Table 2-1*. [8]

It is important to note that similar wage subsidies and training programmes existed in Hungary before the introduction of the YG. Thus, while its measures are not novel, the Youth Guarantee programme has several new features compared to previous programmes, including 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 a coordinated communication.

Table 2-1 Number of participants in YG measures by year

	2015		2016		2017		2018		2019 (Aug)		Total
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	
Wage cost subsidy	4712	4686	5187	4901	7759	7100	8685	7859	4726	4391	60006
Wage subsidy	351	180	576	378	762	455	716	300	431	280	4429
Other (combination)	2214	2223	1933	2149	2592	2861	1647	1960	249	232	18060
Training	4577	3514	3215	2928	4814	5072	3912	4296	432	540	33300
Rent subsidy	117	161	165	244	197	278	224	334	92	123	1935
Entrepreneurship subsidy	157	249	215	326	330	461	528	632	431	472	3801

Source: YG monitoring database

All of the programmes of the YG programme became available from January 2015 onwards in the convergence regions, where they were financed mainly from YEI funds as part of the Economic Development and Innovation Operative Programme (EDIOP); and from October 2015 onwards in Central Hungary, which is not a convergence region, where they were financed mainly from ESF funds as part of the Competitive Central Hungary Operative Programme (CCHOP).

[8] A rent subsidy could accompany other programmes, but was offered in only a few cases.

WAGE SUBSIDIES AND THE 90-DAY JOB TRIAL IN HUGARY

Wage subsidies come in many forms in Hungary. First, the Job Protection Plan (introduced in 2013) gives a substantial wage subsidy (in the form of payroll tax reductions) for employers after all employed young persons (under age 25). The payroll tax reductions apply for two years, and are more generous for employing a labour market entrant (or a person with little prior work experience). This programme is relatively popular, as there are no conditions attached, and the administrative costs are low.

Second, there are a number of hiring subsidies administered by the PES. Here, we will only describe those within the YG (see *Table 2-2*). The hiring subsidies offered within the YG can be of different durations (three, six, eight, or 10 months), depending on the qualifications and needs of the young person. There is an important condition attached: the use of hiring subsidies should lead to an increase in employment at the firm. If an employer takes advantage of the six-, eight-, or 10-month subsidy programmes, a period of unsubsidised employment must follow the period of subsidised employment, and the employer cannot move to end the employment relationship during a period that is 1.5 times the length of the duration of the subsidy.[9] Two of the programmes that are specifically within the YG (the six- and the 10-month programmes, which are referred to as "wage cost subsidy" programmes) cover 100% of the wage costs. The third programme (the eight-month subsidy programme, which is referred to as a "wage subsidy" programme) covers 70% of the wage costs. Finally, there is a three-month wage cost subsidy programme, which covers 100% of the employer's wage costs: the 90-day job trial. In contrast to the hiring subsidies described above, this programme does not require the employer to continue to employ the young person after the end of the subsidy period.

A specific feature of the job trial programme is that a young person may also participate in the "wage subsidy" programme (the 8+4-month programme) directly after participating in the job trial. All jobseekers below 25 years, and those who pass 25 years but are categorized as disadvantaged (for example, have maximum elementary education, are unemployed for more than sixth months, received child transfer or were imprisoned in the last 12 months) are eligible for the subsequent wage subsidy, but the employment services might decide in their own competence whether they allow participation is subsequent wage subsidy. The minimum work schedule of the subsidised employee during the programme is four hours per day. The 90-day job trial accounts for about 40% of all YG wage subsidy programmes and 25% of all YG programmes.

[9] Effectively, the employment relationship is to last for 6+3; 8+4; 10+5 months (subsidisation period + additional employment period).

Both eligible jobseekers and firms might initiate a job trial programmes, and employment services' responsibility to place the young jobseeker to a suitable firm, to a job that fit's the young jobseeker's skills and education. There are cases when the employer and the applicant meet outside the employment service and the young person registers to be eligible to the subsidy.

Table 2-2: Wage and wage subsidy programmes in the Youth Guarantee programme

Programme type	Programme name	Length of subsidised empl. (months)	Length of required non-subsidised empl. (months)	% of wage cost	Comment
Wage cost subsidy	90-day job trial	3		100	Can be combined with 8+4, but not with 10+5
Wage cost subsidy	10+5	10	5	100, maximum of double the min. wage	Low educational level (ISCED 1-2)
Wage cost subsidy	6+3	6	3	100, maximum of 1.5 the min. wage or guaranteed wage minimum	Medium educational level (ISCED 3-8)
Wage subsidy	8+4	8	4		Can be combined with other tax and contr. subsidies, up to 100% of the wage costs Maximum elementary education

In this evaluation, we focus on one of the YG programmes: namely, the 90-day job trial. Among the specific aims of the 90-day job trial are to encourage employers to overcome their negative stereotypes, and to allow young people to get a feel for whether the given line of work interests them. The short duration of the job and the high wage subsidy mean that the risk of participating in the programme is relatively low for both the employer and the young participant, as there is no need for a long-term commitment on either side. The short duration of the subsidy also implies that the cost of the programme is relatively low.

A job trial programme might be an appropriate tool for alleviating an employer's uncertainties about employing a young person arising from a lack of information about the applicant, or even from prejudices. This latter point is particularly important in the context of Hungary, as widespread negative attitudes towards Roma people can be a serious barrier to increasing the employment rate of Roma youth. Having even brief employment experience can enhance a young jobseeker's CV, and increase her bargaining power on the job market. If both the employer and the employee are satisfied with the experience, the participant might stay at the firm after the three-month period. Another characteristic of the job trial programme is that it is

relatively short. Thus, unlike wage subsidy programmes with longer durations, participating in the job trial is less likely to lead young people to drop out of school.

Moreover, there is anecdotal information suggesting that the job trial is also beneficial because it can encourage employers to turn an unreported, grey-market seasonal job into a legal employment arrangement. In some industries, such as tourism, unreported short-term employment spells are relatively common. However, the short duration of the job and the lack of a required period of unsubsidised employment can also be disadvantages, as the employer may not be interested in facilitating the long-term integration and education of the young person, and may consider hiring a young person through the job trial as a way to get a short-term job done at no cost to the firm.

Another concern is that during economic booms, the job trial – and wage subsidies in general – might have large deadweight losses, as the risk is high that those who participate in the programme would have been able to find a job even without the subsidy.

3. Related literature

There is abundant literature on the performance of ALMPs. Meta-analyses that have compared the effectiveness of active labour market policies have generally found that young people benefit less from ALMPs than adults (Kluve (2010), Kluve et al. (2014), and Card et al. (2010)). However, there is an emerging consensus that the effects of such policies depend on the economic environment, and on the type and the design of the programme. In a comprehensive survey of youth employment policies, Caliendo & Schmidl (2016) found that job search assistance (with and without monitoring) resulted in mostly positive effects, the effects on training and wage subsidies varied between positive and zero, while the effects for public works programmes were mainly negative.

Although "work experience" programmes have been implemented in many EU countries[10], there are, to the best of our knowledge, relatively few quantitative evaluations that have focused specifically on this type of measure.[11] The common characteristics of these programmes are that they (i) are of short duration, usually

[10] Liliana Luminita (2017)
[11] Please note that welfare-to-work programmes for the long-term unemployed are more common, but we will not discuss them here.

lasting 3-6 months; (ii) are relatively broadly targeted; (iii) cover a high portion (50% or more) of employers' wage costs; and (iv) have only very light requirements related to employment growth or worker retention. However, there is some variation in the nature of the jobs the employers are expected to offer, and in the wages they are expected to pay. One type of programme (including the 90-day job trial in Hungary) explicitly aims to place young people in "real" jobs at private firms, where they are paid market (minimum) wages. The second type of programme aims to give young people experience in "supplementary" jobs, where they are paid a (flat-rate) below-market wage, often in the form of an allowance paid directly to the young person.[12]

Broadly speaking, the results of the evaluations of these work experience programmes point towards two general conclusions: first, that placing a young person in a real job at a private firm is likely to be beneficial for her employment, including in the medium term; and, second, that the effect of placing a young person in an "additional" job is zero at best, and may even be negative for her employment prospects. An example of the first case is from Australia's Special Youth Employment Training Programme, which operated until 1985. The programme offered a flat-rate subsidy for youths (aged 16 to 24) who had been unemployed for at least four months in the previous year. The subsidy lasted for only 14 weeks, and covered about 50% of typical youth wages. Richardson (1998) and Knight (2002) examined the impact of the programme roughly one year after participation, and found a small positive effect of around 10% on the participants' employment probability. An example of the second type of programme is a Swedish programme that operated between 1992 and 1995. The programme provided subsidised work experience for youth (aged 18 to 24) with a high school education who had been unemployed for four months. The placements, which lasted six months, were heavily subsidised, paid below-market wages, and were meant to be supplementary in nature (i.e., not displacing existing jobs). Evaluations of the short-term (Larsson, 2003) and the medium-term (Costa Dias et al., 2013) impacts of the programme on the participants' employment probability showed small *negative* results.

In overviews of the literature on hiring and wage subsidies, Almeida et al. (2014), Bördős et al. (2016) raise attention that in addition to poor design, implementation issues can decrease the effectiveness of these programmes. They find that more vulnerable jobseekers might need longer-term employment conditions. Furthermore, they show that the take-up rate of subsidies depends on the ease of administration and the way in which the subsidy is paid to the beneficiaries (aka. the 'payment vehicle'). In a more recent paper, Neumark & Grijalva (2017) show that in the US, hiring subsidies were more effective at boosting employment

[12] In some cases, the young person is also expected to participate in job counselling and training sessions, and is still considered a registered jobseeker.

growth if the credits could be recaptured when job creation goals were not met. Cahuc et al. (2019) point out that hiring subsidies are particularly effective if they are not anticipated, are temporary, target a relatively small subset of firms (workers), and are implemented in an environment of rigid wages.

Our work is also related to the debate on the stepping-stone effect of short-term jobs for young people. This literature covers many different forms of 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 market (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 limited to one paper, given that prior to the Great Recession, there were few ALMPs specifically targeting young people. Svraka (2018) evaluates the wage subsidies provided in the Job Protect Act in 2013. These subsidies were in the form of a reduction in social security contributions for employers who hired people under age 25 (and a host of other groups).[13] Using a difference-in-differences methodology to compare the changes in the outcomes of those just below the eligibility cut-off (aged 22-24) and those just above the cut-off (age 25-27), the author showed that the subsidy raised the probability of employment by about two percentage points. He also provided some circumstantial evidence that the subsidy did not lead to the substitution of workers who were slightly older, and hence ineligible, with eligible younger workers.

Prior to the introduction of the Youth Guarantee, there was a work experience programme for career entrants in Hungary that lasted until 2006. However, there was no evaluation of this measure. All we know about this programme is that the (raw) employment rates were relatively high. Evaluations (based on regression adjustments) of longer-term hiring subsidy programmes for adults in Hungary were carried out for the late 1990s (O'Leary, 1998); the early 2000s (Galasi et al., 2007); and most recently, for those entering programmes in 2009-2010 (Csorba & Nagy, 2012). The latter two studies found significant positive effects on employment outcomes (six months after the end of the programme), particularly for programmes that lasted 9-12 months. Survey evidence (self-reports by unemployed individuals) suggests that 20-25% of the jobs subsidised by these programmes would likely have been created even in the absence of the subsidy.

[13] The value of the subsidy was around 14% of the total wage costs for a minimum wage worker. Due to (i) widespread promotion and (ii) easy processing, the take-up was in excess of 90% for young workers.

The Youth Guarantee Programme was evaluated at a very early stage in a study by (Szabó-Morvai et al, 2015). More broadly, Czombos et al. (2018) provided an ex-post evaluation of the early phase of the Youth Guarantee in Hungary that relied on PES register and programme data. They used matching methods to measure the effect of YG participation relative to participation in a programme that offered training to young people in low-skilled and public works employment. They found that participation in the YG was highly beneficial, as the participants were 31.5% more likely than the control group to be working in the primary labour market six months after their participation in the programme ended.[14] The authors also evaluated the YG programme using a regression discontinuity design, in which they compared 24-25-year-old YG participants with 25-26-year-old participants in a programme that was roughly similar. They found a small, significant 2% effect of participation in the YG on the employment rate of the young people six months after their participation in the programme ended.[15]

4. Data

The analysis is based on an individual-level administrative panel database from Hungary. The owner of the database is the Databank of the Centre for Economic and Regional Studies in Hungary.[16] The data cover half of the population aged 5-74 in 2003, who were randomly selected and followed-up until 2017. 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.[17] Linking the PES database to the databases of the pension and health care authorities enables us to observe the employment histories of jobseekers, including their employment and wage outcomes after an arbitrary time span following completion of the programme and their background characteristics.

[14] There are two important caveats. First, given that the authors have access to only a limited set of control variables (for instance, they cannot take into account prior work history), this estimate is likely upward biased. Second, given the composition of the control group, it is likely that this estimate is valid only for the group of youth with low education.
[15] Please note that this estimate excludes the Central Hungary (the most developed) region.
[16] The raw administrative data were cleaned and processed by the Databank.
[17] Furthermore, the YG preparatory phase tends to be very short (median: four days), since counsellors register jobseekers as entering the YG only when they are aware of an offer.

The PES database enables us to observe the type of active labour market programme (wage cost subsidy, wage subsidy, training), and the name and the length of the programme. However, the different YG wage subsidy variants are not distinguished in the database. Thus, were are not able to observe the 90-day trial participants directly. Still, we have a good approximation, as we can detect the actual duration of the wage cost subsidy programme, and the only three-month YG wage cost subsidy is the job trial. The problem with this approximation is that participants in longer-term wage subsidy and wage cost subsidy programmes who left the programme earlier than planned for any reason might be falsely identified as job trial participants. To reduce the risk of including dropouts from longer programmes, we consider only those individuals who participated in a programme that was exactly 90 days long, as the probability of dropping out on exactly the 90th day of another wage cost subsidy programmes is low.[18] *Figure A1* shows an outstanding peak 90 days, indicating that with fixing the programme length at 90 days, we capture vast majority of the job trial participants. The caveat of this approach is that we measure the effect of a completed programme, and loose participants who terminate the program earlier. According to an interview from the PES, drop-out rate from the programme is low, around 5% in case of the 90-day job trial programme.

5. Conceptual framework and empirical strategy

The 90-day job trial belongs to the broader category of wage subsidies in the spectrum of active labour market policies. The economic rationale behind these programmes is to provide a financial incentive for employers to hire young people, as it is assumed that employers may worry that younger workers are less productive, lack work experience, or have low skill levels. In addition, employers might face higher training costs when employing young people than when hiring more experienced employees. The wage subsidy compensates for these factors and increases the wage level the employers can pay, thus giving young people an incentive to take the job. A key assumption about the mechanism of the wage subsidy programmes 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

[18] Based on PES interviews, it is usually below 6%.

another firm. Consequently, we expect an effective programme to improve the future employment prospects and wages of the participants.

Against this background, our research questions are as follows. First, we analyse how the programme participants are selected from the pool of eligible young jobseekers. Second, we examine the effect of participation in the programme on different employment and wage outcomes.

The aim of the empirical strategy is to give a causal estimate of the effect of the policy by applying a quasi-experimental framework. Identifying the causal effect of a labour market programme from observational data, in which the selection into the labour market programme is not random, is always challenging. This is because programme participants might differ in their observable and unobservable characteristics from those who are not participating in the programme. If these characteristics correlate with the outcomes, a simple comparison of the outcomes of participants and non-participants might result in a biased estimation. Finding a valid control group is especially difficult in the case of YG due to its framework. The basic idea of the YG is that all NEETs (who register as jobseekers) should be provided with a good quality offer within a short period of time. To meet the policy goal of reaching every individual in the NEET group, the YG programme has relatively abundant financial resources and high target enrolment numbers. Thus, since the introduction of the YG, the PES has not faced large financial and quantity constraints, and there has been no need for the rationing of applicants.

Another 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, and is based on the specific needs and qualifications of the young person. However, there are no prescribed rules for how such choices are made, and the programmes cannot be differentiated by eligibility. Moreover, some programmes can be combined sequentially. For example, disadvantaged 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. The coexistence and potential combination of the different active labour market programmes of the YG has two important implications. First, programme combinations must be fully taken into account when defining the treatment and the control groups. Second, 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 follow two different identification strategies. In the first strategy, we compare the outcomes of the participants in the 90-day job trial programme with outcomes of participants in public works and training programmes, applying propensity score matching method to address the selection bias.

