To work or not to work? Effects of temporary public employment on future employment and benefits*

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Abstract

We evaluate a temporary public sector employment program targeted at individuals with weak labor market attachment, applying dynamic inverse probability weighting to account for non-random dynamic assignment into the program. We show that the program is successful in increasing employment and reducing social assistance. The positive employment effect is driven by individuals at regular workplaces; for participants with temporary employment at a constructed workplace, we find negative employment effects. The decrease in social assistance is partially countered by an increase in the share receiving unemployment insurance benefits, indicating that municipalities can shift costs from the local to the central budget.

Keywords: Public sector employment programs; Social assistance; Cost-shifting; Dynamic inverse probability weighting

JEL classification: H75; I38; J45

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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. Given that PSEPs provide participants with networks and labor market experiences, this program can be expected to be well suited for marginalized groups with otherwise poor labor market prospects.[1] However, in contexts where different levels of the government are responsible for financing unemployed with and without unemployment insurance (UI) benefits, there are incentives for caseworkers to use PSEPs as a means of providing participants with eligibility to UI benefits and thereby shift costs to the other level, rather than targeting individuals most likely to benefit from the program.[2] Although there is anecdotal evidence that such cost-shifting does occur, empirical evidence is scarce.

In this paper, we thus 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. The paper thus adds to our limited knowledge of what works for this particular group (see, for instance, Bolvig et al., 2003; Cockx and Ridder, 2001; Heinesen et al., 2013; Markussen and Røed, 2016; Thomsen and Walter,

[1] PSEPs 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 is present when one governmental level is in charge of activating unemployed individuals while another is responsible for paying their benefits.

2010), as well as broadens our understanding of the role played by institutional setups when 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 temporary employments at regular and non-regular workplaces. The findings of this paper can therefore improve our understanding of which ingredients that are important for a public sector employment to be successful.

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 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 participants. A similar conclusion is reached by Lundin and Skedinger (2006) who, when

[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 are present 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.

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. The aim of the program 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. Caseworkers thus face several, potentially conflicting, objectives, similar to what is discussed in, for instance, Schmieder and Trenkle (2020). 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.

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 on 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

[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.

an indicator 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 (2022). Earlier studies relying on matching strategies mostly follow Sianesi (2004, 2008) and apply 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. In the dynamic IPW, the group of potential controls is made up of individuals who never take part in the program. The estimand is thus the effect of taking part in the program or not doing so, which 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.

We find that the employment prospect for individuals placed at regular workplaces are improved thanks to the program. The effect is especially pronounced and lasting for Youth employments, where former participants are around 10 percentage points more likely to be employed up to two years after the end of their temporary employment. 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 show that participants that do find an employment after the program to a large extent are employed at the same workplace or in the same sector as they were during their Stockholm job, indicating that it is crucial that the program provides participants with experiences and networks that are relevant in sectors with a demand for labor. We further find that having any type of Stockholm job reduces the likelihood of receiving SA with more than 50 percent during the two years following the employment. To some extent, this is counteracted by an increase in UI, in particular for Stockholm hosts, 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, indicating positive effects on participants' well-being.

2 Institutional setting

Like many other welfare states, Sweden combines relatively generous (earnings-related) UI benefits with mandatory active labor market programs (ALMPs).[5] 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 with very low levels of UI benefits 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.[6]

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

[5]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, or 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 benefits. Parents with children under 18 have access to an additional 150 days.

[6]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 1,060) for a couple with two children aged 5 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.

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, the capital of Sweden, 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. 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 and thereby get access to their services.

The program that we analyze 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 main component of the program is temporary (often subsidized) employment in the municipal sector lasting 6–12 months, where wages are paid out by the municipality and not by the workplace where the individual is employed. 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.

We focus on three types of Stockholm jobs that differ with respect to target group, type of workplace and employment duration. 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

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.

temporary employments do not take place at regular workplaces. 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 UI benefits, 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.[7] During this introduction, participants keep the benefits they received prior to

[7]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.

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 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. When employed, participants above the age of 19 receive a salary of at least SEK 19,000 (approximately EUR 1,800) per month (SEK 18,000 until 2015). During the temporary employment, caseworkers at the job center help participants plan what to do once the Stockholm job ends. This may entail going to the job center one afternoon a week to search for jobs or enrolling in education.[8] Since 2016, all participants are offered additional assistance for three months after the end of their employment.

As opposed to the other activation programs at the job center, which are mandatory for unemployed SA recipients if referred to by the caseworkers, taking up a Stockholm job is voluntary.[9] 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 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.

Taking up a Stockholm job is financially beneficial for participants. The salary

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.

[8]Since 2015, participants are allowed to study half-time simultaneously with their employment. Initially, this opportunity only applied to participants in some types of Stockholm jobs and for some types of educational choices.

[9]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.

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

When the Stockholm job ends, participants returning to unemployment fulfill the work requirement for receiving UI benefits and are entitled earnings-related UI benefits if they have been members of a UI fund for at least one year. 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 (although households with many children might still need to top up with SA). 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.[10] Hence, the 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 finance and attend to the former recipients.

Caseworkers at the 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. 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.

[10]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.

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 (but not the identity of the caseworker). In addition, the data includes information regarding whether the individual him-/herself took the initiative to enroll at the job center. The data from Statistics Sweden covers the years 2008–2019 and includes yearly socio-demographic background characteristics such as age, sex, number and age of children and marital status, region of origin, year of immigration as well as information about the highest attained education level. It also includes 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.[11] 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. 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

[11]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.

