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IZA DP No. 15071

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ISSN: 2365-9793

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ABSTRACT

To Work or Not to Work? Effects of Temporary Public Employment on Future Employment and Benefits*

We evaluate a temporary public sector employment program targeted at individuals with weak labor market attachment, applying dynamic inverse probability weighting to account for dynamic selection. We show that the program is successful in increasing employment and reducing social assistance. However, being at a regular workplace seems crucial: we find negative employment effects for participants employed at a workplace created especially for the purpose. The decrease in social assistance is to some extent countered by an increase in the share receiving unemployment insurance benefits, indicating that municipalities are able to shift costs from the local to the central budget.

JEL Classification: H75, I38, J45

Keywords: public sector employment programs, social assistance, cost-shifting, dynamic inverse probability weighting

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* We are grateful for comments and suggestions from seminar participants at IFAU, Ratio, The Department of Economics and the Urban Lab at Uppsala University, as well from conference participants at the virtual EALE 2021 conference. The comments from Martin Lundin, Simen Markussen, Lukas Mergele, Johan Vikström, Michael Rosholm and Olof Åslund are also highly appreciated. Ottosson and Vikman thank FORTE (Reg. No. 2016-07123) for financial support.

1 Introduction

How to best help individuals with a weak labor market attachment find employment is high up on the agenda for policy makers all over the world. In this paper, we investigate whether temporary public employment in the form of a public sector employment program (PSEP) is a way forward. Since PSEPs provide participants with networks and labor market experiences, these can be expected to work well for marginalized groups that would otherwise face problems finding employment.[1] However, for groups that lack previous labor market experience, and therefore rely on social assistance (SA), temporary employment may also work as a means of providing eligibility to (earnings-related) unemployment insurance (UI) benefits. In contexts where different levels of the government are responsible for financing these benefits, there are thus incentives for the level in charge of SA to use PSEPs as a means of shifting costs to the other level.[2] Although there is anecdotal evidence that such cost-shifting does occur, empirical evidence is scarce.

In this paper, we ask whether having a temporary municipal employment serves as a stepping stone to future employment or whether it mostly works as a means for the welfare office to transfer individuals from SA to UI benefits. Our focus is Sweden, where municipalities finance and activate unemployed SA recipients, whereas UI benefits are paid out by central UI funds. More specifically, we evaluate a PSEP in the city of Stockholm, targeted at unemployed SA recipients and other individuals at risk of becoming long-term unemployed. Our paper thus adds to our knowledge of what works for this particular group (see, for instance, Bolvig et al., 2003; Cockx and

[1] PSEPs targeted at unemployed individuals typically do not fare well in evaluations; at best, they are shown to have negligible employment effects; at worst, they are found to hurt participants' labor market prospects (Card et al., 2010, 2018; Kluve, 2010). One explanation is the presence of lock-in effects that outweigh any potential positive program effects.

[2] Luigjes and Vandenbroucke (2020) discuss cost-shifting or "dumping" as one of two potential types of institutional moral hazard, the other being ineffective activation, which may occur when one governmental level is in charge of activating unemployed individuals while another is responsible for paying their benefits.

Ridder, 2001; Heinesen et al., 2013; Markussen and Røed, 2016; Thomsen and Walter, 2010), as well as broadens our understanding of the role played by institutional setups in terms of determining how individuals are moved between different benefit schemes (see, for instance, Bonoli and Trein, 2016; Schmidt and Sevak, 2004). A specific feature of the program that we study is that we can distinguish between participation at regular and non-regular workplaces. The findings of this paper can therefore improve our understanding in how important networks and experiences from a regular workplace are, as opposed to just having any previous labor market experience.

Earlier evidence on PSEPs for SA recipients is mixed. Whereas Danish evidence concerning subsidized public employment programs shows positive effects for SA recipients overall and non-Western immigrants in particular (Bolvig et al., 2003; Heinesen et al., 2013), results from Germany and Belgium are less promising: no effects are found for Social employments in Belgium (Cockx and Ridder, 2001), nor for Temporary extra jobs in Germany (Thomsen and Walter, 2010). In general, very few programs have turned out to be successful for this particular group. An exception is the Norwegian qualification program that combines full-time (voluntary) activation with a generous non-means-tested benefit (Markussen and Røed, 2016).

Previous evidence regarding to what extent PSEPs are used to provide participants with eligibility to UI benefits is very scarce.[3] What we do know is that decentralized job centers tend to prioritize local objectives. For example, Mergele and Weber (2020) find that decentralized job centers in Germany adjust labor market policies towards programs that are financed by the federal government and potentially generate local public goods, rather than favoring the reemployment prospects of the program par-

[3]Analyzing Canadian provinces, Gray (2003) finds that this kind of cost-shifting is fairly marginal but that there are some instances where provinces finance job-creation programs that generate insurance eligibility. See also Nieminen et al. (2021) for indicative evidence of cost-shifting in the Finnish context. Although the incentives for local governments to shift costs to the central government exist for Social employments in Belgium, Cockx and Ridder (2001) are not able to separate between, on the one hand, going from welfare to employment and, on the other hand, going from welfare to UI benefits.

ticipants. A similar conclusion is reached by Lundin and Skedinger (2006) who, by studying a Swedish pilot program, show that decentralization increased the targeting of individuals with a relatively high level of dependence on SA, which is what we should expect if local governments use their increased influence to improve municipal budgets at the expense of the central government.[4]

The temporary employment program we study is called Stockholm jobs and consists of a 6–12 months long employment in the municipal sector. We evaluate three different types of Stockholm jobs. In two (Youth employments and Other municipal employments), participants work at a regular workplace performing quality-enhancing activities that would otherwise not have been undertaken. In the third (Stockholm hosts), participants are employed at a workplace created especially for this purpose where they are engaged in outdoor cleaning. The aim of the temporary employment is to strengthen the participants' position in the labor market and thereby increase their chances of finding employment or moving on to further education. Through the employment, individuals become eligible for UI benefits, financed by central UI funds, which typically provide individuals with a higher disposable income compared to SA. Hence, in the longer run, having a Stockholm job is financially beneficial both for the individual and the municipality, even if it does not lead to regular employment.[5]

Our analysis is based on administrative data for individuals who enroll at a job center in Stockholm 2010–2015. We follow the participants for three years after the program starts and analyze the effects on subsequent employment, UI benefits and SA receipt, as well as a number of health outcomes. The data includes a rich set of individual background characteristics, such as labor market history, previous SA recipiency, education, health indicators, and time since immigration as well as an indicator

[4]The incentives for local governments to reduce caseloads are also affected by how and the extent to which costs for welfare are reimbursed by the central government. E.g. Baicker (2005), Hayashi (2019) and Kok et al. (2017) show that moving from matching to lump sum grants indeed has an effect on local governments in terms of reducing welfare caseloads.

[5]Caseworkers hence face several, potentially conflicting, objectives, similar to what is discussed in, for instance, Schmieder and Trenkle (2020).

of whether the individual took the initiative to enroll at the job center him-/herself.

In order to address the fact that treatment assignment is not random and that participants can enter the program at any time after enrollment at the job center, we apply the dynamic inverse probability weighting (IPW) approach suggested by Van den Berg and Vikström (2021). Earlier studies relying on matching strategies mostly follow Sianesi (2004, 2008) and rely on dynamic propensity score matching, thus estimating the effect of being assigned to a program at a specific time as opposed to potentially being assigned at a later time.[6] In the dynamic IPW, the group of potential controls is made up of individuals who never take part in the program, and the estimand is thus the effect of taking part in the program or not doing so. The latter is arguable the most relevant question for policy makers. The method accounts for the fact that individuals with short durations at the job center will be over-represented in the potential control group of never-treated by giving greater weights to never-treated individuals who have been registered at the job center for a long time. To the best of our knowledge, our paper is the first program evaluation utilizing dynamic IPW.

We find that the employment prospect for individuals placed at regular workplaces are improved thanks to the program. Up to two years after the temporary employment has ended, they are 7–10 percentage points more likely to be employed than their matched controls. Especially former participants of Youth employments are to a large extent employed at the same workplace or in the same sector as they were during their Stockholm job, indicating that the program provides participants with valuable skills and networks. However, the type of workplace is important; for participants in Stockholm hosts, we find negative employment effects up to two years after the program. We further find that having any type of Stockholm job reduces the likelihood of receiving SA by 50–60 percent during the two years following the employment. To some extent, this is counteracted by an increase in UI, in particular for Stockholm hosts,

[6]E.g. Heinesen et al. (2013) instead use the timing-of-events method suggested by Abbring and van den Berg (2003).

for which more than 60 percent receive UI after the program ended. In addition, we find that individuals' health outcomes improve once they start their temporary employment and that these positive effects to some extent pertain once the program ends.

Taken together, our results are promising for this group of marginalized unemployed individuals with a weak labor market attachment. Even for those that do not get employed after the program, the fact that they are now entitled to UI benefits rather than means-tested SA is likely to improve and reduce their financial stress, which the positive health effects are indicative of.

2 Institutional setting

Like many other welfare states, Sweden combines relatively generous (earnings-related) UI benefits with mandatory active labor market programs (ALMPs).[7] The formal responsibility for providing ALMPs is placed on the Swedish Public Employment Service (PES), a central governmental agency. Unemployed individuals who do not qualify for UI benefits (or for whom UI benefits are not enough to make a living or whose UI benefits have been exhausted) can apply for social assistance (SA) at the local welfare office. To be eligible, all other means, including savings and valuable assets, must be exhausted. The means-testing is performed at the household level, implying that an individual with a spouse with high earnings is not entitled to SA. The (centrally) stipulated benefit level, depends on the number and age of dependent children as well as the number of adults in the household.[8]

[7]In order to qualify for earnings-related UI benefits, individuals need to i) have been a member of a UI fund for at least one year and ii) worked at least 80 hours per month for six months during the last year. Individuals also fulfill the work requirement if they have worked at least 480 hours during six consecutive months and at least 50 hours per month during the last year. Individuals who fulfill condition ii) but not condition i), and are at least 20 years old, receive a basic unemployment benefit up to SEK 8,000 (EUR 740) per month. The UI benefits last for 300 days, with a maximum outtake of 5 days per week, corresponding to approximately 14 months of full-time unemployment and benefits. Parents with children under 18 have access to an additional 150 days.

