

Ottosson, Lillit; Vikman, Ulrika

Working Paper

Bilingual caseworkers and onthe-job training: A pathway to integration?

Working Paper, No. 2025:9

Provided in Cooperation with:

IFAU - Institute for Evaluation of Labour Market and Education Policy, Uppsala

Suggested Citation: Ottosson, Lillit; Vikman, Ulrika (2025) : Bilingual caseworkers and onthe-job training: A pathway to integration?, Working Paper, No. 2025:9, Institute for Evaluation of Labour Market and Education Policy (IFAU), Uppsala

This Version is available at:

<https://hdl.handle.net/10419/322185>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Bilingual caseworkers and on-the-job training: A pathway to integration?

Lillit Ottosson

Ulrika Vikman

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala.

IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

Bilingual caseworkers and on-the-job training: A pathway to integration? ^a

by

Lillit Ottosson^b and Ulrika Vikman^c

May 22, 2025

Abstract

We study an active labor market program aimed at immigrants with very limited language skills. The program consists of a three-month on-the-job training program in a regular workplace, facilitated by bilingual caseworkers speaking the participant's native language. The aim of the program is to improve participants' language skills and to provide labor market experience. We apply dynamic inverse probability weighting to account for dynamic selection into the program. After an initial lock-in effect, we find that the program leads to sizable increases in employment throughout the three-year follow-up period. These effects are explained by both subsidized and regular employment, and are mainly driven by women.

Keywords: Immigrants; Integration; On-the-job training; Language support

JEL-codes: H75, I38, J15

^aWe are grateful for the comments from Henrik Andersson, Iben Bolvig, Jonas Cederlöf, Matz Dahlberg, Gerard van den Berg, Matthew Lindquist, Eva Mörk, Anna Thoreson, Johan Vikström, Alexander Willén, and Olof Åslund as well as the seminar participants at the Institute for the Evaluation of Labor Market and Education Policy (IFAU), the Uppsala Immigration Lab (UIL), the 7th Swedish Conference in Economics, the EALE conference 2024 and the Swedish Institute for Social Research (SOFI). We also thank Rut Haregot, Dzenana Cenovic and Samatar Isse for sharing their valuable insights about the program. We thank FORTE (Reg. No. 2016-07123) and the Jan Wallander and Tom Hedelius Foundation (Ottosson) for financial support.

^bSOFI, Stockholm University, Uppsala Center for Labor Studies (UCLS) and UIL, Uppsala University; IFAU, lillit.ottosson@sofi.su.se.

^cIFAU; UCLS and UCFS, Uppsala University, ulrika.vikman@ifau.uu.se.

Table of contents

1	Introduction	3
2	Institutional setting	7
3	Data and sample	9
3.1	Descriptive statistics	10
3.2	Timing and duration of the program	13
4	Empirical strategy	13
4.1	Controlling for confounding factors	14
4.2	Dynamic inverse probability weighting (IPW)	15
4.3	Implementation	17
4.4	Balance	19
5	Results	20
5.1	Swedish language skills	22
5.2	Sensitivity analysis	23
5.3	Effect heterogeneity	24
6	Mechanisms	28
7	Conclusions	30
A	Additional figures	37
B	Additional tables	45

1 Introduction

The slow labor market integration of immigrants, in particular refugees, that we see in most European countries is costly both from an individual and societal perspective. Even after several years in the host country, refugees on average have lower employment rates and earnings compared to native-born residents and economic migrants (Brell et al., 2020; Cortes, 2004). Finding ways to speed up their labor market integration is therefore an important policy question. Previous literature has shown the merits of using both language training (Foged et al., 2022; Lochmann et al., 2019; Pont-Grau et al., 2023; Sarvimäki and Hämäläinen, 2016) and more traditional active labor market policies (ALMPs) like subsidized employment and job search assistance (Andersson Joona and Nekby, 2012; Battisti et al., 2019; Butschek and Walter, 2014) to accomplish this. This suggests that it is important to provide new immigrants with both language training and labor market oriented activities in their new country.