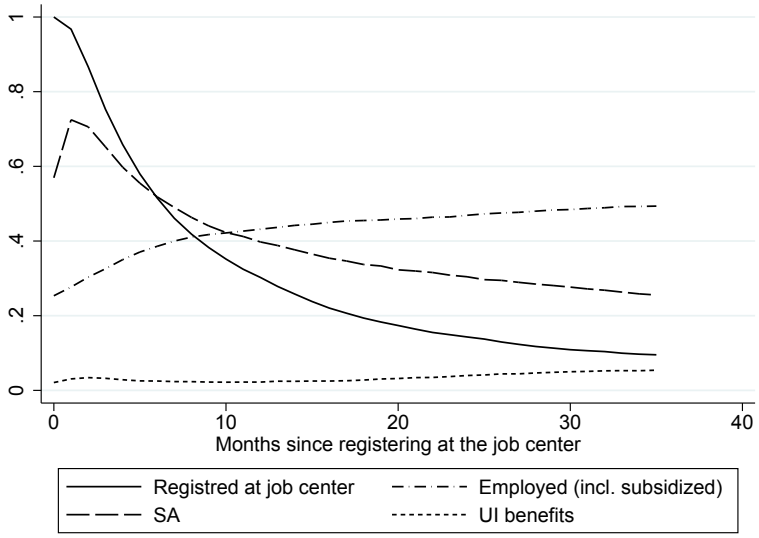
Youth employments, the sample is restricted to those younger than 30. When it comes to Other municipal employments, the sample is restricted to those with a start date in May 2012 or later (since this is the first month that this type of Stockholm job was used). Finally, for Stockholm hosts, we exclude individuals younger than 25 from the sample.

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.[12] We define the start date of the Stockholm job as the day when the individual starts her/his employment, that is after the introduction and any internship.

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 in the short (1 year after the end of the program) and medium (year 2 after the end of the program) run. We define an individual as employed in month m if he/she has positive earnings during that month. 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.

[12]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.

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



3.1 Descriptives

Figure 1 shows how enrollment at the job center (we consider an individual as having exited the job center when he/she starts a Stockholm job), the share of employed individuals, and the share receiving positive SA and UI benefits evolve since time of enrollment at the job center. As is clear from the figure, the share enrolled at the job center goes down over time, and at the end of our follow-up period, only 10 percent are registered at the job center (they may have left and re-entered). Also, the share receiving any SA goes down over time, expect for a small increase in the first month, reaching 25 percent at the end of our period.[13] When first enrolling at the job center, 25 percent are employed (subsidized or non-subsidized). However, their earnings are generally low, implying that they may need SA to top up; see Appendix A (available

[13]As is clear from the figure, not everyone receives SA when registering at the job center. Of these, around 55 percent receive SA the following month, and of those that do not, 60 percent are younger than 30. See Appendix A (available online) for additional descriptions.

online). The share of employed individuals increases over time, reaching 50 percent after three years. The share receiving UI benefits is very low throughout the follow-up period, never reaching more than 5 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. N. Africa	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, 1,000 SEK	50.71	25.52	26.69	35.53
SA, nr of months t-24	6.15	5.18	15.61	8.59
Any psychotropic drug prescribed t-12	0.20	0.13	0.16	0.12
Any pain rel. drug prescribed t-12	0.16	0.09	0.26	0.14
Any hospital visit t-12	0.10	0.08	0.08	0.09
Observations	21,996	970	396	196

Note: $t-24$ refers to 24 months prior to the enrollment and $t-12$ refers to 12 months prior to enrollment at the job center. Individuals may register several times and the observations in column "All" correspond to 17,647 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 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 Stockholm jobs we study. Focusing first on participants in Youth employments, this group consists of equally many males and females, and the participants are also equally likely to be born in or outside Sweden. Compared to the average client at the job center, they are younger and more likely to be born in Sweden, have shorter spells of unemployment and SA, as expected, as well as better health outcomes. Participants in Other municipal employments and Stockholm hosts are instead older than the average client, and to a larger degree born outside Sweden; the share foreign born is almost 80 percent. The participants in Stockholm hosts have been in Sweden for a shorter time and have somewhat lower education than participant in Other municipal employments. More women than men participate in Other municipal employments (60 percent females), whereas Stockholm hosts are dominated by male participants (70 percent males). Compared to the average client at the job center, participants in these two types of Stockholm jobs have longer unemployment and SA-spells. 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 and also exhibit worse health, with more drugs prescribed the previous year. Almost 13 percent of the participants in Other municipal employments 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, and for Youth employments, it is 18 percent.

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" (in which care for elderly is included), for Other municipal employments.

