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To work or not to work?

Effects of temporary public employment on future employment and benefits

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Effects of temporary public employment on future employment and benefits

by

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Abstract

We evaluate a temporary public sector employment program targeted at individuals with weak labor market attachment in the City of Stockholm. Having access to rich high-quality individual-level administrative data, we apply dynamic inverse probability weighting, proposed by Van den Berg and Vikström (2021), to deal with dynamic selection into the program. We find 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 engaged in outdoor cleaning at a workplace created especially for the program. In addition, we find that the decrease in social assistance recipiency to some extent is countered by an increase in the share receiving unemployment insurance benefits. This tendency is especially pronounced for the program with negative employment effects.

Keywords: Public sector employment programs; Social assistance; Cost-shifting; Dynamic inverse probability weighting
JEL-codes: 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. 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.[5] However, for groups that lack previous labor market experience, temporary employment may also work as a means of providing eligibility to (earnings-related) unemployment insurance (UI) benefits. In contexts where a higher level of the government is responsible for financing UI benefits whereas a lower level finances social assistance (SA), there are thus incentives for the lower level to use PSEPs as a means of shifting costs to the higher level.[6] 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, and ask to what extent participating in the program leads to future employment for the participants or whether the program rather transfers individuals from SA to UI benefits. Our paper thus contributes to the empirical literature analyzing what works for this particular group (see, for instance, Markussen and Røed, 2016; Bolvig et al., 2003; Heinesen et al., 2013; Cockx and Ridder, 2001; 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, giving us an opportunity to shed additional light on how type of employment matter for outcomes.

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 employment* 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, which has been shown to have positive employment effects (Markussen and Røed, 2016) .

[5] PSEPs targeted at unemployed individuals in general 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 possible program effects.

[6] 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 a lower governmental level is in charge of activating unemployed individuals while a higher level is responsible for paying their benefits.

Previous evidence regarding to what extent PSEPs are used to provide participants with eligibility to UI is very much lacking.[7] 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, by studying a Swedish pilot program, show that decentralization increased the targeting of individuals with a relatively high level of dependence on social assistance, which is what we should expect if local governments use their increased influence to improve municipal budgets at the expense of the central governments.[8]

The program we study is called *Stockholm jobs* and consists of employment in the municipal sector for 6–12 months, where the individual performs (quality-enhancing) tasks that would otherwise not have been performed. We study three different types of the program, in two of which (*Youth employment* and *Other employment*), participants work at a regular workplace, whereas in the third (*Stockholm hosts*), participants are employed at a workplace created especially for this purpose. 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 their employment, participants become eligible for UI benefits, 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.[9]

Our analysis is based on administrative data for individuals who register 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. The data includes a rich set of individual background characteristics, such as labor market and welfare history, education, health indicators, and time since immigration as well as an indicator of whether the individual took the initiative to register 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 registering 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 typically use dynamic propensity score matching (Sianesi, 2004, 2008), thus estimating the effect of being assigned to a program at a specific time as opposed to potentially being assigned at a later time.[10] In the dynamic IPW, the group of potential controls

[7] 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 employment insurance eligibility. Although the incentives for local governments to shift costs to the central government exist for *Social Employment* 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.

[8] 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), Kok et al. (2017) and Hayashi (2019) show that moving from matching to lump sum grants indeed has an effect on local governments in terms of reducing welfare caseloads.

[9] See, for instance, Schmieder and Trenkle (2020) for a discussion on how caseworkers face several, potentially conflicting, objectives.

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

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. To the best of our knowledge, our paper is the first program evaluation utilizing the dynamic IPW-method.

We find that positive post-program effects for participants who worked at a regular workplace within the municipality. For participants in *Stockholm hosts*, we find a lower probability of being employed up to two years after the program ended compared to the control group. The program further reduces the likelihood of receiving SA for all participants. To some extent, this is due to an increase in UI. Taken together, our results are promising for this group of marginalized unemployed individuals with a weak labor market attachment. However, the type of workplace where the participants get their temporary employment is important. If not taken place at a regular workplace, the risk is that the temporary employment mostly work as a way to transfers individuals from SA to UI benefits.

The outline of the paper is as follows. In the next section, we present the institutional setting and the program under study. Section 3 describes the data, how we select our sample, and gives some descriptive statistics. In Section 4, we present the empirical strategy that we apply to deal with dynamic selection into the program, as well as how we implement the strategy. Section 5 presents the main results as well as sensitivity analysis, whereas Section 6 concludes by summarizing the findings and discussing potential explanations to the results found.

2 Institutional setting

In this section, we first briefly describe the benefit systems and activation programs for unemployed individuals. We then present details about the specific PSEP that we study in the paper. We conclude by discussing the incentives facing, on the one hand potential participants, and on the other hand, the caseworker at the job center, who is responsible for assigning individuals to the program.

2.1 Financing and activating the unemployed

Like many other welfare states, Sweden combines relatively generous (earnings-related) UI benefits with mandatory active labor market programs (ALMPs).[11] 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.[12] To be eligible, all other means, including savings and

[11] 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. Full-time unemployment and benefits last for approximately 14 months. Parents with children under 18 have access to an additional 150 days.

[12] SA makes up the final safety net in the Swedish welfare system and all individuals living in Sweden have the right to apply for SA.

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.[13]

Unemployed SA recipients are required to actively look for work, be registered at PES and take part in ALMPs offered by PES. If 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, and many municipalities have job centers that organize these activities and where unemployed SA recipients are required to register (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 job centers. In addition, it is possible for unemployed individuals aged 16–29 to enroll at a job center even if they do not receive SA. At the job center, the client meets a caseworker who, in collaboration with the client, sets up an action plan containing information about planned activation programs. The client also gets assistance in putting together a CV, contacting potential employers as well as advice regarding study opportunities.

2.2 Stockholm jobs

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 local job centers in the city of Stockholm. The main component of the program is temporary employment in the municipal sector lasting 6–12 months. Different types of employment subsidies are used to finance this employment, either financed by the government, the labor market unit in the city of Stockholm or the local city districts. 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)[14] .

We focus on three types of *Stockholm jobs* that differ with respect to target group, type of workplace and employment duration.[15] Table 1 summarizes the main characteristics of the three program types. The *Youth employment program* targets individuals aged 16–29 in need of extra support to find and maintain employment. Participants are employed at a regular workplace such as childcare centers, schools, nursing homes or the municipal administration. The employment lasts for six months, but the program may be prolonged for an additional six months if it is deemed beneficial for the individual. *Other municipal employment*, which exists since 2012, is in many aspects similar to the *Youth employment program*, except for the target group (SA-recipients in general) and the length of the program (typically 12 months). *Stockholm hosts* differs from the

[13] 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 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.

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

[15] There are also *Stockholm jobs* especially targeted at disabled individuals or former criminals. Since these programs are very small in scale, we exclude them from our analysis.

Table 1: Description of different types of Stockholm jobs

	Youth employment program	Other municipal employment	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: Since 2015, the different city districts in Stockholm administer a large portion of *Other municipal employment* and decide or themselves with regard to the target group.

other two in that it does 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), and the program is targeted at individuals who are 25 years or older with children to care for or other individuals expected to not do well in the open labor market on their own. Individuals are eligible to participate if they 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 introductory phase consisting of general information about the UI system, unions, norms and rights in the workplace and the program itself. During this introductory phase, 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 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.[16] When employed, participants above the age of 19 receive a salary of at least SEK 19,000 (approximately EUR 1,800) per month.[17] During the employment, caseworkers at the job center help participants plan what to do once the *Stockholm job* ends.[18] This may entail going to the job center one afternoon a week to search for jobs or enrolling in education.

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

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

[18] 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.[19] However, it is uncommon that an individual declines an offer to participate in the program.

2.3 Incentives for individuals and municipalities

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.[20] If an individual does not accept an offered *Stockholm job*, he or she is likely to be placed in some other activation program that typically does not consist of employment.

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.[21] 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.[22] Instead, 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.[23] 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 take care of them 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 clients to the workplaces can be expected to counteract these incentives.

[19] 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.

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

[21] 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.

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

[23] 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 the *Youth employment program* are subsidized, while the shares for *Other municipal employment* and *Stockholm host* 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. In addition, the data includes information regarding whether the individual him-/herself took the initiative to enroll at the job center.[24] 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, earnings and monthly employment status. 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 registered 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 registration.[25] This gives us 17,659 unique individuals who enter a new enrollment at the job center in Stockholm 22,012 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.[26]

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.[27] 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

Table 2 presents a description of our study population. Column 1 describes the average client at the job center, while columns 2–4 divide these participants into the three different types of PSEPs we study.

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

[25] Since only individuals who are registered 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.

[26] 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.

[27] We also try a stricter definition where we require yearly earnings to exceed the income base amount. The income base amount tracks the general income growth and amounted to SEK 42,400 in 2010 and SEK 46,500 in 2019.

As expected, the average participant in the *Youth employment program* is younger than the average client and has shorter spells of unemployment and with SA, 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 employment* stand out with respect to the participants' previous labor market history being considerably worse and having a longer history of receiving SA. Participants in the *Youth employment program* 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* is dominated by males, whereas *Other municipal employment* is dominated by females. When it comes to health status, participants in *Other municipal employment* seem to exhibit worse health, with more drugs prescribed and more hospital visits the previous year. On the other hand, almost 23 percent took the initiative to enroll at the job center themselves, rather than being directed by the case worker at the social office. The corresponding share for the *Youth employment program* is 18 percent and for *Other municipal employment*, it is only 1 percent.

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 registration at the job center.[28] Six months after registration, 51 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 8 percent are registered at the job center (they may have left and re-entered). 57 percent receive some SA the same month they register at the job center and this share increases to 72 percent one month after program start.[29] After the first months, the share receiving SA decreases over time, and three years after their registration at the job center, 25 percent receive SA.

When first registering at the job center, 26 percent are employed (subsidized or non-subsidized). However, their earnings are generally low (see Figure B.2 in Appendix B), implying that they may need SA to top up. The share of employed individuals increases over time since registration, 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 registration to 5 percent after 36 months.

Figure 2 shows how long individuals have been registered at the job center (upper graphs) and at PES (lower graphs) when starting a *Stockholm job*. Participants in the *Youth employment program* and *Other municipal employment* typically enter the program quite early on in their job center spell, whereas participants in *Stockholm hosts* enter somewhat later.[30] Most participants enter during their first year at the job center. However, many participants have been registered as unemployed

[28] We consider an individual as having exited the job center when he or she starts a *Stockholm job*.

[29] Since unemployed individuals are required to register at the job center and participate in activities in order to qualify for SA, the fact that the share is higher one month after registering at the job center than one month before is not surprising. There could also be a measurement error in the data due to employment being registered on a monthly level, where an individual shows up as employed if he or she becomes unemployed at the beginning of the month, and register at the job center directly after. In Figure B.1 in Appendix B, 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. It turns out that of those around 9,500 new registrations without SA, 46 percent receive SA the following month. Of the remaining 54 percent, 59 percent are younger than 30.

[30] According to the information from the city of Stockholm, individuals who had been registered at the job center for six months were a prioritized group in terms of joining *Stockholm hosts*.

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

	(1) All	(2) Youth employment	(3) Other municipal employment	(4) Stockholm host
Age	32.96	21.00	41.52	40.25
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	0.50	0.29
Some college education	0.18	0.05	0.23	0.11
No college education	0.77	0.86	0.74	0.82
Education unknown	0.05	0.08	0.02	0.08
Foreign born	0.62	0.51	0.79	0.78
0–2 yrs since immigration	0.14	0.15	0.03	0.23
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	0.01
Born in W. Asia or N. Africa	0.19	0.14	0.22	0.08
Born in Africa, excl. NA	0.21	0.24	0.36	0.55
Other country of birth	0.15	0.09	0.14	0.11
Own initiative to be registered	0.06	0.18	0.23	0.01
Quarter at PES, at JC reg.	3.63	1.87	13.58	7.48
Earnings t-24, SEK 1,000	50.72	25.58	26.62	33.75
SA, nr of months t-24	6.15	5.19	15.69	8.36
Psychotropic drug prescribed t-12	0.20	0.12	0.17	0.12
Pain rel. drug prescribed t-12	0.16	0.09	0.25	0.14
Hospital visit t-12	0.10	0.08	0.09	0.08
No. of observations	22,012	965	396	206

Note: $t-24$ refers to 24 months prior to registration and $t-12$ refers to 12 months prior to registration. Individuals may register several times and the observations in column (1) correspond to 17,659 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

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 the *Youth employment program* for natural reasons). Figure 3 shows how long participants remain in a *Stockholm job*.[31] Most participants stay for the whole planned duration of the program (6 months for the *Youth employment program* 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 employment programs* are not prolonged for the possible additional 6 months. Next, we turn to the empirical strategy and explain how we go about estimating the causal effects of the program.

[31] 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 lasted 12 months.