However, these two goals may be conflicting. A work-first approach, where immigrants enter ALMPs early on, risks crowding out time and effort otherwise spent on language training (Arendt, 2022; Arendt and Bolvig, 2023). It may furthermore not be possible for immigrants without sufficient language skills to fully benefit from ALMP participation if they are unable to communicate with and understand instructions from caseworkers and colleagues in the workplace. On the other hand, the slow integration of immigrants – especially among women and individuals with low educational attainment – may be explained by an excessive focus on preparatory measures like language training (Cheung, 2018; Dahlberg et al., 2024).

One way to tailor early ALMPs to the needs of immigrants who have not yet learned the host-country language, is to combine such programs with support and information in the immigrants' native language. We study such a program in Sweden, which entails an on-the-job training in a regular workplace, facilitated by coaching and information provided by bilingual caseworkers who speak the immigrants' native language. Furthermore, workplaces are selected such that they provide participants with the opportunity to improve their Swedish communication skills (whilst learning about the Swedish labor market). We investigate if this type of program, combining a traditional ALMP with non-formal language training and support by bilingual caseworkers, represents a way forward to increase employment for immigrants with limited language skills.¹

¹Non-formal language training refers to training that takes place outside formal learning environments (as opposed to formal training), but within some organizational context (as opposed to informal training) (Dressman and Sadler, 2020).

The program we evaluate, the On-the-job Language Training (OLT) program, is run by the city of Stockholm and directed toward unemployed immigrants with very limited Swedish language skills, a majority of which are refugees or their next of kin. The program consists of two stages. In the first four-week-long phase, bilingual caseworkers provide an extensive introductory course about the Swedish labor market (how to apply for a job, write a CV, rules and norms in the workplace etc.) and carry out in-depth surveys of participants' formal and informal skills and preferences, all in their native language. After this, participants are matched to a regular workplace where they, in the second stage of the program, participate in on-the-job-training for three months. In the workplace, participants are supported by supervisors who have received training in communication and intercultural understanding.

There is a growing literature studying the effects of different ALMPs on the labor market integration of immigrants. Activities like job search assistance and intensive coaching (Andersson Joona and Nekby, 2012; Battisti et al., 2019) and wage subsidies (Butschek and Walter, 2014) have been found to improve labor market outcomes, the former in particular for immigrants with low levels of education.² However, traditionally, before entering such programs, immigrants invest substantial time in learning the language in their new country. An alternative to this order is to apply a work-first approach. Arendt (2022) and Arendt and Bolvig (2023) study an on-the-job training program targeted to refugees who recently received a residency permit in Denmark. While this work-first program improved labor market outcomes in the short run (Arendt, 2022), it also resulted in less investments in language skills, and had no longer-run effects on employment (Arendt and Bolvig, 2023). As there is a well-established and strong correlation between language skills and labor market success (Chiswick and Miller, 1995), crowding out the acquisition of language skills may be costly. There is also a growing body of work that finds a direct positive causal effect of language training on labor market outcomes (Foged et al., 2022; Heller and Mumma, 2023; Lochmann et al., 2019; Pont-Grau et al., 2023; Sarvimäki and Hämäläinen, 2016). These findings indicate that it is vital that early ALMP participation does not come at the expense of investments in language skills.

To evaluate the effects of participating in the OLT program, we have access to administrative data covering a rich set of individual characteristics, migration, labor market and health history in Sweden. Importantly, we can use information about prior Swedish language training (SFI) courses

²Dahlberg et al. (2024) also find that a program consisting of a bundle of job search assistance, on-the-job training and intensive language training has positive effects on labor market outcomes for this group.

and grades to capture language skills upon being assigned to the program. We study all individuals who enroll at a local job center in Stockholm 2010–2017 and follow participants who entered the program for three years after program start. We study how the program affects labor market outcomes and SFI grades, and investigate if this differs based on sex or prior educational attainment.

Assignment to the program is not random, but rather depends on a combination of factors. In Stockholm, unemployed recipients of SA register at local job centers where they meet with a caseworker who assigns them to an ALMP. Caseworkers may assign individuals with sufficiently limited language skills to the OLT program. Whether or not they do, and at what point in time, may also depend on the caseworkers' awareness about the program (which takes place at a separate unit), the local job center's priorities, or the social situation of the unemployed individual. There is also some variation in program start caused by the fact that the first introduction course is carried out in a group, which implies that participants may have to wait a while for there to be a sufficient number of other participants speaking the same native language. Because we lack quasi-random variation in program assignment, we rely on our rich set of covariates to find observably similar non-participants.³ In addition, we deal with a dynamic treatment assignment problem (see e.g., Fredriksson and Johansson, 2008). Given that individuals who are registered at a local job center can be assigned to the OLT program at any point in time, individuals with short spells at the job center (who, for example, may have found employment before being assigned) will be over-represented among individuals who are never assigned to the program. To address these problems, we apply dynamic inverse probability weighting (IPW) (van den Berg and Vikström, 2022). With this approach, we compare individuals with similar durations at the local job centers and give a greater weight to non-participants that have been registered at the job center for a long time.⁴