[8]The stipulated benefit level in 2010, excluding housing costs, was SEK 3,680 (EUR 360) per month for a single person without children and SEK 10,770 (EUR 1060) for a couple with two children aged 5

Unemployed SA recipients are required to actively look for work, be registered at the PES and take part in ALMPs offered by the PES. If the PES cannot offer a suitable program, municipalities have the right to condition benefits on taking part in activation programs organized by the municipalities. This right is used by most municipalities (Forslund et al., 2019).

In Stockholm, which is the focus of this paper, unemployed SA recipients are sent by the welfare office to one of six local job centers. At the job center, the client meets a caseworker who, in collaboration with the client, sets up an action plan including information about planned activities. The client also gets assistance in putting together a CV and contacting potential employers, and advice regarding study opportunities. Unemployed individuals aged 16–29 that do not receive SA are also allowed to enroll at the job centers in order to get access to their services.

The program that we analyze in this paper is called Stockholm jobs and was introduced in 2010 as one of the activation programs provided by the job centers in the city of Stockholm (the capital of Sweden). The main component of the program is temporary employment in the municipal sector lasting 6–12 months, typically financed through different types of wage subsidies.[9] Hence, the workplace where the individual is employed faces no salary costs. The purpose of the program is to, by providing labor market experience and networks, strengthen the participants' position in the labor market and thereby increase their chances to find employment or to go on to further education. After ending a Stockholm job, former participants fulfill the work requirement for receiving UI benefits and are entitled to at least the basic unemployment benefit (if above the age of 20).[10]

and 13. In 2019, the corresponding numbers were SEK 4,080 and SEK 12,960. The municipalities are allowed to deviate both upwards and downwards from the stipulated benefit level if they can motivate these deviations.

[9]The wages subsidized are financed through the government, the labor market unit in the city of Stockholm or the local city districts.

[10]Before starting the temporary employment, participants are informed about the conditions for receiving UI benefits.

We focus on three types of Stockholm jobs that differ with respect to target group, type of workplace and employment duration.[11] Table 1 summarizes the main characteristics of these three program types. Youth employments target individuals aged 16–29 in need of extra support to find and maintain employment. Participants are employed at regular workplaces such as childcare centers, schools, nursing homes or the municipal administration. The employment lasts for six months, but may be prolonged for an additional six months if it is deemed beneficial for the individual. Other municipal employments, introduced in 2012, are in many aspects similar to the Youth employments, except for the target group (SA recipients in general) and the length of the program (typically 12 months). Stockholm hosts differ from the other two in that the temporary employments do not take place at a regular workplace. Instead, participants work outdoors, together in teams with other participants and supervisors. Their work tasks include picking litter, clearing snow and assisting tourists with directions. The employment lasts for 6 (2010–2011)/12 months (2012–2016). The program is targeted at individuals who are 25 years or older with children to care for or individuals who have been registered at the job center for at least 6 months or are considered at great risk of remaining at the job center for a long time.

Before being directed to the workplace, most participants take part in an introduction consisting of general information about the UI system, unions, how to behave at a workplace and the program itself. The introduction can also contain a 4–8 weeks long internship aiming at ensuring a good match between the participants and the workplace.[12] During this introduction, participants keep the benefits they received prior to the program (typically SA). Once at the workplace, the participants are provided with a supervisor and perform quality-enhancing activities outside the scope of the regular

[11]There are also two other types of Stockholm jobs, targeted at disabled individuals or former criminals respectively. Since these programs are very small in scale, we exclude them from our analysis.

[12]For the period we study, introductory internships have mainly been used paired with Youth employments, where the share that had an internship before entering their workplace is 89 percent. For the other two programs, the corresponding shares are 1 and 13 percent, and for these two programs, internships have mainly been used for those starting a Stockholm job after 2014.

Table 1: Description of different types of Stockholm jobs

	Youth employments	Other municipal employments	Stockholm hosts
Target group	16–29 years with poor labor market prospects	SA recipients	SA recipients ≥25 years with children or at risk of becoming long-term unemployed
Workplace	Regular workplace in the municipal sector	Regular workplace in the municipal sector	Outdoor cleaning
Employment length	6+6 months	12 months	6 months (2010–2011) 12 months (2012–2016)

Note: Other municipal employments were introduced in 2012. Since 2015, the different city districts in Stockholm are in charge of administering most Other municipal employments and also decide on specific targets groups.

tasks. This may include playing with the children in a childcare facility (but not engaging in pedagogical work), taking residents for a walk in homes for the elderly, or helping elderly individuals with simple IT-related questions in a library. They may also perform regular tasks under supervision. Since 2015, participants are allowed to study half-time simultaneously with their employment.[13] When employed, participants above the age of 19 receive a salary of at least SEK 19,000 (approximately EUR 1,800) per month.[14] During the employment, caseworkers at the job center help participants plan what to do once the Stockholm job ends.[15] This may entail going to the job center one afternoon a week to search for jobs or enrolling in education.

As opposed to the other activation programs at the job center, which are mandatory

[13]Initially, this opportunity only applied to participants in some types of Stockholm jobs and for some types of educational choices.

[14]The salary was raised from SEK 18,000 to SEK 19,000 in 2015.

[15]Since 2016, all participants are offered additional assistance for three months after the end of their employment.

for unemployed SA recipients if referred to by the caseworkers, taking up a Stockholm job is voluntary.[16] As it is uncommon that an individual declines an offer to take up a Stockholm job, selection into the program is mainly driven by the priorities made by the caseworkers. These vary somewhat across local job centers and type of Stockholm job. As a general rule, caseworkers prioritize individuals with dependent children, clients that are judged to be in need of additional assistance before they can enter the regular labor market and long-term recipients of SA. For Youth employments, motivation plays an important role, and for Stockholm hosts participants must, e.g., be able to walk long distances.[17]

Taking up a Stockholm job is financially beneficial for participants. The salary received is higher than the stipulated SA level and is not means-tested at the household level. In addition, having a job with a salary, even if it is subsidized, may offer a sense of pride and purpose for the participant.[18] If an individual does not accept an offered Stockholm job, he/she is likely to be placed in some mandatory activation program.

When the Stockholm job ends, participants returning to unemployment are entitled to UI benefits, which will provide individuals with a higher disposable income compared to if they were to receive SA.[19] In addition, they no longer need to apply for SA and undergo the means-testing and the scrutiny this implies, nor are they required to visit the job center.[20] Instead, the PES will be responsible for directing them to ALMPs. Participants who find employment will continue to receive a salary.

Most Stockholm jobs are financed via a subsidy from the government.[21] Hence, the

[16]The argument from the city of Stockholm is that participants must be motivated in order for the program to be successful. Furthermore, sending motivated participants is important in order to maintain a good relationship with the workplaces, thereby ensuring future collaboration.

[17]The information about priorities is provided by the Labor market unit in the city of Stockholm.

[18]This view was expressed by several participants when we visited their workplace.

[19]As mentioned above, in order to receive earnings-related UI benefits, individuals must have been a member of a UI fund. This is something they are informed about when entering the program.

[20]Households with many children might still need to top up with SA.

[21]In our data, the share of PSEPs financed by the government is 65 percent. This share differs between the program types: Only 46 percent of the employments in Youth employments are subsidized, while the shares for Other municipal employments and Stockholm hosts are 94 and 100 percent, respectively.

municipality will not bear the full wage cost. Given that participants are expected to perform quality-enhancing activities at the workplace, the municipality can reap the benefits of better municipal services. In the long run, it is clearly financially beneficial for the municipality to place individuals in Stockholm jobs as they either become employed or eligible for UI benefits. In both cases, costs for SA will go down and the municipality no longer needs to attend to the former recipients at the job center and welfare office.

Caseworkers at the local job center face a potential conflict of interest. On the one hand, they might want to prioritize individuals who are the most likely to benefit from the program in terms of future employment prospects. On the other hand, they may be tempted to instead prioritize clients who are hard to place with the intention of getting them off their desk: when individuals qualify for UI benefits, municipalities are relieved both from the financial burden and the responsibility for activation. In addition, as mentioned above, this is likely to also benefit the client. However, the intention of the job center to only send motivated individuals to the workplaces can be expected to counteract these incentives.

3 Data and sample selection

We combine administrative data from several different sources: the city of Stockholm, Statistics Sweden, the Public Employment Service (PES), the Swedish Unemployment Insurance Board (IAF) and the National Board of Health and Welfare (NBHW). The data from the city of Stockholm covers the period from January 2010 to June 2019 and includes information about the start and end date of each spell of enrollment at the job center, as well as the name, type, start and, in most cases, the end date of each activity an individual has participated in. In addition, the data includes information regarding

whether the individual him-/herself took the initiative to enroll at the job center.[22] The data from Statistics Sweden covers the years 2008–2019 and includes yearly socio-demographic background characteristics such as age, gender, number and age of children and marital status, region of origin, year of immigration as well as information about the highest attained education level. We also have monthly information about earnings, workplace and sector. The PES data includes information about enrollments at PES and program participation for the period 1991–2019. The data from IAF includes all UI payments between 2008 and 2019. From NBHW, we have access to (monthly) information about medical prescriptions, hospitalizations and SA payments for the period 2008–2019.