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

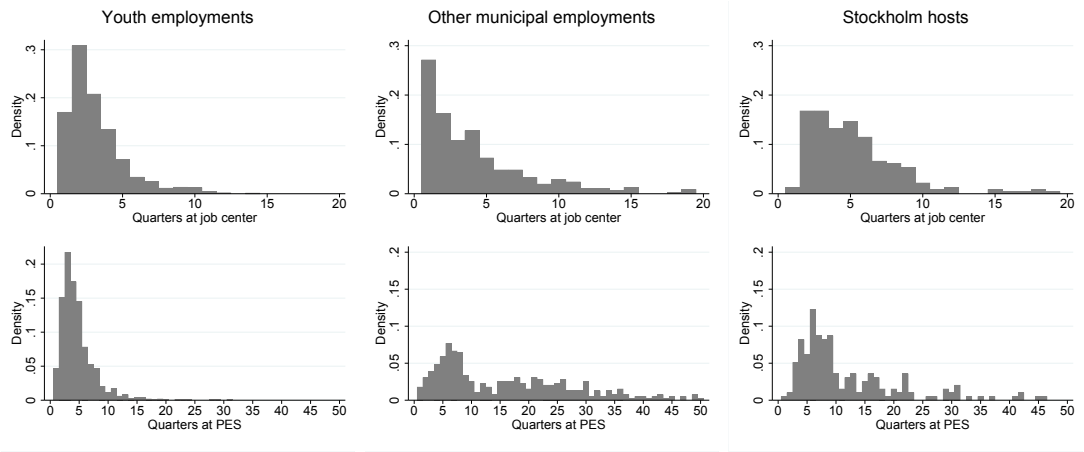
Table 3: Sector, Stockholm jobs (percent)

	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 work in the municipal sector.

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

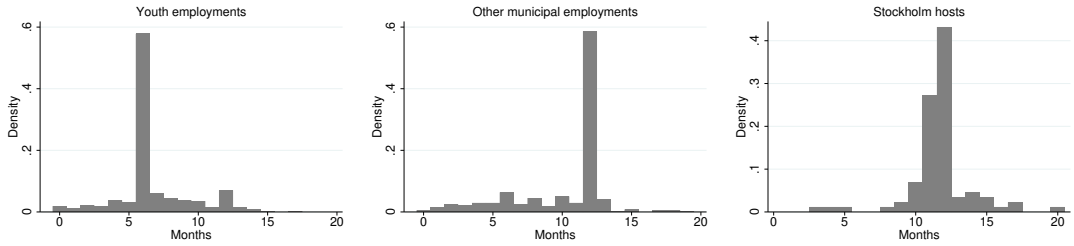


Note: Two observations (Stockholm hosts) that lasted longer than 20 months are censored. One observation (Stockholm host) with a duration at the job center longer than 20 months and seven observations (one Stockholm hosts and six in Other municipal employments) with durations at the PES longer than 50 quarters at the PES are censored.

Figure 3 shows how long participants remain in a Stockholm job. Most participants stay for the whole planned duration of the program (6 months for Youth employments and 12 months for the other programs[14]) 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.

[14]Historically, participants were registered as leaving the job center when they started Stockholm job, which implies that very few end dates were registered before 2014. Consequently, most observations in Figure 3 are from the period when the program lasted 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 observations (Stockholm hosts) that lasted longer than 20 months are censored.

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 after spending different amount of time at the job center. Next, we turn to the empirical strategy and explain how we handle these issues 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. Lacking random variation, we rely on selection on observables, also known as the Conditional Independence Assumption (CIA).

In addition to non-random selection, we have to account for the dynamic selection into the program, since individuals can be assigned to a Stockholm job at any point in time while at the job center. Some individuals will leave the job center without being assigned to a Stockholm job, whereas those who are assigned will be so after spending different amounts of time in the job center. Not taking the dynamic nature of the assignment into account will lead to biased estimates (Fredriksson and Johansson,

2008).

To account for the non-random, dymamic selection, we apply the dynamic IPW-strategy proposed by Van den Berg and Vikström (2022). The dynamic IPW estimates the effects of being treated at a certain elapsed duration compared to never being treated at any subsequent time. This is accomplished by weighting observations with both the propensity scores for being assigned at each assignment period and the propensity to be treated for each future assignment period, if still at the job center. In principle, this implies giving greater weights to never-treated individuals who have been at the job center for a long time.[15]

For (dynamic) CIA to hold, it is crucial that we have access to all potential confounders.[16] 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. However, when meeting the client, the caseworker also 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. Finally, since we also observe at which job center an individual is registered, we can control for in which part of Stockholm he/she lives and for any job center specific assignment strategy of clients into different programs.

Taken together, the rich set of individual specific characteristics makes it likely that

[15]See Appendix B (available online) for a a formal description of the estimator.

[16]Dynamic CIA implies that, conditional on observable, the sequence of potential outcomes id independent of treatment at each given point of time.

CIA is fulfilled in our setting.[17] 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.

When implementing the dynamic IPW, there are a number of issues that need to be settled. First, because of the limited number of program participants, it will not be possible to estimate effects by assignment day. Instead, we need to aggregate over longer assignment periods (see for instance Biewen et al. (2014); Fitzenberger et al. (2008) for a similar approach).[18] See Appendix C (available online) for a detailed description. Second, we need to estimate propensity scores. In the main analysis, we limit the set of covariates to the following set of confounders: age, schooling, own initiative to register at the job center, previous labor market experiences and SA recipiency.[19] In Section 5.4, we test for the robustness of including more extensive sets of confounders. See Appendix D (available online) for details about the propensity score estimation as well as balancing results. Third, we impute fictitious start dates for individuals in the control group to be able to compare outcomes for the participants with those of their weighted controls. See Appendix E (available online) for details.

[17] 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.

[18] The ATET that we present in the next sections are the average ATET weighted by the number of program participants in each given assignment period.

[19] We limit the set of confounders since the bootstrapping procedure that we apply to estimate standard errors does not always converge with a larger set.