We find an initial negative lock-in effect on employment from participating in the OLT program. After the program ends, employment increases among participants compared to non-participants. They are 5–6 percentage points more likely to be employed 1–3 years after entering the program, which corresponds to a 15 percent increase in employment by the end of this period. We show that these effects are explained by both regular and subsidized employment and that they lead to sizable increases in earnings corresponding to almost 20 percent over the 2.5 years we follow participants after having left the program. Dividing our sample by sex, we furthermore find that the positive

³We use a data-driven LASSO approach to limit our set of confounders to the most important determinants of assignment to the program.

⁴The estimator also censors the outcomes of the non-participants if and when they enter the program. Compared to the commonly used dynamic propensity score matching approach (Sianesi, 2004, 2008), the effect we estimate is thus the effect of participating in the program, rather than the effect of participating now as opposed to potentially later on.

employment and earnings effects are essentially driven by women. The effects for men are noisy but very close to zero.

Furthermore, in contrast with the results from Arendt and Bolvig (2023), we find no indications that participation crowds out investments in Swedish language skills. In fact, when we divide our sample by educational attainment, we see an imprecise increase in the likelihood of passing a Swedish language course of 12 percent for individuals with at most compulsory schooling. This may indicate that the program is indeed working as non-formal language training, in particular for a group we may expect to struggle more in classroom training (Lochmann et al., 2019).

Our study adds to the literature on which ALMPs work to improve immigrants' labor market integration. More specifically, it adds to a very sparse number of papers studying the effect on ALMPs targeting individuals with weak language skills. Compared to the related Danish program, the on-the-job-training in the OLT program is facilitated by caseworkers who provide information and support in the participants' first language, and thereafter carefully match them to a workplace prioritizing the possibility to practice Swedish. Both these factors may help explain its relative success compared to the Danish program. Our results also place our study among the few that identify ALMPs which work well for immigrant women, whose average employment rates are much lower than men's.⁵

Finally, while there is a number of studies that investigate the importance of bilingual support in schools (Attar et al., 2020; Chin et al., 2013), not much is known about the impact of programs providing bilingual support to unemployed adults. Using bilingual caseworkers to convey important information could provide a way forward for work-first strategies. If not, the limited language skills of the participants may prevent them from acquiring the intended knowledge of the labor market and successfully completing the program. It may also be easier for a caseworker to coach and motivate the participant if they share the participant's native language.⁶

The remainder of the paper is structured as follows. Section 2 presents the institutional setting and the program we study in more detail, while Section 3 provides a description of the data and sample used. The empirical strategy and its implementation is presented in Section 4. Section 5 presents the results from our empirical analysis, Section 6 discusses potential mechanisms and, finally, Section 7 concludes.

⁵Some exceptions, also in a Swedish context, include Bratu et al. (2025) and Helgesson et al. (2020).

⁶This is mentioned as an important factor of success by one of the bilingual caseworkers.

2 Institutional setting

There are two cornerstones of introducing new immigrants to Sweden. The first is Swedish for Immigrants (SFI) classes.⁷ These language courses are available to all immigrants who lack basic Swedish language skills, and are run and financed by the local governments at the municipal level. The second cornerstone is participation in ALMPs. Access to the latter to some extent depends on whether the immigrant has received a residency permit as a refugee (or a refugee's next of kin) or for another reason. Upon receiving their residency permit, refugees and their relatives are eligible to participate in the two-year Introduction Program, run by the Public Employment Service (PES).⁸ Participants receive benefits conditional on participating in SFI and other activities specified in an integration plan, set up together with a caseworker. These other activities have however been found to mostly include preparatory activities, especially for women (Andersson Joona, 2020; Cheung, 2018). Other immigrants, as well as refugees who have not found a job within the two years of the Introduction Program may also register with the PES. If they do not have savings or family members who can support them, they can apply for means-tested social assistance (SA) at the social welfare office run and financed by each municipality. Municipalities are however allowed to condition the receipt of SA on participation in skill-enhancing local activities, and most do (Vikman and Westerberg, 2017).