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

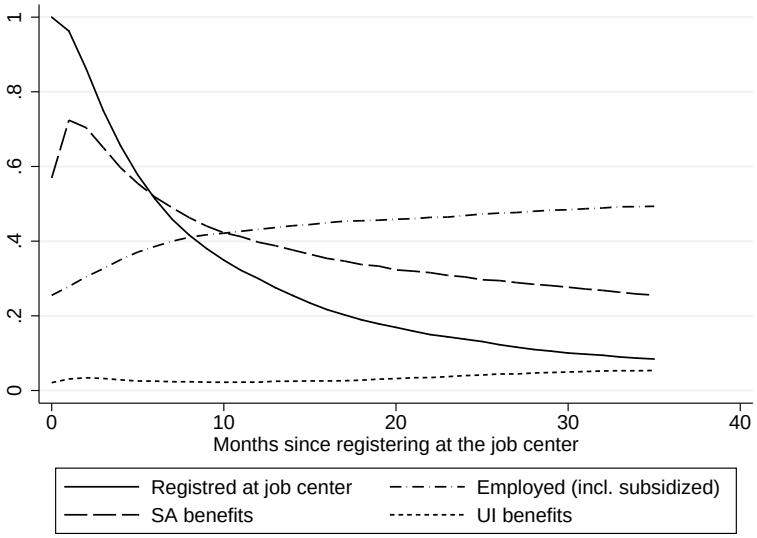


Figure 2: Time registered at the job center/at the Public employment service (PSE) before program start

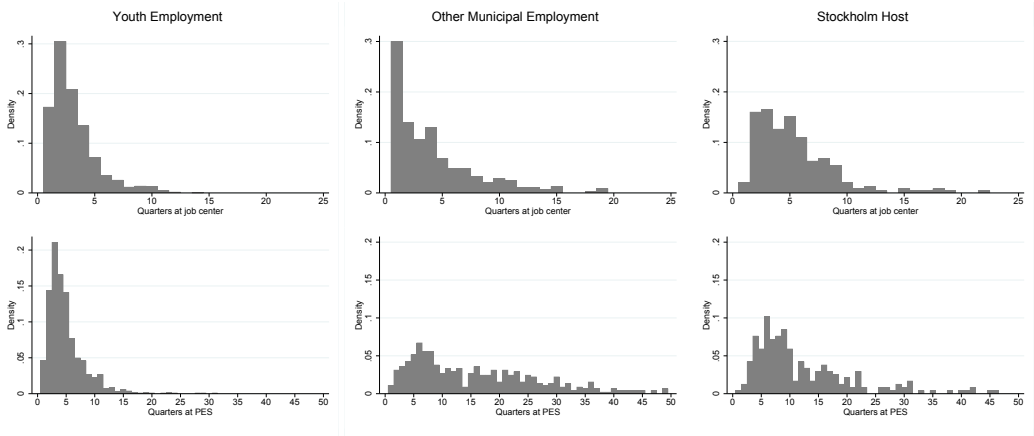
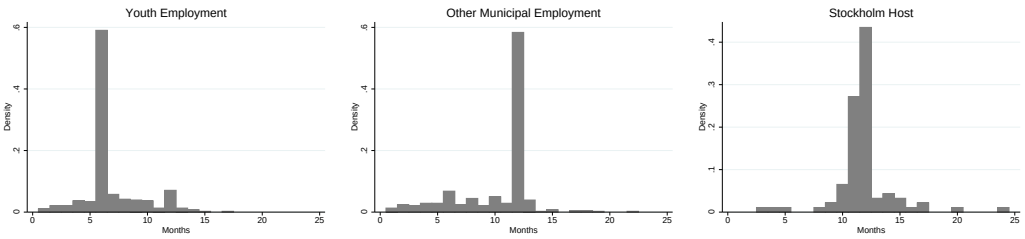


Figure 3: Duration of *Stockholm jobs* by program type



Note: Displayed for observations where end date is registered, which was rare before 2014.

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.[32] Previous literature (Heckman et al., 1998; Lechner and Wunsch, 2013; Biewen et al., 2014; Caliendo et al., 2017) 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.

As discussed in Section 3, we have access to this information. 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. Together, this information is very similar to the information available to the caseworker at the job center. However, 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

[32] 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.

Stockholm he or she lives.

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. 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. 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 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 register at the job center. In the language of Van den Berg and Vikström, we denote being registered 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 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".[33] The average treatment effect of the treated ($ATET$), when assigned to treatment at t_s compared to never being treated is then given by

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

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

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

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

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

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

where

$$p(t, X) = Pr(T_s = t | T_s \geq t, T_u \geq t, X) \quad (4)$$

and

$$\rho_t = Pr(T_s = t | T_s \geq t, T_u \geq t) \quad (5)$$

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 non-treated individuals is 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

$$\widehat{ATET}(t_s) = \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \frac{1}{\rho_{t_s} N_{t_s}} \left[\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i}) \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \right.$$

$$\sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} \Bigg] \tag{6}$$

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

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

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 $N_t / \sum_{m=1}^{T_u^{max}} N_m$. To obtain standard errors, we bootstrap with 99 replications.

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.[34] 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 job* and the pattern on 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 registered 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 employment*, there are fewer individuals taking part in this program compared to *Youth employment*, and most participants enter already in their first quarter at the job center. We thus we define $t_s = [1]$ as quarters 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. Table A.1 in Appendix A displays the number of treated individuals for each program and assignment period.[35]

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, given the relatively limited number of treated individuals in each assignment period and the data-intensive bootstrap-procedure, we limit the set of covariates in our main analysis and control for the following set of confounders: age, schooling, own initiative to register at the job center, previous labor market attachment and SA usage.[36] Propensity scores are estimated

[34] 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).

[35] When estimating the weights, we also consider a seventh/fifth)′′ period where we aggregate all participants who enter a *Youth employment*/ *Other municipal employment* or *Stockholm host* after more than two years.

[36] This set of controls was chosen to achieve similar patterns for participants and their weighted controls in the outcomes of interest before participants entered the program. See column (1) in Tables A.3–A.5 for a list of the variables included. In Section 4.1, we test for the robustness of including more extensive sets of confounders.

using logistic regression models for each type of *Stockholm job* and for each 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.[37]

Before estimating ATET, we need to impute fictitious start dates for individuals in the control group. We do this by for each type of *Stockholm job* and time of assignment, draw a date with replacement from the pool of start dates for the treated individuals.[38] 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,[39] 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. Observations that are later treated will be excluded from that follow-up month. Observations with a (simulated) treatment date after 2016 are also excluded once the weights have been calculated.[40]

Since the different types of *Stockholm jobs* have different target groups, we have restricted our estimation samples accordingly. This implies that when estimating the effects for *Youth employment*, the sample is restricted to those younger than 30. When it comes to *Other municipal employment*, the sample is restricted to those with a simulated start date in May 2012 or later (since this is the first month that this type of *Stockholm job* was used). Finally, for *Stockholm host*, we exclude individuals younger than 25.

5 Results

Stockholm jobs are intended to offer participants labor market experience and labor market contacts, thereby increasing their future employment chances. If the program works as intended, we thus expect to find positive effects on employment and earnings and negative effects on social assistance receipt once the *Stockholm job* has ended.[41] As being employed in a *Stockholm job* makes the participants entitled to UI benefits, we also expect a positive effect on uptake of UI benefits. In this section, we present the results from the main analysis for each program type separately. Thereafter, we present a number of sensitivity analyses, to test the robustness of our results. We next analyze how different types of health outcomes are affected by program participation.

[37] It turns out that this constraint is only binding for *Other employment*, see Tables A.9–A.11 in Appendix A. Propensity score estimates (Tables A.6–A.8), descriptive statistics over means of treated and weighted controls in each period (Tables A.12–A.14), as well as normalized differences for the covariates included in the propensity scores specification before and after weighting (Tables A.15–A.17) are shown in Appendix A.

[38] Figures B.3–B.5 in Appendix B shows the resulting distributions of actual and simulated start dates.

[39] 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

[40] The number of excluded individuals at each follow up month (pooled over the periods) is shown in Tables A.9–A.11 in Appendix A.

[41] A typical finding in the literature evaluating ALMPs is that these have lock-in effects. Since our data does not always allow us to separate between employment in a *Stockholm job* and other types of employment, we do not investigate lock-in effects.

5.1 Results: Youth employment

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 left 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 right panel shows the ATET in each month relative to program start as well as 95-percent confidence intervals.[42] 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.[43]

Once the program starts, the share of employed individuals (top 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, employment is still higher in the treatment group compared to the control group. The right panel shows a statistically significant effect on the likelihood of being employed corresponding to 40 percentage points higher among former participants than among the control group in month 7 and corresponding to 30 percentage points in month 8. The effect decreases somewhat over time but stabilizes at about 10 percentage points around month 18.

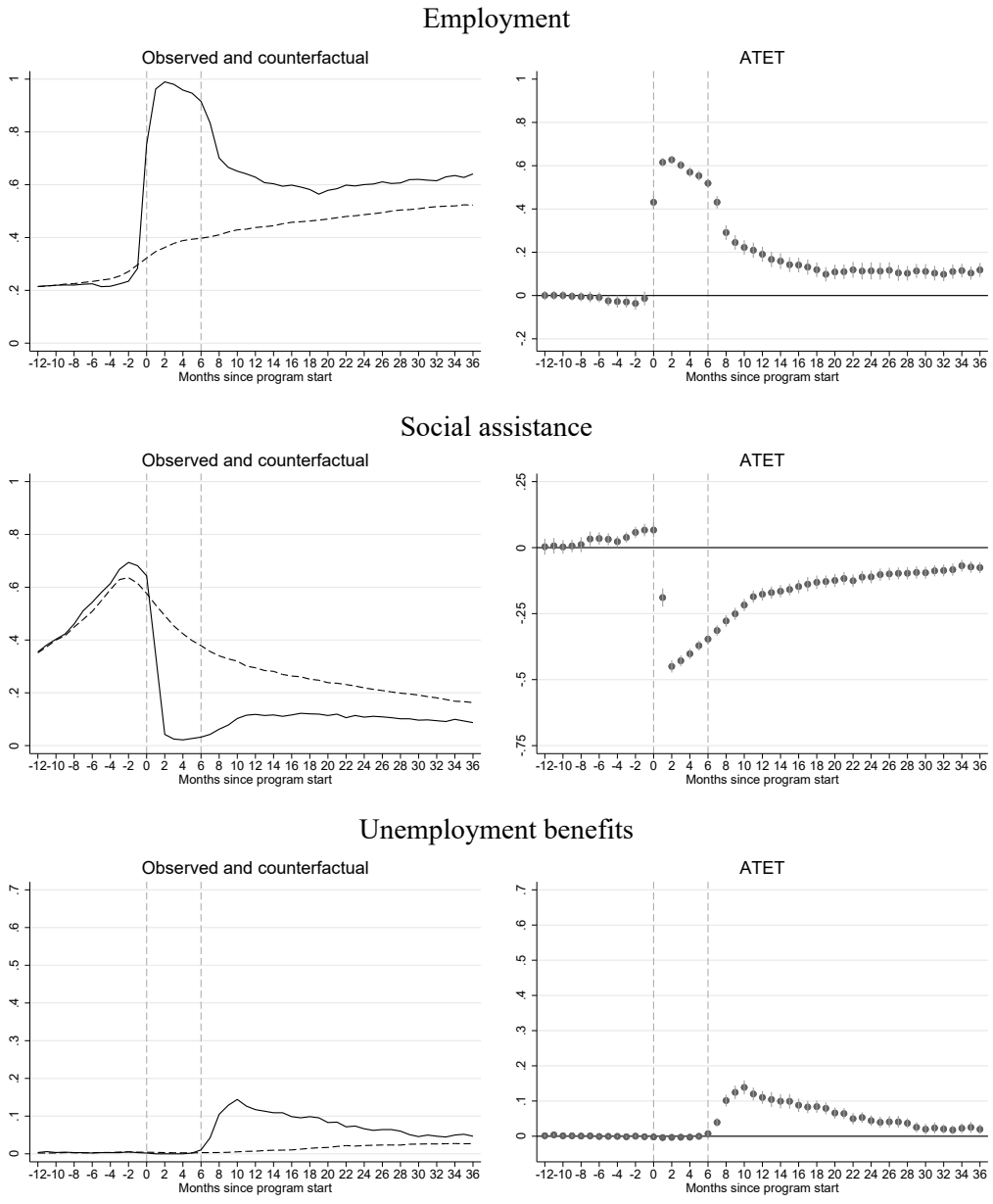
There is a corresponding mechanical sharp drop in the share receiving SA the first two months after individuals enter the program.[44] Whereas the share receiving SA decreases gradually in the control group, it increases somewhat after the first initial drop among the treated. However, there is a negative 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 (bottom 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 than among non-participants.

[42] A regression table corresponding to the right panel in Figure 4 is available in Table A.18 in Appendix A.

[43] As mentioned Section 4.3, the explanation to why effects are negative for employment and positive for SA the months just before program start, is that some individuals in the control group may already have left the job center at the time of their simulated start date. Excluding those individuals, the ATETs for these months are closer to zero, see Figure B.6 in Appendix B.

[44] The fact that the share does not drop to zero can be explained that for some (large) households, the salary received may not be sufficient to reach the stipulated benefit level.

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



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. The weights in the pre-period (-12 to 0) are based on the weights at time 1.

Table 3 shows the cumulative effects on the number of months employed, receiving any SA and UI benefits as well as earnings and amounts received from SA and UI benefits, 13–36 months after program start.[45] According to the estimates, participating in *Youth employment* increases employment during these two years by approximately 2.7 months, reduces the number of months with any SA by 2.6 and increases the number of months with any UI benefit by 1.2 months.[46] These effects correspond to a 23-percent increase in employment and a 50-percent decrease in the likelihood of receiving any SA compared to the averages in the (weighted) controls.[47] Total earnings increase by SEK 39,600 (23 percent) during the same period, whereas the amount received in SA decreases by SEK 16,000 (53 percent) and the amount received in UI benefits increase by SEK 4,550 (137 percent). If we add these numbers together, the resulting figure is a SEK 28,000 higher income on average over two years for those that took part in the *Youth employment* program.