We define our study population as all individuals who enroll at a job center in Stockholm at some point between January 1, 2010, and December 31, 2015, and aged 18–61 at the time of enrollment.[23] Since the different types of Stockholm jobs have different target groups, we also restrict our estimation samples accordingly. This implies that when estimating the effects for Youth employments, the sample is restricted to those younger than 30. When it comes to Other municipal employments, the sample is restricted to those with a start date in May 2012 or later (since this is the first month that this type of Stockholm job was used). Finally, for Stockholm hosts, we exclude individuals younger than 25. This gives us 17,647 individuals who enter a new enrollment at the job center in Stockholm 21,996 times to be included in our analysis.

We define treatment as the first participation in a Stockholm job within two years after registration at the job center or in December 2016 at the latest.[24] We define the start date of the Stockholm job as the day when the individual starts her/his

[22]Since youths are able to register at the job center without receiving SA, this mostly includes individuals under the age of 30.

[23]Since only individuals who are enrolled at the job center are considered for a Stockholm job and since young people, who are the target group of the largest program, can be registered at the job center and participate in the program without receiving SA, we define the study population as the inflow to the job center, as opposed to the inflow to SA.

[24]We choose this end date in order to be able to follow participants for three years after program start. If a former participant later returns to the job center, the new spell is excluded from the analysis.

employment, that is after the introduction.

We analyze how employment, SA and UI benefit receipt status evolve month by month up to 36 months after program start, as well as the total number of months in, and amounts received from, employment, with SA and UI benefits during two years after the program has ended. We define an individual as employed in month m if he/she has positive earnings during that month. We are thus able to examine whether individuals return to SA after their UI benefits expire after 14 months. In addition, we analyze three health outcomes (medical prescriptions for pain relief, psychiatric drugs and hospitalization for any cause) in order to capture effects on participants' well-being.

3.1 Descriptives

Figure 1 shows how enrollment at the job center, the share of employed individuals, and the share receiving positive SA and UI benefits evolve since time of enrollment at the job center.[25] Six months after enrolling, 52 percent are registered at the job center, and after one year this share has decreased to 30 percent. At the end of our follow-up period (after 36 months), only 9 percent are registered at the job center (they may have left and re-entered). 57 percent receive some SA the same month they enroll and this share increases to 72 percent after one month.[26] After the first months, the share receiving SA decreases over time, and after three years, 25 percent receive SA. When first enrolling at the job center, 25 percent are employed (subsidized or non-subsidized). However, their earnings are generally low (see Figure B.1 in the Online Appendix), implying that they may need SA to top up. The share of employed individuals increases over time, and after three years, 50 percent are employed. The share receiving UI benefits is very low throughout the follow-up period but increases from 2 percent at month of enrollment to 5 percent after 36 months.

[25] We consider an individual as having exited the job center when he/she starts a Stockholm job.

[26] In the Online Appendix, we divide the study population into those who receive SA when registering at the job center and those who do not, and then analyze the second group in more detail.

Figure 1: Share at the job center, in employment (incl. subsidized), with SA and UI benefits since time of enrollment at the job center

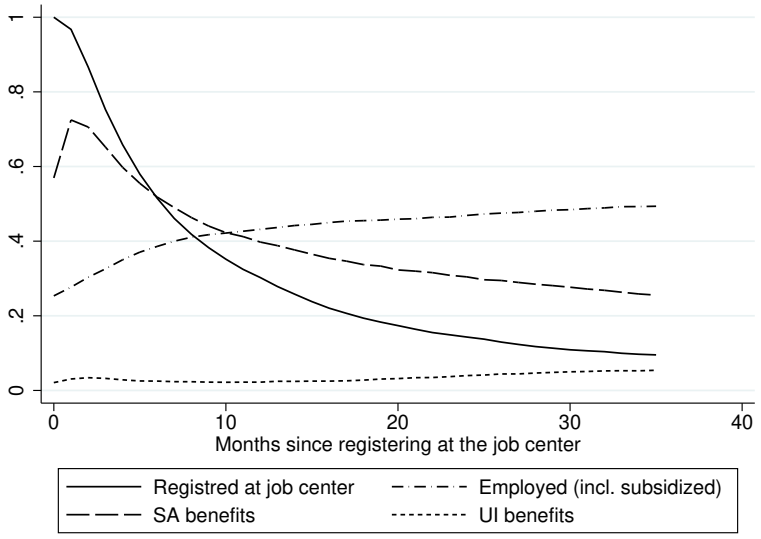


Table 2 presents a description of our study population. Column 1 describes the average client at the job center, while columns 2–4 divide the participants into the three different types of PSEPs we study. The average participant in Youth employments is younger than the average client and has shorter spells of unemployment and SA, as expected, whereas the average participant in the other two types of programs is older and has been unemployed and received SA for a longer time compared to the average client. Participants in Other municipal employments stand out with respect to the participants' previous labor market history being considerably worse and having a longer history of receiving SA. Participants in Youth employments are more likely to be natives than the average client. The two other employment types are dominated by individuals born in Africa and the Middle East. In these programs, the share of foreign-born individuals is well above the same share at the job center in general. Stockholm hosts are dominated by males, whereas Other municipal employment are dominated by females. When it comes to health status, participants in Other municipal employments

seem to exhibit worse health, with more drugs prescribed the previous year. On the other hand, almost 13 percent took the initiative to enroll at the job center themselves, rather than being directed by the caseworker at the welfare office. The corresponding share for the Stockholm hosts is only 1 percent. For Youth employments, it is 18 percent.

Table 2: Description of job center clients and participants in Stockholm jobs at enrollment at the job center

	All	Youth employments	Other municipal employments	Stockholm hosts
Age	32.96	20.99	41.50	41.16
Female	0.47	0.43	0.61	0.27
Married	0.26	0.16	0.31	0.42
Child in household	0.38	0.38	0.51	0.30
Some college education	0.18	0.05	0.23	0.11
No college education	0.77	0.86	0.75	0.82
Education unknown	0.05	0.08	0.02	0.07
Foreign born	0.62	0.51	0.79	0.78
0-2 yrs since immigration	0.14	0.15	0.03	0.21
3-5 yrs since immigration	0.13	0.13	0.15	0.16
Born in Nordics or W. Europe	0.05	0.02	0.05	0.03
Born in E. Europe or C. Asia	0.03	0.01	0.02	0.02
Born in W. Asia or N. Africa	0.19	0.14	0.22	0.09
Born in Africa , excl. NA	0.21	0.24	0.36	0.54
Other country of birth	0.15	0.09	0.14	0.11
Own initiative to enroll at the job center	0.05	0.18	0.13	0.01
Ith quarter at PES when enr. at the job center	3.63	1.86	13.48	7.76
Earnings t-24, 1000 SEK	50.71	25.52	26.69	35.53
SA, nr of months t-24	6.15	5.18	15.61	8.59
Psychotropic drug prescribed t-12	0.20	0.13	0.16	0.12
Pain rel. drug prescribed t-12	0.16	0.09	0.26	0.14
Hospital visit t-12	0.10	0.08	0.08	0.09
Observations	21996	970	396	196

Note: $t-24$ refers to 24 months prior to the assignment period and $t-12$ refers to 12 months prior to the assignment period. Individuals may register several times and the observations in column "All" correspond to 17,658 unique individuals. For individuals participating in Stockholm jobs, later registrations are excluded from the sample. Earnings are reported in 2019 SEK. Psychotropic drugs are drugs with ATC code levels N03–N07 and pain-related drugs are those with ATC code levels N01–N02

Table 3 shows in which sector participants in Youth employments and Other municipal employments worked during the temporary employment (Stockholm hosts all work in the same sector). The most common sectors are "Education" (for Youth employments) and "Human health and social work activities" (for Other municipal employments). Participants in Other municipal employments also work within "Public administration" to a large extent, whereas participants in Youth employments work within "Human health and social work activities" as well as in "Arts, entertainment and recreation".

Table 3: Sector, Stockholm jobs (percent)

	Youth employments	Other municipal employments
Accommodation and food service activities	0.21	0
Real estate activities	0.21	0
Public administration and defence; compulsory social security	5.57	24.2
Education	47.7	3.03
Human health and social work activities	19.2	49.5
Arts, entertainment and recreation	13.5	8.33
Other service activities	2.47	0
No sector registered	7.42	11.4
No workplace registered	3.71	3.54

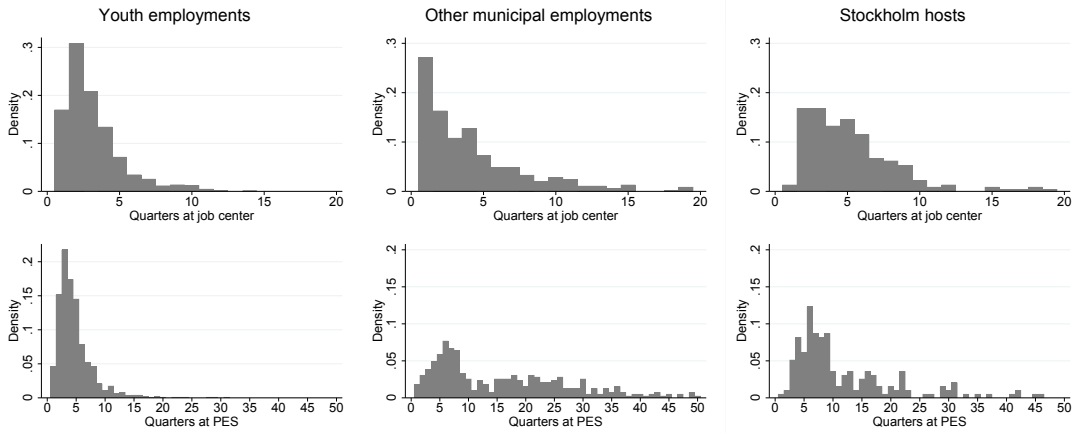
Note: Sectors are characterized according to The Swedish Standard Industrial Classification (SNI 2007) which is based on the EU's recommended standards, NACE Rev.2. For participants that have earnings from several workplaces, we select the workplace from which he/she had the highest earnings during the first month of program participation (or if missing, up to 3 months later), conditioning on that they in the municipal sector.