The second strategy is to implement an intention-to-treat (ITT) identification, which exploits the fact that the Youth Guarantee started nine months later in the Central Hungarian region for administrative reasons, by applying a difference-in-differences framework. As this identification strategy does not allow us to distinguish between different YG programmes, the aim of the estimation is to assess the effect of participation in the YG scheme as a whole by answering the question of whether registered jobseekers had better labour market outcomes after the introduction of the YG.

5.1. Propensity score matching framework

In this section, we discuss how we compare the outcomes of participants in the 90-day job trial programme with the outcomes of two control groups: participants in public works programmes and participants in training programmes with propensity score matching.

First, we look at the treatment effect of the 90-day job trial on the treated (ATT), which is defined as follows.

$$\tau_{ATT} = E(\tau|D_i == 1) = E(Y_i(1)|D_i == 1) - E(Y_i(0)|D_i == 1)]$$

D_i is the treatment indicator, $Y_i(1)$ is the outcome of population observation i when receiving the treatment, and $Y_i(0)$ is the counterfactual for those being treated.

The propensity score matching relies on the conditional independence assumption; that is, given a set of observable covariates X that are not affected by the treatment, the potential outcomes are independent of the treatment assignment. This assumption is strong, and assumes that conditional on the observable characteristics, there is no selection on unobservable variables that are unrelated to the observables, and could hence threaten the identification (e.g., Imbens (2004)). We assume 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. Given the high dimensionality of our vector of observable variables X, we use the propensity score as a balancing score, which is the probability of being included in the treatment group, conditional on a set of observable characteristics (Rosenbaum and Rubin, 1983).

$$\mathrm{PS} = \hat{P}[D_i == 1|X_i]$$

and the propensity score estimator of the ATT can be written as:

$$\tau_{ATT}^{PSM} = EP_{P(X_i)|D_i}\{E(Y_i(1)|D_i == 1, P(X_i)) - E(Y_i(0)|D_i == 0, P(X_i))$$

Our propensity score estimation is based on a logit model, conditional on a large set of explanatory variables described below. We apply kernel matching and use weighted averages of the non-treated to form a counterfactual control group. In other words, we match each observation in the treated group to multiple observations in the control group that are within a certain radius (bandwidth) of the propensity score. The weights used for calculating the weighted mean decrease as the difference in the propensity score increases. The matching is exact on the individuals' gender and level of education. Our chosen bandwidth is 1.5 times the 90% quartile of the distribution of (non-zero) distances between observations in one-to-one matching (one-nearest-neighbour matching with replacement), based on the method proposed by Huber et al. (2015).

We prefer kernel matching over the commonly used one-to-one or nth neighbour matching for two main reasons. First, a kernel is simply more efficient, as it allows us to exploit more variation from the control sample. Second, kernel matching is likely to give us a smaller difference between the estimated and the underlying distributions' variation, lowering the error in the estimated treatment effects. (Caliendo & Kopeinig (2008), Blundell & Costa-Diaz (2009). Note that using a kernel gives us smoother estimates, and it could smooth away some of the less crucial features of the distribution, introducing some bias. The former effect can be expected to be more substantial than the latter, meaning that using a kernel should reduce the mean squared error.

As the shape of the weight function is relatively unimportant (see Caliendo & Kopeinig (2008)), we simply opt for the widely used parabolic (aka. Epanechnikov) kernel. We impose a common support restriction by dropping treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls.
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, and may cause bias when using a kernel. Monte Carlo simulations suggest that this bias usually leads to estimates with a relatively small bias (Jann (2020) which could be conservative (i.e., it can lead to standard errors that are too large).

5.2. Treatment and control groups

TREATMENT GROUP: 90-DAY JOB TRIAL PARTICIPANTS

The treatment group consists of participants in the 90-day job trial. Participation in the job trial is approximated by participation in a YG wage cost subsidy programme with exactly 90 days, as described in Section 3. A major difficulty arises from the fact that some of the job trial participants were allowed to start an 8+4-month-long wage subsidy programme immediately after finishing the 90-day 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 programme, and all of the job trial participants were eligible to participate in such a programme.

We ignore this selection problem in our baseline estimations, but address it by exploiting county-level "house rules" in enrolment into YG programs in Section 6.4.

About 30% of the 90-day job trial participants also took part in the wage subsidy programme. Between 2015 and 2017, there were 6556 YG wage cost subsidy participants with 90 days of participation, without subsequent wage subsidy. As the Admin3 database includes half of the population, this is about half of the total number of Hungarian job trial participants in this period. However, as our outcome variables are available until the end of 2017, and we are measuring the outcomes six months after completion of the programme, we have chosen to restrict the matching analysis to participants who entered between January 2015 and April 2017. After excluding nine cases with serious data errors[19], our treated group consists of 3760 individuals.

We argue that registered jobseekers who have not participated in any programme or public works scheme do not constitute a valid control group. In the period of our analysis, the majority of the registered jobseekers were enrolled in an active programme or a public works scheme within half a year from becoming registered

[19]Crucial variables are missing (e.g. age , gender or education) or inconssistent. These rare errors are random.

jobseeker, or they found a job by themselves and exited the register. The extensive public works scheme was available in all regions, and the macro-economic boom starting in 2014 resulted in increasing labour market demand. For the above reasons, we assume that the unobserved characteristics of the young registered jobseekers who had not been enrolled in either an ALMP or a public works scheme within half a year of registration and also could not find a work by themselves might be so different from those of the treatment group that it would threaten the analysis. For example, they may more likely participate in illegal work, have a disability, or have other family care obligations. Therefore, we apply two other control groups in our analysis: participants in public works and training programmes.

CONTROL GROUP 1: PARTICIPANTS IN PUBLIC WORKS SCHEMES

Participants in public works programmes constitute a natural control group. The public works scheme was launched in 2011 in Hungary as an answer to the high unemployment rate following the financial crisis. The public works programmes offer unskilled jobs, and pay a salary equal to about 75% of the minimum wage, which was about 170 euros for a low-skilled worker in 2015. The scheme has been widely criticised for its lock-in effect. Specifically, it has been argued that participants might get stuck in the programme, as it does not develop their human capital and future labour market prospects. From 2017 onwards, young people under age 25 could be enrolled in the public works scheme only if the Youth Guarantee had failed to help them. However, until 2018, participation in the public works scheme was relatively widespread, even among young people who could be eligible for the programmes of the YG. The majority of the public works programme participants work in unskilled physical jobs.

Table 5-1 Number of young people under age 25 enrolled into programs of YG and public works

	2015	**2016**	**2017**
Youth Guarantee Programme	23141	22217	32681
Public works	43390	40046	24444

Source: Admin3 database and YG monitoring database.

In addition, even more young people were enrolled in the public works scheme than in the programmes of the YG in our study period (see Table 5-1) Roughly 1.8% of women and 3.2% of men aged 20-24 were employed in a PW programme in 2017.[20] Although public works programmes are aimed to function as a "last chance", many young people, who could have a chance to find a better option ended up in public works programme with relatively short time after registration. For example, nearly 20 percent of newly registered 16-19-year-olds became a public employee within 90 days (Molnár,2019). Consequently, the participants in public works programmes may qualify as a control group. We considered participants in the public works scheme who were under age 25 when they started the programme, and who were enrolled between 1 January 2015 and April 2017. We excluded those individuals who had also participated in any of the YG programmes. The average length of the public works programmes the young people in the control group participated in was somewhat longer than that of the job trial: the median length was 136 and the mean duration was 158 days, and 99% of the participants were enrolled in a programme that was shorter than one year.

A person can participate in a public works programme several times. We decided that each person would be included in the control group once, with the first spell starting after 1 January 2015. Excluding only a few cases with serious data errors, our first control group consists of 26,631 individuals.

CONTROL GROUP 2: PARTICIPANTS IN TRAINING PROGRAMMES

The other control group consists of training programme participants who were under age 25 at the beginning of the training, and did not enter any other wage subsidy programme. To ensure that we had an outcome variable six months after completion of the programme, the control group includes participants who entered between January 2015 and April 2017, as the outcome variables are available until the end of 2017. Participants in YG education programmes account for 90% of the control group 2.

According to the principles of the YG, the PES counsellor and the young jobseeker choose among the different ALMPs together, while taking into account the qualifications and needs of the jobseeker and the labour market environment. However, as there are no clear-cut rules and there are differences in the eligibility criteria, this exercise might give us a picture of the relative effect of participating in the job trial compared to participating in the training programmes. (A comparison of the 90-day job trial programme with the longer-

[20] At the same time, the NEET rate was 14% and 20%, for young men and young women, respectively.

term wage subsides was not possible because of the short sample period, which would result in a low number of observations in the control group.)

The control group 2 consists of training programme participants who had never participated in any wage subsidy programme, and are not members of the control group 1. This control group consists of 6330 persons. We excluded less than 10 observations from all the three groups because of serious data errors or missing values of an important observable characteristic, such as education. These exclusions can be regarded as random.

As the employment status and the wages are available only until the end of 2017, we had to restrict our treatment group to those who entered the YG90 programme between 1 January 2015 and 1 April 2017. As the duration of public works programmes varies between 30-428 days, the control group includes workers who reported participating in a public works programme in May 2017.

5.3. Outcome variables

We are interested investigating how successful the job trial was in improving the labour market prospects of the participants. Accordingly, we consider the following outcome variables. We present the variables to assess the employment outcomes six months after completion of the programme:

- Works as an employee;
- Works under any employment contract (includes employees, those employed with a temporary contract, and the self-employed), if the monthly wage reaches 80% of the statutory minimum wage;
- Works under any employment contract, including a public works contract; and
- Cumulative days spent with any employment contract in the six months after completion of the programme, in all types of contrasts, as an employee, and in any contract with a monthly wage and including public works contracts.

Our other two outcome variables are the cumulated earnings relative to the statutory minimum wage, accumulated within six months after completion of the programme, excluding and including earnings from public works programmes. We focus on the cumulative earnings variable, which includes wages from public

works programmes. This is because our primary interest is to evaluate whether participation in the job trial enabled the young person to earn wages through regular employment, rather than from an alternative source, such as from a public works programme.

5.4. Observables and selection into the treatment group

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

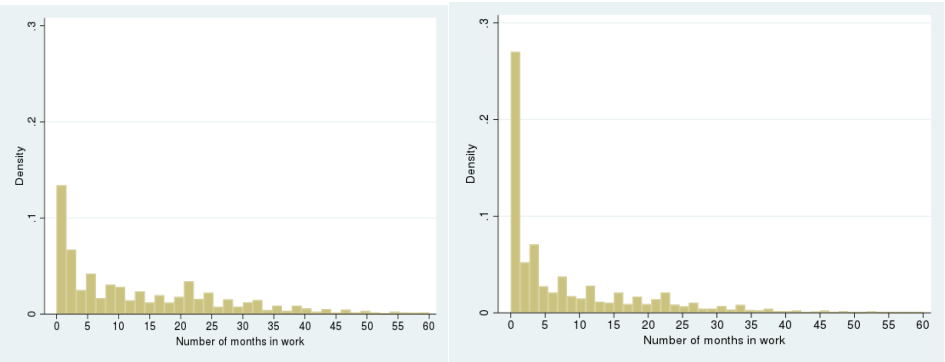
spent between the last registration with the public employment service and the start of the programme.

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.[21]

Figure 5-1 Employment history: number of months an individual was employed (excluding public works) preceding the programme

a) 90-day job trial *b) public works programme*



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

c) training

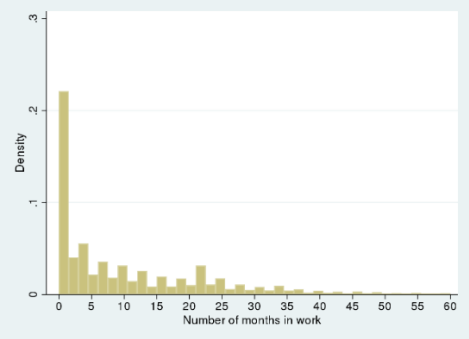
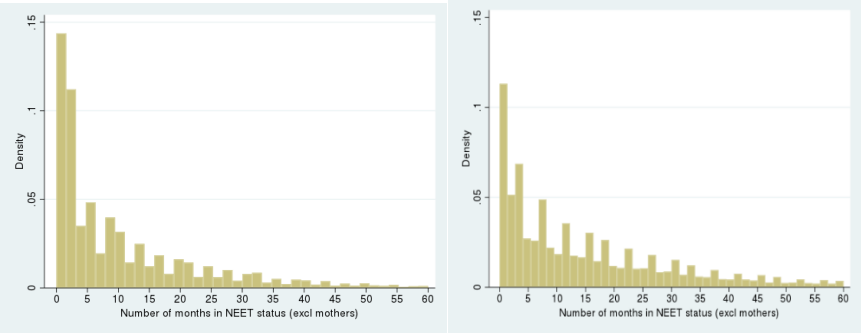


Figure 5-2: *NEET history (excluding periods of child-related benefits)*

a) 90-day job trial *b) public works programme*



c) training

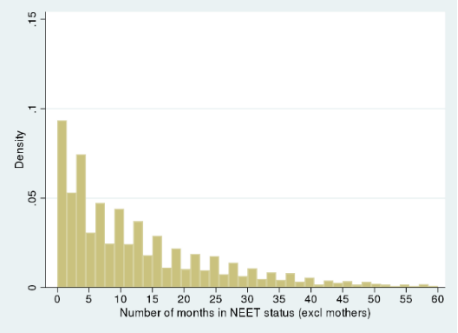


Figure 5-1 and *Figure 5-2*. indicate that there is a marked difference between the labour market histories of the treatment and the control groups: namely, the job trial participants had more work experience and spent less time as a NEET preceding their participation in the programme than both the public works participants and the training participants. This gap is especially noticeable with the public works participants, but the training participants also seem to have less favourable labour market histories. We will formally test these differences at the inspection of the covariate balance. shows that the share of newly registered jobseekers

with zero months of NEET history is lower among public works participants. However, 11% of these individuals enrolled in a public works programme immediately after leaving school or registering with the PES.

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.

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. The maternity benefits and the role of parenthood in the selection of the programme are described in detail in Section 6.6.

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[22] based on the assumption that places differ strongly in terms of the proximity of

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

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.

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.

SELECTION INTO THE PROGRAMME

Understanding the drivers of the selection mechanism is of great importance. It has often been observed that participants in active labour market programmes are, on average, in a better labour market position than eligible non-participants, even before they enter the programme (e.g., Bell and Orr, 2002; Lechner and Smith, 2007). This may be due to “cream-skimming”, which occurs when participants with a higher probability

inhabitants.

of finding employment are selected to participate in order to facilitate the programme's implementation and to improve its results; or due to self-selection, as more capable and motivated participants are more likely to agree to participate in the programme.

However, according to the principles of the YG, priority should be given to the long-term unemployed, the vulnerable, and the socially excluded groups in providing jobseekers with good quality offers, and allowing them to choose any programme. This principle[23] implies that YG participants should be selected from less employable jobseekers. Bratti et al. (2018), who evaluated a YG vocational training programme in Latvia, found that in line with the YG guidelines, the participants in the YG vocational training were indeed less employable in terms of their observable characteristics, and, probably, also in terms of their unobservable characteristics: they were less educated, the proportion of individuals living in the capital or in other cities was lower than in the pool of young jobseekers, and the labour market experience of programme participants was also higher on average than that of the control group.

By analysing the selection process, we aim two answer two questions. First, did the YG meet the goal of enrolling the most vulnerable young people? Second, when we contrast the selection and the heterogeneity of the treatment effect, do we find that the programme was well-targeted?

Table A1 shows the covariate balance of the treatment group and job trial and public works programme participants. The table suggests that YG job trial participants were in a much more favourable position based on most of the characteristics measured. For the majority of the variables, the differences between the treatment group and the public works participants were significant. The differences in the level of education were sizable: 61% of public works participants, compared to 28% of job trial participants, had elementary education or less. Accordingly, the type of job the jobseekers were looking for differed markedly among the two groups: 43% of public works programme participants, compared to 18% of job trial participants, were seeking a job with elementary work responsibilities. When we look at the jobseekers' labour market histories, we see that compared to public works participants, the job trial participants had longer work experience and spent less time as a public works participant or as a NEET. The pattern of child-related transfers also differed between the two groups: over their whole previous histories and in the previous two years, the public works programme participants were more likely than the job trial participants to have received any child-related benefits, and the average number of months they received these benefits was also higher.