Table 3: Cumulative ATET: Youth employment

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 13–36 after program start			
ATET	2.76	-2.61	1.24
St err	.347	.227	.118
Mean	11.8	5.18	.49
	Earnings (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 13–36 after program start			
ATET	39,597	-16,021	4,549
St err	7,638	1,514	594
Mean	172,785	30,439	3,328

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

5.2 Results: Other municipal employment

Other municipal employments last for twelve months, take place at regular workplaces and are targeted at SA-recipients older than 29. Figure 5 shows the evolution of outcomes and estimated effects for this program.[48]

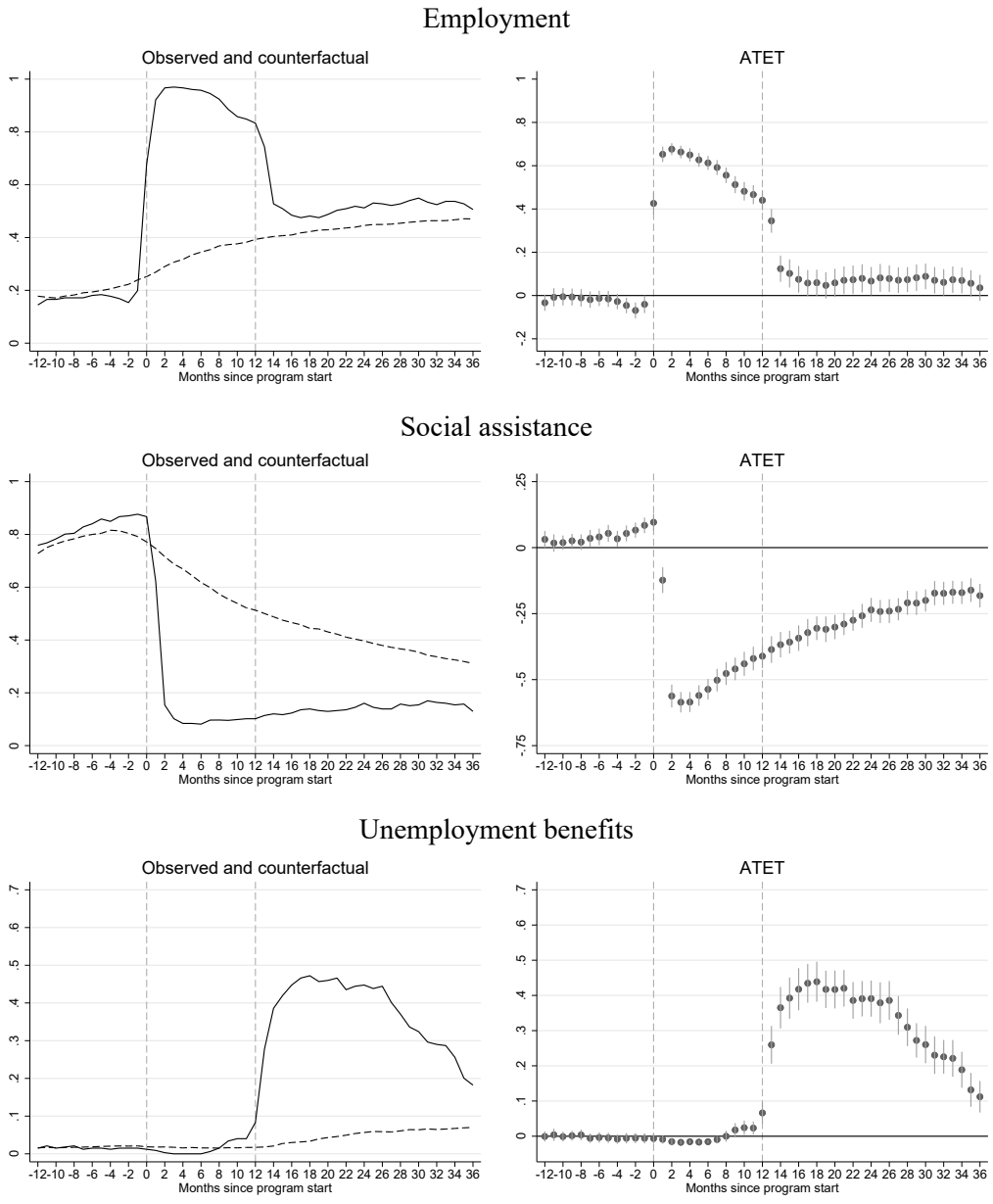
[45] Table A.21 in Appendix A shows the cumulative effects for other follow-up periods.

[46] If we instead use a stricter definition of employment, requiring individuals to have earnings larger than one income base amount, the corresponding increase in the number of months employed is 2.3 months, to be compared with a mean of 9.5 for the (weighed) controls.

[47] Since very few in the latter group receive any UI benefit, it does not make sense to calculate the relative increase for this outcome.

[48] A regression table corresponding to the right panel in Figure 5 is available in Table A.19 in Appendix A

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



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. The weights in the pre-period (-12 to 0) are based on the weights at time 1.

Once the program starts, employment goes up and the share receiving SA goes down, as expected. When the program ends after one year, the share of employed individuals among former participants goes down but remains higher compared to their (weighted) controls. The ATET is 34.5 percentage points in month 13, 8 percentage points in month 25 and 3.5 in month 36. The share receiving SA increases somewhat once the program ends but remains at a lower level compared to the share among the weighted controls. In months 13, the ATET is 38.5 percentage point, in month 25 it has gone down to 24 percentage points and in month 36 to 18 percentage points.

Turning to the share receiving UI benefits, there is a sharp increase among former participants once the program ends and the corresponding ATET is 26 percentage points. This effect increases the following months, reaching a maximum of 42 percentage points in month 21. 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 4 shows the cumulative effects on number of months (top panel) and amounts (bottom panel) for the two years after the program ended.[49] By participating in the program, individuals gain 1.8 months in employment and SEK 23,700 in earnings.[50] These effects correspond to increases of around 13–17 percent compared to those in the control group. The number of months with SA decreases by 5.8, corresponding to a decrease of 63 percent, whereas the amount received decreases by 70 percent. The increase in the number of months with any UI is 7.8 months and the corresponding amount is SEK 33,000. 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 employment* results in SEK 12,500 more in income.

Table 4: Cumulative ATET: Other employment.

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 13–36 after program start			
ATET	1.84	-5.83	7.77
St err	.623	.449	.453
Mean	10.8	9.24	1.24
	Earnings (SEK)	**SA receipt (SEK)**	**UI benefit receipt (SEK)**
Months 13–36 after program start			
ATET	23,700	-44,119	32,966
St err	13,650	3,204	2,220
Mean	182,850	62,918	8,242

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

[49] Table A.22 in Appendix A shows the cumulative effects for other follow-up periods.

[50] If we instead use a stricter definition of employment, requiring individuals to have earnings larger than one income base amount, the corresponding increase in the number of months employed is 1.5 months, to be compared with a mean of 9.3 for the (weighed) controls.

5.3 Results: 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.[51]

As opposed to the findings for the other two *Stockholm jobs*, the share of employed individuals among former program participants drops to a level below the corresponding share for non-participants once the program has ended. The negative employment effect is the largest in month 26 reaching -14.4 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 is around 15 percentage points in month 36. There is a large increase in the share receiving UI benefits once the program ends, but this share drops dramatically over the following two years. The ATETs are 47 percentage points in month 13, 44 percentage points in month 25 and 5.6 percentage points in month 36.

The negative employment effects are also visible in Table 5, which shows the cumulative effects of participating in the program.[52] During the follow-up period, former participants are employed 1.8 fewer months (a 20-percent decrease) and earn SEK 41,000 less (a 27 percent decrease) compared to non-participants.[53] Participating in the program reduces the number of months receiving SA by 5.2 (63 percent) and the amount received by SEK 41,800 (70 percent). The time receiving UI benefits increases by 9 months and SEK 42,700. Taken together, income is SEK 40,100 lower for participants compared to non-participants during these two years.

Table 5: Cumulative ATET: Stockholm host

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 13–36 after program start			
ATET	-1.8	-5.24	8.95
St err	.609	.601	.455
Mean	9.09	10.3	1.03
	Earnings (SEK)	**SA receipt (SEK)**	**UI benefit receipt (SEK)**
Months 13–36 after program start			
ATET	-41,041	-41,839	42,726
St err	12,262	3,836	2,743
Mean	150,315	67,195	6,694

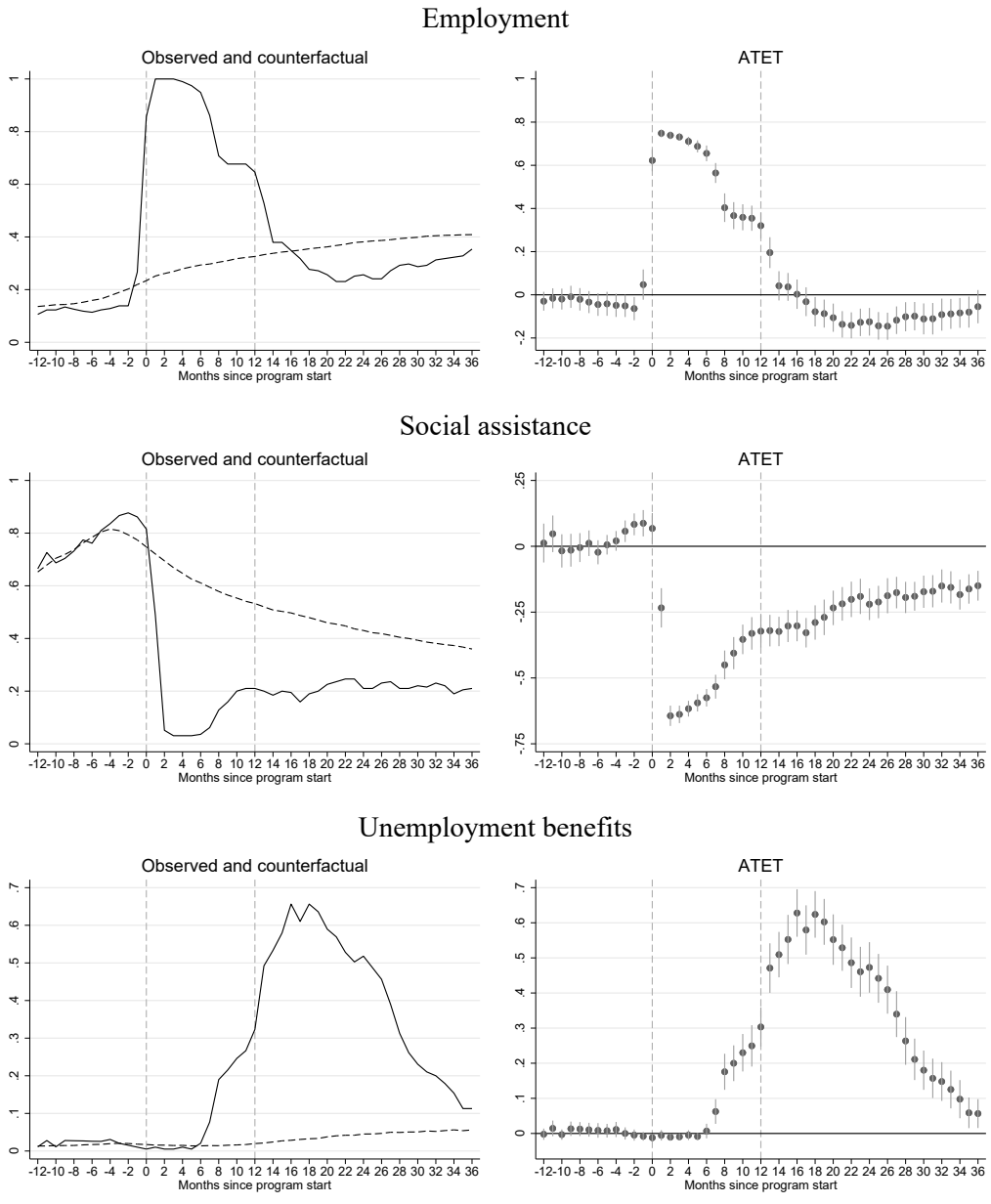
Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 replications.

[51] A regression table corresponding to the right panel in Figure 6 is available in Table A.20 in Appendix A.

[52] Table A.23 in Appendix A shows the cumulative effects for other follow-up periods.

[53] If we instead use a stricter definition of employment, requiring individuals to have earnings larger than one income base amount, the corresponding decrease in the number of months employed is 1.3 months, to be compared with a mean of 7.8 for the (weighed) controls.

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



Note: Solid line indicates treated group while dashed line indicates weighted control group. 95% CI based on 99 bootstrap replications. The weights in the pre-period (-12 to 0) are based on the weights at time 1.