Figure 2 shows how long individuals have been enrolled at the job center (upper graphs) and at the PES (lower graphs) when starting a Stockholm job. Participants in Youth employments and Other municipal employments typically enter the program quite early on in their job center spell, whereas participants in Stockholm hosts enter somewhat later. Most participants enter during their first year at the job center. However, many participants have been registered as unemployed at PES for a long time

when they are assigned to a Stockholm job; unemployment spells longer than two years are not unusual (an exception is Youth employments for obvious reasons).

Figure 3 shows how long participants remain in a Stockholm job.[27] Most participants stay for the whole planned duration of the program (6 months for Youth employments and 12 months for the other programs – at least since 2012) but some end earlier, whereas some employments are prolonged for over a year. The majority of the Youth employments are not prolonged for the possible additional 6 months.

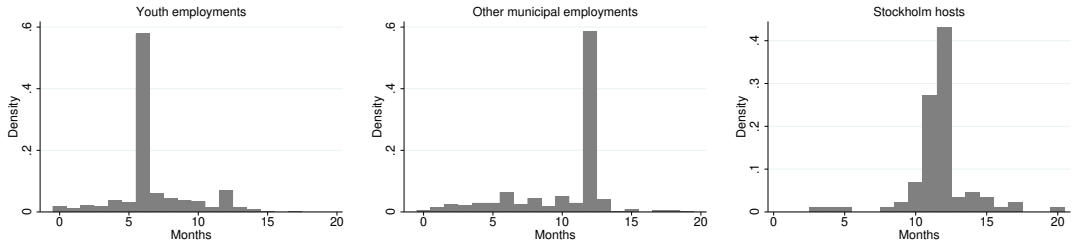
Figure 2: Time registered at the job center/Public employment service (PSE) before program start



Note: One participant in Stockholm hosts had been registered more than 20 months at the job center, and one participant in Stockholm hosts and six in Other municipal employments had been registered longer than 50 quarters at the PES.

[27]Historically, starting the PSEP as part of the Stockholm jobs program was registered as leaving the job center, which implies that very few end dates were registered before 2014. During this period, the duration of Stockholm hosts was six months. Since 2012, when most end dates in Figure 3 were registered, the program lasts 12 months.

Figure 3: Duration of Stockholm jobs by program type



Note: Displayed for observations where end date is registered, which was rare before 2014. Two Stockholm hosts have employments lasting longer than 20 months.

As this section has shown, participants and non-participants are different in terms of individual characteristics. It is also clear that participants enter the program at different points in time. Next, we turn to the empirical strategy and explain how we handle this when estimating causal effects.

4 Empirical strategy

We are interested in estimating the average treatment effect on the treated (ATET); that is, to compare the outcome for those that participate in a Stockholm job with what would have happened had they not participated. Since the latter is not observed, we need to impute the potential outcome under no treatment. Just using the observed outcomes for those who were not treated will most likely lead to biased estimates, since selection into treatment is not random, but determined by the caseworker together with the client. Lacking random variation, we rely on selection on observables, also known as the Conditional Independence Assumption (CIA). By conditioning on all variables that affect both treatment assignment and outcomes variables, the dependence between treatment assignment and outcomes is removed.

As in many evaluations of ALMPs, individuals can be assigned to treatment at any point in time during their unemployment spell. This causes a dynamic selection problem

as one might expect that all individuals will be assigned to treatment eventually, given that they remain at the job center long enough. If we do not take this into account, a static evaluation will lead to biased estimates, since the choice of the control group relies on future outcomes (Fredriksson and Johansson, 2008).

In the rest of this section, we first argue that the extensive set of individual-specific covariates available in our data makes it likely that we are able to take all potential confounders into account. Thereafter, we describe how we address the dynamic treatment assignment by applying the dynamic IPW suggested by Van den Berg and Vikström (2021). Finally, we provide the details of how the empirical strategy is implemented.

4.1 Selection on observables

Since the CIA can not be tested, it is crucial that we have access to all potential confounders.[28] As discussed in Section 3, our data includes a rich set of individual background characteristics such as sex, age, family situation, time since migration and education. In addition, tax registers give us information on previous earnings. We also have information on previous SA uptake, UI benefits and prior participation in ALMPs at PES. This information is very similar to the information available to the caseworker at the job center. When meeting the client, the caseworker forms an opinion about the client's health situation as well as her/his intrinsic motivation. In our data, we have access to information about the client's previous drug prescriptions and hospitalizations, which we include in order to control for potential health problems. Our data also includes information on whether the individual him-/herself took the initiative to enroll at the job center. We use this information as a proxy for motivation. Since we also know at which job center an individual is registered, we can control for in which part of Stockholm he/she lives.

[28]Since we are working in a dynamic setting, explained in more detail in the next section, this assumption needs to be extended to a dynamic CIA. This implies that given our observable characteristics at a given point of time, a sequence of potential outcomes needs to be independent of treatment at that time.

Taken together, the rich set of individual specific characteristics, including information on individual background, previous labor market history, SA and UI history, health and motivation, makes it likely that CIA is fulfilled in our setting.[29] Still, there might be additional important variables that we do not observe in our data. As a way to evaluate our set of confounders, we estimate effects for the period before the participants enter into the program (and also prior to the period for which we include pre-treatment outcomes in the conditioning set). We interpret the absence of such pre-effects as suggestive evidence that our empirical strategy is successful. As matching on this large set of covariates is very demanding, we apply propensity score matching as suggested by Rosenbaum and Rubin (1983).

4.2 Dynamic inverse probability weighting (IPW)

To account for the fact that individuals are assigned to treatment at different points in time, we apply the dynamic IPW-strategy proposed by Van den Berg and Vikström (2021). The dynamic IPW estimates the effects of being treated at a certain elapsed duration compared to never being treated at any subsequent time.

To be eligible for a Stockholm job, individuals need to be enrolled at the job center. In the language of Van den Berg and Vikström, we denote being enrolled at the job center as being in the *initial state* and being assigned a Stockholm job as being *treated*. Some individuals will leave the initial state without being assigned to the treatment, whereas those who are treated will be assigned after spending different amounts of time in the initial state.

Let T_u denote duration at the initial state and T_s the duration until treatment. If $T_u < T_s$, the individual leaves the initial state before treatment. Let the potential time

[29] Previous literature (Biewen et al., 2014; Caliendo et al., 2017; Heckman et al., 1998; Lechner and Wunsch, 2013), focusing on a somewhat stronger group of unemployed, has shown that in addition to individual characteristics, previous labor market history is of great importance, as is regional information, pre-treatment outcomes and information regarding the current unemployment spell. In our setting, previous SA uptake is probably equally relevant.

at the initial state, if the individual is assigned to treatment at t_s, be denoted by $T_u(t_s)$. Further, let Y denote the outcome of interest and $Y(t_s)$ the potential outcome if the individual is assigned to treatment at time t_s. $T_u(\infty)$ and $Y(\infty)$ capture the potential duration and the potential outcome if the individual is assigned to "never treated".[30] The average treatment effect of the treated ($ATET$), when assigned to treatment at t_s compared to never being treated is then given by

$$ATET(t_s) = E(Y(t_s) - Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s) \quad (1)$$

Since we do not observe the outcome under "never treatment" for treated individuals, we need to compute this outcome from those who were never treated. However, the potential control group of never-treated will, in general, be a selective sample since individuals with relatively short durations at the job center will be over-represented in that group. The solution, proposed by Van den Berg and Vikström, is to give greater weights to never-treated individuals who have been at the initial state (at the job center) for a long time. Van den Berg and Vikström show that under the assumptions of sequential unconfoundness, "no anticipation" (Abbring and van den Berg, 2003), common support and SUTVA, an unbiased estimator of $ATET(t_s)$ is given by

$$\begin{aligned} \widehat{ATET}(t_s) = & \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i} = t_s, T_{u,i} \geq t_s} Y_i - \\ & \frac{1}{\sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i)} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_i \end{aligned} \quad (2)$$

where N_{t_s} is the number of never-treated survivors at the beginning of t and the weights w^{t_s} are given by

[30] In practice, infinity will be replaced by some upper bound.

$$w^{t_s}(t_u, X) = \frac{p(t_s, X)}{\rho_{t_s}(1 - p(t_s, X))} \frac{1}{\prod_{m=t_s+1}^{t_u}(1 - p(m, X))} \tag{3}$$

$$p(t, X) = Pr(T_s = t | T_s \geq t, T_u \geq t, X) \tag{4}$$

$$\rho_t = Pr(T_s = t | T_s \geq t, T_u \geq t) \tag{5}$$

The first part of Equation (3) corresponds to the weights from the static IPW, where $p(t_s, X)$ is the propensity to be treated in period t_s, given by Equation (4). The second part takes the duration at the job center (for never-treated individuals) into account by including the propensity to be treated for each following period, if still at the job center, in the denominator. In practice, the weights will be replaced by estimated weights based on estimated propensity scores for each period the never-treated individuals are still at the job center.

Equation (1) is formulated for the effects on outcomes realized after all individuals have left the initial state. We are mainly interested in measuring shorter run outcomes and thus need to take into account that there are individuals who, at the time when outcomes are measured, are still in the initial state. Let Y_t denote the observed outcome in period t and $Y_t(t_s)$ the corresponding potential outcome. The estimand of interest is the ATET of treatment at t_s on the outcome in period $t_s + \tau$ (i.e. τ periods after treatment start). Van den Berg and Vikström show that under no-anticipation (short-run) and unconfoundness (short-run) assumptions, an unbiased estimator of $ATET(t_s)$ is given by

$$\widehat{ATET}(t_s) = \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \frac{1}{\rho_{t_s} N_{t_s}} \Bigg[\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i}) \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} \Bigg] \quad (6)$$

where w^{t_s} is given by Equation (3) and

$$w_\tau^{t_s}(X) = \frac{p(t_s, X)}{\prod_{m=t_s}^{t_s+\tau}(1 - p(m, X))} \quad (7)$$

The weights in Equation (7) are applied to non-treated individuals who are still in the initial state when the outcome is measured (at τ). Since $t_s+\tau < t_u$ for these individuals, only information available at τ is used when estimating these weights.