5 Results

Stockholm jobs are intended to provide participants with labor market experience and networks, thereby increasing their future employment chances. If the program works as intended, we should thus observe positive effects on employment and earnings and negative effects on SA receipt once the Stockholm job has ended. If the program instead 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.

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.[20] 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

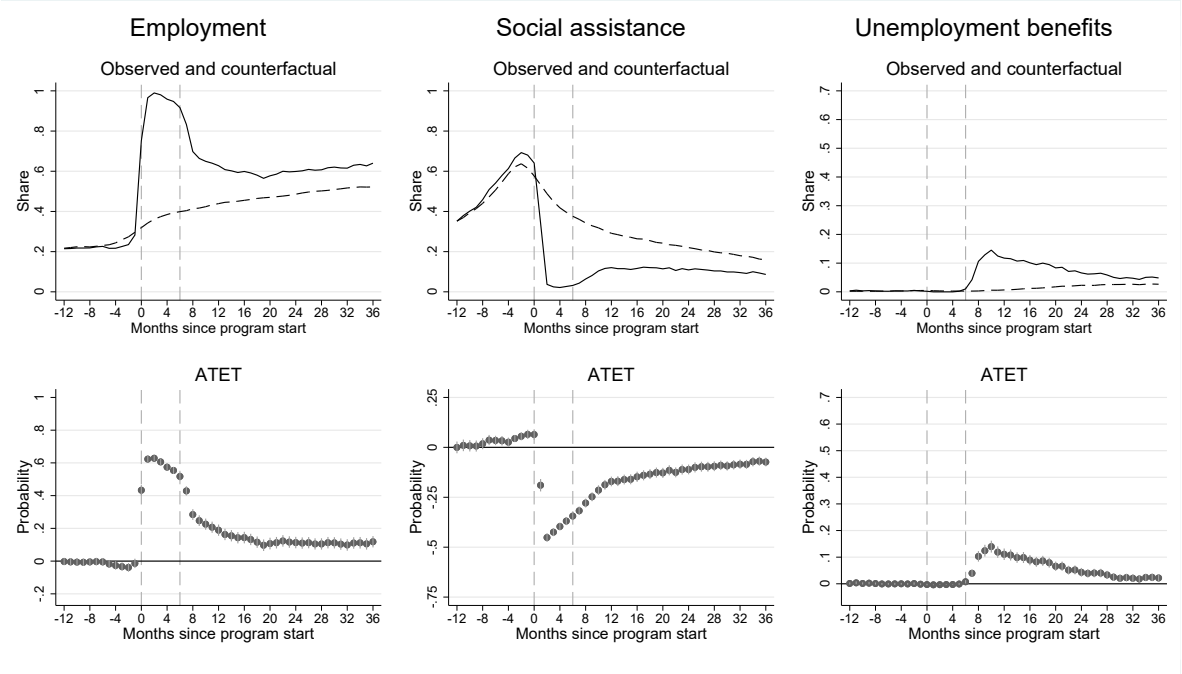
[20] A small decrease in the share employed just before the participants start their employment can be detected, which could be explained by participation in a pre-program internship. The positive pre-effect present for SA the months just before program start may be because some individuals in the control group already have left the job center at the time of their simulated start date (see Appendix E, available online). This interpretation is reinforced by our sensitivity analysis, where we show that these positive pre-effects disappear when we aggregate over shorter assignment periods, see Section 5.4.

gradually for the latter group. Once most Youth employments have come to an end, the share employed goes down, but remains higher than the corresponding share in the control group (and is considerable higher than before the program started). As a result, there is a statistically significantly ATET of the program during the full follow-up period that stabilizes at about 10 percentage points one year after the temporary employment has ended.

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

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 around 15 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 than among non-participants.

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-percent confidence intervals estimated through bootstrapping with 997 replications. Months related to program start. Weights estimated for month 1 are used for the pre-period (months -12 to -1). Point estimates and standard errors corresponding to the lower panel are available in Appendix F (available online).

In order to get a better impression of how large the estimated effects are, Table 4 shows the cumulative effects on the number of months employed, receiving any SA and UI benefits, as well as on earnings and amounts received from SA and UI benefits respectively, the year before program start, the six months that the program typically lasts, and in the short (first post-program year) and medium (the second year after the program has ended) run. As a comparison, the table also provides the means for the weighted controls.

Table 4: Cumulative ATET: Youth employments

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Before program (months -12– -1)			
ATET	-.189	.308	.00633
St err	.102	.0915	.0169
Mean	2.8	5.79	.0339
During program (months 1–6)			
ATET	3.49	-2.17	-.00579
St err	.0562	.0575	.00506
Mean	2.27	2.66	.0182
Short run (months 7–18)			
ATET	2.41	-2.29	1.2
St err	.174	.121	.0804
Mean	5.27	3.52	.0937
Medium run (months 19–30)			
ATET	1.31	-1.26	.555
St err	.186	.131	.0731
Mean	5.88	2.58	.264
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Before program (months -12– -1)			
ATET	-1,178	1,226	-13
St err	841	793	43.2
Mean	12,959	34,035	103
During program (months 1–6)			
ATET	66,539	-12,760	-37.2
St err	1,119	388	14.9
Mean	21,788	15,418	58.7
Short run (months 7–18)			
ATET	36,379	-13,659	3,638
St err	3,347	835	306
Mean	66,479	20,587	484
Medium run (months 19–30)			
ATET	17,648	-7,886	1,961
St err	4,109	840	401
Mean	86,215	15,148	1,809

Note: Means are calculated for the weighted controls. Standard errors are estimated through bootstrapping with 997 bootstrap replications. Months relate to program start.