5.4 Sensitivity analyses

We conduct a number of sensitivity analyses. First, we include a larger set of confounders in the propensity score matching. Second, we use shorter time intervals when aggregating over assignment periods. Third, we use different weights when estimating pre-program effects. Below, we summarize the results from these 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 local 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.[54] As is clear from Figure B.7 in Appendix B, the estimated ATETs are more or less identical for all these different sets of confounders.

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 with the cost of having fewer participants entering the program at each assignment time.[55] It turns out that our results are insensitive to the length of the time periods, see Figure B.8 in Appendix B.

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 the periods when participants enter the program. In the main analysis, we apply the weights from month 1 for this 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.9 in Appendix B.

5.5 Results: Health outcomes

Participating in the program may also affect 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.[56] In addition, an increased income gives individuals opportunities to invest in their health, and may reduce negative stress associated with living with limited resources. Finally, not having to rely on SA and undergo the monthly means testing may also reduce this type of negative stress. We do not have access to any self-reported health or well-being from the participants. We do, however, have access to administrative data on medical prescriptions and hospitalizations, and in this section, we analyze how these outcomes are affected by the program. We focus on medical prescriptions for pain relief, psychiatric drugs and hospitalization for any cause.

Table 6 shows cumulative ATETs for the three health outcomes 13–36 months after program

[54] See Tables A.3–A.5 in Appendix A for information on the variables included.

[55] Table A.2 in Appendix A shows how we define assignment periods as well as the number of treated individuals within each assignment period.

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

start, as well as ”placebo”effects for months -12 to -1 (i.e., the year preceding entering the *Stockholm job*). The pre-effects are all very close to zero, indicating that we do control for all important differences between participants and non-participants.[57] The post-program effects are negative and considerably larger than the placebo effects for all outcomes and employment types expect hospitalization among former *Stockholm hosts*, indicating that having a *Stockholm job* is beneficial for well-being. However, standard errors are relatively large, implying that in many cases, we cannot reject the null hypothesis that the point estimates are zero. Focusing on the most significant effects, from an economic as well as statistical point of view, we conclude that former participants in *Other employment* are less likely (30 percent) to have a prescription for psychiatric drugs than non-participants and that former participants in *Stockholm hosts* are less likely (21.5 percent) to have a prescription for pain relief compared to non-participants.

Table 6: Cumulative ATET: Health outcomes

	Prescription:		
	Any pain relief	Any psychiatric	Hospitalization
Youth Employment		Months -12–0	
ATET	.00108	-.00188	.00152
St err	.00201	.00246	.00214
Mean	.0985	.132	.0649
		Months 13–36	
ATET	-.0124	-.00962	-.016
St err	.0147	.015	.0141
Mean	.192	.22	.184
Other Employment		Months -12–0	
ATET	-.000319	.000291	-.00578
St err	.00691	.00592	.00503
Mean	.246	.184	.0733
		Months 13–36	
ATET	-.0152	-.0844	-.0205
St err	.0293	.0228	.0183
Mean	.331	.281	.143
Stockholm hosts		Months -12–0	
ATET	.000478	.00112	-.00149
St err	.00334	.00838	.00396
Mean	.143	.148	.125
		Months 13–36	
ATET	-.0723	-.0329	.0187
St err	.0288	.0256	.0281
Mean	.335	.249	.172

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 99 (97 for *Stockholm hosts*) replications.

[57] 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. Adding these variables implies that two of the bootstrap replications for *Stockholm hosts* fail. The standard errors for this employment type are thus based on 97 replications.

6 Concluding discussion

In this paper, we study three different types of temporary municipal employment targeted at unemployed social assistance 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 access to networks and labor market experiences, 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 for *Stockholm jobs* that take place at a regular workplace. This is true for both *Youth employment*, which is targeted at youths and lasts for six months, and *Other municipal employment*, which is targeted at SA recipients older than 29 and lasts for twelve months. For the former group, on average, we find that program participation results in almost three more months of employment and 23 percent higher earnings during month 13–36 after the program started. For the latter group, the effects are 1.8 months more in employment and 13 percent higher earnings during the same time period. However, the result for the third program, *Stockholm hosts*, which takes place at a workplace created especially for the group, are less promising. Being a *Stockholm host* leads to 1.8 fewer months of employment and reduces earnings by 27 percent.

Having a temporary municipal employment reduces the likelihood of receiving SA after the program has ended. In absolute terms, this effects is smallest for *Youth employment* (2.6 fewer months with SA during month 13–36 after the program started). The corresponding figures for *Other municipal employment* and *Stockholm host* are 5.8 and 5.2 months respectively. Measures in relative terms, the effects are similar in size across the three programs (an increase in 50–60 percent). For all three employment types, the decrease in the share receiving SA is counteracted by an increase in the share receiving UI. This effect is most pronounced for *Stockholm hosts* and least for *Youth employment*. For the former, the number of months receiving UI benefits increases by 9 (from a baseline of 1) during month 13–36 after the program started. Hence, it is clear that the program to some extent worked as a way to transfer SA-recipients to UI benefits.

Taken together, former participants in both *Youth employment* and *Other employment* increase their aggregate income from earnings, SA and UI benefits, by SEK 28,000 and SEK 12,000 respectively. Compared to their (weighted) controls, this corresponds to an increase by 14–15 percent during months 13–36 after program start. Former participants in *Stockholm hosts*, however, loses SEK 40,000 (18 percent) by taking part in the program. On the positive side, there are some indications that health, measured by a decrease in medical prescriptions, improves among former *Stockholm hosts*, as for former participants in *Other employment*.

There may be several explanations for the less promising employment effects found for former participants of *Stockholm hosts*. One 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 employment*, even if there are some indications that the latter group would have done somewhat better in the labor market had they not taken part in the program.[58]

[58] Comparing the counterfactual outcomes (during months 13–36), measured by how well the participants' (weighted) controls did, we conclude that participants in *Youth employment* are positively selected with respect to the number of months employed (11.8 compared to 10.8 and 9.1) and with SA (5.2 compared to 9.2 and 10.3), whereas participants

A more probable explanation is the type of workplace. Both *Youth employment* and *Other employment* take place at real workplaces that typically have a shortage of personnel. *Stockholm hosts*, on the other hand, takes place at a workplace where there is no possibility of getting employed after the program ends.[59] It is therefore likely that the skills and experiences that participants acquire at the two former workplaces are more valued in the labor market than those required at the latter. In addition, working at a regular workplace can also provide participants with valuable networks, which can convey useful knowledge on how and where to apply for work after the program ends. The manager can also keep the former participant in mind as he or she hires new staff in the future, or recommend the participant to other managers. Having had a *Stockholm job* can also lead to positive labor market outcomes for participants if it signals willingness and ability to maintain an employment. It is not clear that this will affect the program types differently, but experience from a regular work place could be a stronger signal to employers. Finally, it may also be the job search assistance provided by caseworkers toward the end of the temporary employment that are crucial. However, this support is expected to be similar across program types, and it is therefor not obvious how it could explain the differing employment outcomes.

In relation to previous evaluations of public sector employment programs, our findings are more promising than the German and Belgian evidence for *Temporary extra jobs* and *Social employment* 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 provided tailored activation to hard-to-employ SA recipients. The latter combined activation, job practice with generous non-means-tested benefits and has been shown to raise employment among participants by 18 percentage points four years after entering the program (Markussen and Røed, 2016). Our findings are also 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).[60]

For most individuals, having a *Stockholm job* is likely to have improved their well-being. Income from earnings and UI benefits increase more than income from SA decreases and is not means tested. However, this is not true for *Stockholm host* participants. In addition to this, if anything, we find indications of positive effects on the participants health. It is also possible that individuals' general satisfaction increases thanks to the program. By becoming eligible for UI benefits, the individual is also more likely to take part in active labor market programs implemented by the Public Employment Service 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

in *Other employment* are positively selected with respect to earnings (SEK 183,000 compared to SEK 173,000 (*Youth employment*) and SEK 150,000 (*Stockholm hosts*)).

[59]The closest type of job is probably a janitor, an occupation that, according to the Swedish Public Employment Service, is one of those involving the toughest competition among professions with the shortest education (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).

[60]One potential unintended consequence of providing temporary public employment in regular workplaces, is that it crowds out other employment. Even if participants in all *Stockholm jobs* are supposed to perform tasks outside the scope of the regular tasks, this might be hard to monitor. Due to the weak labor market attachment of the participants, crowding-out effects can be acceptable given that the program increases labor force participation among participants.

and Langenskiöld, 2008).

To conclude, we mainly find positive effects of the temporary public employment we evaluate in this paper in terms of increased employment and reduced SA recipiency. This is promising for this group of marginalized unemployed individuals with a weak labor market attachment. However, the type of workplace where the participants get their temporary employment is important; being employed at a regular workplace in a sector seems crucial for future employment prospects. In addition, we find that the program transfers individuals from SA to UI benefits, hence indicating cost-shifting from the lower to the central governmental level.

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Appendix A Additional tables

A.1 Program participants by assignment period

Table A.1: Program participants, per assignment period. Main analysis

Quarter	Youth employment	Other employment	Stockholm hosts
1	172	136	42
2	305	111	
3	208		65
4	134	90	
5	107		61
6		59	
7	38		27
8			
(> 8)	35	56	30

Note: Program participants at quarter > 8 included in the propensity score estimations but not in the estimations of ATET.

Table A.2: Program participants, per assignment period. Sensitivity analysis

Months	Youth employment	Other employment	Stockholm hosts
1	57	4	136
2	26		
3	94		
4	109	38	63
5	117		
6	84		
7	75	38	48
8	81		
9	54		
10	43	28	47
11	49		
12	42		
13–18	101	61	65
19–24	37	28	37
(> 24)	35	31	57

Note: Program participants at months > 24 included in the propensity score estimations but not in the estimations of ATET.

A.2 Variables included in the propensity score

Table A.3: Variables included in the propensity score estimations: Youth employment

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +LM hist	(7) all	(8) Data-driven		
								Emp	SA	UI
Age 25–29	X		X	X	X	X			X	X
Less than high school	X		X	X	X	X				
Non-western immigrant	X	X	X	X	X	X	X			
Own initiative to be reg	X	X	X	X	X	X	X			
0 quarter at PES, at JC reg	X	X	X	X	X					
Employed in t0-6	X	X	X	X	X	X	X			
SA in t-1	X	X	X	X	X	X	X			
Age 21–23		X					X			
Age 24–26		X					X			
Age 27–29		X					X			
Female		X					X			
High school		X					X			
Some college education		X					X			
0–2 yrs since immigration		X					X			
3–5 yrs since immigration		X					X			
Jobbtorg unga Globen			X				X			
Jobbtorg Skärholmen			X				X			
Jobbtorg Kista			X				X	X		X
Jobbtorg Farsta			X				X			
Jobbtorg City			X				X			
Year 2011				X			X			
Year 2012				X			X			
Year 2013				X			X			
Year 2014				X			X			
Year 2015				X			X			
Year 2016				X			X			
Psychotropic drug prescr. t-12					X		X			
Pain rel. drug prescr. t-12					X		X			
1-2 quarter at PES, at JC reg						X	X			
3-8 quarter at PES, at JC reg						X	X			
$>$ 8 quarter at PES, at JC reg						X	X			
Employed in t0-24						X	X			
1–12 months with SA, t-24						X	X			
13–24 months with SA, t-24						X	X			

Note: Variables in column (8) are selected using the algorithm proposed by de Luna et al. (2011).

Table A.4: Variables included in the propensity score estimations: Other employment

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +LM hist	(7) all	(8) Data-driven		
								Emp	SA	UI
Age 18–29	X		X	X	X	X		X	X	X
Age 30–39	X		X	X	X	X				
Age 40–49	X	X	X	X	X	X	X	X	X	
Employed in t0-6	X	X	X	X	X	X	X			
Subsidized empl in t0-6	X	X	X	X	X	X	X	X	X	X
Reason for SA, unempl	X	X	X	X	X	X	X	X	X	
SA, nr of months t-24	X	X	X	X	X	X	X	X	X	
SA in t-1	X	X	X	X	X	X	X			
Own initiative to be reg	X	X	X	X	X	X	X		X	
Age 25–29		X					X			
Age 30–39		X					X			
Age 50–		X					X			
Female		X					X			
Married		X					X			
Child in household		X					X			
High school		X					X			
Some college education		X					X			
0–2 yrs since immigration		X					X			
3–5 yrs since immigration		X					X			
Non-western immigrant		X					X			
Jobbtorg unga Globen			X							
Jobbtorg Skärholmen			X				X			
Jobbtorg Kista			X				X		X	
Jobbtorg Farsta			X				X			
Jobbtorg City			X				X			
Year 2014				X			X			
Year≥2015				X			X			
Psychotropic drug prescr. t-12					X		X			
Pain rel. drug prescr. t-12					X		X			
1–2 quarter at PES, at JC reg						X	X			
3–8 quarter at PES, at JC reg						X	X			
$>$ 8 quarter at PES, at JC reg						X	X			
Employed in t0-24						X	X			
1–12 months with SA, t-24						X	X			
13–24 months with SA, t-24						X	X			
SA in t-2								X	X	
JC registration 2015									X	
Log earnings, t0-24, SEK 1,000								X	X	X
Log earnings, t-24, SEK 1,000								X	X	X

Note: Variables in column (8) are selected using the algorithm proposed by de Luna et al. (2011).