The ATET aggregated over all possible t_s, is obtained by using the average over the distribution of T_s, where the fraction of treated individuals after t is given by $N_t / \sum_{m=1}^{T_u^{max}} N_m$.

4.3 Implementation

Even though we observe the exact day of assignment, we need to aggregate over larger time intervals in order to estimate the dynamic IPW because of the limited number of individuals entering the program each day.[31] When doing so, we face the trade-off between having enough treated individuals in each assignment period and losing important variation in the data when aggregating over too long time intervals. As guidance, we base our decision on the number of participants in each type of Stockholm

[31] This is similar to what has been done in applications of dynamic propensity score matching, see, for instance, Biewen et al. (2014); Fitzenberger et al. (2008).

job and when they typically enter the program. As is clear from the top panel in Figure 2, most individuals who enter a Youth Employment do so during the first year enrolled at the job center. This is also the program type with the most participants. We thus define $t_s = [1, 4]$ as quarters of a year, and $t_s = [5, 6]$ as six-month periods when evaluating this program. For Other municipal employments, there are fewer individuals taking part in this program compared to Youth employments, and most participants enter already in their first quarter at the job center. We thus we define $t_s = [1]$ as quarter of a year, $t_s = [2, 3]$ as six-month periods and the last period $t_s = [4]$ as the remaining 9 months. For Stockholm hosts, very few enter during the first quarter. This is also the program type with the smallest number of participants. We thus define $t_s = [1, 4]$ as six-month periods. In the Online Appendix we displays the number of treated individuals for each program and assignment period.[32]

The next step is to estimate the propensity scores in Equation (4) and the weights in Equations (3) and (7). As is clear from Section 4.1, we have access to an extensive set of potential confounders. However, we limit the set of covariates in our main analysis since we apply a bootstrap-procedure to obtain standard errors, and initially include the following set of confounders: age, schooling, own initiative to register at the job center, previous labor market attachment and SA usage.[33] Propensity scores are estimated using logistic regression models for each type of Stockholm job and for each assignment period (t_s). Since IPW has been shown to be sensitive to extreme values of the propensity score, we trim our sample following the suggestion by Huber et al. (2013), excluding individuals with weights larger than 1 percent of the sum of weights for the controls.[34]

[32]When estimating the weights, we also consider a seventh/fifth period where we aggregate all participants who start a Youth Employment/Other municipal employment or Stockholm host program after more than two years.

[33]This set of confounders was chosen to achieve similar patterns for participants and their weighted controls in the outcomes of interest before participants entered the program. See Tables C.3-C.5 in the Online Appendix for a list of the variables included. In Section 4.1, we test for the robustness of including more extensive sets of confounders.

[34]It turns out that this constraint is only binding for Other municipal employments, where at most

Before estimating ATET, we need to impute fictitious start dates for individuals in the control group, to know when to measure the outcomes. We do this by, for each type of Stockholm job and time of assignment, drawing a date with replacement from the pool of start dates for the treated individuals.[35] Since we, in the estimations, aggregate over assignment periods and do not condition on non-participants to remain at the job center for the full length of the assignment period,[36] there will be some individuals who have left the initial state before their imputed start date. This, in turn, implies that the estimates for the months closest to the program start might be different from zero for mechanical reasons. For each follow-up month, observations that are later treated are excluded. Observations with a (simulated) treatment date after 2016 are also excluded once the weights have been calculated.

5 Results

Stockholm jobs are intended to provide participants with labor market experience and contacts, thereby increasing their future employment chances. If the program works as intended, it should have positive effects on employment and earnings and negative effects on SA receipt once the Stockholm job has ended. If the program is used as a way transferring individuals from SA to UI benefits, we expect the negative effects on SA recipiency to be counteracted by positive effects on uptake of UI benefits.

27 treated and 12 controls are excluded. Propensity score estimates, descriptive statistics over means of treated and weighted controls in each period, as well as normalized differences for the covariates included in the propensity scores specification before and after weighting are shown in the Online Appendix.

[35]Figures B.2–B.4 in the Online Appendix show the resulting distributions of actual and simulated start dates.

[36]For each assignment period, we consider all individuals who are still registered at the job center at the beginning of that period as our pool of potential controls.

5.1 Youth employments

Youth employments typically last for six months, take place at regular workplaces and are targeted at individuals aged 16–29, who may or may not take up SA. The upper panel in Figure 4 shows how the likelihood of employment (having positive earnings), receiving any SA and receiving any UI benefits evolve before, during and after the participants enter the program, as well as the corresponding evolution for their (weighted) controls. The lower panel shows the ATET in each month relative to program start as well as 95-percent confidence intervals. In the year preceding the program (i.e., at months -12 to -1), the differences between participants and their weighted controls are small, implying that our empirical strategy is successful.[37]

Once the temporary employment starts, the share of employed individuals (left panel) in the treatment group mechanically increases to 1. During the six months that a Youth employment last, employment rates are constantly higher for the treatment group than for the control group, even though employment increases gradually for the latter group. Seven months after program start, when most Youth employments have come to an end, the share employed drops in the treatment group, but is still higher than the corresponding share in the control group (and is considerable higher than before the program started). The lower panel shows a statistically significant effect on the likelihood of being employed corresponding to 30 percentage points higher share employed among former participants than among the control group in month 8. The effect decreases somewhat over time but stabilizes at about 10 percentage points around month 18.

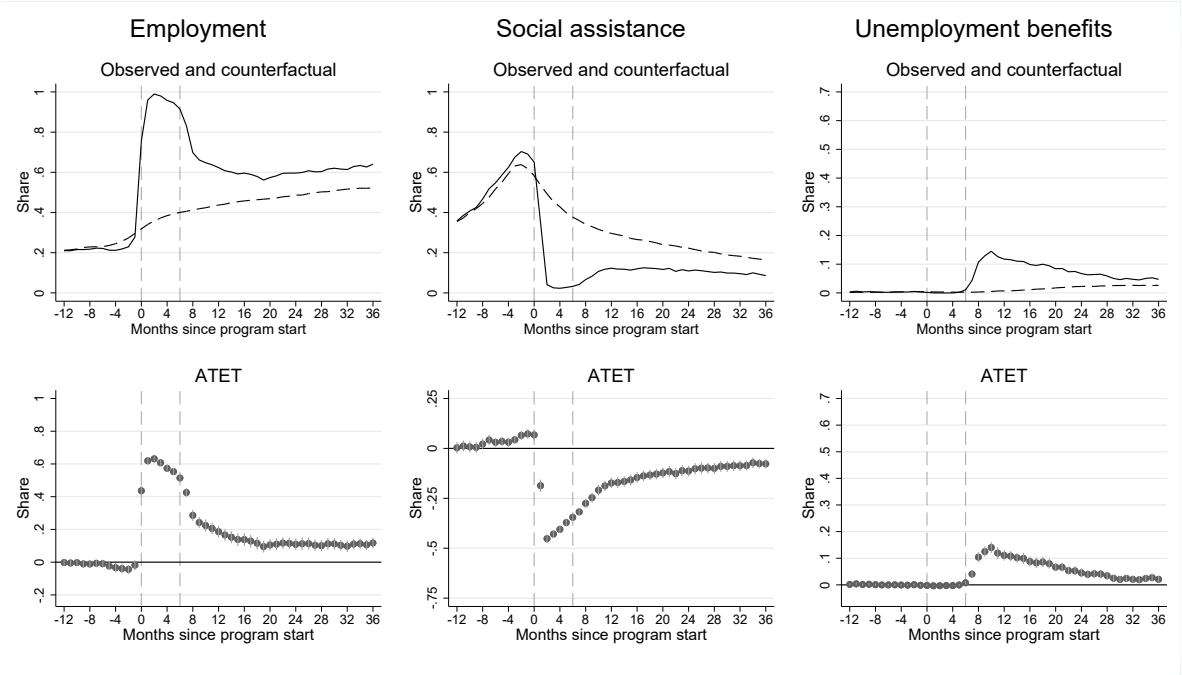
There is a corresponding mechanical sharp drop in the share receiving SA the first two months after individuals enter the program.[38] After the six months that the Youth

[37] A small decrease in the share employed just before the participants start their employment can be detected, which could be explained by participation in pre-program internships. The positive pre-effect present for SA the months just before program start, may be a consequence of that some individuals in the control group already have left the job center at the time of their simulated start date, as mentioned in Section 4.3.

[38] The fact that the share does not drop to zero can be explained that for some (large) households,

employments typically last, the share receiving SA among the former participants increases somewhat, but remains considerably lower than the corresponding share before program start as well as the share in the control group, even though the latter decreases over time. Hence, there is a negative effect on SA recipiency for the full follow-up period, reaching around 7.5 percentage points three years after program start.

Figure 4: Outcomes and ATET by month since program start: Youth employments



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in the Online Appendix.

The likelihood of receiving UI benefits (right panel) increases sharply in the treatment group in month 6, when most Youth employments have come to an end. The effect is at it's largest 10 months after program start when it amounts to 14 percentage points. The effect then diminishes, but three years after program start, the share receiving any UI benefit is still 2 percentage points higher among former participants

the salary received may not be sufficient to reach the stipulated benefit level.

than among non-participants.

Table 4 shows the cumulative effects on the number of months employed, receiving any SA and UI benefits, and on earnings and amounts received from SA and UI benefits respectively, 13–36 months after program start, as well as corresponding effects estimated for the year prior to program start. Reassuringly, the pre-effects are all close to zero, lending support to our identification strategy.