Moreover, the treatment and the first control group exhibited clear geographical differences. The public works participants were more likely than the job trial participants to be from a less developed district or

[23] Hungary's National Youth Guarantee Implementation Plan

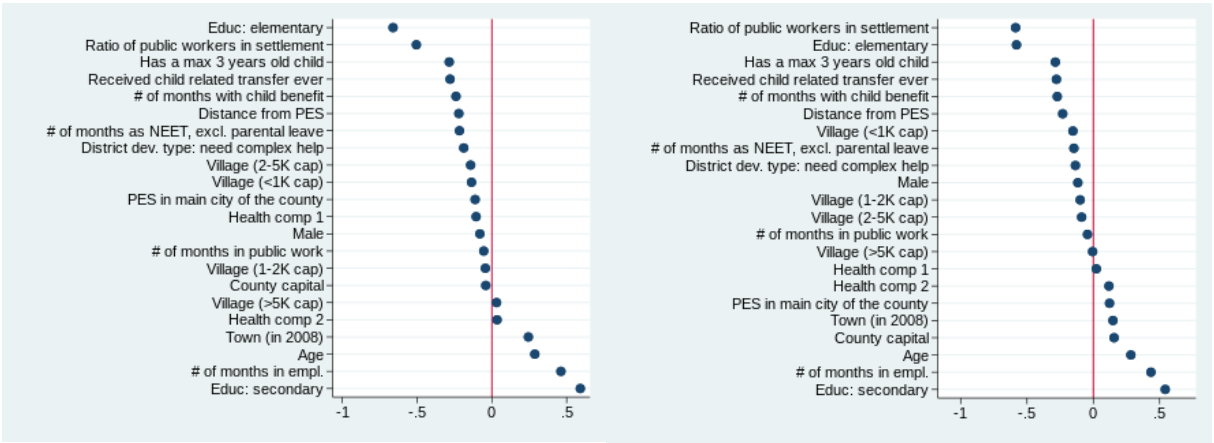
region, and from a settlement with a high incidence of public work. The job trial participants were almost balanced in terms of their gender composition, but among the public works participants, the share of males was significantly higher. The sample period covers two and a half years, but the public works participants were more concentrated in the first year, and within each year, the inflow was the greatest in the first quarter. As the labour market environment improved gradually over the sample period, we add the quarter of entry as an explanatory variable. Moreover, as the duration of the programmes differ, we also added the year of the end date of the programme.

While many of the above variables were correlated, the pseudo R square from the logit regression of the treatment indicator on the above variables was high, at 0.46. The difference between the treatment group and the training participants is similar in direction, albeit for most of the variables, the differences between the job trial participants and the training participants were smaller in magnitude (see *Table A2).*

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 5-3*). 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.

Figure 5-3: Covariate balance: a) 90-day job trial participants and b) all YG programme participants vs registered jobseekers under age 25

a) 90-day job trial *b) Youth Guarantee participants*



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.

6. Propensity score matching results

6.1. Baseline results

Figure A3 displays averages of the outcomes 6 months after the programme for the treatment and the control groups. In average, job trial participants perform the best, the second is the group of training participants and usually the public works participants have the worst value. The only exception is the cumulative wages including the wage from public works within 6 months after the programme, as public works participants are more likely to be enrolled in another public works programme than the participants of trainings and public works. The difference is sizeable in case of most outcomes, for example, the probability of being an employee 6 months after the programme is 44% in case of a public works participant, while only 18% in case of a public works participant. However, our matching results reveal that strong differences in observable characteristics explain bulk of the raw difference between the treatment and the control groups.

Figure 6-1 displays the propensity score distributions before and after the matching in the control and the treatment groups. There is a sharp difference between the treatment and control groups in the raw sample, nevertheless, the common support assumption holds, and the distribution of propensity score are in line in the matched sample. *Figure 6-2* indicates that no significant difference remains for the main covariates in the matched sample.

Figure 6-1: Distribution of the propensity score in the raw and the matched sample

a) Control group: public works participants *b) Control group: training participants*

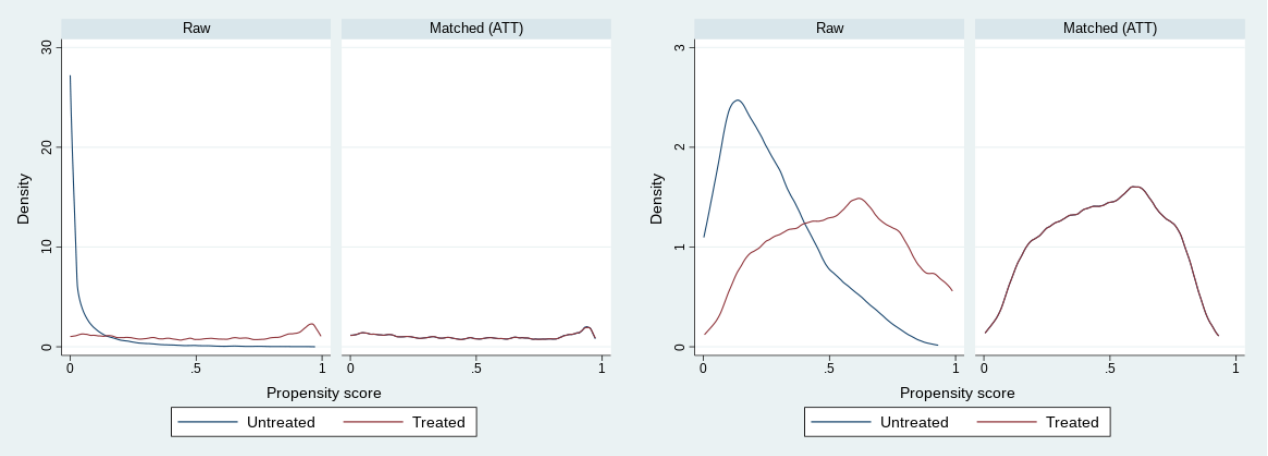
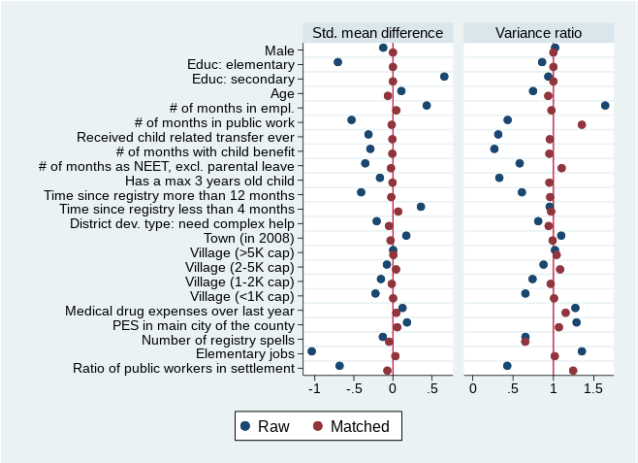
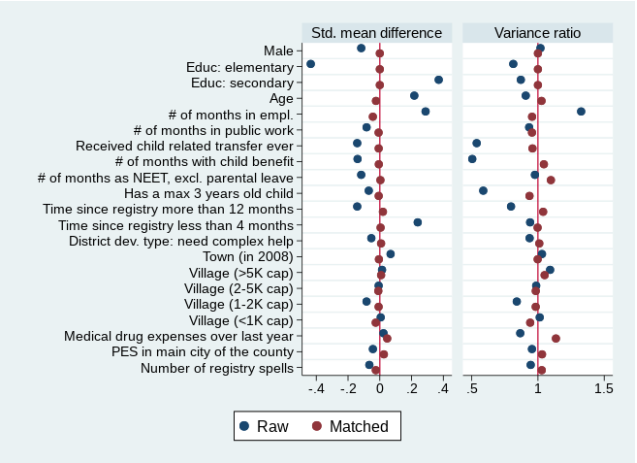


Figure 6-2: Standardised mean difference and variance ratio in selected covariates in the raw ad matched samples

a) Control group: public works participants



b) Control group: training participant



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 considered to be more balanced if the post-matching statistics are closer to the red lines. Note that the training participants were not matched based on industry, as people who are in training often do not work.

The baseline matching results are summarised in Table *6-1*. Compared to the control group of public works participants, the job trial participants were 7.6 percentage points more likely to be employed six months after completing the programme. When we consider that the baseline value for the public works participants was less than 0.2, the ATT in relative terms was close to 40%. When we consider all employment contracts with earnings above 80% of the minimum wage (column (1)), the effect was weaker, at 4% points. In cumulative terms, job trial participants worked 14 -23 days more in the six months following the programme, depending on the definition of work.

Taking into account that according to both international (Card et al, 2018; Caliendo et al, 2016) 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 does not necessarily imply that the job trial has a positive causal impact compared to non-participation.

Strikingly, there is no difference in the total labour income accumulated during the six months after the completion of the programme. The estimated ATT is even negative, but not significant. This implies that the wages the participants received from public works, and, perhaps, from temporary work, counterbalanced

their lower cumulated earnings from their employee status. One potential explanation for this finding is that although the wages for public works jobs were very low (below 75% of the minimum wage), on average, public works participants received labour income for a longer time than job trial participants.

Compared to that of the training participants, the ATT for the probability of being an employee or employed with wages above 80% of the minimum wage was close to zero for the job trial participants, though in cumulative terms, we find a moderate, but significant impact: job trial participants worked more over a six-month period than the training participants, by 7.5-11 days (column (5)- (7)).

The effect on total labour income of participation in the job trial was not shown to be significant when wages from public works programmes were included (column (6)). However, the effect on wages from the primary labour market (column (7)) for job trial participants was found to be significant. Compared to training participants, job trial participants accumulated more earnings only if we include wages from public works, which implies that job trial participants had a higher probability than training participants of working in a public works programme.

The 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, and even the whole difference in case of cumulative earnings including wage from public works.

Table 6-1: Matching results: employment and cumulated earnings six months after the programme

ATT	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative days worked	(4) Cumulative days worked earning above 80% of min. wage	(5) Cumulative days worked as employee	(6) Cumulative wage, incl. public work	(7) Cumulative wage, excl. public work
Control: Public works participants	0.0408**	0.0760***	22.77***	13.99***	19.96***	-0.201*	0.667***
	(0.0197)	(0.0203)	(2.435)	(2.415)	(2.421)	(0.116)	(0.117)
n_treat	3291	3291	3291	3291	3291	3291	3291
n_used_cont	18670	18670	18670	18670	18670	18670	18670
Control: Training Participants	-0.0165	0.00880	10.60***	7.725***	11.60***	0.243**	0.121
	(0.0143)	(0.0145)	(2.052)	(2.056)	(2.074)	(0.0986)	(0.101)
n_treat	3313	3313	3313	3313	3313	3313	3313
n_used_cont	6016	6016	6016	6016	6016	6016	6016

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

Table shows the estimates of average treatment effect on the treated using two separate non-treated groups as controls. The outcomes are measured six months after completion of the 90-day job trial programme. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposal of Huber et al. (2015). Standard errors in parentheses.

6.2. Results for sub-sample with competence test scores

As a robustness check, we estimated the model with a variable that is presumably correlated with abilities, the so called competence test scores. The competence test is written in all schools of the country of pupils in 6. and 10. class (roughly age 12 and 16). As tests scores are available only about 70% of the sample, and missing test scores cannot be regarded as random[24], we estimated the propensity score with and without the 10th class standardized competence test score on the subsample with competence scores available. The results (*Table 6-2*) indicate that the competence test score does not alter the results significantly.

Table 6-2: Matching results for sub-sample with competence test scores

ATT	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative wage, incl. public work	(4) Cumulative wage, excl. public work	(5) Cumulative days worked	(6) Cumulative days worked earning above 80% of min. wage	(7) Cumulative days worked as employee
Results with standardized test scores included in the propensity score estimation							
Control: Public works participants	0.0219	0.0648*	-0.385***	0.643***	23.34***	14.09***	21.29***
	(0.0347)	(0.0340)	(0.144)	(0.150)	(3.032)	(3.066)	(3.001)
n_treat	2492	2492	2492	2492	2492	2492	2492
n_used_cont	6794	6794	6794	6794	6794	6794	6794
Control: Training Participants	-0.0123	0.0115	0.197	0.0721	11.69***	7.746***	11.65***
	(0.0184)	(0.0186)	(0.145)	(0.148)	(2.576)	(2.659)	(2.675)
n_treat	2527	2527	2527	2527	2527	2527	2527
n_used_cont	3685	3685	3685	3685	3685	3685	3685
Results without standardized test scores (same base sample)							
Control: Public works participants	0.0302	0.0670**	-0.335**	0.699***	23.67***	14.75***	20.81***
	(0.0264)	(0.0267)	(0.146)	(0.143)	(3.108)	(3.071)	(3.102)
n_treat	2475	2475	2475	2475	2475	2475	2475
n_used_cont	6824	6824	6824	6824	6824	6824	6824
Control: Training Participants	-0.00551	0.0152	0.215	0.0960	11.89***	8.134***	12.10***
	(0.0176)	(0.0178)	(0.132)	(0.135)	(2.508)	(2.599)	(2.589)
n_treat	2506	2506	2506	2506	2506	2506	2506
n_used_cont	3662	3662	3662	3662	3662	3662	3662

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

Table shows the estimates of average treatment effect on the treated using two separate non-treated groups as controls. The outcomes are measured six months after completion of the 90-day job trial programme. The underlying matching algorithm is Epanechnikov kernel propensity score matching combined with exact matching on gender and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposal of Huber et al. (2015). Standard errors in parentheses. Differences in sample size are due to differences in the common support.

[24] Table A3 shows the comparison of those with and without competence test score in the sample of job trial or public work programme participants

6.3. Heterogeneity by the level of education

Table 6-3 summarises the estimated treatment effect on the treated 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. The wage above 80% of the minimum wage was stronger for participants with elementary education or less, but we find somewhat stronger effect for higher educated participants in case of cumulative working days in all types of employment and as an employee. While young jobseekers with low education had a lower chance of being enrolled in the programme, the impact of participation on their employment probability six months after completion of the programme 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. We found no robust heterogeneity in the effect of being employed six months after completion of the programme with respect to level of education compared to that for training participants. However, compared to training participants, higher educated job trial participants found a job in the primary labour market more quickly than the lower educated job trial participants (column (5)-(7)).

In contrast to the treatment effect, the raw mean of the employment probability six months after participation was significantly higher for job trial participants with at least secondary education than for those with elementary education or less (see *Figure 6-3*). As the programme results are assessed (by the PES) based on the raw employment probability 6 months after the programme, instead of their impact, this gives an incentive to select jobseekers with a better labour market position, although the program can achieve a comparable improvement for participants who have worse prospects before the programme. This could be easily improved by defining separate outcome indicators for subgroups of participants, ie. for those with low / high level of education, or those living in low/high unemployment regions.

Similar differences can be observed when we compare the raw mean differences in cumulative earnings between the elementary-educated and the higher-educated participants: while the raw mean cumulative wages indicate that the higher-educated job trial participants accumulated higher earnings, on average, within the six months after completing the programme, the ATT is similar for participants with lower education, with public works participants as a control group.

Compared to the training participants, the effect in almost all outcomes is stronger for the higher-educated jobseekers, indicating that jobseekers with elementary education can profit even more from trainings.