Table A.5: Variables included in the propensity score estimations: Stockholm host

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +labor market hist	(7) all
Age 50-	X	X	X	X	X	X	X
Less than high school	X		X	X	X	X	
Non-western immigrant	X	X	X	X	X	X	X
Own initiative to be registered	X	X	X	X	X	X	X
0 quarter at PES, at JC reg	X	X	X	X	X		
Employed in t0-6	X	X	X	X	X	X	X
SA in t-1	X	X	X	X	X	X	X
Age 30–39		X					X
Age 40–49		X					X
Female		X					X
High school		X					X
Some college education		X					X
0–2 yrs since immigration		X					X
3–5 yrs since immigration		X					X
Jobbtorg unga Globen			X				X
Jobbtorg Skärholmen			X				X
Jobbtorg Kista			X				X
Jobbtorg Farsta			X				X
Jobbtorg City			X				X
Year 2011				X			X
Year 2012				X			X
Year 2013				X			X
Year 2014				X			X
Year 2015				X			X
Year 2016				X			X
Psychotropic drug prescr. t-12					X		X
Pain rel. drug prescr. t-12					X		X
1–2 quarter at PES, at JC reg						X	X
3–8 quarter at PES, at JC reg						X	X
$>$ 8 quarter at PES, at JC reg						X	X
Employed in t0-24						X	X
1–12 months with SA, t-24						X	X
13–24 months with SA, t-24						X	X

A.3 Propensity score estimations

Table A.7: Propensity score estimates by assignment period: Other employment

	(1) Quarter 1	(2) Quarter 2–3	(3) Quarter 4–5	(4) Quarter 6–8	(5) Quarter > 8
Age 18–29	0.204	-1.105**	-1.610***	-0.0446	0
	(0.353)	(0.384)	(0.470)	(0.467)	(.)
Age 30–39	-0.131	-0.320	-0.0341	0.432	0.211
	(0.354)	(0.284)	(0.307)	(0.365)	(0.373)
Age 40–49	0.585*	0.241	0.178	-0.258	0.225
	(0.288)	(0.252)	(0.304)	(0.423)	(0.370)
Employed in t0-6	-0.663	-0.376	-0.629	-0.194	0.123
	(0.759)	(0.538)	(0.531)	(0.536)	(0.490)
Subsidized empl in t0-6	2.665***	2.197***	1.369***	0.776*	0.689
	(0.288)	(0.220)	(0.232)	(0.309)	(0.362)
Reason for SA, unemployment	1.079**	0.156	1.079*	1.934**	0.817*
	(0.351)	(0.270)	(0.428)	(0.745)	(0.413)
SA, nr of months t-24	0.123***	0.0559***	-0.0212	0.0518	0.0922*
	(0.0198)	(0.0129)	(0.0168)	(0.0285)	(0.0407)
SA in t-1	0.545	0.291	1.030*	0.447	-0.0926
	(0.340)	(0.333)	(0.430)	(0.544)	(0.533)
Own initiative to be registered	3.587***	1.041*	4.250***	2.981*	0
	(0.257)	(0.489)	(0.521)	(1.256)	(.)
Constant	-9.580***	-6.091***	-5.846***	-7.192***	-5.781***
	(0.549)	(0.435)	(0.580)	(1.007)	(0.963)
Observations	11,440	8,813	4,625	2,783	1,144
Pseudo R^2	0.499	0.183	0.147	0.056	0.036

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: Propensity score estimates by assignment period: Stockholm hosts

	(1) Quarter 1–2	(2) Quarter 3–4	(3) Quarter 5–6	(4) Quarter 7–8	(5) Quarter > 8
Age 50-	1.746***	0.412	0.233	-0.306	-0.0354
	(0.321)	(0.296)	(0.321)	(0.549)	(0.470)
Less than high school	0.647*	-0.129	0.702**	0.812*	0.196
	(0.314)	(0.262)	(0.263)	(0.399)	(0.368)
Non-western immigrant	-0.384	0.267	0.727*	1.519*	0.748
	(0.318)	(0.290)	(0.354)	(0.741)	(0.504)
Own initiative to be registered	3.098***	0	2.892*	0	0
	(0.771)	(.)	(1.138)	(.)	(.)
0 quarter at PES, at JC reg	-1.065	-0.881*	-0.955*	-0.744	0.0826
	(0.603)	(0.403)	(0.405)	(0.498)	(0.386)
Employed in t0-6	-1.483*	-1.283*	-0.105	0	0.0753
	(0.731)	(0.594)	(0.436)	(.)	(0.621)
SA in t-1	0.438	-0.366	0.769	0.128	0.0344
	(0.321)	(0.305)	(0.449)	(0.546)	(0.550)
Constant	-6.486***	-4.481***	-5.697***	-5.856***	-4.382***
	(0.383)	(0.374)	(0.560)	(0.887)	(0.681)
Observations	15,245	7,934	4,308	2,024	1,231
Pseudo R^2	0.100	0.022	0.043	0.052	0.012

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Propensity score estimates by assignment period: Youth employment

	(1) Quarter 1	(2) Quarter 2	(3) Quarter 3	(4) Quarter 4	(5) Quarter 5–6	(6) Quarter 7–8	(7) Quarter >8
Age 25–29	-0.638**	-0.984***	-1.137***	-1.384***	-0.806***	-0.753*	-1.785***
	(0.243)	(0.172)	(0.186)	(0.234)	(0.220)	(0.368)	(0.490)
Less than high school	-0.407*	0.119	-0.296*	-0.245	0.170	0.469	-0.650
	(0.166)	(0.123)	(0.147)	(0.184)	(0.208)	(0.369)	(0.410)
Non-western immigrant	-0.116	-0.104	-0.0897	-0.211	0.544*	0.0274	1.139*
	(0.170)	(0.123)	(0.148)	(0.186)	(0.228)	(0.358)	(0.508)
Own initiative to be registered	2.358***	0.958***	0.373	0.667	0.592	0.971	0
	(0.190)	(0.198)	(0.314)	(0.428)	(0.526)	(1.170)	(.)
0 quarter at PES, at JC reg	-0.973***	-0.240	-0.223	-0.317	-0.00825	-0.0925	0.484
	(0.201)	(0.148)	(0.173)	(0.215)	(0.222)	(0.360)	(0.392)
Employed in t0-6	-0.371*	-0.00282	-0.222	-0.404	-0.410	-0.267	0.246
	(0.187)	(0.140)	(0.192)	(0.273)	(0.349)	(0.630)	(0.705)
SA in t-1	0.144	0.243	0.470*	0.893***	0.654*	0.792	1.332*
	(0.195)	(0.158)	(0.186)	(0.248)	(0.264)	(0.499)	(0.657)
Constant	-3.891***	-2.930***	-2.480***	-2.548***	-2.957***	-3.164***	-3.087***
	(0.176)	(0.181)	(0.208)	(0.271)	(0.329)	(0.588)	(0.720)
Observations	9,273	5,847	3,330	2,007	1,262	536	251
Pseudo R^2	0.100	0.035	0.037	0.066	0.038	0.038	0.151

Standard errors in parentheses
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

A.4 Trimming

Table A.9: Number of observations excluded: Youth employment

	Trimmed		Censored	
Month	Treated	Untreated	Treated	Untreated
1	0	0	0	474
2	0	0	0	651
3	0	0	0	922
4	0	0	0	1,097
5	0	0	0	1,220
6	0	0	0	1,409
7	0	0	0	1,516
8	0	0	0	1,607
9	0	0	0	1,697
10	0	0	0	1,760
11	0	0	0	1,825
12	0	0	0	1,900
13	0	0	0	1,947
14	0	0	0	1,999
15	0	0	0	2,032
16	0	0	0	2,079
17	0	0	0	2,108
18	0	0	0	2,152
19	0	0	0	2,182
20	0	0	0	2,209
21	0	0	0	2,242
22	0	0	0	2,268
23	0	0	0	2,283
24	0	0	0	2,304
25	0	0	0	2,315
26	0	0	0	2,326
27	0	0	0	2,341
28	0	0	0	2,356
29	0	0	0	2,364
30	0	0	0	2,375
31	0	0	0	2,388
32	0	0	0	2,406
33	0	0	0	2,418
34	0	0	0	2,425
35	0	0	0	2,436
36	0	0	0	2,444

Note: Trimmed observations have weights larger than 1 percent of the sum of weights for the controls as suggested by Huber et al. (2013). Censored observations have simulated start dates after December 31, 2016, or participate in a *Stockholm job* in this follow-up month.

Table A.10: Number of observations excluded: Other employment

	Trimmed		Censored	
Month	Treated	Untreated	Treated	Untreated
1	64	16	0	326
2	64	16	0	356
3	63	15	0	464
4	63	15	0	507
5	63	15	0	549
6	64	13	0	600
7	65	12	0	621
8	66	10	0	655
9	72	5	0	734
10	72	5	0	758
11	72	5	0	783
12	72	5	0	828
13	72	5	0	870
14	72	5	0	904
15	72	5	0	958
16	72	5	0	980
17	72	5	0	1,011
18	72	5	0	1,041
19	72	5	0	1,061
20	72	5	0	1,090
21	72	5	0	1,117
22	72	5	0	1,155
23	72	5	0	1,176
24	72	5	0	1,226
25	72	5	0	1,255
26	72	5	0	1,278
27	72	5	0	1,307
28	72	5	0	1,336
29	72	5	0	1,371
30	72	5	0	1,389
31	72	5	0	1,415
32	72	5	0	1,443
33	72	5	0	1,471
34	72	5	0	1,494
35	72	5	0	1,516
36	72	5	0	1,538

Note: Trimmed observations have weights larger than 1 percent of the sum of weights for the controls as suggested by Huber et al. (2013). Censored observations have simulated start dates after December 31, 2016, or participate in a *Stockholm job* in this follow-up month.

Table A.11: Number of observations excluded: Stockholm hosts

	Trimmed		Censored	
Month	Treated	Untreated	Treated	Untreated
1	0	0	1	385
2	0	0	1	418
3	0	0	1	443
4	0	0	1	473
5	0	0	1	504
6	0	0	1	540
7	0	0	1	578
8	0	0	1	609
9	0	0	1	632
10	0	0	1	650
11	0	0	1	680
12	0	0	1	707
13	0	0	1	728
14	0	0	1	747
15	0	0	1	770
16	0	0	1	790
17	0	0	1	811
18	0	0	1	827
19	0	0	1	863
20	0	0	1	881
21	0	0	1	901
22	0	0	1	923
23	0	0	1	940
24	0	0	1	960
25	0	0	1	970
26	0	0	1	981
27	0	0	1	997
28	0	0	1	1,010
29	0	0	1	1,022
30	0	0	1	1,035
31	0	0	1	1,053
32	0	0	1	1,060
33	0	0	1	1,071
34	0	0	1	1,088
35	0	0	1	1,098
36	0	0	1	1,114

Note: Trimmed observations have weights larger than 1 percent of the sum of weights for the controls as suggested by Huber et al. (2013). Censored observations have simulated start dates after December 31, 2016, or participate in a *Stockholm job* in this follow-up month.

A.5 Balance

Table A.12: Means for participants and (weighted) controls by assignment period: Youth employment

	Quarter 1		Quarter 2		Quarter 3		Quarter 4		Quarter 5–6		Quarter 7–8	
	Part.	Contr.	Part.	Contr.	Part.	Contr.	Part.	Contr.	Part.	Contr.	Part.	Contr.
Age	20.8	20.5	20.9	21.2	21.8	22	21.7	22.2	22.9	23.4	23.2	24.3
Female	.308	.389	.42	.44	.404	.471	.5	.478	.542	.51	.658	.53
Married	.11	.0918	.125	.133	.173	.171	.172	.178	.308	.273	.211	.273
Child in household	.395	.371	.374	.349	.385	.322	.336	.309	.393	.313	.342	.288
Some college education	.0291	.0548	.0459	.0567	.0385	.0779	.0896	.0721	.0935	.0822	.0526	.0637
No college education	.936	.852	.902	.878	.933	.853	.888	.873	.888	.882	.947	.923
Education unknown	.0349	.0936	.0525	.0652	.0288	.0692	.0224	.0554	.0187	.036	0	.013
Foreign born	.401	.396	.462	.46	.514	.528	.507	.521	.729	.725	.684	.668
0–2 yrs since immigration	.0523	.108	.0951	.135	.125	.169	.142	.162	.243	.227	.211	.208
3–5 yrs since immigration	.0698	.0844	.134	.106	.173	.139	.172	.149	.196	.232	.132	.23
Born in Nordics or W. Europe	.0349	.0333	.0262	.0261	.00962	.0263	.00746	.0258	.0187	.0156	.0526	.0224
Born in E. Europe or C. Asia	.0116	.0189	.0131	.023	.00962	.0237	.0224	.0161	.00935	.0184	0	.0122
Born in W. Asia or N. Africa	.145	.113	.118	.139	.149	.169	.104	.177	.196	.249	.158	.211
Born in Africa, excl. NA	.157	.145	.203	.181	.274	.219	.299	.22	.346	.325	.316	.349
Other country of birth	.0523	.0859	.102	.0908	.0721	.0898	.0746	.0819	.159	.116	.158	.0735
Own initiative to be registered	.465	.463	.2	.2	.0721	.0723	.0597	.0626	.0467	.0411	.0263	.0244
Quarter at PES, at JC reg.	2.45	1.63	1.88	1.52	1.68	1.68	1.69	1.68	1.64	1.71	1.34	2
Earnings t-24, SEK 1,000	40	32.5	23.2	27.8	22.9	20.4	16.6	17.2	15.5	15.8	12.1	11.7
SA, nr of months t-24	3.62	3.82	6.62	6.08	9.22	8.76	9.71	10.9	13.2	12.6	15.6	16.2
Psychotropic drug prescr. t-12	.151	.158	.144	.159	.111	.167	.157	.183	.0654	.185	.105	.239
Pain rel. drug prescr. t-12	.0872	.0759	.0852	.094	.125	.108	.0597	.129	.15	.162	.132	.16
Hospital visit t-12	.064	.0852	.0852	.0872	.0577	.083	.0746	.0808	.0374	.0821	.0263	.082

Note: Weights used for the controls are based on information known 24 months after program start.