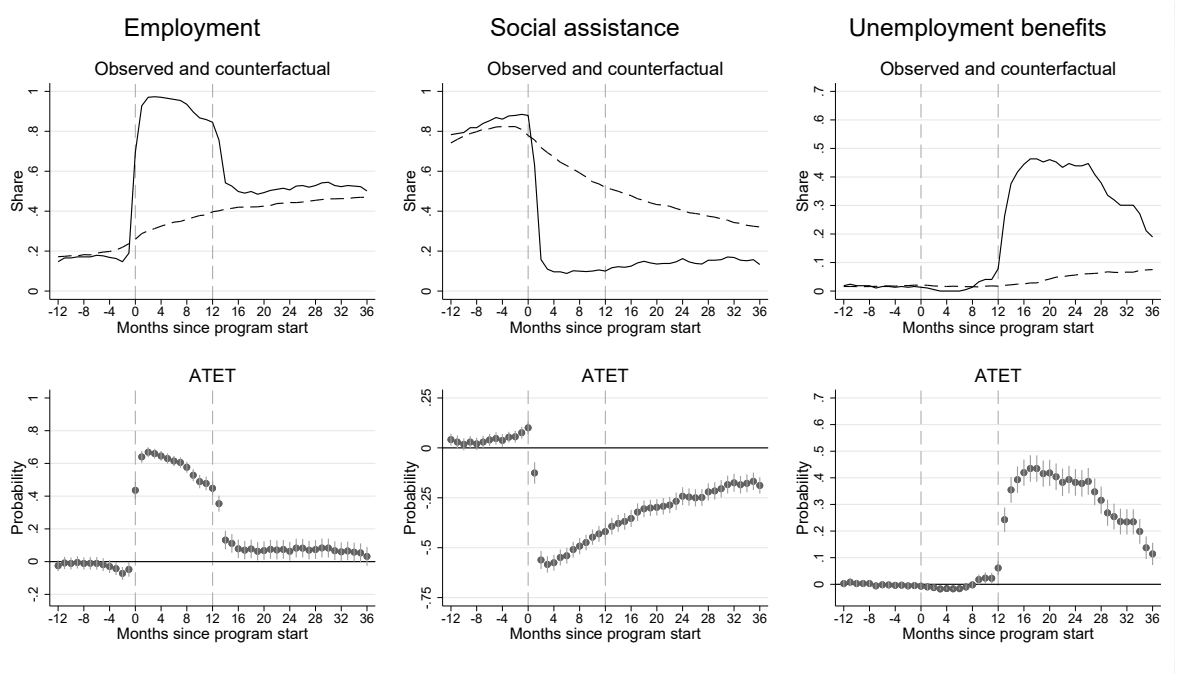
Reassuringly, the pre-effects are all small, lending support to our identification strat-

egy. It is also evident that employment and earnings go up during the six months that participants are employed and that SA recipiency goes down during this period. The more interesting thing is what happens once the temporary employment has ended. From the table, we see that having had a Youth employment increases employment in the short run by approximately 2.4 months and increases earnings by SEK 36,400 during the same period. These effects correspond to a 45–55 percent increase in employment and earnings compared to the averages in the weighted controls. In the medium run, the effects are smaller (approximately 50 percent of the short run-effects), but still economically (and statistically) significant, corresponding to increased employment and earnings with around 20 percent. The program further reduces the number of months with any SA by 2.3/1.25 months (65 and 50 percent) in the short/medium run, and increases the number of months with any UI benefits with 1.2/0.5 months respectively. The amount received in SA decreases by SEK 13,500/7,900 whereas the amount in UI benefits increases by SEK 3,600/2,000. 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 38,000 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-percent confidence intervals estimated through bootstrapping with 997 replications. Months related to program start. Weights estimated for month 1 are used for the pre-period (months -12 to -1). Point estimates and standard errors corresponding to the lower panel are available in Appendix F (available online).

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, but not to the level of the weighted controls. Hence, there is a positive employment effect of around 5–10 percentage points for the two years that follow after the temporary employment has ended. 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, with a treatment effect of just below 20 percentage points at the end of our follow-up period. Turning to the share receiving UI benefits, there are indications of a small negative effect 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, that is not present among the weighted controls, implying a positive ATET of around 25 percentage points. The effect increases the following months, reaching a maximum of just above 40 percentage points. Two years after the temporary municipal employment has ended, the share among former participants is around 10 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). By participating in the program, individuals gain 1.2/0.8 months in employment and SEK 19,700/10,200 in earnings in the short/medium run. These effects correspond to increases of around 20/10 percent compared to those in the control group. The number of months with SA decreases by 3.7/2.4, corresponding to a decrease of around 70/75 percent, whereas the amount received decreases by SEK 47,000 during these two years. The increase in the number of months with any UI is 4.7/3.1 months and the amount received increased by SEK 33,000 during the two post-program years. 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 15,900 more in income in the short and medium run. All pre-program effects are economically insignificant.

Table 5: Cumulative ATET: Other municipal employments.