Table 4: Cumulative ATET: Youth employments

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 12–0 before program start			
ATET	-.00481	.00133	-.000123
St err	.00846	.0138	.00147
Mean	.186	.284	.00219
Months 13–36 after program start			
ATET	2.72	-2.6	1.26
St err	.35	.279	.112
Mean	11.8	5.21	.486
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 12–0 before program start			
ATET	-.592	-.0204	-.0171
St err	.764	.735	.0417
Mean	12	31.3	.0997
Months 13–36 after program start			
ATET	37,651	-16,046	4,586
St err	7,120	1,946	676
Mean	172,934	30,686	3,337

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 replications.

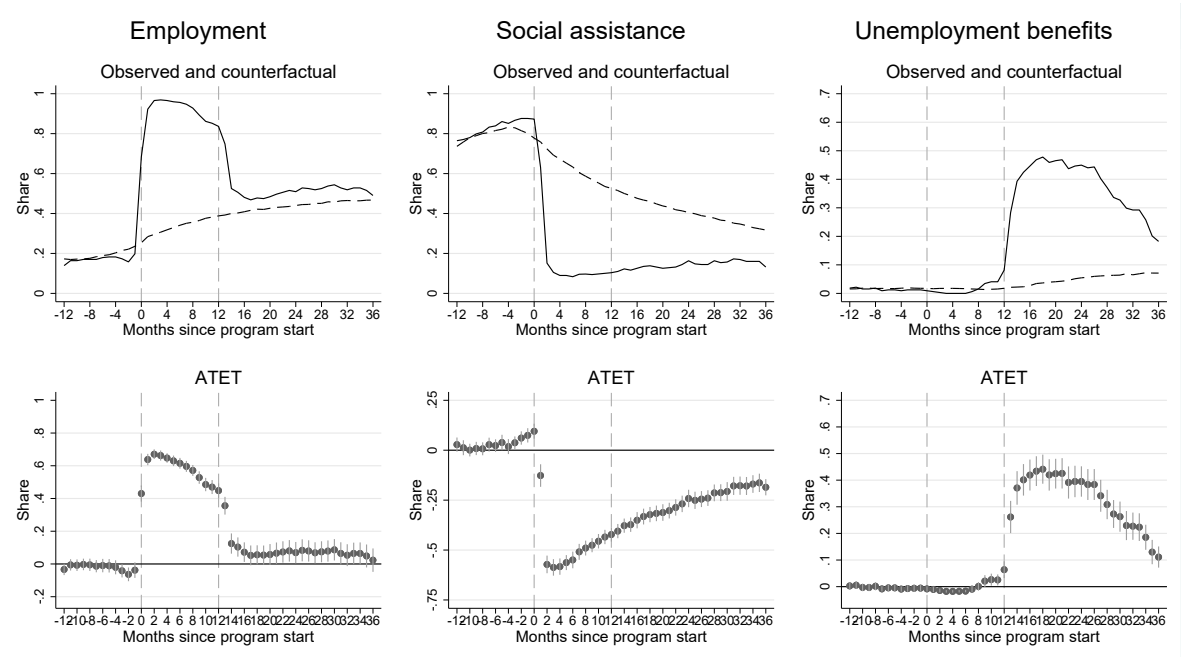
Having had a Youth employment increases employment during this period by approximately 2.7 months, and increases earnings by SEK 37,700 during the same period. These effects correspond to a 22–23-percent increase in employment and earnings compared to the averages in the (weighted) controls. Having a Youth employment further reduces the number of months with any SA by 2.6 (50 percent) and increases the num-

ber of months with any UI benefit by 1.3. The corresponding amounts are a decrease in SA by SEK 16,000 (52 percent) and an increase in UI benefits by SEK 4,600 (137 percent). Comparing the amounts gained in earnings and UI benefits with the amount lost in SA, we conclude that taking part in the Youth employment program results in SEK 26,300 higher income on average over two years after the program ended.

5.2 Other municipal employments

Other municipal employments last for twelve months, take place at regular workplaces and are targeted at SA recipients. Figure 5 shows the evolution of outcomes and estimated effects for this employment type.

Figure 5: Outcomes and ATET by month since program start: Other municipal employments



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in the Online Appendix.

Once the temporary employment starts, the share employed goes up, whereas the share receiving SA goes down, as expected. When the program ends, after one year, there is a distinct drop in the share employed among former participants. However, former participants are employed to a larger extent than their (weighted) controls: The ATET is 35.6 percentage points in month 13; 8 percentage points in month 25 and 2.3 in month 36. The share receiving SA increases only marginally once the program ends and remains at a lower level compared to the share among the weighted controls. In month 13, the ATET is -40.5 percentage points, in month 25 it has gone down to -25 percentage points and in month 36 to -18.5 percentage points.

Turning to the share receiving UI benefits, there are indications of a small negative ATET during the period when the employment lasts, which is partly mechanical given that employed individuals are not entitled to UI benefits. Once the employment ends, there is a sharp increase among former participants and the corresponding ATET is 26 percentage points. This effect increases the following months, reaching a maximum of 44 percentage points in month 18. Three years after program start (in month 36), the share among former participants is still 11 percentage points higher compared to had they not taken part in the program.

Table 5 shows the cumulative effects on number of months (top panel) and amounts (bottom panel) for one year before participants started their Stockholm job and two years after the program ended. By participating in the program, individuals gain 1.8 months in employment and SEK 21,300 in earnings. These effects correspond to increases of around 12–17 percent compared to those in the control group. The number of months with SA decreases by 6, corresponding to a decrease of 64 percent, whereas the amount received decreases by 45,900. The increase in the number of months with any UI is 7.8 months and the corresponding amount is SEK 33,200. Whereas the increase in the number of months receiving UI benefits is larger than the corresponding decrease in the number of months receiving SA, the amount gained in UI benefits is smaller than the amount lost in SA. Also taking into account the increase in earnings, participating

in Other municipal employments results in SEK 8,600 more in income. All pre-program effects are economically insignificant.

Table 5: Cumulative ATET: Other municipal employments.

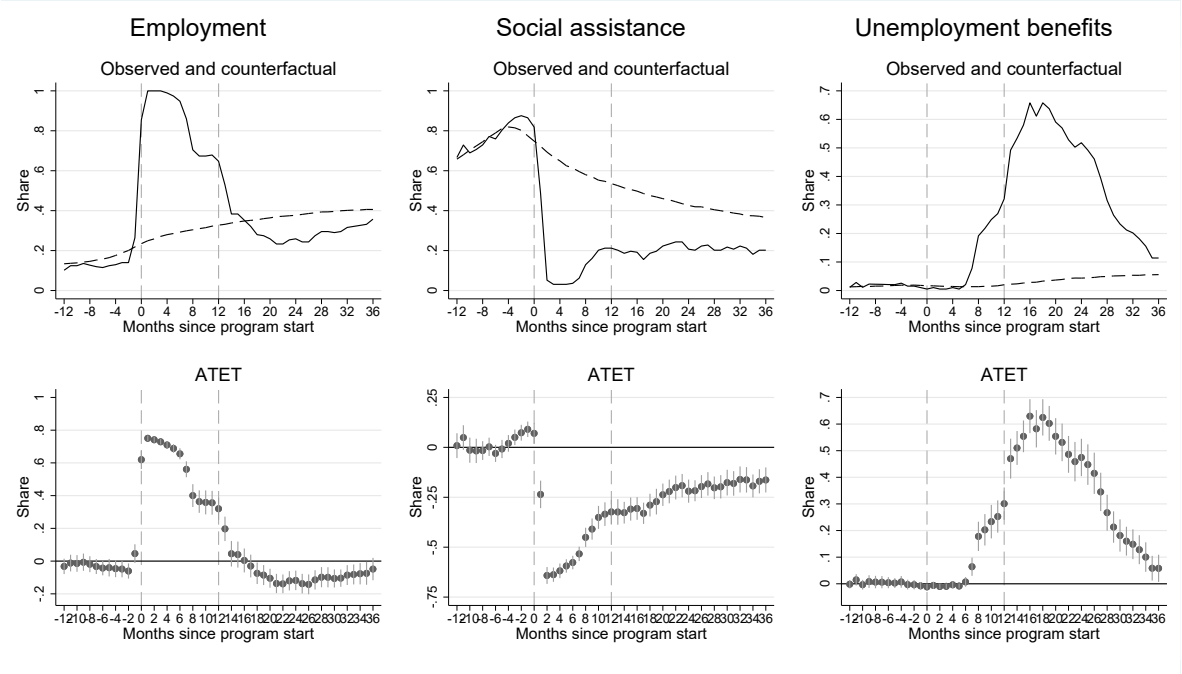
	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 12–0 before program start			
ATET	-.00332	-.00807	.00631
St err	.00802	.0172	.00741
Mean	.0488	.711	.0178
Months 13–36 after program start			
ATET	1.76	-6	7.81
St err	.67	.42	.496
Mean	10.7	9.44	1.25
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 12–0 before program start			
ATET	-2.04	-5.94	.0101
St err	2.07	1.59	.244
Mean	14.9	62.4	.755
Months 13–36 after program start			
ATET	21,312	-45,862	33,232
St err	14,274	3,206	2,450
Mean	182,149	64,889	8,320

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 replications.

5.3 Stockholm hosts

Stockholm hosts differ from the other two types of Stockholm jobs in that participants are not employed at a regular workplace, but at a workplace created especially for program participants. The program is targeted at SA recipients older than 25 or other individuals at risk of becoming long-term unemployed. The length of the program has been either six or 12 months. The results for this program are shown in Figure 6.

Figure 6: Outcomes and ATET by month since program start: Stockholm hosts



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in the Online Appendix.