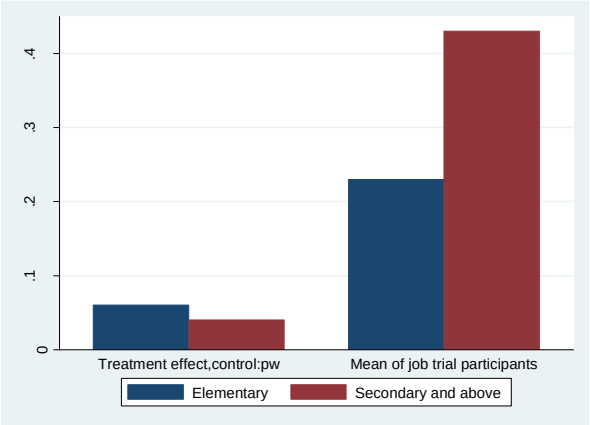
Table 6-3: Average treatment effect on the treated by level of education

ATT	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative wage, incl. public work	(4) Cumulative wage, excl. public work	(5) Cumulative days worked	(6) Cumulative days worked earning above 80% of min. wage	(7) Cumulative days worked as employee
				Primary education			
Control: Public works participants	0.0610*** (0.0186)	0.0815*** (0.0195)	0.336*** (0.119)	0.684*** (0.124)	17.96*** (3.010)	14.90*** (2.937)	18.16*** (3.028)
n_treat	951	951	951	951	951	951	951
n_used_cont	11422	11422	11422	11422	11422	11422	11422
Control: Training Participants	-0.0363* (0.0205)	-0.0285 (0.0213)	0.133 (0.135)	-0.0565 (0.138)	3.454 (2.968)	3.721 (2.854)	4.963* (2.955)
n_treat	937	937	937	937	937	937	937
n_used_cont	2834	2834	2834	2834	2834	2834	2834
				Secondary and tertiary education			
Control: Public works participants	0.0402* (0.0243)	0.0847*** (0.0250)	-0.389** (0.154)	0.681*** (0.158)	24.42*** (3.220)	13.24*** (3.220)	20.63*** (3.188)
n_treat	2335	2335	2335	2335	2335	2335	2335
n_used_cont	6488	6488	6488	6488	6488	6488	6488
Control: Training Participants	-0.0133 (0.0184)	0.0171 (0.0185)	0.274** (0.124)	0.177 (0.128)	12.13*** (2.626)	7.769*** (2.701)	12.37*** (2.692)
n_treat	2389	2389	2389	2389	2389	2389	2389
n_used_cont	3046	3046	3046	3046	3046	3046	3046

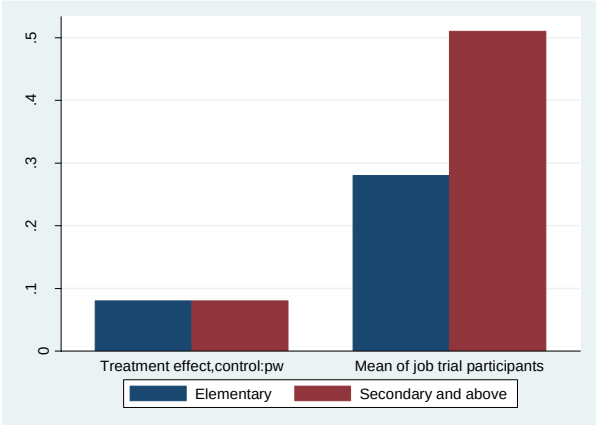
Epanechnikov kernel propensity score matching combined with exact matching on gender, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposition of Huber et al. (2015). Standard errors in parentheses. Propensity score is estimated on sub-sample.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6-3: Probability of employment 6 months after the programme : treatment effect compared to public works and the mean of job trial participants

a)Employment abowe 80% of mw



b)Employee in primary labour market



6.4. Programme combinations

As we discussed in Section 5, we excluded job trial participants who were enrolled in a longer-term wage subsidy after completing the job trial, as the probability of employment 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. Indeed, *Table 6-4* suggests that those who combined participation in the job trial with participation in a longer-term subsidy programme were in an even more favourable labour market position than the members of the treatment group.

We addressed the problem of eliminating the most employable 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[25] 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.

[25] Győr-Moson-Sopron, Tolna, Vas and Zala

Table 6-4: Comparison of 90-day job trial participants with and without subsequent wage subsidy

Variable	Mean		%bias	t	p>|t|
	No wage subs	Wage subs			
Male	0.51	0.50	1.3	0.49	0.623
Age	21.26	21.50	-12.4	-4.54	0.000
# of months in empl.	14.46	17.00	-16.6	-6.19	0.000
# of months in public work	1.77	1.34	9.1	3.31	0.001
# of months as NEET, total	12.94	10.40	17.5	6.33	0.000
# of months as NEET, excl. parental leave	12.20	10.07	15.5	5.61	0.000
# of months with child benefits	0.97	0.43	10.9	3.8	0.000
Received child-related transfer ever	0.03	0.02	11.2	3.96	0.000
Education elementary	0.28	0.16	30.6	10.99	0.000
Education secondary	0.68	0.79	-25	-9.08	0.000
Education tertiary	0.04	0.05	-7.4	-2.81	0.005
District needs complex help	0.23	0.16	15.8	5.75	0.000
Public works share in settlement	0.08	0.07	9.8	3.57	0.000
Town	0.41	0.49	-15.6	-5.71	0.000
Elementary occ	0.33	0.21	25.7	9.35	0.000

The results based on the restricted sample are broadly in line with the baseline results. However, as the sample size is much smaller, the coefficients are not precisely estimated in case of probability of being employee and the effects are stronger for training participants. 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.

Table 6-5: Average treatment effect on the treated for counties where no subsequent wage subsidy is present applied after the job trial

	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative wage, incl. public work	(4) Cumulative wage, excl. public work	(5) Cumulative days worked	(6) Cumulative days worked earning above 80% of min. wage	(7) Cumulative days worked as employee
Control: Public works participants	-0.0129	0.0682	0.141	0.876**	34.69***	22.43**	35.18***
	(0.0631)	(0.0605)	(0.373)	(0.406)	(8.033)	(8.715)	(8.466)
n_treat	331	331	331	331	331	331	331
n_used_cont	938	938	938	938	938	938	938
Control: Training Participants	0.0756*	0.142***	1.023***	1.026***	37.02***	30.01***	42.01***
	(0.0420)	(0.0414)	(0.265)	(0.268)	(5.751)	(5.935)	(5.923)
n_treat	359	359	359	359	359	359	359
n_used_cont	450	450	450	450	450	450	450

Epanechnikov kernel propensity score matching combined with exact matching on gender and level of education, with replacement. Bandwidth is calculated with a pair-matching based algorithm following the proposal of Huber et al. (2015). The estimation is restricted to the following counties: Győr-Moson-Sopron, Tolna, Vas, and Zala.

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

6.5. Outcomes six vs 12 months after the program

Up to this point, we have evaluated our employment and wage outcomes 6 months after completing the programme. In this section, we compare the results for the outcome variables 6 vs. 12 months after completing the programme. Note that to ensure that the estimations on the two horizons were compatible with each other, we have restricted our sample here to young people whose outcomes could be observed 12 months after completing the programme. This implies that the job trial participants who entered between 1 January 2015 and 30 September 2016 are included in the treatment group.

The results (*Table 6-6*) 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 6-6: Comparison of outcomes six and 12 months after the programme

ATT	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative wage, incl. public work	(4) Cumulative wage, excl. public work	(5) Cumulative days worked	(6) Cumulative days worked earning above 80% of min. wage	(7) Cumulative days worked as employee
				Control: Public works participants			
6 mo.	0.0513**	0.0876***	-0.0617	0.677***	22.04***	13.92***	21.07***
	(0.0220)	(0.0230)	(0.150)	(0.145)	(3.077)	(2.880)	(2.943)
12 mo.	0.0159	0.0595**	-0.158	0.906***	35.40***	18.04***	33.08***
	(0.0236)	(0.0239)	(0.322)	(0.328)	(6.580)	(6.367)	(6.449)
n_treat	2799	2799	2799	2799	2799	2799	2799
n_used_cont	14817	14817	14817	14817	14817	14817	14817
				Control: Training Participants			
6 mo.	-0.00827	0.0176	0.200	0.102	9.494***	8.820***	11.79***
	(0.0176)	(0.0179)	(0.132)	(0.135)	(2.546)	(2.467)	(2.507)
12 mo.	0.00230	0.0206	0.0463	-0.192	11.69**	8.568*	15.01***
	(0.0178)	(0.0180)	(0.270)	(0.278)	(5.012)	(4.963)	(4.999)
n_treat	2820	2820	2820	2820	2820	2820	2820
n_used_cont	4692	4692	4692	4692	4692	4692	4692

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

6.6. Gender differences in the selection and programme effect

As we discussed in Section 2, the male NEET rate is below while the female NEET rate is above the EU average. Moreover, Hungary has the largest gap between the female and the male NEET rate in the whole EU. This is probably because of the low activity rate of young mothers in Hungary, which can be traced back to the low supply of child care and the relatively generous maternity allowance. There is empirical evidence that entitlement to prolonged maternity leave reduces the labour supply of women and negatively affects the employment rates and wages of mothers (Kunze, 2016), while the availability of child care services increases the female labour supply (Kunze and Liu, 2019).

6.6.1. Parenthood and employment - institutional background

The parental benefits in Hungary have insurance-based and non-insurance-based elements (see *Table 6-7*). Among the insurance-based benefits, the CSED (infant care benefit) lasts for six months, covers 70% of the parent's pre-birth wages, and requires the parent to have at least two years of previous employment and health insurance coverage. All of the benefits can be claimed by one of the two parents. The GYED (child care benefit) follows the CSED and is available until the child reaches age two. The GYED covers 70% of the previous wage, but only up to 1.4 times the minimum wage. The non-insurance type benefit, the GYES (child care allowance), provides health care and pension insurance coverage, and a low benefit amount (about 25% of the minimum wage) for one of the parents. Parents without sufficient health insurance coverage are eligible for the GYES for the first two years of parenthood. One of the parents in a family with at least three children is entitled to collect the GYET (childbearing support) until the youngest child reaches age eight.

Table 6-7.: Parental transfers in Hungary

Age of the child	Worked before motherhood	Employment restrictions	No working history	Employment restrictions
0-6 month	CSED: 70% of previous earnings	employment not allowed	GYES: fixed amount	from 2015: No restriction above age of 1 year
6-24 months	GYED: 70% of previous earnings, until max 140% of the stat min wage	from 2015: no limit above 6-month age		
24-36 months	GYES: fixed amount*	from 2015: no limitations		
3-8 years	GYET, with at least 3 children, fixed amount*	20 hours per week	GYET	20 hours per week

*In 2015, 28500 Ft, 27% of the statutory minimum wage.

The regulation of a parent's earning activities for the period when she receives the benefit has been gradually eased since 2010; and since 2015, GYES and GYED recipients have been allowed work without any restrictions after the child turns six months old. In parallel with this easing of restrictions on earning activities, parental benefit recipients became eligible to receive the same services as registered jobseekers. Hence, parents could participate in all programmes of the YG.

Under the institutional framework of parental benefits in Hungary, all mothers, regardless their working history, are entitled to receive some kind of maternity benefit for a given period. Moreover, we can assume that all parents have been receiving a maternity benefit at least until the child turns age three after 2013, when engaging in earning activities while receiving benefits was allowed after the child's first birthday. Consequently, we argue that we are able to detect almost all mothers in the database.

Although the majority of these transfers might also be claimed by fathers, fathers in Hungary still rarely take parental leave. Accordingly, 84-92% of child-related transfer recipients are to women, and the share of child-related transfer recipients is much higher among female participants than among males. Against this

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

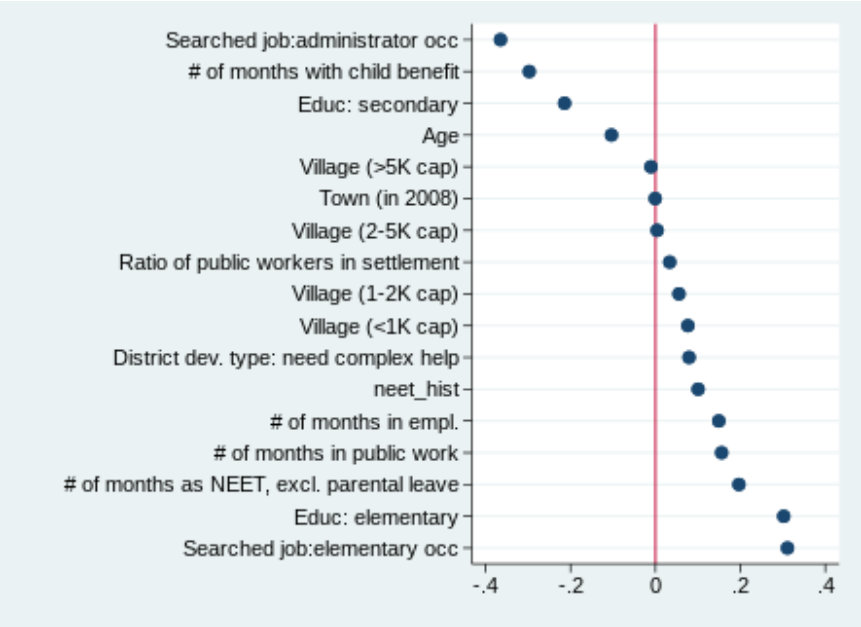
6.6.2. Gender differences in the selection into the job trial programme

The gender composition of the job trial participants was close to 50-50%. Nevertheless, taking into account that the NEET rate of young women was higher, the gender balance of participation implies that the outreach to women was weaker.

A comparison of female job trial participants with the control group indicates that, on average, the 90-day job trial participants had received parental benefits for shorter periods of time than the public works programme participants: compared to the latter group, the former group spent 5.9 fewer months receiving any parental transfers (and three fewer months than the training participants), and were 5.6% points less likely to have a child under age three (and 2% less likely than the training participants) (see *Table A11*). As the decision to become a parent might be correlated with other variables, we present the differences in the above variables after controlling for other age, educational, and regional differences. The results show that mothers were less likely to participate in job trials than in public works (and training programmes) even after controlling for differences in all characteristics, which implies that being a mother decreased the probability of participating in the YG programmes, and particularly in the job trials.

Moreover, 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. This indicates that female job trial participants constitute an even more strongly selected group, with even better labour market prospects that male participants (see *Figure 6-4* and *Table A12*).

Figure 6-4: Comparison of and male and female job trial participants: standardised mean difference in observable characteristics



Notes: The standardised difference is calculated as the difference between the mean of the male and female job trial participants over the standard deviation of the male participants

Table 6-8: Differences in the parental benefit histories of the female members of the treatment and control groups

CONTROL GROUP	(1)	(2)	(3)	(4)
VARIABLES	public works # months on parental benefits	training # months on parental benefits	public works child under 3 years	training child under 3 years
Raw differences between treatment and control groups	-6.088***	-2.548***	-0.0558***	-0.0209***
	(0.401)	(0.330)	(0.00646)	(0.00552)
Controlling for observables[1]	-2.945***	-1.304***	-0.0484***	-0.0109*
	(0.373)	(0.329)	(0.00693)	(0.00609)
Observations	10,293	4,574	10,293	4,574

Standard errors in parentheses

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

[1]OLS, with age, educational, and regional variables as controls

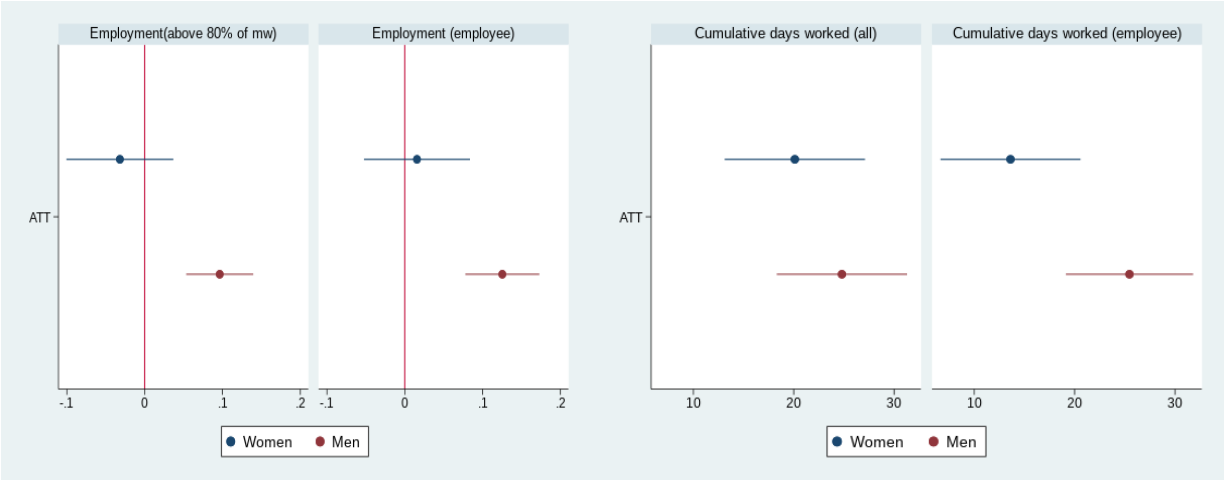
6.6.3. Gender differences in the effect of the job trial

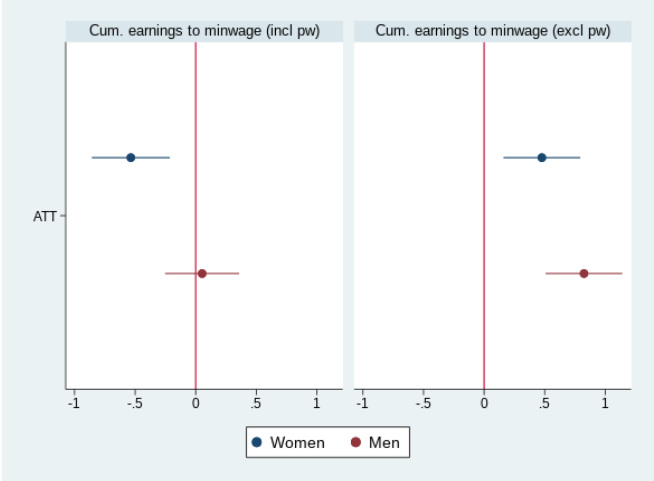
The majority of the existing studies on the gender differences in the effects of active labour market policies have found that women benefit more than men from labour market programmes. (e.g., see a survey by Bergemann and van den Berg (2008) for Europe). In contrast to this general conclusion, our results 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 improved both the employment probability six months after the programme, and the cumulative earnings and employment within the six months following the programme more for men than for women (*Figure 6-5*).