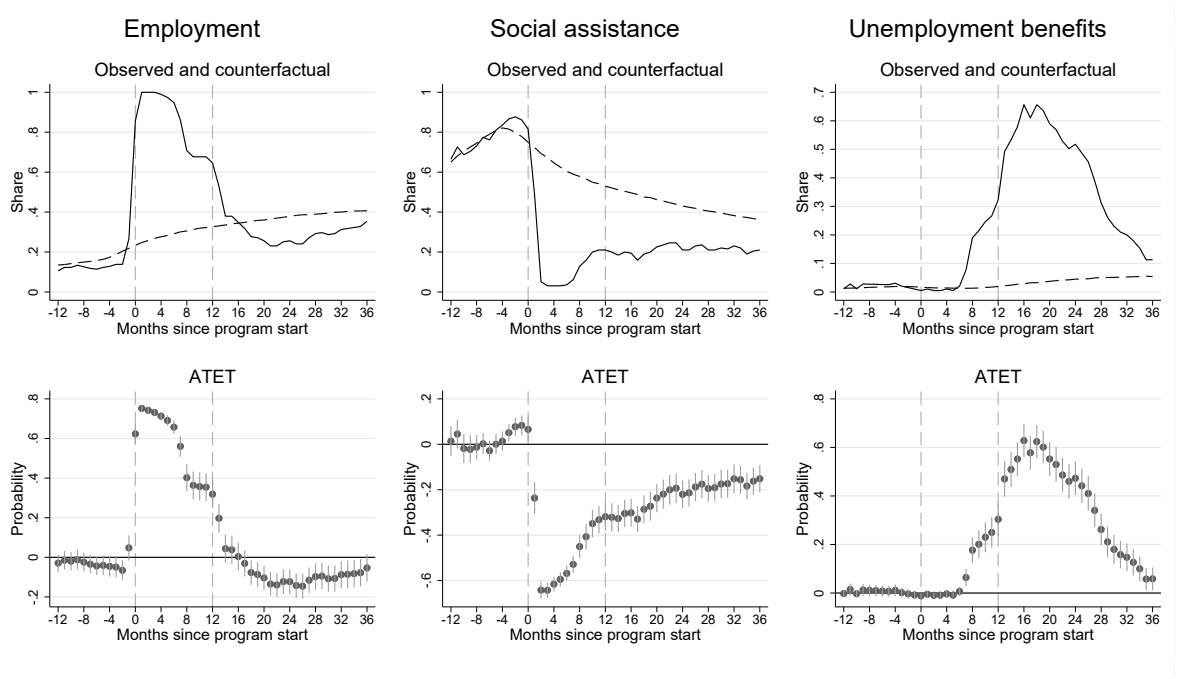
	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Before program (months -12– -1)			
ATET	-.293	.477	-.00756
St err	.19	.102	.0684
Mean	2.31	9.59	.208
During program (months 1–12)			
ATET	6.99	-5.73	.0212
St err	.173	.2	.0486
Mean	4.13	7.49	.206
Short run (months 13–24)			
ATET	1.17	-3.72	4.67
St err	.299	.224	.243
Mean	5.16	5.36	.441
Medium run (months 25–36)			
ATET	.767	-2.38	3.1
St err	.313	.24	.231
Mean	5.55	4.2	.809
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Before program (months -12– -1)			
ATET	-2,750	-3,845	-131
St err	2,386	1,627	246
Mean	17,250	64,611	807
During program (months 1–12)			
ATET	148,148	-42,528	-322
St err	3,918	1,439	182
Mean	56,154	51,207	983
Short run (months 13–24)			
ATET	19,726	-27,897	22,404
St err	6,283	1,553	1,417
Mean	83,414	36,280	2,917
Medium run (months 25–36)			
ATET	10,158	-19,114	10,598
St err	7,242	1,761	1,095
Mean	98,222	29,539	5,455

Note: Means are calculated for the weighted controls. Standard errors are estimated through bootstrapping with 995 replications. Months relate to program start.

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-percent confidence intervals estimated through bootstrapping with 997 replications. Months relate to program start. Weights estimated for month 1 are used for the pre-period (months -12 to -1). Point estimates and standard errors corresponding to the lower panel are available in Appendix F (available online).

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.[21] As opposed to

[21]In Section 6 we further discuss how the length of the program may matter for the effects.

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 two years after program start, reaching almost 15 percentage points. The negative effect decreases over time, and towards the end of our follow-up period, we 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 15 percentage points. For the share receiving any UI benefits, there is a positive effect already after six months, when the temporary employments in 2010 and 2011 had come to an end, and an additional increase after one year. The effect is at its largest shortly thereafter, amounting to around 60 percentage points, and then diminishes over time. At the end of our follow-up period former participants are around 5 percentage points more likely to receive any UI benefits than their controls.

The negative employment effect is also visible in Table 6, which shows the cumulative effects. In the short/medium run, former participants are employed 0.5/1.2 fewer months (a 10/20 percent decrease) and earn SEK 15,300/24,400 less (a 20/30 percent decrease) compared to non-participants. The negative employment effect is thus larger in the medium run than in the short run. Participating in the program reduces the number of months receiving SA by 3.2/2 (45–55 percent) and the amount received by SEK 23,500/18,800 (60–65 percent). The time receiving UI benefits increases by 6.5/2.5 months and the amount received by SEK 31,800/10,900. Taken together, income is SEK 39,400 lower for participants compared to non-participants during these two years. However, this loss in disposable income is lower than the increase in disposable income while being in the program (SEK 87,000).