That the program length varied over time is evident from the graphs: for all three outcomes, there is a drop/increase in the share employed/receiving SA or UI benefits after six months and a corresponding change after twelve months.[39] As opposed to the findings for the other two types of Stockholm jobs that we analyze, the share of employed individuals among former program participants drops to a level below the corresponding share for non-participants one year after the employment begins (and when the majority of temporary employments have come to an end). The negative employment effect is the largest in month 26 reaching -14.2 percentage points. The negative effect decreases over time, and towards the end of our follow-up period, we

[39]Figure B.9 in the Online Appendix show the ATET when excluding participants entering the program before 2012, i.e. when the employment was shorter. In Section 6 we further discuss how the length of the program may matter for the effects.

cannot reject that it is zero (at the five-percent significance level).

The share receiving any SA hovers around 20 percent once the program has ended. Compared to the corresponding share among the weighted controls, this is considerably lower, and the ATET amounts to around 16 percentage points in month 36. For the share receiving any UI benefits, there is a positive effect already from month 7, when the temporary employment in 2010 and 2011 had come to an end, and the effect increases further in month 13. The effect is at its largest in months 16–19, amounting to around 60 percentage points. Thereafter, the ATET diminishes over time, and at the end of our following up period former participants are 5.8 percentage points more likely to receive any UI benefits than their controls.

Table 6: Cumulative ATET: Stockholm hosts

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 12–0 before program start			
ATET	-.0393	-.000121	.00317
St err	.00916	.0382	.00869
Mean	.07	.539	.0122
Months 13–36 after program start			
ATET	-1.7	-5.37	8.99
St err	.57	.564	.454
Mean	9.05	10.3	1.03
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 12–0 before program start			
ATET	-3.99	-1.26	.736
St err	1.95	2.26	.703
Mean	12.6	51.8	.61
Months 13–36 after program start			
ATET	-39,110	-43,082	42,915
St err	10,621	3,457	3,069
Mean	149,412	67,315	6,750

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 replications.

The negative employment effects are also visible in Table 6, which shows the cu-

mulative effects of participating in the program, as well as cumulative effects for the year prior to program participation. During the follow-up period, former participants are employed 1.7 fewer months (a 19 percent decrease) and earn SEK 39,100 less (a 26 percent decrease) compared to non-participants. Participating in the program reduces the number of months receiving SA by 5.4 (52 percent) and the amount received by SEK 43,100 (64 percent). The time receiving UI benefits increases by 9 months and the amount received by SEK 42,900. Taken together, income is SEK 39,300 lower for participants compared to non-participants during these two years.

5.4 Sensitivity analyses

As mentioned in Section 4.3, we limit the number of confounders in the main analysis due to issues with the bootstrap procedure. The fact that the pre-effects are all very close to zero indicates that this limited set does the job. To further test whether we miss any important underlying differences between the two groups, we include additional individual characteristics, dummies for the different job centers, year effects, additional health indicators, as well as additional labor market history, one by one and jointly. Finally, we apply the algorithm suggested by de Luna et al. (2011) for covariate selection.[40] As is clear from Figure B.5 in the Online Appendix, the estimated ATETs are more or less identical for all these different sets of confounders.

Another way to allow for a larger set of confounders and still be able obtain bootstrapped standard errors is to pool over assignment periods when estimating the propensity scores in Equation 4, but adding assignment periods dummies.[41] Doing this we can include all variables mentioned above but the downside is that we restrict the parameters to be the same for all assignment periods. As seen in Figure B.6 in the Online Appendix, we find very similar estimates when using this alternative way to estimate

[40] See Tables C.3–C.5 in the Online Appendix for information on the variables included.

[41] See columns (8) in Tables C.3–C.5 in the Online Appendix for information on the variables included.

the propensity scores.

The limited number of program participants forces us to aggregate over several months when defining assignment periods. To investigate whether our results are sensitive to the way in which we aggregate, we have shortened the time periods somewhat, which comes at the cost of having fewer participants entering the program at each assignment period.[42] It turns out that our results are insensitive to the length of the time periods, see Figure B.7 in the Online Appendix.

When estimating ATET for the period before participants enter the program (months -12 to -1), we need to weigh the non-participants to make them comparable with the participants. However, the weights in Equation (7) are only estimated for periods when participants have already entered the program. In the main analysis, we apply the weights from month 1 for the pre-program period. As a consequence, we might worry that the pre-period is less relevant when it comes to evaluating the balance for participants who enter late during their job center spell. Instead using weights from months 12, 24 and 36 respectively does not change the ATET for the pre-period, see Figure B.8 in the Online Appendix.

5.5 Health outcomes

Participating in the program may also have positive effects on participants' health and general well-being, as having a job with a salary, even if it is subsidized, may offer a sense of pride and purpose for the participant.[43] In addition, an increased income gives individuals opportunities to invest in their health, and may reduce the negative stress associated with living with limited resources. To investigate health effects we analyze medical prescriptions for pain relief, psychiatric drugs and hospitalization for any cause.

Table 7 shows the ATET for the likelihood of having any drug prescribed/any hos-

[42]Table C.2 in the Online Appendix shows how we define assignment periods in this sensitivity analysis as well as the number of treated individuals within each assignment period.

[43]E.g., Ivanov et al. (2020) find that job creating schemes improve the social integration and well-being of long-term unemployed individuals in the German setting.

pitalization for the year before the individual enters the program (months -12 to 0), while they take part in the program, and two years after the temporary employment has ended (months 13 to 36). The pre-effects are all very close to zero, indicating that we do control for all important differences between participants and non-participants.[44]

For participants in Youth employments, the likelihood of any prescription of psychiatric drugs and hospitalizations decrease during the program, compared to non-participants (by 28 percent and 39 percent), but the effects diminish once the temporary employments end. The group that seems to benefit most with respect health outcomes are participants in Other municipal employments. For this group, both the likelihood of prescriptions of psychiatric drugs and hospitalization decrease by 45 percent during the program. The positive health effects pertain also after the temporary employments have ended; during the two following years, former participants are less likely to be prescribed any pain relief (20 percent), any drugs for psychiatric conditions (40 percent) and less likely to be hospitalized (30 percent). Stockholm hosts, finally, are less likely to get any prescription for pain relief both when upholding the Stockholm job and afterwards – a reduction of just over 20 percent.

6 Mechanisms

One conclusion from our analysis is that the type of workplace seems to matter for the program's success. There are several possible explanations to this finding. It is likely that working at a regular workplace is a stronger positive signal to future employers than having worked at a more created workplace, and provides more relevant skills. In addition working at a regular workplace can provide participants with valuable networks as well as useful references and referrals from the manager. Former participants may

[44]When estimating the effects on these outcomes we use the same covariates as in the main analysis except that we also condition on whether the individual has received any pain relief the year before registering at the job center, whether he/she received any psychiatric drugs and whether he/she was hospitalized during the same period.

Table 7: Cumulative ATET: Health outcomes

	Prescription:		Hospitalization
	Any pain relief	Any psychiatric	
Youth employments		Before: Months -12–0	
ATET	-.000519	-.000341	.000989
St err	.000841	.000952	.000683
Mean	.102	.133	.066
		During: Months 1–6	
ATET	-.00779	-.0295	-.0261
St err	.00779	.00875	.00745
Mean	.0717	.106	.0673
		After: Months 13–36	
ATET	-.0144	-.0111	-.0209
St err	.0134	.0139	.0127
Mean	.191	.221	.186
Other municipal employments		Before: Months -12–0	
ATET	-.00047	.00468	-.00166
St err	.00568	.00547	.00276
Mean	.252	.182	.0739
		During: Months 1–12	
ATET	-.0296	-.0972	-.0443
St err	.0236	.0169	.0146
Mean	.261	.218	.098
		After: Months 13–36	
ATET	-.0625	-.111	-.0478
St err	.025	.0221	.0177
Mean	.337	.286	.153
Stockholm hosts		Before: Months -12–0	
ATET	.000661	.00138	-.000278
St err	.00182	.00106	.00119
Mean	.143	.147	.123
		During: Months 1–12	
ATET	-.0543	-.0332	.00627
St err	.0267	.0227	.0251
Mean	.229	.192	.127
		After: Months 13–36	
ATET	-.0714	-.032	.0189
St err	.0325	.0228	.0265
Mean	.333	.247	.171

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 replications. Months relate to program start.

even get a regular employment at the same workplace as in which they had their Stockholm job, something that is not possible, or a least to a very limited extent, for former Stockholm hosts.

To further analyze the importance of workplace for future employment prospects, Table 8 shows to what extent former participants work at the same workplace and/or the same sector as they did during their temporary employment, sometime between 18–36 months after they enrolled in the program. Many former participants are employed in the same sector as their temporary employment. In this regard, former participants in Youth employments stand out, with as many as 50 percent of those that do work are working in in the same sector, and 32.8 percent at the same workplace. The corresponding figures for previous participants in Other municipal employments and Stockholm hosts are lower: 15.5 and 20.4 percent are employed at the same workplace, and 36–37 percent work in the same sector.[45] Note, however, that much fewer former Stockholm host have an employment during this period compared to former participants in Other municipal employment. The fact that participants in Youth employments are much more likely to remain in the same workplace and sector, indicates that employer contacts can be particularly important for young individuals, something which might also explain why we find larger positive employment effects for former participants in Youth employments.

In Table 9, we instead explore in which sectors former participants end up three years after the program started. This is for instance informative of whether the program prepares individuals to enter sectors with a relatively high labor demand, like education and health. In fact, half of former participants in Youth employments and Other municipal employments work in the education or health sector. These are also

[45]For Stockholm hosts, these figures should be interpreted with caution. As is shown in Figure 3, some temporary employments lasted longer than 18 months and are therefore included in the 20.4 percent working in the same workplace 18–36 months after program start. It is however also possible that a few of them, after finishing the Stockholm hosts program, were employed as supervisors at the same workplace.