Interestingly, the comparison with training participants painted a different picture: no gender differences could be detected in the effects of participating in job trial program on the probability of being employed in the primary labour market, but the effect is larger for women in case of cumulative days and wages (see *Table A13*. This result indicates that women profit more from job trial programmes than from classroom trainings, that in part can be explained by the higher education level of female than male job trial participants.

We argue that the main explanation for these diverging results is that the factors mentioned in the literature as explaining the impact of ALMPs on women are missing in the case of the 90-day job trial. Bergemann and van den Berg (2008) focused on the different labour market status of women. Women have more options than men to split their time between paid employment and housework and child care. The so-called Chatelier principle, which states that individuals with more options have more elastic supply functions, implies that the female labour supply is more responsive to wage changes than the male labour supply. This principle has largely been confirmed by empirical studies (see, e.g., Evers, M., De Mooij, R., & Van Vuuren, D., 2008). Although female labour force participation has increased in recent decades, there are still a significant difference between men and women in employment rates. These differences might also explain the large programme effects for women. 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 men. A related argument is based on the assumption that many women are less engaged in the labour market.

Figure 6-5: Average treatment effect by gender compared to the public work participants on the six months horizon





The above mechanisms imply that the greater the gender gap in labour market participation, the larger the gender differences in the programme effects in favour of women are likely to be. As we showed in Section 2, the gender gap in Hungary is larger than the EU average, which would imply that the effects would be greater for women. However, despite their lower activity rate, the share of women was somewhat lower among registered jobseekers, which implies that outreach to women has been less successful. In addition, the comparison between male and female job trial participants indicated that the 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 *Figure 6-4* and *Table A12*). The weaker impact found for women is also in line with the results presented in Section 6.2, which indicated that the effects of the programme is slightly weaker on better educated participants.

An alternative explanation for the greater impact of the programme on females focuses on the role of motherhood. Lechner & Wiehler (2007) argued that the observed gender gap in the impact of labour market policy programmes can be partly traced back to a measurement problem: i.e., that the failure to control for pregnancy among non-participants may lead to upward biased results. Analysing Austrian labour market policies, the authors found that removing this bias significantly reduced, but did not eliminate the gender gap in the programme effects.

One potential factor behind the remaining gender gap is that as an unintended side effect, programme participants may postpone motherhood to increase their attachment to the labour market. That is, an unintended side effect of participation in the programmes is that it decreases fertility.

We also found that job trial participants were indeed less likely to have a child 12 months after the end of the programme, but that the difference in the probability of receiving any parental transfers disappeared in the matched sample. Hence, we conclude that participation in the job trial did not have a causal impact on women's fertility.

Table 6-9: Propensity score matching estimates for the parental status of young women, 12 months after the beginning of treatment

	Control: Public works participants		Control: Training participants	
	Raw difference	Matched difference	Raw difference	Matched difference
ATT	-0.03643***	-0.000673	-0.00361	0.00640
	(0.00584)	(0.00489)	(0.00454)	(0.00409)
n_treat	1562	1361	1562	1340
n_used_cont	8592	6192	2074	1954

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

6.7. Deadweight losses and displacement effect

An obvious caveat of any wage subsidy programme is the high probability of deadweight effects arising from employers hiring an employee with a wage subsidy even though they would have hired her without the subsidy. The likelihood of deadweight might be even higher in case of short-term job trials without obligation for further employment.

Wage subsidy programmes of the Youth Guarantee require from firms that they increase average workforce by at least the number of the subsidized young employees. The aim of this rule is to reduce the displacement

effect, that is the risk that subsidised young person simply substitutes a non-subsidised employee. Nevertheless, the Youth Guarantee was launched in 2015, in a generally favourable economic environment, when the employment growth exceeded yearly 2% in the 2015-2017 period. This implies that the prescribed increase in the firm size might not prevent deadweight losses to materialize from YG wage subsidies.

The magnitude of deadweight losses and the displacement effect is generally difficult to measure and the empirical literature addresses this problem is scarce (Caliendo, 2016). We try to assess the impact of the programme on net employment and share of young persons by linking the administrative dataset with the dataset of tax files of firms, which includes balance sheets and other firm details for all double-entry bookkeeping firms in Hungary. About 60% of the employers of Youth Guarantee wage subsidies can be found in the tax file database

In order to assess deadweight losses, we compare change in the number of employees and share of employees under age 25 at firms who hired a young person with one of the wage subsidy programmes of the Youth Guarantee with all other similar firms that have not hired a subsidized young employee. Here we do not concentrate exclusively on the on the 90-day job trial, but treat all Youth Guarantee wage subsidies together.

The basic specification is the following:

$$\Delta emp_{it} = emp\_YGnew_{it} + X_i + Z_{i,t-1} + v_t + \varepsilon_{it}$$

where emp is the yearly average number of employees at the firm; emp_YGnew is the number of new hires under a YG wage subsidy programme, i is the firm identifier and t denotes years. X includes firm specific variables and Z predetermined variables that capture the factors influencing the employment growth at the firm in addition to hiring a young person. Specifically, we use the following controls: dummy for geographical location (district), industry (two-digit NACE code), lagged values of share of employees under age 25 year and share of employees in unskilled jobs (jobs with ISCO code 7-9), average wages (thousand HUF per person), labour productivity[26], size category in t-1[27]and change in number of employees in the 2 years preceding the

[26] Productivity is calculated as (sales+activated own performance- cost of goods and services purchased from other firms)/ number of employees.
[27] Size categorieas are the following: firm with 1-10, 11-20, ..91-100 employees

actual year (between years t-3 to t-1) . We constrained our analysis to firms at most 100 employees in the preceding year (98% of YG wage subsidies is used by firms at most 100 employees). As the individual administrative database contains half of the population, we multiplied the number of new YG subsidized employees in order to be able to assess the magnitude of the estimated coefficient.

The results are summarized in *Table 6-10.* The first column contains results for all firms with maximum 100 employees, the second column shows results for firms under 25, the third column displays results for firms 26-100 employees and the fourth columns show firms with maximum 50 employees in the preceding year. The standard errors are clustered by firm. The estimated coefficient is significantly positive, indicating that firms with subsidized young persons increased their size more than similar firms. However, the parameter is well below one, indicating that the increase in the workforce due to the subsidized young employee lags behind the number of subsidized hires at the firm, suggesting the presence of deadweight losses. Our results suggest that the deadweight is equivalent roughly half of hires.

Table 6-10: *Estimation of deadweight loss*

	(1) All firms max 100 employees	(2) Firms below 25 employees	(3) Firms 26-100 employees	(4) Firms max 50 employees
Independent/outcome variable	d (emp)	d(emp)	d(emp)	d(emp)
# of new hires with YG wage subs	0.483***	0.451***	0.618***	0.461***
	(0.0388)	(0.0381)	(0.111)	(0.0385)
d(emp) (between t-3 and t-1)	0.00352	-0.0209	0.0204	-0.0147
	(0.0172)	(0.0130)	(0.0280)	(0.0248)
average wage (t-1)	0.0854**	0.0772**	0.304***	0.0799**
	(0.0405)	(0.0390)	(0.0528)	(0.0392)
productivity (t-1)	0.00153	0.00174*	-0.00226	0.00155*
	(0.000933)	(0.00103)	(0.00149)	(0.000931)
share of staff below age 25 (t-1)	0.213***	0.230***	0.704	0.238***
	(0.0388)	(0.0335)	(1.065)	(0.0407)
share unskilled jobs (t-1)	0.00876	0.0802	-0.920	0.0558
	(0.0610)	(0.0591)	(0.631)	(0.0600)
Constant	-0.291**	-0.228**	4.239*	-0.252**
	(0.122)	(0.108)	(2.241)	(0.117)
District dummy	YES	YES	YES	
2 digit industry dummy	YES	YES	YES	
Year dummy	YES	YES	YES	
Size category dummy	YES	YES	YES	
Observations	376,232	354,821	21,411	368,472
R-squared	0.005	0.004	0.029	0.005

For OLS: clustered standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Productivity is calculated as value added per employee (sales+activated own performance costs of purchased goods and services.).

*NN1 Propensity score matching, with exact matching on district, 2-digit industry, firm size category and year. Additional variables are share of employees below age 25, share un unskilled workers, log of average wage at the firm and revenue/ capital at t-1.

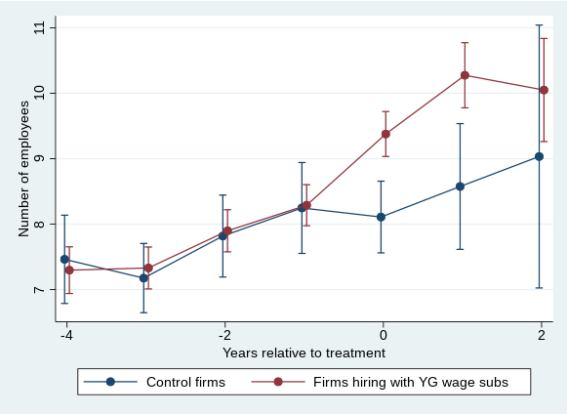
In addition, we applied propensity score matching to compare firms who hired a subsidized young person with those firms that did not, regardless the number of subsidized employees. A matching of firms with subsidized and nonsubsidized hiring is applied in Lombardi et al (2016), in order to estimate the impact of subsidized hiring on firm performance.

We matched firms which hired at least one young employee with an YG wage subsidy with firms that have never hired with YG subsidy, but other subsidies, targeting for example older employees, have not been excluded. Our primary interest is the impact of the programme on the number of total employees and the share of employees under the age of 25 in the two groups. We apply propensity score nearest neighbour matching using change of the firm size in two years preceding hiring a subsidized young person, average wage at the firm, share of unskilled employees and employees at most age 25 years in the preceding year. We combine this with exact matching on the district, two-digit industry NACE code, year and the firm size category in the preceding year.

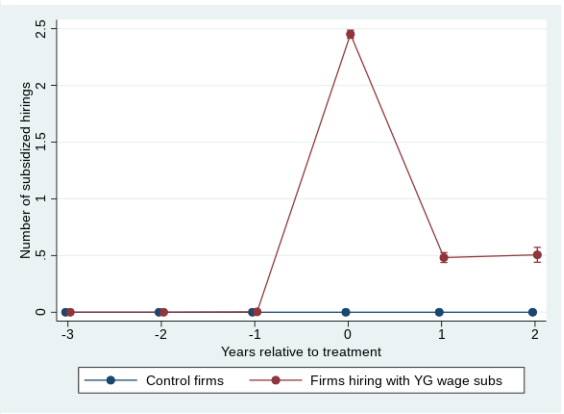
The following charts show the evolution of number of total employees, number of subsidized hires and the share of young workers at the firm, relative to the year of the first hiring of a young with YG wage subsidy in the treated and the matched control group. The *Figure 6-6* leads to a similar conclusion than our regressions :it implies that firms, hiring a wage subsidy participant increase their size more than similar firms without subsidized employees by 1,5 employees, but the difference is smaller than the number of new subsidized hires (2,5 at the first year and additional 0,5-0,5 employees in average in the following two years.). The share of young employees under the age of 25 also increases significantly after hiring a subsidized young person. Meanwhile, a slight decrease can be observed in the rate of young employees at the control firms, suggesting that young employees tend to concentrate at the firms hiring with YG wage subsidy.

Figure 6-6: Evolution of a) total employment, b) new subsidized hires, c) share of employees at 25 years at firms which hire with YG wage subsidy and control firms

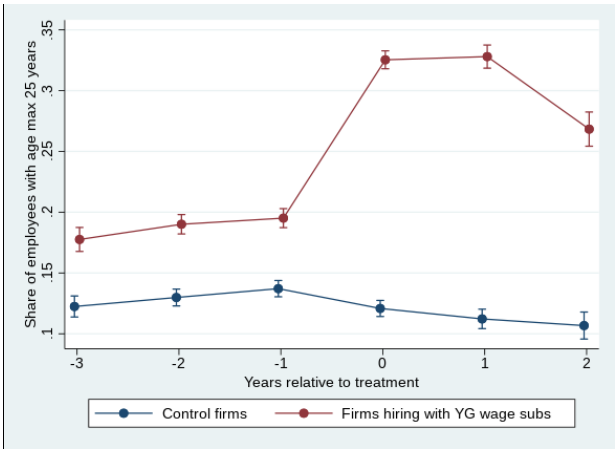
a)



b)



c)



7. Difference-in-differences framework

We also provide the results of a first attempt at evaluating the Youth Guarantee programme by taking advantage of the fact that the rollout strategy was staggered in Hungary. More precisely, in the six regions where the financing of the programme relied on YEI/ESF funds, the programme started on 1 January 2015; while in Central Hungary, where a combination of national and ESF funding was used, the implementation

did not start until after 1 October 2015[28]. Hence, the delay in the rollout of the YG in Central Hungary can be regarded as an exogenous factor stemming from the fact that the administration and the financing of the programme was delegated to different administrative units. However, the YG programmes offered in the Central Hungarian region had the same eligibility criteria and subsidies as those offered in the other six regions. Hence, we can exploit this rollout strategy by applying a difference-in-differences (diff-in-diff) framework to estimate the effect of the YG. It is important to highlight that this method identifies the intention-to-treat (ITT) effect, which shows the impact of the policy on the eligible subpopulation. Given this constraint, we cannot separate the effects of the 90-day job trial from those of the other YG programmes in this estimation. Thus, the estimation might capture the effects of the introduction of the Youth Guarantee as a whole.

More precisely, in our empirical strategy, we will contrast the evolution of the employment of young people who were (newly) registered as unemployed in Central Hungary during the 2014-2015 period with that of young people in Central and Western Transdanubia. These latter two regions were not eligible for YEI funds, and generally represent the two most developed regions of Hungary outside of Central Hungary. For our comparison, we needed to find regions where the general economic trends were similar to those of Central Hungary. This is important because the difference-in-differences strategy relies on the assumption that in the absence of the (early) introduction of the YG in these two regions, the evolution of the labour market outcomes of registered unemployed youth would have been similar to those in Central Hungary. Our hypothesis is that in Central and Western Transdanubia, the employment outcomes of young unemployed people improved (relative to Central Hungary) in 2015 thanks to the introduction of the YG, which gave them access to quality job offers.

Before proceeding to the details of our empirical strategy, we present the basic results on the inflow of youth into ALMPs in the relevant years. In the table below, we display our findings for a sample of young people who were registered as jobseekers in a given half-year (semester), and who were not registered in the previous six months. Then, for this inflow sample, we ask the following question: Where did these young people end up in the six months following their registration? We use the following broad categories: (1) started an ALMP; (2) participated in a public works programme; (3) left the register for a job (in the primary labour market); (4) left the register for an unknown destination (presumably: inactivity); and (5) was still unemployed, but did not participate in any measures. We present these statistics for the two larger regions: Central Hungary and Western Hungary (which includes Central and Western Transdanubia).

[28] This was due to the fact that Central Hungary was not a convergence region according to EU standards.