Table 6: Cumulative ATET: Stockholm hosts

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Before program (months -12– -1)			
ATET	-.359	.0692	.053
St err	.224	.174	.0981
Mean	1.94	8.92	.198
During program (months 1–12)			
ATET	6.64	-5.67	1.2
St err	.22	.199	.144
Mean	3.51	7.31	.175
Short run (months 13–24)			
ATET	-.542	-3.19	6.46
St err	.326	.323	.323
Mean	4.27	5.68	.409
Medium run (months 25–36)			
ATET	-1.2	-2.09	2.49
St err	.347	.325	.257
Mean	4.76	4.68	.615
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Before program (months -12– -1)			
ATET	-3,729	733	443
St err	1,890	2,096	539
Mean	14,846	56,862	676
During program (months 1–12)			
ATET	122,552	-37,761	2,394
St err	4,786	1,577	403
Mean	48,221	46,602	656
Short run (months 13–24)			
ATET	-15,346	-23,498	31,772
St err	5,629	2,068	2,029
Mean	67,590	36,328	2,498
Medium run (months 25–36)			
ATET	-24,441	-18,818	10,936
St err	6,782	2,089	1,524
Mean	81,471	31,344	4,214

Note: Means are calculated for the weighted controls. Standard errors are estimated through bootstrapping with 997 replications. Months relate to program start.

5.4 Sensitivity analyses

As mentioned in Section 4, we limit the number of confounders in the main analysis. 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, indicators for the different job centers, year effects, additional health indicators, as well as additional controls for labor market history, one by one and jointly, in addition to applying the algorithm suggested by de Luna et al. (2011) for covariate selection.[22] The estimated ATETs are more or less identical for all these different sets of confounders, see Appendix G (available online).

Another way to allow for a larger set of confounders is to pool over assignment periods when estimating the propensity scores (instead of estimating propensity scores by assignment periods), but adding assignment periods dummies. Doing this we can include all variables mentioned above, but we have to restrict the parameters to be the same for all assignment periods. We find very similar estimates when estimating propensity scores this alternative way, see Appendix G (available online).

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, see Appendix C (available online) for details. When doing this, the positive pre-effects that were detected in the likelihood of receiving any SA that were present in Figures 4–6 are considerably smaller, which we take as evidence that the former were a consequence of the aggregation over assignment periods rather than "true" pre-effects. Apart from this the results are insensitive to the length of the assignment periods, see Appendix G (available online).

[22] See Appendix D, available online, for information on the variables included.

When estimating ATET for the period before participants enter the program (months -12 to -1), we need to weight the non-participants to make them comparable with the participants. However, the weights 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 later in their job center spell. Instead using weights from months 12, 24 and 36 respectively does not change the ATET for the pre-period, see Appendix G (available online).

5.5 Health outcomes

Participating in the program may also affect participants' health and general well-being. Having a job with a salary, even if it is subsidized, may offer a sense of pride and purpose for the participant.[23] This view was expressed by several participants when we visited their workplace. Also when asked in interviews, participants respond that they do tell their family and friends about acquiring a Stockholm job, which we interpret as further evidence of satisfaction and pride with having a temporary employment. In addition, for those participants whose disposable income increases, there are opportunities to invest in their health, and potentially a reduced negative stress associated with living with limited resources.

To test whether participants health was affected by the program, we estimate cumulative ATET for the likelihood of having any drug prescribed/any hospitalization for the year before the individual enters the program (months -12 to -1), while they take part in the program, as well as in the short and medium run for the three types of Stockholm jobs, see Table 7 for results.[24]

[23]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.

[24]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 received any pain relief the year before

Focusing first on participants in Youth employments (column a) we find indications of positive health effects for participants. Compared to their weighted controls, they are 25 percent less likely to be hospitalized and almost 40 percent less likely to be prescribed any psychiatric drugs while upholding their Stockholm job. The latter effect pertains in the short run, corresponding to a reduced likelihood of having any psychiatric drug prescribed with almost 15 percent. Turning next to participants (column b) Other municipal employments, we conclude that also for this group, getting a Stockholm job reduces the likelihood of being prescribed any psychiatric drugs, both while in the program (with around 35 percent) and in the short and medium run (with around 30 percent).[25] Finally, the corresponding results for Stockholm hosts (column c) show that having this type of Stockholm job reduces the likelihood of receiving any pain relief with almost 25 percent while in the program and with 30/20 percent in the short/medium run. One explanation to these positive effects may be the very active nature of the employment where participants spend the day outdoor walking long distances.

registering at the job center, whether he/she received any psychiatric drugs and whether he/she was hospitalized during the same period. See Appendix D (available online) for details.

[25]When it comes to hospitalization, there are however indications that those that take part in the program are somewhat less likely to be hospitalized already before starting their Stockholm job. However, these differences disappear once we aggregate over shorter assignment periods, but so do the short run effects, see Appendix G (available online).