Table 8: Workplace and sector of employment, 18–36 months after program start (percent)

		Youth employments	Other municipal employments	Stockholm hosts
Having a workplace		83.8	71.7	55.4
	Whereof same sector	*49.7*	*35.9*	*37.0*
	Whereof same workplace	*32.8*	*15.5*	*20.4*
No workplace		16.2	28.3	44.6
No. of observations		970	396	195

Note: Sectors are characterized according to The Swedish Standard Industrial Classification (SNI 2007) which is based on the EU's recommended standards, NACE Rev.2. Production units are classified according to the activity carried out.

the sectors where most temporary employments took place (see Table 3). Former participants in Stockholm hosts are instead most likely to work with transportation or storage.

Another potential explanation for the less promising employment effects found for former participants of Stockholm hosts may be selection into the program. The participants in Stockholm hosts are to a larger extent males but in other dimensions, they do not differ that much from to the participants in Other municipal employments except having somewhat better health status before enrolling at the job center. There are however some indications that those that were Stockholm hosts would have done somewhat better in the labor market had they not taken part in the program than those that took part in Other municipal employments.[46] However, even if the participants in Stockholm hosts were negatively selected, this would not explain the negative employment effects found, since we compare the outcome of those participating in Stockholm hosts, not with participants in the other two employment types but with their (weighted) controls

[46]Comparing the counterfactual outcomes (during months 13–36), measured by how well the participants' (weighted) controls did, we conclude that participants in Youth employments are positively selected with respect to the number of months employed (11.8 compared to 10.7 and 9.1) and with SA (5.2 compared to 9.4 and 10.3), whereas participants in Other municipal employments are positively selected with respect to earnings (SEK 182,000 compared to SEK 173,000 (Youth employments) and SEK 149,000 (Stockholm hosts)).

Table 9: Sector of employment, 36 months after program start (percent)

Sector, conditional of being employed	Youth employments	Other municipal employments	Stockholm hosts
Manufacturing	1.86	0.61	1.67
Water supply; sewerage, waste management ...	0.34	1.21	1.67
Construction	4.07	0.61	3.33
Wholesale and retail trade; repair of motor vehicles ...	8.47	3.03	3.33
Transportation and storage	3.90	4.85	18.3
Accommodation and food service activities	6.10	3.64	8.33
Information and communication	1.02	1.82	0
Financial and insurance activities	0.51	0	0
Real estate activities	2.03	2.42	0
Professional, scientific and technical activities	2.20	2.42	0
Administrative and support service activities	13.4	10.9	25
Public administration ...	2.71	6.06	3.33
Education	19.0	19.4	5
Human health and social work activities	20	32.7	10
Arts, entertainment and recreation	4.94	1.82	0
Other service activities	2.03	0.61	10
Missing sector	7.46	7.88	10
No workplace	39.2	58.3	69.2
No. of observations	970	396	195

Note: Sectors are characterized according to The Swedish Standard Industrial Classification (SNI 2007) which is based on the EU's recommended standards, NACE Rev.2. Production units are classified according to the activity carried out. If a participant has several workplaces 36 months after the start of the program, the workplace from which the individual receives the the highest earnings is selected.

of never-treated individuals. The differing employments effects could instead be a result of the empirical strategy being differently successful for the three program types. From the estimated pre-effects, there are however no such indications. The negative effects are hence likely driven by negative lock-in effects of the program.

A common feature of all three employment types is that participating in the program decreases the likelihood of receiving SA and increases the likelihood of receiving UI benefits once the temporary employment is over. This tendency is less pronounced for Youth employments, whose temporary employments only last six months. To be

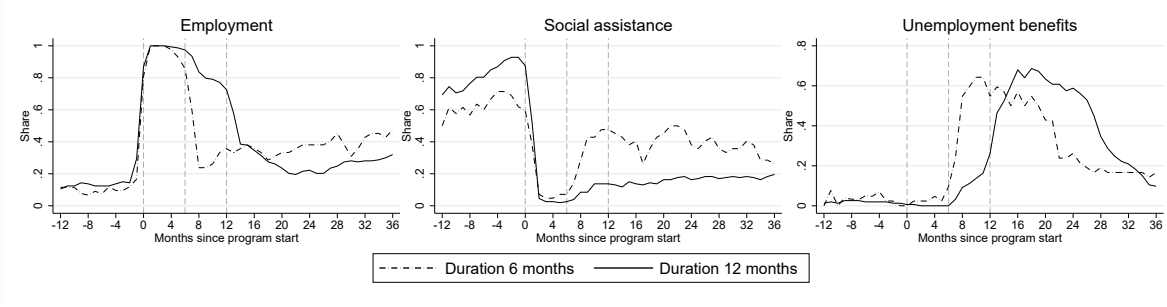
entitled to earnings related UI benefits, individuals must have worked for at least six months and been a member of a UI fund for at least one year, and it is hence likely that those participating in Youth employments do not fulfill the membership requirement when their Stockholm job finishes. To analyze the importance of the length of the employment, we utilize the fact that the duration of Stockholm hosts was shorter (six months compared to twelve months) during the first two years (2010 and 2011) of our study period.

Figure 7 shows observed outcomes by program length.[47] Comparing the employment outcomes for those that took part in the program when it lasted six months and those that took part in the program when its duration was longer, we find that, regardless of the length of the program, the share receiving any UI benefits increases almost to the same extent when the program ends, stabilizing around 20 percent towards the end of our follow up period. However, participants in the shorter program receive SA to a larger extent than those taking part in the longer program, once the temporary employment is finished. A likely explanation is that the former group does not fulfill the membership condition and hence receive lower levels of UI benefits and need to top up with SA.[48]

[47]Given the small number of participants we are not able to estimate ATET separately for those entering the program before and after 2012. Results excluding participants that enter the program before 2012 are available in Figure B.9 and Table C.7 in the Online Appendix.

[48]This explanation is supported when comparing the cumulative ATET on the amount UI benefits received including (Table 6) and excluding (Table C.7 in the Online Appendix) participants entering the program before 2012, i.e. when the employment was shorter.

Figure 7: Observed outcomes by program length: Stockholm hosts



Note: The number of participants with employments lasting for 6 (12) months is 42 (182).

7 Concluding discussion

In this paper, we study three different types of temporary municipal employment targeted at unemployed social assistance (SA) recipients or other unemployed individuals with a weak labor market attachment. Participants are given temporary employment in the municipal sector for 6–12 months. Besides providing labor market experiences and access to networks, the program makes participants eligible for UI benefits. We ask whether having such a temporary municipal employment serves as a stepping stone to future employment or whether it mostly works as a means for the welfare office to transfer individuals from SA to UI benefits.

We find positive employment effects of having a Stockholm job taking place at regular workplaces, a result that differs from what previous evaluations of public sector employment programs have found (Card et al., 2010, 2018; Kluve, 2010). One explanation is probably that the program we study is targeted at SA recipients and other individuals with a similar weak position in the labor market, whereas most earlier work focuses on groups with stronger labor market attachment. Hence, the value of networks and work experience is likely to outweigh potential negative lock-in effects. But also for this specific group, our results are more promising than the ones found for the German

and Belgian evaluations of Temporary extra jobs and Social employments and more in line with the Danish evidence on subsidized employment for SA recipients.

The fact that taking up a Stockholm job is voluntary is potentially one reason for the positive employment effects. In that vein, the program resembles the Norwegian qualification program, which provides tailored activation to hard-to-employ SA recipients in combination with generous non-means-tested benefits. This program has been shown to raise employment among participants (Markussen and Røed, 2016). Another possible explanation to the relatively good outcome of the program we evaluate is that the job search assistance provided by caseworkers toward the end of the temporary employment is effective. This would be in line with the results in Dahlberg et al. (2020) who evaluate a program for another vulnerable group, low-educated refugees, and find large positive effects on employment. The program in their study included intensive language training, work practice and ended with intensive job search assistance.

However, having the temporary employment at a regular workplace seems to be crucial for future employment prospects. Our findings are thus in line with previous evidence indicating that programs that more resembles regular employment, such as subsidized employment, work better (see e.g. Calmfors et al., 2002). For Stockholm hosts, who work at a workplace created especially for the program, we instead find negative employment effects. One explanation to the differing results is Youth employments and Other municipal employments often take place at workplaces with a shortage of personnel, whereas Stockholm hosts have their temporary employment at a workplace with very limited possibility of prolonged employment. The closest type of job is probably a janitor, an occupation that, according to the Swedish PES, is one of those involving the toughest competition among professions with the shortest education.[49] This conclusion is supported by the fact that several participants get employed at the same workplace as in which they had their temporary employment. This pattern is

[49]see https://arbetsformedlingen.se/for-arbetssokande/sa-hittar-du-jobbet/tips-inspiration-och-nyheter/artiklar/2021-03-25-har-finns-jobben-i-framtiden—listan-med-jobb-att-satsa-pa.

especially pronounced for young people, a finding that is in line with previous work by Müller (2021), who shows that early employer links account for more than 30 percent of Swedish vocational high school students' first regular employment, and that losing this link before graduation has a long-lasting negative impact on earnings and employment.

A common feature of all three employment types is that participating in the program decreases the likelihood of receiving SA and increases the likelihood of receiving UI benefits once the temporary employment is over. Municipalities are thus able to shift cost from the local budget to the UI funds by placing individuals into Stockholm jobs. However, the extent to which this is possible seems to depend on whether the temporary employment is long enough to make participants fulfill the membership condition for being entitled to earnings related UI benefits.

Being transferred from SA to UI benefits could be beneficial also for the individual. By becoming eligible for UI benefits, the individual no longer needs to apply for means-tested SA and undergo the scrutiny and uncertainty it pertains. They are also more likely to take part in active labor market programs implemented by the PES instead of municipal activation programs. Although there is limited evidence comparing the effectiveness of these two alternative activation programs, the existing literature points to an advantage for the former (Forslund and Nordström Skans, 2006; Johansson and Langenskiöld, 2008).

To conclude, our results are promising for the group of marginalized unemployed individuals with a weak labor market attachment where few previous programs have been shown to be successful. Not only do we find positive employment effects when having a temporary employment at a regular workplace, for most individuals having had a Stockholm job is likely to have improved their income and well-being.

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