Table 7-1: Outcomes six months after initial registration as jobseeker, by region, year, and semester

Central Hungary	2014, 1st half	2014 2nd half	2015, 1st half	2015 2nd half	2016, 1st half	2016 2nd half
ALMP total	5.28	6.02	4.45	7.38	18.67	19.61
Public Works	9.55	7.42	7.62	7.51	5.81	4.39
Employed	40.92	40.72	44.75	42.08	41.84	39.01
Inactive	18.88	17.04	18.16	15.51	16.43	17.39
Unemployed	25.37	28.8	25.03	27.52	17.24	19.61

Western Hungary	2014, 1st half	2014 2nd half	2015, 1st half	2015 2nd half	2016, 1st half	2016 2nd half
ALMP total	16.11	14.6	20.6	21.82	23.12	22.42
Public Works	11.15	10.6	7.76	7.85	8.52	5.31
Employed	43.95	43.07	43.66	41.79	45.48	47.39
Inactive	11.88	11.4	11.45	11.73	11.34	11.28
Unemployed	16.9	20.33	16.52	16.82	11.53	13.6

The results displayed in *Table 7-1* point to five important phenomena. First, (early) access to ALMPs was much greater in Western Hungary than in Central Hungary even before the implementation of the YG. Second, participation in measures increased substantially in 2015 following the implementation of the YG; and by 2016, participation had grown by 150% compared to two years earlier. Third, we can clearly see the staggered implementation of the YG in Central Hungary: by 2016, young people in this region had only a slightly lower probability of participating in a measure than in Western Hungary. Fourth, thanks to the increase in participation in ALMPs, the proportion of young people who were still in the register six months after entry without receiving a substantive offer fell, and there was also a decrease in the use of public works programmes among young people. Finally, as the proportion of young people participating in an ALMP even after the introduction of the Youth Guarantee was no more than 20%, we cannot expect to observe large effect sizes in our results.

The empirical strategy

We base our estimates on the following regression specification:

$$y_i^r = \beta_1 WH_i * After + \beta_2 WH_i + \beta_3 After_i + X_i'\delta + Z_i'\tau + \varepsilon$$

where the outcome variable represents the employment outcome of young people (detailed below). *WH* is a dummy variable equal to one for those living in Western Hungary, and equal to zero for those living in Central Hungary. The dummy variable *After* switches from zero to one in the year the jobseeker registered, if it happened in the year the YG was implemented. In other words, it is equal to zero in 2014, while it is equal to one in 2015. The vector X represents a host of variables describing each young person's background, including gender, the level of education, age, work history in the previous two years, and the type of occupation the person is looking for. The vector Z stands for a set of district-level (local labour office level) fixed effects. Thus, we ask whether the outcomes of young people who registered in Western Hungary *after* the implementation of YG improved more (relative to the previous year) than the outcomes of young people living in Central Hungary (where the YG was not yet implemented). Thus, we measure to what extent the greater likelihood of participating in an active measure (thanks to the YG) might have improved young people's outcomes. In the specification above, we use inflow samples from the first nine months of each year (2014, 2015), since the implementation of the YG in Central Hungary started in October 2015.

The outcomes we use are cumulated outcomes over three six-month intervals following registration, starting half a year after the month of registering as a jobseeker. In other words, we looked at cumulative outcomes in months 7-1, 13-18, and 19-24 in order to estimate the short- and medium-term effects of the YG. As a robustness check, we also estimated employment probabilities for the sixth, 12th, and 18th months following entry into the unemployment register (these results are displayed in the Appendix, *Table A14-A15*). We did not estimate outcomes for the first six months following registration as unemployed, since this is the period when participation in an ALMP is likely to have started. We measured the total number of days worked and the total number of days worked in a job that paid at least 80% of the minimum wage. This effectively means that we excluded temporary and low-paid jobs, including participation in a public works programme. As further outcomes (which will be shown in the annex), we also looked at the number of days worked as an employee and the number of days worked without a wage subsidy. While the first of these outcomes is an alternative measure of having a stable job, the second is useful for understanding whether potential participation in an ALMP (within the YG programme) had a beneficial effect on the likelihood of retaining a job without financial support.

The intention-to-treat effect of the YG in the approach above is identified under the "common trends" assumption, including that there were no changes in selection on (unobservable) characteristics. First, we are unaware of (other) any policy changes during the same period that would have affected the two regions differentially. Second, given that these regions of Hungary are the most developed and have the strongest connections to Germany, Austria, and other trade partners, we can assume that the economic environment

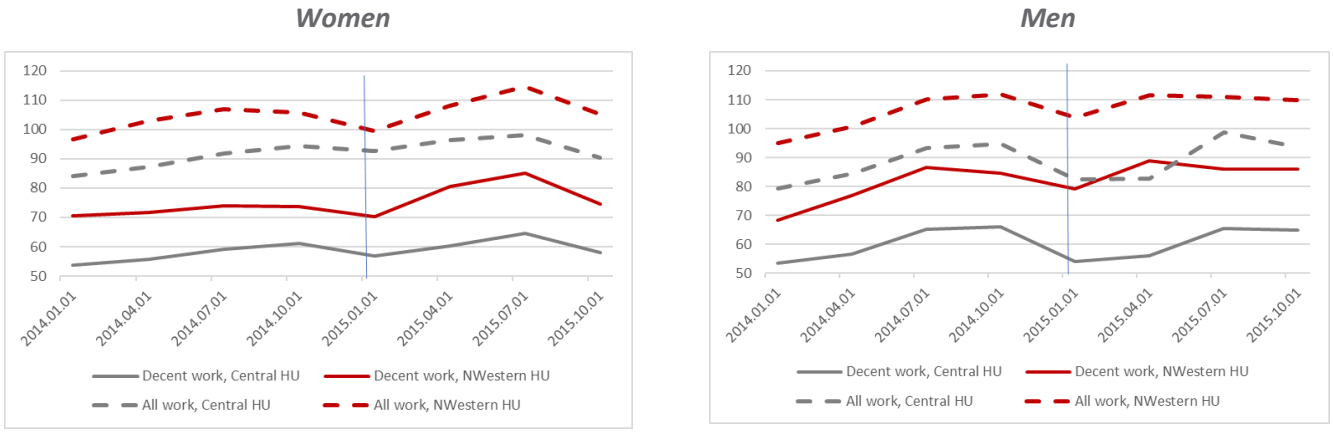
in these regions changed in a similar manner. Third, we will provide some graphical evidence on parallel trends by presenting outcomes from years prior to the period we are studying. Finally, we can also provide some evidence on selection into jobseeker status based on observable characteristics, and can thus speculate that selection based on unobservables might follow similar patterns.

It is worth mentioning some issues regarding our sample selection and timing. In the baseline case, we selected young people who were not registered as unemployed in the previous six months, but became jobseekers in 2014 or 2015. Our intention in using only new entrants (and not those who were already in the stock of unemployed) was to have clearly delimited periods. In principle, during the introductory phase of the Youth Guarantee (up to 2016), young people who had been registered for six (later: 4) months were to be given priority. However, this was not obligatory, and prior research has shown that during actual implementation, it was respected for a short period only. This uncertainty means that there might have been contamination of the different groups if we had not restricted the sample to those who were newly registered. More specifically, those who entered the registry in the second half of 2014 in Western Hungary might have entered a measure within the YG programme after 4-6 months (at the very beginning of implementation). A similar argument can be made for young people entering the register in Central Hungary in the second half of 2015. Thus, in a robustness analysis, we will limit the sample to the first half of each year.

7.1. Diff-in-Diff results

We first present the basic results in *Table 7-1* and *Figure 7-1*. The results suggest that the implementation of the Youth Guarantee had very limited effects. While the employment trends seem very similar before the implementation of the YG, and the increase in number of days worked was slightly higher in Western Hungary (which includes Central and Western Transdanubia), these trends do not appear to be much more pronounced there than in Central Hungary. However, we do see marked differences in the trends for women and men. While for women, the increase in the number of days worked did not follow a pattern that corresponds closely to the implementation of the YG; for men, the differences in the number of days worked between Central and Western Hungary rose quickly after the initial implementation of the YG.

Figure 7-1: Number of days worked in months 7-12 after registration as jobseeker, by quarter of registration and gender



Note: We include those who registered in a given calendar quarter and in a given region. The outcome is the total number of days worked in months 7-12 after registration as jobseeker. All work: all employment relationships (including public work); decent work: only those employment relationships in which the (monthly) earnings were above 80% of the minimum wage (and hence does not include public works).

In *Table 7.2,* we show the results of the difference-in-differences regressions. In the top panel, we display the raw comparisons (i.e., estimates without any control variables). Overall, we do not see an increase in the number of days worked, but we do see an increase in the number of days worked in the primary labour market above minimum wage. This corresponds to a roughly 7% increase in the number of days worked (as the baseline value was 70 days worked). However, this trend was limited to the period 7-12 months following registration as a jobseeker. This finding is consistent with the observation that some youth entered active measures instead of public works thanks to the YG, which led to an increase in the number of days worked for decent pay. This result is supported by the additional evidence (in the annex) that the number of days worked in unsubsidised jobs was not higher due to the introduction of the YG. In the bottom panel, we show the effects estimated while applying the full set of controls. They indicate that the estimated parameters changed only slightly, and that the effect of the introduction of the YG was small, and was only marginally significant.

Table 7-2: Main results of outcomes, cumulated over 6-24 months after entry into the unemployment register

	(1) Days of work months 7-12	(2) Days of work; month 13-18	(3) Days of work; month 18-24	(4) Days of work, above 80% MW; month 7-12	(5) Days of work, above 80% MW; month 13-18	(6) Days of work, above 80% MW; month 19-24
No controls	1.079	-1.209	1.220	4.987**	1.302	2.639
	(2.155)	(2.200)	(2.216)	(2.153)	(2.245)	(2.277)
Full controls	0.033	-2.068	0.393	3.901*	0.282	1.536
	(2.032)	(2.069)	(2.093)	(2.035)	(2.119)	(2.152)

Note: Days of work include all employment relationships, including public works, while the second outcome refers to those employment relationships in which the earnings were at least 80% of the minimum wage in a given month (effectively excluding public works). Full controls include gender, level of education, age, type of occupation, work history in the two years prior to registration, and micro-region level of development. Number of observations: 27,288. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In

Table 7-2, we also display results showing that these positive changes occurred exclusively among young males, and that young women did not benefit at all from having access to the YG. For young men, there was a small, but significant increase in the number of days worked in jobs in which they earned more than the minimum wage in the period of 7-12 months after registration as jobseeker. This observation is line with our findings in Section 6.6 that the effects of the programme were somewhat weaker for women.[29]

Table 7-2: Main results of outcomes, cumulated over 6-24 months after registration as jobseeker

	(1) Days of work months 7-12	(2) Days of work; month 13-18	(3) Days of work; month 18-24	(4) Days of work, above 80% MW; month 7-12	(5) Days of work, above 80% MW; month 13-18	(6) Days of work, above 80% MW; month 19-24
Women	-2.992	-3.974	-1.796	2.114	0.339	-0.032
	(2.902)	(2.975)	(3.028)	(2.909)	(3.028)	(3.087)
Men	2.975	-0.054	2.456	5.690**	0.398	2.915
	(2.839)	(2.877)	(2.894)	(2.845)	(2.962)	(3.000)

Note: Days of work include all employment relationships, including public works, while the second outcome refers to those employment relationships in which the earnings were at least 80% of the minimum wage in a given month (effectively excluding public works). Full controls include level of education, age, type of occupation, work history in the two years prior to registration, and micro-region level of development. Number of observations: women 10,560; men 11,265. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[29] Note that when looking at the probability of working (see Annex), we can see that women had a significantly lower probability after the implementation of the YG programme.

Overall, our results imply that the introduction of the YG led to a small positive short-term effect: i.e., it helped to put young people into decent work (but this was limited to young men). However, this impact did not seem to have had a knock-on or long-term effects, in the sense that early participation in decent work (thanks to ALMP participation) did not induce further positive developments later in the careers of those who participated. When interpreting these effects, we need to keep in mind that while the introduction of the YG led to a roughly 50% increase in the proportion of young people who started an active measure within six months of their initial registration as unemployed, this proportion remained under 25% in these regions.

8. Conclusion

Our propensity score matching estimations indicate that compared to participation in the public works programme, participation in the 90-day job trial improves the employment probability of young people 6 months after they finished the programme by 4-7.7% for the whole sample, and by close to 20-37% in relative terms. The young people earn more by 0,67 times the monthly minimum wage within the six months after the programme than public works participants. However, having a higher employment probability 6 months after terminating the programme is not reflected in the total earnings of the participants, including wages from the public works programme: on average, participation in the job trial did not increase a young person's cumulated earnings within this half-year period after the end of the programme. This is because public works participants spent more time in public works even after the programme, which counterbalanced the lower wage level of public works programmes.

The difference is lower compared to the training participants: we found no difference in the probability of being employed six months after the programme, job trial participants spent 7.5-12 days more in employment status.

There is a marked difference between the treatment group and public work participants in terms of the observable characteristics. There is evidence that those who are selected into the programme were in a better labour market position, had a higher level of education, were living in a more developed region, had more work experience, and spent less time in NEET status. This suggests that the programme may not have been reaching those who needed help the most. All in all, based on observable characteristics, the individuals participating in the job trial were probably the most employable young jobseekers in terms of their observable characteristics (past work experience, education, etc.). This increases the risk of high deadweight losses, especially given that the wage subsidy was introduced at a time of strong economic growth.

Observable characteristics explained more than half of the difference in the mean of the employment rate six months after completion of the programme, and all of the raw difference in cumulative wages. At the same time, compared to participation in public works, participation in the 90-day job trial was found to have a stronger impact on the employment and wage outcomes of participants with basic education than of participants with secondary or higher education. This result suggests that giving young unemployed people with less education a higher priority in the programme could increase the average treatment effect, and decrease the deadweight loss of the programme.

In contrast to the usual findings in the literature, we found that the effects of job trial participation on wages and employment prospects are weaker for women (when we compare the job trial programme to the public works programme). This finding might be related to our observation that being a mother lowered the probability of participating in the job trial, even after controlling for the other observable characteristics. In addition, the female participants were, on average, better educated and had a more favourable labour market position than the male participants. Consequently, the enrolled women were more engaged in the labour market, and their labour supply was not expected to be more elastic than that of the male participants. Moreover, we showed that participation in the programme did not have a causal effect on maternity six and 12 months after completing the programme, which is a potential explanation for the positive employment effect on women.

We found that firms, which hired young persons with YG wage subsidy increased their size more than similar firms without YG subsidized hires. However, the increase in the workforce due to the subsidized young employee lags behind the number of subsidized hires at the firm, suggesting the presence of deadweight losses.

We also provide a preliminary assessment of the early phase of the Youth Guarantee scheme as a whole to complement the analysis above, as such an assessment can provide us with a better understanding of the policy context. For this analysis, we exploit the fact that in the Central Hungarian region, the programme started nine months later, and apply a difference-in-differences framework to estimate the effect of the Youth Guarantee on the outcomes of eligible jobseekers. The diff-in-diff specifications indicate that in the short run, the implementation of the YG as a whole led to an improvement in the outcomes of young people by giving them access to decent jobs, but this positive impact was concentrated on young males. Moreover, our results show that the initial phase of the YG programme did not lead to knock-on (or long-term effects).

Bibliography

Almeida, R., Orr, L., & Robalino, D. (2014). Wage subsidies in developing countries as a tool to build human capital: Design and implementation issues. *IZA Journal of Labor Policy, 3(1)*, 12.

Anghel, Liliana Luminita (2017): Report on PES Implementation of the Youth Guarantee, ICON-INSTITUT Public Sector GmbH, 2017

Azmat, G., Güell, M., & Manning, A. (2006). Gender gaps in unemployment rates in OECD countries. Journal of Labor Economics, 24(1), 1-37.

Bell, S. H., & Orr, L. L. (2002). Screening (and creaming?) applicants to job training programs: The AFDC homemaker–home health aide demonstrations. *Labour Economics, 9(2)*, 279–301. https://doi.org/10.1016/S0927-5371(02)00006-4

Bentolila, S., Dolado, J. J., & Jimeno, J. F. (2019). *Dual Labour Markets Revisited*. Bergemann, A., & Berg, G. J. V. D. (2008). Active Labor Market Policy Effects for Women in Europe—A Survey. *Annals of Economics and Statistics / Annales d'Économie et de Statistique, 91/92*, 385–408.

Blundell , R., & Dias, M. C. (2009). Alternative approaches to evaluation in empirical microeconomics. *Journal of Human Resources, 44(3)*, 565-640.

Bördős, K., Csillag, M., & Scharle, Á. (2016). *What works in wage subsidies for young people: A review of issues, theory, policies and evidence*. ILO Employment Working Paper No. 199.

Bratti, M., Ghirelli, C., Havari, E., & Santangelo, G. (2018). Vocational Training for Unemployed Youth in Latvia: Evidence from a Regression Discontinuity Design. *IZA Discussion Papers 11870*, Institute of Labor Economics (IZA).

Cahuc, P., Carcillo, S., & Le Barbanchon, T. (2019). The effectiveness of hiring credits. *The Review of Economic Studies, 86(2)*, 593-626.