Table A.13: Means for participants and (weighted) controls by assignment period: Other employment

	Quarter 1		Quarter 2–3		Quarter 4–5		Quarter 6–8	
	Part.	Contr.	Part.	Contr.	Part.	Contr.	Part.	Contr.
Age	39.2	37.5	42.3	41.8	40.9	40.3	39.9	39
Female	.556	.385	.622	.431	.659	.477	.695	.54
Married	.264	.255	.315	.287	.293	.281	.254	.291
Child in household	.486	.39	.523	.367	.524	.38	.576	.427
Some college education	.222	.157	.198	.211	.207	.212	.237	.21
No college education	.708	.821	.802	.775	.793	.764	.712	.774
Education unknown	.0694	.0215	0	.0143	0	.0238	.0508	.0155
Foreign born	.75	.682	.811	.699	.768	.707	.831	.728
0–2 yrs since immigration	.0278	.052	.018	.077	.0488	.112	.0339	.0981
3–5 yrs since immigration	.111	.151	.162	.146	.134	.152	.153	.182
Born in Nordics or W. Europe	.0556	.0383	.045	.0472	.0732	.0532	.0339	.0457
Born in E. Europe or C. Asia	.0417	.0319	.018	.0374	.0122	.0294	.0169	.0246
Born in W. Asia or N. Africa	.208	.25	.189	.24	.171	.223	.153	.259
Born in Africa, excl. NA	.347	.235	.405	.223	.366	.246	.492	.244
Other country of birth	.0972	.127	.153	.151	.146	.156	.136	.155
Own initiative to be registered	.0972	.168	.0541	.0293	.0854	.0838	.0169	.0221
Quarter at PES, at JC reg.	17.3	10.9	14.1	10.2	7.8	6.84	5.47	5.51
Earnings t-24, SEK 1,000	13.7	26.1	36.5	41.9	29	34.4	17.9	17.4
SA, nr of months t-24	18.9	18.4	16.1	15.8	14.1	13.9	18.8	18.7
Psychotropic drug prescr. t-12	.153	.265	.153	.302	.207	.257	.22	.276
Pain rel. drug prescr. t-12	.139	.275	.279	.28	.268	.245	.254	.291
Hospital visit t-12	.0833	.123	.0811	.123	.061	.0802	.0339	.0916

Note: Weights used for the controls are based on information known 24 months after program start.

Table A.14: Means for participants and (weighted) controls by assignment period: Stockholm hosts

	Quarter 1–2		Quarter 3–4		Quarter 5–6		Quarter 7–8	
	Part.	Contr.	Part.	Contr.	Part.	Contr.	Part.	Contr.
Age	46.7	44.9	40.8	40.5	40.7	40.4	38.7	39.5
Female	.19	.437	.262	.515	.295	.566	.333	.629
Married	.238	.277	.415	.369	.426	.389	.593	.449
Child in household	.167	.297	.323	.412	.295	.477	.407	.539
Some college education	.0476	.176	.2	.249	.082	.177	.037	.157
No college education	.929	.805	.754	.713	.852	.81	.889	.829
Education unknown	.0238	.019	.0462	.0373	.0656	.0126	.0741	.0146
Foreign born	.548	.616	.785	.783	.869	.869	.926	.943
0–2 yrs since immigration	.0238	.096	.2	.186	.18	.157	.296	.202
3–5 yrs since immigration	.0714	.104	.2	.164	.197	.21	.222	.259
Born in Nordics or W. Europe	.0238	.0924	.0462	.0455	.0328	.027	0	.0111
Born in E. Europe or C. Asia	0	.0257	.0462	.0386	0	.0296	0	.0286
Born in W. Asia or N. Africa	.167	.203	.0923	.263	.0164	.323	.111	.341
Born in Africa, excl. NA	.238	.165	.492	.257	.705	.315	.741	.378
Other country of birth	.119	.131	.108	.179	.115	.174	.0741	.184
Own initiative to be registered	.0476	.0195	0	0	.0164	.00741	0	0
Quarter at PES, at JC reg.	9.69	6.77	10.2	5.35	6.36	5.08	2.48	4.54
Earnings t-24 ,SEK 1,000	37.8	44.2	30.7	29.3	25.1	19.3	10.2	13.5
SA, nr of months t-24	11	9.15	12	10.9	14	15.4	14.2	17.8
Psychotropic drug prescr. t-12	.19	.274	.215	.254	.082	.252	.0741	.231
Pain rel. drug prescr. t-12	.0952	.239	.169	.243	.0984	.316	.259	.323
Hospital visit t-12	.143	.129	.123	.0876	.131	.0727	.0741	.0751

Note: Weights used for the controls are based on information known 24 months after program start.

Table A.15: Normalized difference for participants and (weighted) controls by assignment period: Youth employment

	ND2m_24											
	Treated1	Cpost1	Treated2	Cpost2	Treated3	Cpost3	Treated4	Cpost4	Treated5	Cpost5	Treated6	Cpost6
Age 25–29	.128	.129	.141	.145	.183	.189	.179	.186	.327	.344	.316	.329
Less than high school	.541	.537	.577	.575	.428	.433	.448	.452	.579	.584	.684	.699
non_western	.366	.363	.436	.434	.505	.502	.5	.495	.71	.71	.632	.646
Own initiative to be registered	.465	.463	.2	.2	.0721	.0723	.0597	.0626	.0467	.0411	.0263	.0244
0 quarter at PES, at JC reg	.267	.267	.272	.269	.24	.239	.239	.243	.318	.324	.342	.343
Employed in t0-6	.233	.236	.243	.246	.173	.177	.127	.128	.0935	.0961	.0789	.0783
SA in t-1	.227	.224	.679	.679	.769	.765	.806	.803	.785	.785	.842	.849

Note: Variables included in the propensity score estimations. Weights used for the controls are based on information known 24 months after program start.

Table A.16: Normalized difference for participants and (weighted) controls by assignment period: Other employment

	ND2m_24							
	Treated1	Cpost1	Treated2	Cpost2	Treated3	Cpost3	Treated4	Cpost4
age_18_29	.25	.254	.0901	.0937	.11	.108	.153	.123
Age 30–39	.167	.165	.243	.256	.341	.352	.458	.489
Age 40–49	.417	.39	.423	.408	.329	.331	.203	.205
Employed in t0-6	.0278	.0271	.036	.0381	.0488	.0497	.0678	.0699
Subsidized empl in t0-6	.708	.712	.676	.66	.439	.426	.271	.267
Reason for SA, unemployment	.861	.851	.82	.821	.89	.907	.966	.97
SA, nr of months t-24	18.9	18.4	16.1	15.8	14.1	13.9	18.8	18.7
SA in t-1	.806	.773	.874	.882	.866	.87	.915	.915
Own initiative to be registered	.0972	.168	.0541	.0293	.0854	.0838	.0169	.0221

Note: Variables included in the propensity score estimations. Weights used for the controls are based on information known 24 months after program start.

Table A.17: Normalized difference for participants and (weighted) controls by assignment period: Stockholm hosts

	ND2m_24							
	Treated1	Cpost1	Treated2	Cpost2	Treated3	Cpost3	Treated4	Cpost4
Age 50-	.524	.524	.246	.245	.213	.211	.111	.136
Less than high school	.5	.5	.354	.353	.574	.568	.63	.633
non_western	.524	.524	.738	.737	.836	.842	.926	.932
Own initiative to be registered	.0476	.0195	0	0	.0164	.00741	0	0
0 quarter at PES, at JC reg	.0714	.0726	.108	.108	.115	.12	.185	.195
Employed in t0-6	.0476	.0455	.0462	.0377	.0984	.054	0	0
SA in t-1	.571	.579	.785	.786	.902	.907	.852	.863

Note: Variables included in the propensity score estimations. Weights used for the controls are based on information known 24 months after program start.

A.6 Estimation results - Main analysis

Table A.18: Estimation results: Youth employment

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	.000353	.0109	.00351	.0155	.00141	.00205
-11	.000616	.0107	.00684	.0152	.00383	.00223
-10	.000276	.0102	.00286	.013	.00123	.00168
-9	-.00343	.0101	.00653	.0122	.00121	.00204
-8	-.0054	.0106	.0115	.0145	.000461	.00179
-7	-.00675	.0125	.0328	.0145	.000829	.00171
-6	-.00867	.0116	.0345	.012	-.000767	.00155
-5	-.0247	.0123	.0316	.0117	-.000394	.00176
-4	-.0278	.0142	.0228	.0104	-.000446	.00192
-3	-.0297	.0138	.0387	.00988	-.00154	.00189
-2	-.0366	.0152	.0578	.0109	.0000988	.00234
-1	-.0136	.0162	.0665	.0121	-.00137	.00193
0	.431	.0168	.0666	.0144	-.00214	.00158
1	.616	.0104	-.189	.0178	-.00381	.000576
2	.627	.00941	-.45	.0118	-.00327	.000548
3	.602	.0103	-.429	.0106	-.00286	.000502
4	.57	.011	-.402	.00967	-.00272	.000494
5	.554	.0114	-.371	.00969	-.000265	.00171
6	.519	.0115	-.347	.0104	.00726	.0034
7	.431	.0144	-.314	.00989	.0393	.00622
8	.291	.0171	-.278	.0113	.101	.00854
9	.245	.0173	-.251	.0117	.125	.00988
10	.222	.0171	-.217	.0119	.139	.0103
11	.209	.0178	-.186	.0118	.12	.00934
12	.191	.0176	-.177	.0121	.11	.00926
13	.167	.0182	-.17	.0117	.104	.0109
14	.159	.0186	-.165	.0118	.0994	.0102
15	.142	.0174	-.159	.0114	.0992	.0106
16	.141	.0174	-.147	.0129	.0881	.00964
17	.131	.018	-.138	.0139	.0831	.00888
18	.119	.0173	-.131	.0131	.084	.00896
19	.0982	.017	-.128	.0117	.0793	.00862
20	.109	.0172	-.124	.0126	.0661	.00814
21	.11	.0172	-.117	.0126	.0644	.00794
22	.119	.0188	-.126	.0121	.0498	.00825

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Table A.18 – continued from previous page

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
23	.113	.0197	-.112	.0119	.0532	.00767
24	.114	.0201	-.111	.0123	.0444	.00762
25	.113	.0199	-.102	.0119	.0396	.00783
26	.117	.019	-.0997	.012	.041	.00835
27	.105	.0188	-.0975	.0115	.0409	.00854
28	.103	.0171	-.0971	.0118	.037	.00708
29	.113	.0167	-.0939	.0119	.0254	.00747
30	.111	.0175	-.0951	.0118	.0198	.00787
31	.103	.0174	-.088	.0108	.0234	.00798
32	.0981	.0161	-.0866	.0115	.0205	.00676
33	.111	.0169	-.0835	.0118	.0178	.00661
34	.115	.0162	-.0685	.0114	.0226	.0072
35	.104	.0155	-.0733	.0111	.0252	.00787
36	.118	.0165	-.0758	.0107	.0197	.00685

Table A.19: Estimation results: Other employment

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	-.0333	.0191	.0312	.0163	-.000652	.00737
-11	-.00825	.0215	.0175	.0167	.00401	.00886
-10	-.00546	.0205	.019	.0137	-.00103	.0071
-9	-.00679	.02	.0258	.0134	.00157	.00714
-8	-.0107	.0205	.0209	.015	.0038	.0077
-7	-.0191	.0193	.035	.0164	-.00579	.00661
-6	-.0131	.018	.0405	.0161	-.00354	.00749
-5	-.0157	.0189	.0542	.0163	-.00455	.00746
-4	-.0277	.0182	.0334	.015	-.00822	.00678
-3	-.0459	.0183	.0539	.0153	-.00583	.00755
-2	-.069	.0188	.0663	.0147	-.00542	.00731
-1	-.0406	.0207	.0844	.015	-.00598	.00744
0	.426	.0258	.0961	.0165	-.00653	.00669
1	.653	.0181	-.123	.0248	-.00889	.00613
2	.677	.0141	-.562	.0221	-.0152	.00438
3	.663	.0148	-.586	.0197	-.0174	.00242
4	.65	.0158	-.585	.0192	-.0157	.00234
5	.627	.0163	-.56	.0199	-.0168	.00232

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Table A.19 – continued from previous page