Table 7: Cumulative ATET: Health outcomes for (a) Youth employments, (b) Other municipal employments and (c) Stockholm hosts

	(a) Youth employments			(b) Other municipal employments			(c) Stockholm hosts		
	Prescription:		Hospital	Prescription:		Hospital	Prescription:		Hospital
	Pain relief	Psychiatric		Pain relief	Psychiatric		Pain relief	Psychiatric	
Before program									
ATET	-.0102	-.00973	-.00841	-.0126	-.0157	-.0271	-.0187	-.0202	.00616
St err	.00548	.00469	.00434	.0134	.0084	.00872	.0168	.0092	.0158
Mean	.104	.134	.0713	.261	.195	.0806	.173	.164	.122
During program									
ATET	-.00619	-.0262	-.0252	.00144	-.0795	-.0373	-.0542	-.0321	.00704
St err	.00836	.00829	.00734	.0248	.0166	.0139	.0254	.0247	.0238
Mean	.0722	.104	.0674	.262	.219	.0992	.229	.191	.126
Short run									
ATET	-.0127	-.0212	-.000523	-.0217	-.0603	-.0139	-.0691	-.0304	.0472
St err	.0111	.0119	.0115	.023	.0186	.0154	.0269	.0229	.0263
Mean	.116	.153	.112	.251	.214	.0975	.228	.184	.112
Medium run									
ATET	-.0127	.00155	-.00982	-.0321	-.0687	-.0166	-.0451	-.00879	.0188
St err	.0108	.0128	.0113	.0233	.0191	.0149	.027	.0256	.0228
Mean	.117	.159	.116	.24	.222	.0921	.23	.188	.0991

Note: Means are calculated for the weighted controls. Standard errors are estimated through bootstrapping with 999 (Youth employments), 995 (Other municipal employments) and 996 (Stockholm hosts) replications, respectively. Before program refers to months -12– -1, during program to months 1–6 (a) or 1–12 (b and c), short run to months 7–18 (a) or 13–24 (b and c) and medium run to months 19–30 (a) or 25–36 (b and c). Months relate to program start.

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. One is that working at a regular workplace provides a stronger positive signal to future employers than having worked at a constructed workplace, and/or that the skills acquired are more valuable. In addition, working at a regular workplace may provide participants with valuable networks as well as useful references and referrals from the manager. Former participants may 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.[26]

When analyzing to what extent former participants are employed at the same workplace and/or in the same sector as they had their temporary employment in/at, sometime between 18–36 months after they enrolled in the program (see Appendix H, available online), we indeed find that a relatively large proportion of those that have an employment during these two years indeed are at the same workplace as they had their Stockholm job. This tendency is especially pronounced among former participants in Youth employments, where as many as one third work at the same workplace. This finding 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. It is also evident that many get their future employment in the same sector; 50 percent of former participants in Youth

[26]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, have been a shorter time in Sweden and have somewhat lower education than the participants in Other municipal employments. On the other hand, they have somewhat better health status before enrolling at the job center. 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 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 due to negative lock-in effects of the program.

employments and around 35 percent for the other two employment types.

It thus seems to be important that the temporary employment takes place in a sector that is in demand of labor. Whereas many Youth employments and Other municipal employments take place in sectors characterized by shortage of staff, such as childcare and care for elderly, the closest type of job to a Stockholm host 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.[27] When exploring in which sectors former participants end up three years after the program started (see Appendix H, available online), it also turns out that half of former participants in Youth employments and Other municipal employments work in the education or health care sector, whereas former participants in Stockholm hosts are instead most likely to work with transportation or storage.

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. 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 (see Appendix H, available online), we find that, regardless of the length of the program, the share receiving any UI benefits increases almost to the same

[27] 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.

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.

One concern with the program is that case workers place individuals in the program in order to shift costs from the local to the central budget instead of prioritizing individuals that are most likely to benefit from the program. To investigate whether these concerns are valid, we estimate treatment effects of Youth employments for individuals that received SA once enrolling at the job center and those that did not. It turns out that even though the effects on UI benefits are larger for the first group, the employment effects are very similar (see Appendix H, available online), indicating that the incentives to shift cost do not come at the expense of employment effects.

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 that to a large extent lack previous labor market experiences, whereas most earlier work focuses on groups with stronger labor market attachment.[28] The conclusion that a temporary employment can act as a stepping stone to future employment for new entrants at the labor market is in line with the findings in e.g. Pallais (2014). Our results are also 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 that they study includes intensive language training, work practice and ends 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 constructed workplace, we instead find negative employment effects. One explanation for the differing results is that Youth employments and Other

[28] Another potential explanation is that our estimation strategy takes the dynamic nature of program assignment into account. When comparing the dynamic IPW with the static version, Van den Berg and Vikström (2022) find negative treatment effects of a Swedish training program when using the latter, but positive effects using the former. Hence, it seems like the static estimator, which utilizes a possible positively selected control group, produces estimates that are downward biased.

municipal employments often take place at workplaces with a shortage of personnel, whereas Stockholm hosts have their temporary employment at a workplace with a very limited possibility of prolonged employment. 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 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.[29]

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.[30] 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 is potentially 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

[29]The U.S-evidence on summer job-programs are less promising, mostly finding no or negative effects on future earnings and employment, except for young people highly engaged in schooling, see e.g. Gelber et al. (2015) and Davis and Heller (2020). However, these programs are typically targeted at children at risk and have shorter durations.

[30]A back-of-the-envelope cost-benefit analysis shows that this strategy is not financially beneficial in the short and medium run. However, if the reductions in SA payments pertain, although at lower levels, it will soon be. Also, our cost-benefit analysis does not take into account the reduced administrative and personnel costs at the job center or the potential value-added by the participants when employed. See Appendix I (available online) for details.

points to an advantage for the former (Forslund and Nordström Skans, 2006).

That participants may benefit from having a Stockholm job is also indicated by the reductions in prescriptions for psychiatrics and pain relief that we find, once the participants start their temporary employment. For older participants, these positive effects pertain once the program ends.

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 well-being.

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