Caliendo, M., & Kopeinig, S. (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys, 22(1)*, 31–72. https://doi.org/10.1111/j.1467-6419.2007.00527.x

Caliendo, M., & Schmidl, R. (2016). Youth unemployment and active labor market policies in Europe. *IZA Journal of Labor Policy, 5(1)*.

Card, D., Kluve, J., & Weber, A. (2010). Active Labour Market Policy Evaluations: A Meta-Analysis*. *The Economic Journal, 120(548)*, F452–F477. https://doi.org/10.1111/j.1468-0297.2010.02387.x

Costa Dias, M., Ichimura, H., & van den Berg, G. J. (2013). Treatment Evaluation with Selective Participation and Ineligibles. *Journal of the American Statistical Association, 108(502)*, 441–455. https://doi.org/10.1080/01621459.2013.795447

Cseres-Gergely, Zs.–Molnár, Gy. (2015): Labour market situation following exit from public works. In: Fazekas, K.–Varga, J. (eds.): The Hungarian Labour Market in 2015. IE HAS, Budapest, pp. 148–159.

Csillag, Márton: Unemployment among labour market entrants (2019)in: Hungarian Labour Market, eds: Fazekas, K. Csillag, M., Hermann, Z. and Scharle, Á Institute of Economics, Centre for Economic and Regional Studies, https://EconPapers.repec.org/RePEc:has:lmbook:2019.

Csorba, J., & Nagy, Z. É. (2012). The evaluation of training, wage subsidy and public works programs in Hungary. In G. Kézdi (Ed.), *The Hungarian Labour Market, 2012* (Vols. 1586-460X, pp. 96-122.).

Czombos, I., Bagó, A., Béres, A., Imre, B. O., Kelemen, B., Kugler, A., Nagy, T., Soha, B., Somkuti, M., Szabó, N., & Tolonics-Magony, M. (2018). *Evaluating the Youth Employment Initiative in Hungary*. Equinox Consulting. Manuscript. [In Hungarian].

Evers, M., De Mooij, R., & Van Vuuren, D. (2008). The wage elasticity of labour supply: A synthesis of empirical estimates. *De Economist, 156(1)*, 25–43.

Galasi, P., Hudomiet, P, Kézdi, G, & Nagy, Gy. (2007). *Evaluating the effectiveness of Active Labour Market Policies in Hungary*. Manuscript, Budapest. [In Hungarian]

Frölich, M. (2004). Finite-Sample Properties of Propensity-Score Matching and Weighting Estimators. *Review of Economics and Statistics, 86(1)*, 77–90. https://doi.org/10.1162/003465304323023697

Huber, M., Lechner, M., & Steinmayr, A. (2015). Radius matching on the propensity score with bias adjustment: Tuning parameters and finite sample behaviour. *Empirical Economics, 49(1)*, 1–31. https://doi.org/10.1007/s00181-014-0847-1

Imbens, G. W. (2004). Nonparametric estimation of average treatment effects under exogeneity: A review. *Review of Economics and statistics, 86(1)*, 4-29.

Jahn, E. J., & Rosholm, M. (2018). *The Cyclicality of the Stepping-Stone Effect of Temporary Agency Employment*. IZA Discussion Paper No. 11377.

Jann, B. (2019). *Influence functions for linear regression (with an application to regression adjustment)*. Retrieved on 2020.10.20. from https://ideas.repec.org/p/bss/wpaper/32.html

Jann, B. (2020). I*nfluence functions continued. A framework for estimating standard errors in reweighting, matching, and regression adjustment*. Retrieved on 2020.10.21. from https://ideas.repec.org/p/bss/wpaper/35.html

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics, 17(6)*, 904–918.

Kluve, J., Puerto, S., Robalino, D., Rother, F., Weidenkaff, F., Stoeterau, J., Tien, B., Witte, M., Kluve, J., Puerto, S., Robalino, D., Rother, F., Weidenkaff, F., Stoeterau, J., Tien, B., & Witte, M. (2014). *Interventions to improve labour market outcomes of youth: A systematic review of training, entrepreneurship promotion, employment services, mentoring, and subsidized employment interventions*. http://campbellcollaboration.org/lib/project/306/

Knight, G. M. (2002). *Evaluation of the Australian Wage Subsidy Special Youth Employment and Training Program*, SYETP [School of Economics and Political Science, Faculty of Economics and Business, University of Sydney]. http://www.psi.org.uk/docs/2004/genevievePhDfull.pdf

Köllő, J., Scharle, Á. (2012): The impact of the expansion of public works programs on long-term unemployment. In: Fazekas, K.–Kézdi, G. (eds.): The Hungarian Labour Market, 2012 Research Centre for Economic and Regional Studies and Hungarian Academy of Sciences. National Employment Non-profit Public Company Ltd, Budapest, Budapest, pp. 123–137

Kunze, A., & Liu, X. (2019). *Universal Childcare for the Youngest and the Maternal Labour Supply*. NHH Dept. of Economics Discussion Paper.

Kunze, A. (2016). *Parental leave and maternal labor supply*. IZA World of Labor.

Larsson, L. (2003). Evaluation of Swedish youth labor market programs. *Journal of Human Resources*, *38*(4), 891-927.

Lechner, M., & Smith, J. (2007). What is the value added by caseworkers? *Labour Economics, 14(2)*, 135–151. https://doi.org/10.1016/j.labeco.2004.12.002

Lechner, M., & Wiehler, S. (2007). Kids or Courses? Gender Differences in the Effects of Active Labor Market Policies. *Journal of Population Economics*. 10.2139/ssrn.981323

Lechner, M., & Wunsch, C. (2013). Sensitivity of matching-based program evaluations to the availability of control variables. *Labour Economics, 21*, 111-121.

Lombardi, Stefano & Skans, Oskar & Vikström, Johan. (2018). Targeted wage subsidies and firm performance. Labour Economics. 53. 10.1016/j.labeco.2018.04.002.

Molnár György (2019): Youth in public employment, with particular emphasis on early secondary school leavers, in: Hungarian Labour Market, eds: Fazekas, K. Csillag, M., Hermann, Z. and Scharle, Á Institute of Economics, Centre for Economic and Regional Studies, https://EconPapers.repec.org/RePEc:has:lmbook:2019.

Neumark, D., & Grijalva, D. (2017). The Employment Effects of State Hiring Credits. *ILR Review, 70(5)*, 1111–1145.

Nilsen, Oivind Anti and Reiso, Katrine Holm, Scarring Effects of Unemployment. IZA Discussion Paper No. 6198, Available at SSRN: https://ssrn.com/abstract=1976529

O'Leary, C. J. (1998). *Evaluating the Effectiveness of Active Labor Programs in Hungary*. Upjohn Institute Technical Report No. 98-013.

Oskamp, F., & Snower, D. (2006). *The Effect of Low-Wage Subsidies on Skills and Employment* [Kiel Working Paper]. Kiel Institute for the World Economy. http://econpapers.repec.org/paper/kiekieliw/1292.htm

Rosenbaum, P. R., & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika, 70(1*), 41–55. https://doi.org/10.1093/biomet/70.1.41

Richardson, J. (1998). *Do Wage Subsidies Enhance Employability? Evidence from Australian Youth*. Discussion Paper / Centre for Economic Performance 387. London: Centre for Economic Performance, London School of Economics and Political Science.

Szabó-Morvai Ágnes–Balás Gábor–Remete Zsuzsa–Grócz Márton–Hollósy Bálint (2015). (In Hungarian) Analysis of the Youth Employment Initiative. Hétfa Reseach Institute

Svraka András (2018). *The Effect of Labour Cost Reduction on Employment of Vulnerable Groups - Evaluation of the Hungarian Job Protection Act*. Munich Personal RePEc Archive Paper No. 88234.

Tóth, Endre (2019): K4.1 What are the consequences of young people entering the labour market during an economic crisis? International outlook in: Hungarian Labour Market, eds: Fazekas, K. Csillag, M., Hermann, Z. and Scharle, Á Institute of Economics, Centre for Economic and Regional Studies, https://EconPapers.repec.org/RePEc:has:lmbook:2019.

Webb, M., Sweetman, A., & Warman, C. (2012). *How targeted is targeted tax relief? Evidence from the unemployment insurance youth hires program*. Queen's Economics Department Working Paper. http://www.econstor.eu/handle/10419/67761

Appendix

Figure A1: Distribution of YG wage cost subsidy participants by the length of the programme

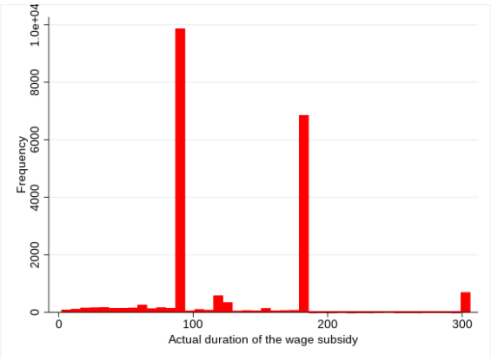
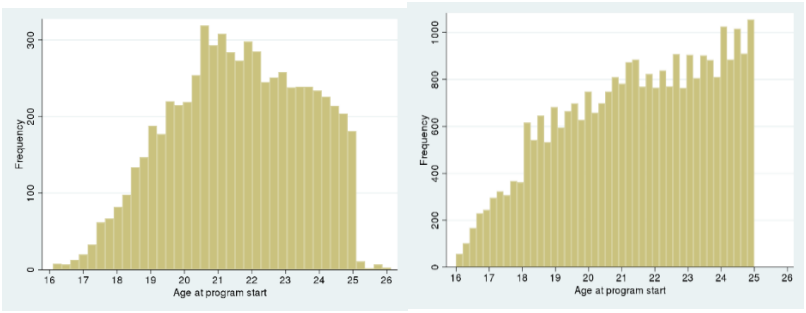


Figure A2: Age distribution of the treatment and control groups: participants in

a) 90-day job trial *b) public works programme*



c) training, by the age of the participants.

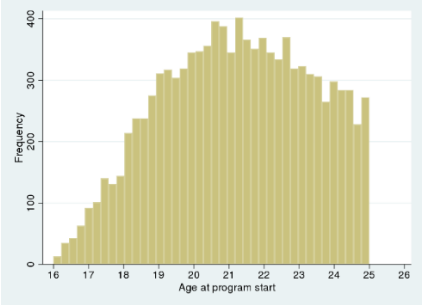


Figure A3: Outcomes after 6 months in the treatment group and the two control groups (raw)

Table A1: Descriptive statistics: job trial and public work programme participants

	Mean			T-test	
Variable	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 help)	0.095	0.105	-3.200	-1.820	0.069
District, cat.4 (need complex help)	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	0.076	0.139	-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 A2: Descriptive statistics: job trial and training programme participants

	Mean			T-test	
Variable	Treated	Control	%bias	t	p>|t|
Male	0.510	0.568	-11.50	-5.590	0.000
Age	21.26	20.83	21.60	10.45	0.000
Education: elementary	0.282	0.489	-43.50	-20.89	0.000
Education: secondary	0.680	0.503	36.70	17.69	0.000
Education: tertiary	0.038	0.008	19.70	10.48	0.000
medical history					
Medical drug expenses in last year (HUF)	2416	2321	2	0.960	0.335
Inpatient expenses in last year (HUF)	7406	10579	-7	-3.310	0.001
Days spent in hospital last year	0.361	0.557	-6.7	-3.100	0.002
Outpatient expenses last year (HUF)	7352	7794	-3	-1.470	0.142
Visits to the General Practitioner	4.186	5.193	-19	-9.010	0.000
working history					
# of months in empl.	14.46	10.43	29.20	14.49	0.000
# of months in empl. in last 2 years	8.235	5.752	33.70	16.70	0.000
# of months in public work	1.835	2.254	-8.100	-3.920	0.000
# of months in public work in last 2 years	1.317	1.748	-11.20	-5.410	0.000
# of months as NEET, excl. parental leave	12.90	14.96	-12.30	-5.960	0.000
# of months as NEET in last 2 years, excl. parental leave	6.341	8.176	-26.80	-13	0.000
# of months with child benefit	0.997	2.049	-14	-6.540	0.000
# of months with child benefit in last 2 years	0.497	0.878	-10.70	-5.040	0.000
Received child related transfer ever	0.033	0.063	-14.20	-6.660	0.000
Has a max 3-year-old child	0.012	0.021	-6.700	-3.180	0.001
Time since registry more than12 months	0.159	0.215	-14.40	-6.910	0.000
Time since registry less than 4 months	0.625	0.506	24.10	11.69	0.000
Number of registry spells	1.815	1.899	-6.700	-3.240	0.001
geographic characteristics					
District, cat.2 (preferential)	0.275	0.272	0.600	0.300	0.763
District, cat.3 (need help)	0.095	0.074	7.400	3.640	0.000
District, cat.4 (need complex help)	0.225	0.246	-4.900	-2.360	0.018
PES in county capital	0.270	0.294	-5.500	-2.670	0.008
settlement type					
County capital	0.162	0.167	-1.500	-0.710	0.476
Town (in 2008)	0.409	0.375	6.900	3.310	0.001
Village (>10K cap)	0.001	0.000	1.300	0.650	0.519
Village (5-10K cap)	0.024	0.022	1.200	0.580	0.562
Village (2-5K cap)	0.162	0.164	-0.700	-0.350	0.730
Village (1-2K cap)	0.125	0.154	-8.300	-3.960	0.000
Village (<1K cap)	0.118	0.116	0.600	0.280	0.783

Table A3: Descriptive statistics: those with and without competence test score in the sample of job trial or public work programme participants

Variable	Mean Without Score	With Score	%bias	T-test t	p>|t|
Male	0.638	0.479	32.50	27.25	0.000
Age	20.97	21.18	-9.700	-7.980	0.000
Education: elementary	0.807	0.294	120.4	101.5	0.000
Education: secondary	0.182	0.675	-114.9	-97.09	0.000
Education: tertiary	0.011	0.031	-13.90	-11.86	0.000
medical history					
Medical drug expenses in last year (HUF)	1546	2481	-22.50	-18.98	0.000
Inpatient expenses in last year (HUF)	10916	9444	2.400	2.050	0.040
Days spent in hospital last year	0.662	0.532	2.600	2.160	0.030
Outpatient expenses last year (HUF)	5906	7355	-9.400	-7.890	0.000
Visits to the General Practitioner	4.283	4.861	-10.60	-8.910	0.000
working history					
# of months in empl.	7.935	11.26	-27.60	-23.22	0.000
# of months in empl. in last 2 years	3.520	6.023	-40.20	-34.15	0.000
# of months in public work	6.034	3.640	32	26.59	0.000
# of months in public work in last 2 years	4.649	3.120	25.80	21.54	0.000
# of months as NEET, excl. parental leave	26.80	9.949	87.50	71.53	0.000
# of months as NEET in last 2 years, excl. parental leave	9.521	5.714	57	47.48	0.000
# of months with child benefit	4.744	1.744	27.30	22.42	0.000
# of months with child benefit in last 2 years	1.776	0.908	17.60	14.62	0.000
Received child related transfer ever	0.141	0.057	28.30	23.39	0.000
Has a max 3-year-old child	0.045	0.023	12.30	10.19	0.000
Time since registry more than12 months	0.372	0.242	28.40	23.67	0.000
Time since registry less than 4 months	0.416	0.529	-22.70	-19.03	0.000
Number of registry spells	2.267	1.650	42.30	35.08	0.000
geographic characteristics					
District, cat.2 (preferential)	0.289	0.294	-1.200	-1.040	0.300
District, cat.3 (need help)	0.103	0.104	-0.300	-0.270	0.790
District, cat.4 (need complex help)	0.336	0.269	14.60	12.20	0.000
PES in county capital	0.191	0.222	-7.600	-6.390	0.000
settlement type					
County capital	0.078	0.110	-11.30	-9.410	0.000
Town (in 2008)	0.323	0.352	-6.200	-5.140	0.000
Village (>10K cap)	0.000	0.000	0.600	0.490	0.625
Village (5-10K cap)	0.026	0.022	2.600	2.110	0.035
Village (2-5K cap)	0.193	0.179	3.400	2.850	0.004
Village (1-2K cap)	0.180	0.162	4.700	3.890	0.000
Village (<1K cap)	0.199	0.173	6.700	5.520	0.000

Table A4: Job trial and public work programme participants with >=0.8 propensity score