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
6	.613	.0164	-.537	.0199	-.0156	.00198
7	.592	.0175	-.502	.0218	-.00951	.00512
8	.556	.0182	-.477	.022	-.000085	.00794
9	.513	.02	-.46	.0218	.0177	.0101
10	.482	.0218	-.44	.0233	.0242	.0102
11	.467	.0224	-.42	.0228	.0232	.00954
12	.44	.0235	-.411	.0228	.0662	.0154
13	.345	.028	-.386	.0258	.26	.0273
14	.124	.0309	-.368	.0245	.365	.0301
15	.102	.0332	-.358	.0219	.392	.0299
16	.0748	.0313	-.343	.0247	.418	.0305
17	.0577	.0307	-.322	.0263	.435	.0281
18	.0591	.0306	-.305	.0228	.439	.029
19	.0471	.0309	-.309	.0247	.417	.0271
20	.0584	.0332	-.301	.0234	.417	.0275
21	.0705	.0324	-.29	.0213	.42	.0265
22	.0729	.0336	-.275	.0202	.386	.0265
23	.0792	.0332	-.258	.0232	.391	.0256
24	.0667	.0333	-.236	.0232	.391	.0262
25	.0815	.0328	-.242	.0223	.379	.0297
26	.0783	.0314	-.241	.0231	.386	.0281
27	.0708	.0301	-.233	.0214	.343	.028
28	.0735	.0294	-.209	.0232	.31	.0273
29	.0823	.0312	-.21	.0229	.272	.0249
30	.0885	.0302	-.2	.0214	.26	.0271
31	.0703	.0313	-.172	.0234	.23	.0271
32	.0608	.0312	-.173	.0222	.226	.0244
33	.073	.0312	-.169	.0223	.221	.0266
34	.0697	.0304	-.17	.022	.189	.0261
35	.0567	.0308	-.161	.0228	.132	.0245
36	.0356	.0305	-.182	.0227	.112	.0228

Table A.20: Estimation results: Stockholm hosts

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	-.0297	.0223	.0123	.0376	-.00215	.00814
-11	-.0161	.0236	.0474	.0355	.0144	.0114
-10	-.0198	.0247	-.0175	.0322	-.00322	.0075
-9	-.00943	.0261	-.0149	.0318	.0134	.0104
-8	-.0209	.0265	-.00485	.028	.0126	.0102
-7	-.0338	.026	.0116	.0248	.0106	.0101
-6	-.0452	.0268	-.0227	.0233	.0087	.0106
-5	-.0418	.0283	.00588	.0189	.00782	.00994
-4	-.0488	.0274	.0204	.0191	.0113	.011
-3	-.0513	.0263	.0572	.0209	-.00003	.00887
-2	-.0639	.0275	.0834	.0212	-.0046	.00809
-1	.0473	.0358	.0875	.0255	-.00798	.00627
0	.622	.0256	.0678	.0297	-.0121	.00489
1	.748	.00934	-.234	.0381	-.00543	.00756
2	.739	.00926	-.644	.0197	-.0106	.00527
3	.731	.00878	-.638	.0169	-.0095	.00482
4	.711	.0107	-.617	.0152	-.0048	.00706
5	.688	.0142	-.595	.0167	-.00857	.00478
6	.655	.0185	-.575	.017	.00688	.0106
7	.564	.0236	-.533	.0231	.0625	.0179
8	.404	.0338	-.45	.0278	.176	.0262
9	.367	.0319	-.406	.0311	.2	.026
10	.359	.0307	-.353	.0286	.23	.0272
11	.355	.0297	-.331	.0315	.249	.0301
12	.32	.0304	-.322	.0314	.303	.0285
13	.195	.0364	-.32	.0305	.471	.036
14	.0416	.0345	-.323	.0284	.509	.033
15	.0367	.0329	-.303	.0311	.552	.0357
16	.00276	.0347	-.302	.0298	.628	.0345
17	-.032	.0341	-.328	.0285	.579	.0358
18	-.0782	.0348	-.289	.0332	.624	.0338
19	-.0871	.0331	-.27	.0344	.603	.0333
20	-.106	.0333	-.234	.0334	.552	.0366
21	-.137	.0312	-.219	.0326	.529	.0336
22	-.142	.0307	-.202	.0343	.486	.0367
23	-.128	.0324	-.19	.0341	.46	.0362

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Table A.20 – continued from previous page

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
24	-.125	.0327	-.22	.0306	.473	.0367
25	-.144	.0326	-.212	.0314	.442	.0355
26	-.146	.032	-.188	.034	.41	.0349
27	-.118	.0326	-.176	.0305	.34	.0333
28	-.102	.0344	-.194	.0302	.263	.0346
29	-.099	.0335	-.19	.0279	.211	.0301
30	-.112	.0365	-.173	.0312	.18	.0285
31	-.111	.0372	-.171	.0312	.157	.0286
32	-.0918	.0379	-.151	.0321	.148	.0282
33	-.0884	.0354	-.156	.0312	.125	.0274
34	-.0838	.0353	-.183	.029	.0976	.0277
35	-.0802	.0366	-.162	.0285	.0588	.0222
36	-.055	.0393	-.15	.0289	.0566	.0209

A.7 Cumulative ATET

Table A.21: Cumulative ATET: Youth employment

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 7–12 after program start			
ATET	1.59	-1.41	.634
St err	.0867	.0593	.0377
Mean	2.53	1.93	.0296
Months 13–24 after program start			
ATET	1.51	-1.6	.915
St err	.189	.128	.0798
Mean	5.59	2.98	.183
Months 25–36 after program start			
ATET	1.29	-1.03	.331
St err	.186	.117	.0656
Mean	6.14	2.22	.306
	Employment (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 7–12 after program start			
ATET	25,753	-8,344	1,383
St err	1,494	418	120
Mean	30,118	11,235	112
Months 13–24 after program start			
ATET	19,657	-9,776	3,733
St err	3,873	867	394
Mean	76,687	17,443	1,126
Months 25–36 after program start			
ATET	20,592	-6,423	830
St err	4,369	770	356
Mean	95,446	13,174	2,189

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

Table A.22: Cumulative ATET: Other employment

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 13–24 after program start			
ATET	1.12	-3.69	4.73
St err	.335	.249	.276
Mean	5.11	5.28	.453
Months 25–36 after program start			
ATET	.818	-2.29	3.05
St err	.328	.239	.249
Mean	5.55	4.11	.779
	Employment (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 13–24 after program start			
ATET	17,051	-27,004	22,761
St err	6,932	1,615	1,468
Mean	82,564	35,237	2,924
Months 25–36 after program start			
ATET	8,593	-18,094	10,283
St err	7,640	1,781	1,129
Mean	98,342	28,660	5,240

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

Table A.23: Cumulative ATET: Stockholm hosts

	Employment (months)	SA receipt (months)	UI benefit receipt (months)
Months 13–24 after program start			
ATET	-.564	-3.18	6.47
St err	.323	.329	.315
Mean	4.29	5.67	.407
Months 25–36 after program start			
ATET	-1.23	-2.09	2.49
St err	.365	.321	.249
Mean	4.79	4.68	.619
	Employment (SEK)	SA receipt (SEK)	UI benefit receipt (SEK)
Months 13–24 after program start			
ATET	-15,821	-23,362	31,798
St err	5,637	2,093	1,965
Mean	68,064	36,192	2,472
Months 25–36 after program start			
ATET	-25,037	-18,723	10,945
St err	7,768	1,956	1,450
Mean	82,067	31,249	4,205

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

Appendix B Additional figures

B.1 Description of the study population

Figure B.1: Description of the study population with respect to SA-resipiency

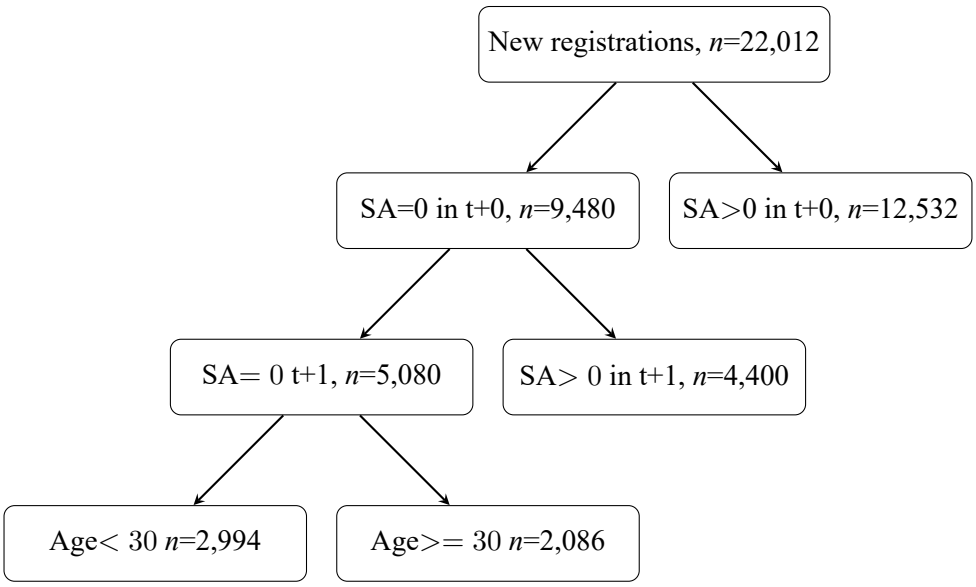
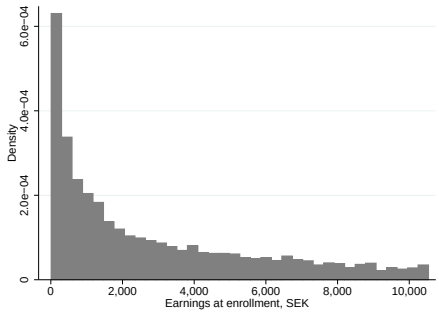


Figure B.2: Average monthly earnings at time of enrollment at the job center



Note: Zero earnings and earnings above $p(95) = 11,300$ SEK excluded.

B.2 Actual and simulated start dates

Figure B.3: Actual (left) and simulated (right) start dates: Youth employment

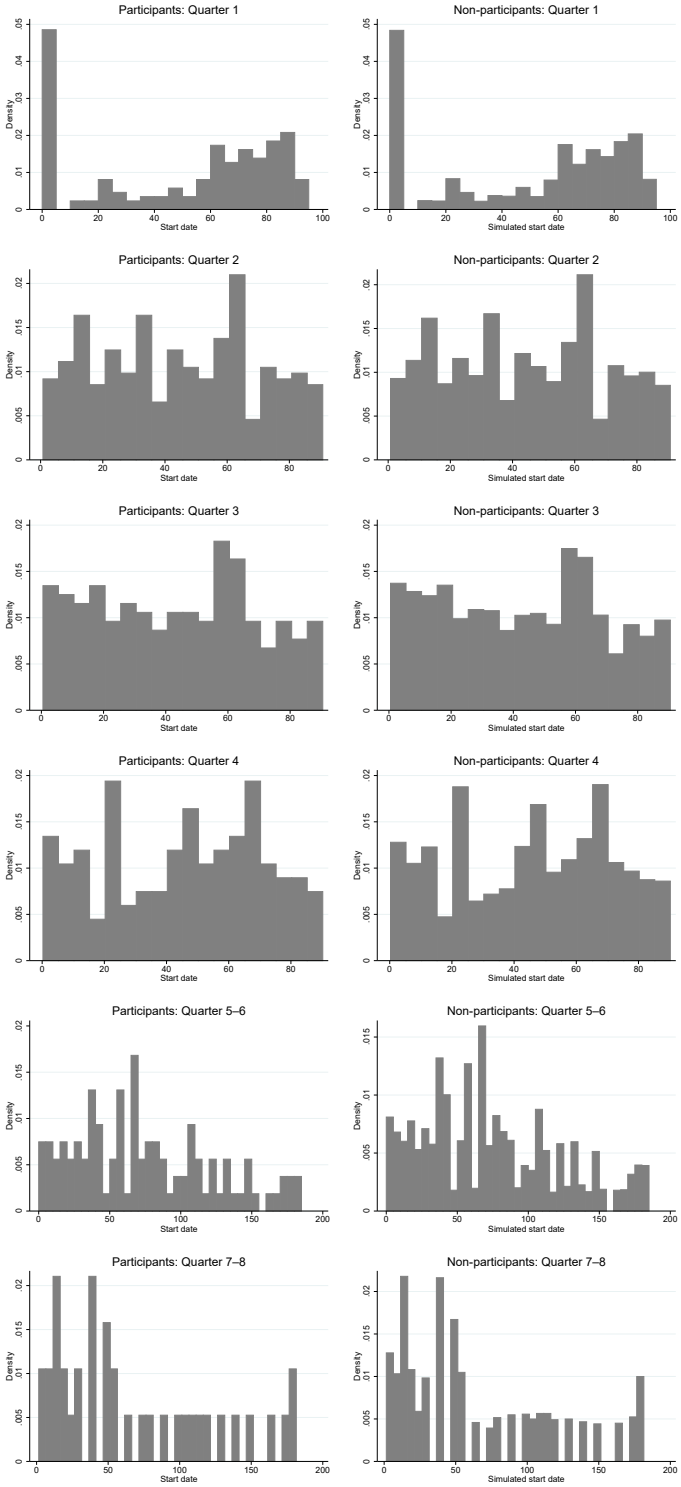


Figure B.4: Actual (left) and simulated (right) start dates: Other municipal employment