	Mean			T-test	
Variable	Treated	Control	%bias	t	p>|t|
Male	0.448	0.471	4.700	0.500	0.616
Age	21.29	21.50	11	1.200	0.229
Education: elementary	0.128	0.157	8.300	0.920	0.356
Education: secondary	0.845	0.818	-7.200	-0.790	0.431
Education: tertiary	0.027	0.025	-1.400	-0.150	0.884
medical history					
Medical drug expenses in last year (HUF)	2577	2062	-13	-1.160	0.246
Inpatient expenses in last year (HUF)	5957	5918	-0.100	-0.010	0.991
Days spent in hospital last year	0.260	0.240	-1.300	-0.120	0.902
Outpatient expenses last year (HUF)	7002	8412	11.10	1.220	0.224
Visits to the General Practitioner	4.562	4.859	5.800	0.660	0.510
working history					
# of months in empl.	20.46	18.13	-14.30	-1.520	0.127
# of months in empl. in last 2 years	11.83	10.25	-18.90	-2.020	0.043
# of months in public work	1.308	0.868	-11.80	-1.040	0.298
# of months in public work in last 2 years	0.955	0.636	-10.80	-0.990	0.322
# of months as NEET, excl. parental leave	8.703	10.86	17	1.810	0.070
# of months as NEET in last 2 years, excl. parental leave	4.404	5.223	14.10	1.520	0.128
# of months with child benefit	0.486	0.455	-0.800	-0.080	0.935
# of months with child benefit in last 2 years	0.254	0.157	-4.800	-0.460	0.643
Received child related transfer ever	0.015	0.017	1.300	0.140	0.885
Has a max 3-year-old child	0.005	0.008	4.100	0.490	0.624
Time since registry more than12 months	0.106	0.074	-11.20	-1.110	0.265
Time since registry less than 4 months	0.692	0.628	-13.50	-1.470	0.143
Number of registry spells	1.705	1.868	12.40	1.510	0.132
geographic characteristics					
District, cat.2 (preferential)	0.295	0.273	-5	-0.530	0.595
District, cat.3 (need help)	0.100	0.149	14.70	1.690	0.090
District, cat.4 (need complex help)	0.190	0.240	12.20	1.350	0.177
PES in county capital	0.245	0.215	-7.200	-0.760	0.450
Ratio of public workers	0.050	0.071	41.90	4.580	0.000
Travelling distance from PES (min.)	513.0	557.0	6.700	0.770	0.440
settlement type					
County capital	0.153	0.182	7.600	0.840	0.401
Town (in 2008)	0.440	0.455	2.900	0.310	0.754
Village (>10K cap)	0.002	0	-5.800	-0.450	0.655
Village (5-10K cap)	0.028	0.008	-14.60	-1.280	0.200
Village (2-5K cap)	0.142	0.099	-13.10	-1.310	0.191
Village (1-2K cap)	0.115	0.132	5.300	0.580	0.559
Village (<1K cap)	0.121	0.124	0.800	0.090	0.930

Table A5: Job trial and public work programme participants with <=0.2 propensity score

	Mean			T-test	
Variable	Treatment	Control	%bias	t	p>|t|
Male	0.532	0.569	7.500	2.580	0.010
Age	20.66	20.90	10.60	3.570	0.000
Education: elementary	0.667	0.662	-1	-0.350	0.725
Education: secondary	0.319	0.324	1.100	0.370	0.712
Education: tertiary	0.014	0.013	-0.100	-0.0500	0.959
medical history					
Medical drug expenses in last year (HUF)	2116	1860	-5.500	-2.070	0.039
Inpatient expenses in last year (HUF)	15871	11214	-5.200	-2.380	0.017
Days spent in hospital last year	0.602	0.661	1.200	0.380	0.700
Outpatient expenses last year (HUF)	7600	6255	-9.300	-3.060	0.002
Visits to the General Practitioner	4.507	4.633	2.300	0.780	0.437
working history					
# of months in empl.	8.920	8.106	-7.400	-2.550	0.011
# of months in empl. in last 2 years	4.719	3.797	-16	-5.730	0.000
# of months in public work	3.701	5.440	21.90	7.590	0.000
# of months in public work in last 2 years	2.371	4.384	35.50	11.21	0.000
# of months as NEET, excl. parental leave	16.44	19.96	17.30	5.580	0.000
# of months as NEET in last 2 years, excl. parental leave	8.306	8.053	-3.400	-1.220	0.221
# of months with child benefit	4.084	4.470	3	1.030	0.303
# of months with child benefit in last 2 years	1.913	1.860	-0.900	-0.320	0.751
Received child related transfer ever	0.123	0.133	3.100	1.060	0.289
Has a max 3-year-old child	0.056	0.048	-3.500	-1.260	0.207
Time since registry more than12 months	0.221	0.335	25.60	8.370	0.000
Time since registry less than 4 months	0.598	0.449	-30.10	-10.30	0.000
Number of registry spells	1.931	1.961	2.100	0.680	0.498
geographic characteristics					
District, cat.2 (preferential)	0.314	0.303	-2.400	-0.850	0.397
District, cat.3 (need help)	0.113	0.107	-2	-0.700	0.484
District, cat. (4 need complex help)	0.325	0.334	2	0.690	0.490
PES in county capital	0.171	0.169	-0.400	-0.140	0.887
Ratio of public workers	0.120	0.149	26.50	8.670	0.000
Travelling distance from PES (min.)	660.1	762.7	16.30	5.570	0.000
settlement type					
County capital	0.089	0.064	-9.700	-3.600	0.000
Town (in 2008)	0.383	0.317	-13.90	-4.890	0.000
Village (>10K cap)	0.000	0.000	1.600	0.410	0.684
Village (5-10K cap)	0.021	0.024	2.100	0.700	0.485
Village (2-5K cap)	0.176	0.194	4.500	1.520	0.128
Village (1-2K cap)	0.159	0.190	8.100	2.710	0.007
Village (<1K cap)	0.212	0.172	10.30	3.430	0.001

Table A6: Results for Employment (above 80% of mw), robustness check

	(1) NN1	(2) NN5	(3) Epan. Kernel with CV on outcome
Control: Public work participants	0.0300	0.0492**	0.0520***
	(0.0236)	(0.0193)	(0.0171)
n_treat	3569	3569	3566
n_used_cont	1848	5592	18964
Control: Training participants	0.149***	0.135***	0.136***
	(0.0120)	(0.0101)	(0.00947)
n_treat	3758	3758	3758
n_used_cont	2825	8280	23496

Robustness test for main results with different matching methods. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Results for Employment (employee), robustness check

	(1) NN1	(2) NN5	(3) Epan. Kernel with CV on outcome
Control: Public work participants	0.0499**	0.0719***	0.0712***
	(0.0250)	(0.0199)	(0.0192)
n_treat	3569	3569	3556
n_used_cont	1848	5592	18954
Control: Training participants	0.188***	0.171***	0.173***
	(0.0126)	(0.0105)	(0.00976)
n_treat	3758	3758	3758
n_used_cont	2825	8280	23496

Robustness test for main results with different matching methods. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Cumulative wage, incl. public work, robustness check

	(1) NN1	(2) NN5	(3) Epan. Kernel with CV on outcome
Control: Public work participants	-0.167	-0.134	-0.149
	(0.137)	(0.130)	(0.117)
n_treat	3569	3569	3552
n_used_cont	1848	5592	18877
Control: Training participants	0.283***	0.206***	0.182***
	(0.0800)	(0.0672)	(0.0624)
n_treat	3758	3758	3758
n_used_cont	2825	8280	23537

Robustness test for main results with different matching methods. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Cumulative wage, excl. public work, robustness check

	(1) NN1	(2) NN5	(3) Epan. Kernel with CV on outcome
Control: Public work participants	0.471*** (0.142)	0.515*** (0.137)	0.543*** (0.123)
n_treat	3569	3569	3552
n_used_cont	1848	5592	18877
Control: Training participants	1.261*** (0.0839)	1.139*** (0.0710)	1.137*** (0.0661)
n_treat	3758	3758	3758
n_used_cont	2825	8280	23500

Robustness test for main results with different matching methods. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Cumulative days worked, robustness check

	(1) NN1	(2) NN5	(3) Epan. Kernel with CV on outcome
Control: Public work participants	16.31*** (3.499)	17.98*** (2.998)	18.83*** (2.875)
n_treat	3569	3569	3551
n_used_cont	1848	5592	18854
Control: Training participants	35.55*** (1.989)	33.31*** (1.655)	33.64*** (1.541)
n_treat	3758	3758	3758
n_used_cont	2825	8280	23496

Robustness test for main results with different matching methods. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Female job trial and public work programme participants

	Mean			T-test	
Variable	Treated	Control	%bias	t	p>|t|
Age	21.36	20.87	24.70	8.110	0.000
Education: elementary	0.213	0.436	-49	-15.99	0.000
Education: secondary	0.729	0.550	38.10	12.52	0.000
Education: tertiary	0.057	0.014	23.60	8.350	0.000
medical history					
Medical drug expenses in last year (HUF)	3063	2914	3	0.980	0.325
Inpatient expenses in last year (HUF)	7778	11861	-9.300	-3.010	0.003
Days spent in hospital last year	0.364	0.618	-11.40	-3.640	0.000
Outpatient expenses last year (HUF)	9401	10311	-5.700	-1.860	0.063
Visits to the General Practitioner	4.844	5.853	-18.40	-5.980	0.000
working history					
# of months in empl.	13.28	8.918	34.10	11.52	0.000
# of months in empl. in last 2 years	7.901	5.006	40.10	13.56	0.000
# of months in public work	1.421	1.613	-4.400	-1.470	0.141
# of months in public work in last 2 years	1.141	1.389	-7	-2.330	0.020
# of months as NEET, excl. parental leave	11.21	14.04	-18	-5.930	0.000
# of months as NEET in last 2 years, excl. parental leave	5.596	7.682	-31	-10.21	0.000
# of months with child benefit	1.922	4.487	-24	-7.700	0.000
# of months with child benefit in last 2 years	0.944	1.915	-19.30	-6.210	0.000
Received child related transfer ever	0.059	0.131	-25	-8.040	0.000
Has a max 3-year-old child	0.024	0.043	-10.70	-3.460	0.001
Time since registry more than12 months	0.147	0.214	-17.60	-5.760	0.000
Time since registry less than 4 months	0.634	0.501	27.10	8.980	0.000
Number of registry spells	1.664	1.679	-1.400	-0.480	0.631
geographic characteristics					
District, cat.2 (preferential)	0.266	0.295	-6.500	-2.140	0.032
District, cat.3 (need help)	0.097	0.067	11	3.710	0.000
District, cat. (4 need complex help)	0.208	0.231	-5.700	-1.870	0.061
PES in county capital	0.287	0.294	-1.600	-0.530	0.594
Ratio of public workers	0.075	0.080	-6.400	-2.130	0.033
Travelling distance from PES (min.)	510.0	563.8	-9	-2.920	0.004
settlement type					
County capital	0.183	0.162	5.600	1.850	0.065
Town (in 2008)	0.409	0.380	5.800	1.910	0.056
Village (>10K cap)	0.001	0.001	-2	-0.620	0.534
Village (5-10K cap)	0.025	0.021	2.400	0.800	0.422
Village (2-5K cap)	0.161	0.166	-1.300	-0.420	0.673
Village (1-2K cap)	0.116	0.164	-13.90	-4.510	0.000

Table A12: Comparison of male and female 90-day job trial participants

	Mean				
	Male	Female	%bias	t	p>|t|
Age	21.16	21.37	-10.3	-3.17	0.002
# of months in empl.	15.60	13.28	15.7	4.79	0.000
# of months in public works	2.12	1.39	14.7	4.51	0.000
# of months as NEET, total	13.78	12.06	10.9	3.35	0.001
# of months as NEET, excl. parental leave	13.72	10.61	21.3	6.53	0.000
# of months with child benefits	0.11	1.87	-29	-8.99	0.000
Received child related transfer ever	0.00	0.02	-19.2	-5.93	0.000
Has a max 3-year-old child	0.35	0.21	30.2	9.24	0.000
Education secondary	0.63	0.73	-20.9	-6.4	0.000
Education tertiary	0.02	0.06	-19.5	-5.99	0.000
District, cat. 4 (needs complex help)	0.24	0.21	8.1	2.48	0.013
Time since registry more than 12 months	0.17	0.15	6.8	2.08	0.038
Time since registry less than 4 months	0.62	0.63	-3.8	-1.17	0.242
Public works share in settlement	0.08	0.07	3.5	1.08	0.281
Town	0.14	0.18	-11.6	-3.51	0.000
Village between 1-2th inhab	0.13	0.12	5.5	1.68	0.093
Village under 1th inhab	0.13	0.11	7.6	2.32	0.021
Clerical Support Workers	0.07	0.21	-40.3	-12.39	0.000
Services and Sales Workers	0.15	0.38	-53.4	-16.42	0.000
Craft and Related Trades Workers	0.17	0.01	56.6	17.21	0.000
Elementary occ	0.42	0.23	42.8	13.08	0.000

Table A13: Average treatment effects by gender

ATT	(1) Employment (above 80% of mw)	(2) Employment (employee)	(3) Cumulative wage, incl. public work	(4) Cumulative wage, excl. public work	(5) Cumulative days worked	(6) Cumulative days worked earning above 80% of min. wage	(7) Cumulative days worked as employee
				Women			
Control: Public works participants	-0.0318	0.0157	-0.536***	0.477***	20.10***	7.277**	13.61***
	(0.0350)	(0.0347)	(0.164)	(0.162)	(3.569)	(3.506)	(3.556)
n_treat	1602	1602	1602	1602	1602	1602	1602
n_used_cont	7994	7994	7994	7994	7994	7994	7994
Control: Training Participants	0.0153	0.0359*	0.555***	0.447***	14.88***	11.68***	13.61***
	(0.0211)	(0.0216)	(0.130)	(0.135)	(3.059)	(3.020)	(3.556)
n_treat	1605	1605	1605	1605	1605	1605	1605
n_used_cont	2593	2593	2593	2593	2593	2593	2593
				Men			
Control: Public works participants	0.0964***	0.125***	0.0530	0.825***	24.80***	19.42***	25.46***
	(0.0220)	(0.0243)	(0.156)	(0.161)	(3.310)	(3.282)	(3.235)
n_treat	1684	1684	1684	1684	1684	1684	1684
n_used_cont	10439	10439	10439	10439	10439	10439	10439
Control: Training	-0.0462**	-0.0173	-0.0508	-0.184	6.734**	3.963	8.570***
	(0.0192)	(0.0194)	(0.147)	(0.149)	(2.768)	(2.796)	(2.821)

Participants							
n_treat	1697	1697	1697	1697	1697	1697	1697
n_used_cont	3410	3410	3410	3410	3410	3410	3410

Epanechnikov kernel propensity score matching combined with exact matching on gender, with replacement. Bandwidth is calculated with a pair-matching-based algorithm following the proposal of Huber et al. (2015). Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Diff-in-diff estimation: probability of employment at months 6,12,18 after entry into the unemployment register

	(1) Work, incl. pw month 6	(2) Work, incl. pw month 12	(3) Work, incl. pw month 18	(4) Work, income > 80% MW; month 6	(5) Work, income > 80% MW; month 12	(6) Work, income > 80% MW; month 18
No controls	-0.012	-0.009	-0.000	-0.003	-0.007	0.004
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Full controls	-0.018	-0.015	-0.005	-0.009	-0.013	-0.001
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)

Note: Full controls include gender, level of education, age, type of occupation, work history in the two years prior to registration, and micro-region level of development. Number of observations: 27,288. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A15: Diff-in-diff estimation: probability of employment at months 6,12,18 after entry into the unemployment register, by gender

	(1) Work, incl. pw month 6 i	(2) Work, incl. pw , month 12	(3) Work, incl. pw , month 18	(4) Work, income > 80% MW; month 6	(5) Work, income > 80% MW; month 12	(6) Work, income > 80% MW; month 18
Women	-0.049**	-0.046**	-0.012	-0.037*	-0.042**	-0.004
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Full controls	0.011	0.016	0.002	0.018	0.015	0.002
	(0.019)	(0.018)	(0.018)	(0.019)	(0.019)	(0.018)

Note: Full controls include level of education, age, type of occupation, work history in the two years prior to registration, and micro-region level of development. Number of observations: women 10,560; men 11,265. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Iceland Liechtenstein Norway grants

Norway grants

Youth employment partnerSHIP

Authors:

Márton Csillag - Budapest Institute for Policy Analysis, Centre for Economic and Regional Studies

Judit Krekó - Budapest Institute for Policy Analysis, Centre for Economic and Regional Studies

Balázs Munkácsy - Budapest Institute for Policy Analysis

Ágota Scharle - Budapest Institute for Policy Analysis

Budapest Institute for Policy Analysis

Address 1074 Budapest Dohány utca 84.
www.budapestinstitute.eu