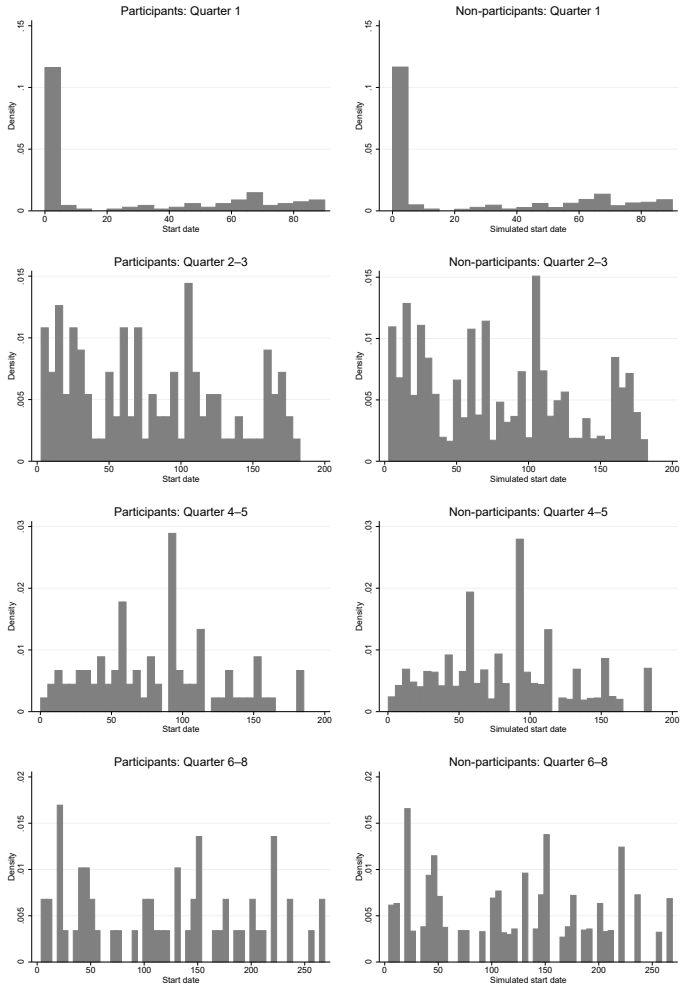
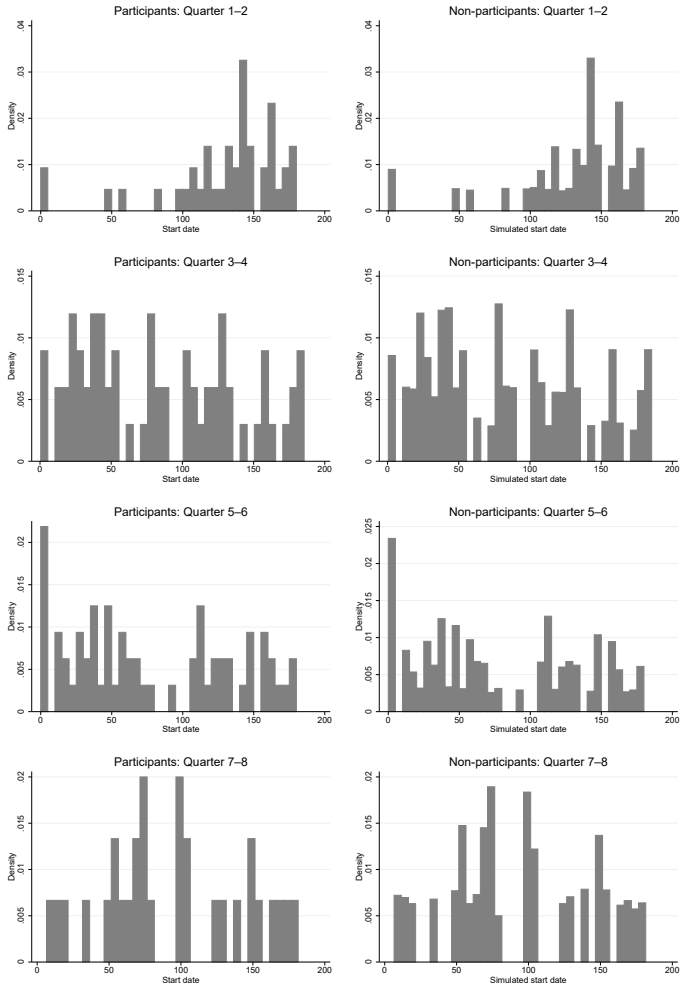


Figure B.5: Actual (left) and simulated (right) start dates: Stockholm hosts



B.3 Sensitivity analysis

Figure B.6: ATET by months since program start: Excluding non-participants with $t_s^{sim} > t_u$

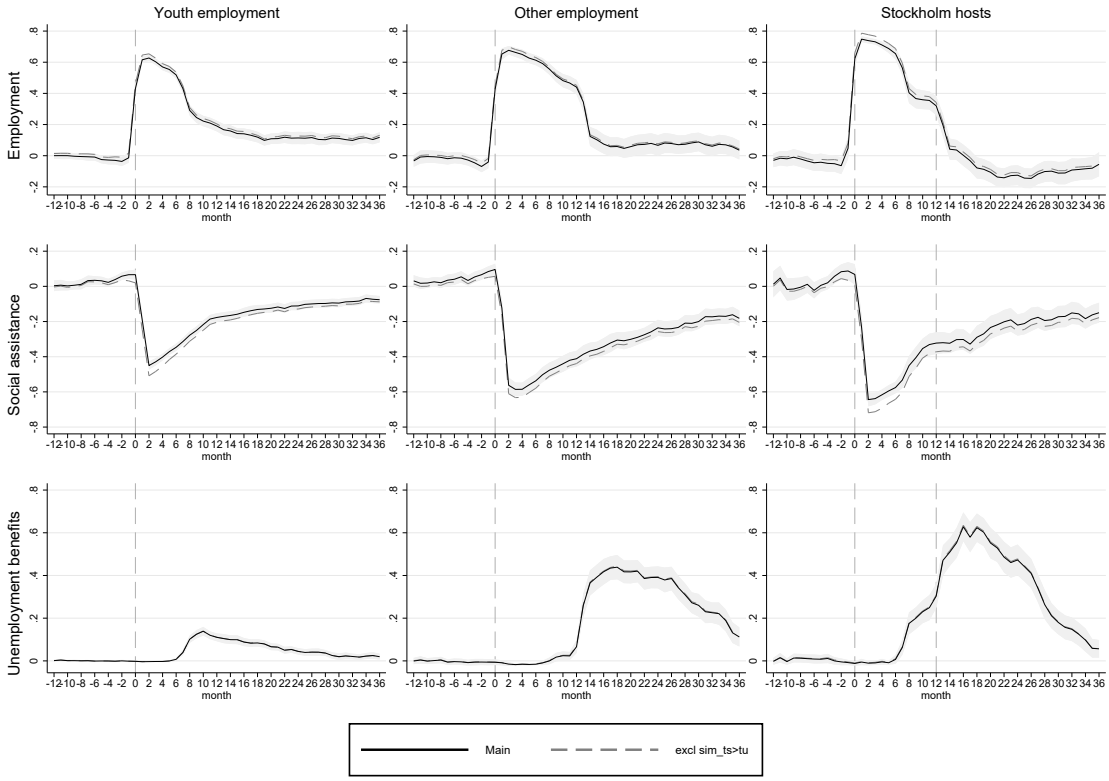
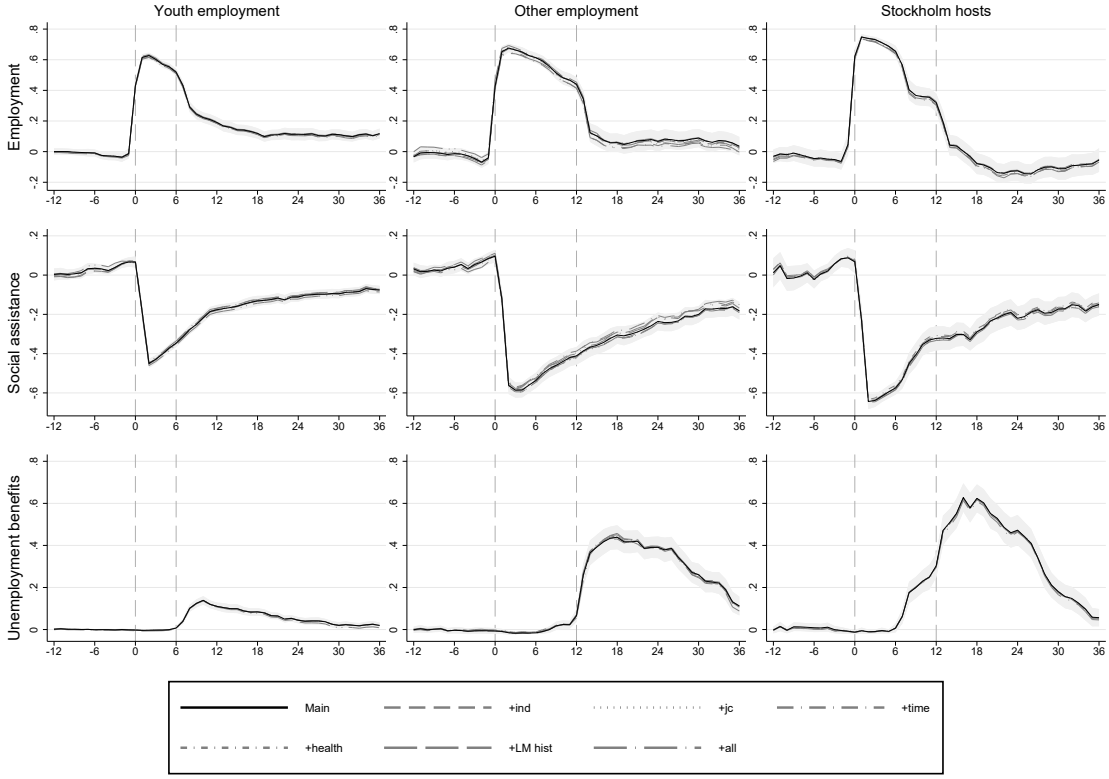
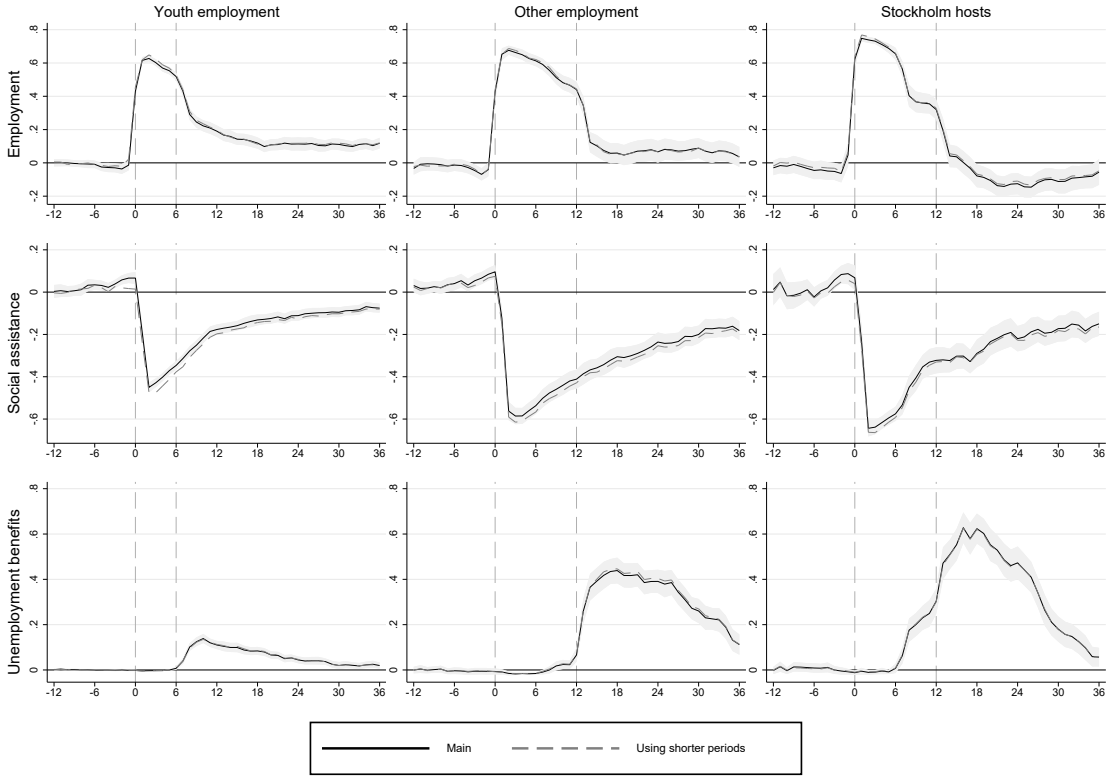


Figure B.7: ATET by month since program start: Different set of confounders



Note: 95% CI based on 99 bootstrap replications for main analysis. The weights in the pre-period (-12 to 0) are based on the weights at time 1. Other lines represent more variables included in the PS estimations according to tables A.3-A.5. Differences between ATET with different set of confounders are shown in Figure **??**

Figure B.8: ATET by month since program start: Different assignment periods



Note: Dashed line indicates using shorter periods. Solid line from main analysis with 95% CI based on 99 bootstrap replications. The weights in the pre-period (-12 to 0) are based on the weights at time 1.

Figure B.9: ATET by month before program start: Different weights