In the city of Stockholm, local ALMPs are organized by 6 local job centers. Both native and foreign-born unemployed SA recipients must register at their local job center in order to receive SA. At the job center, they meet with a caseworker who can assign them to different activities, including job-search activities, coaching, courses, on-the-job training and public sector employment programs. One of the available programs is the OLT program.

The OLT program exists since 2009 and is run by a separate unit within the municipality administration, the Unit for Language Support Interventions (ULSI). Two types of caseworkers work in the ULSI: bilingual and matching caseworkers, and it is the role of the former that makes the unit unique. They cover the four most common languages spoken by job center clients with limited language skills: Arabic, Somali, Dari, and Tigrinya.⁹

⁷SFI is structured in three study paths adapted to participants' educational background: SFI 1 consists of courses A–D, SFI 2 of courses B–D and SFI 3 of C and D. Participants can progress to the next course as they pass the previous.

⁸The Introduction Program was reformed in December 2010. Prior to this, municipalities (as opposed to the PES) in Sweden were responsible for introducing new refugees. For a description of this reform see, e.g., Andersson Joona et al. (2016) or Bratu et al. (2025).

⁹These languages cover a large majority of the participants, but remaining participants (about 15 percent of the participants in our sample) are provided with information and support in “simple” Swedish. They thus need to understand Swedish to a somewhat higher degree than individuals with bilingual coaches. Dari was provided from 2018.

Immigrants who are registered at a local job center and who have limited language skills can be assigned to the OLT program by their local caseworker.¹⁰ Whether or not they are assigned, and at what point in time, may depend on several factors: how the caseworker perceives the client's language skills, their social situation, and how familiar the caseworker is with the OLT program. Furthermore, because the first part of the program consists of an introductory course carried out by the bilingual caseworkers in a larger group, the exact timing of the start of the program will depend on when there is a sufficient number of new participants speaking a certain language. The introductory course lasts for four weeks¹¹ and involves an introduction to the Swedish labor market (how to write a CV, how to apply for jobs, rules and norms at the workplace, etc.). During this period, the bilingual caseworker also meets with each participant to conduct an in-depth survey of participants' formal and informal skills and preferences, coach and motivate them. The information collected during these meetings is subsequently used by the matching caseworkers to match participants with employers. Another important criteria is that the workplace should provide the participants with the opportunity to practice their Swedish language skills, e.g. through communicating with coworkers. The matching process, i.e. finding a suitable workplace for the participant's on-the-job-training, is allowed to take up to 60 days.

The aim of providing on-the-job training in a regular workplace (in the private or public sector) is to provide both labor market experience and opportunities to improve the participant's language skills.¹² In the workplace, the participant has a supervisor who has been offered training in communication and intercultural understanding, and to allow for continued studies at SFI, the program is part-time.¹³ Participants must however be at work for parts of the day five days a week. Participants do not receive a salary, but continue to receive income support (mainly means-tested SA) throughout their participation in the program. To avoid crowding-out effects, participants are only allowed to perform tasks outside the scope of the tasks performed by regular employees. The caseworker has continuous contact with the participant and the workplace throughout the training.

The fact that the introductory course, motivational work, and support throughout the program are provided in the participants' native language, allows them to understand the information and

¹⁰The PES and SFI schools can also send their participants to the program, but these groups are not included in our data.

¹¹The duration of the introductory course was eventually extended to five weeks, but for most of our study period, it lasted four weeks.

¹²We do not observe the workplace of each on-the-job training placement, but according to the ULSI, approximately 35 percent take place at municipal workplaces and 65 percent at private workplaces.

¹³The supervisor can also participate in networking events with lectures and discussions with supervisors from other workplaces and staff from the ULSI.

instructions provided, and to share their previous labor market experience and preferences, despite very limited Swedish language skills. Even if the caseworkers at the regular job centers could in principle use interpreters when they meet these clients, it may be difficult to coach and motivate the client if the caseworkers cannot communicate directly with them.

If, by the end of the on-the-job training, the participant has not found employment, they return to the caseworkers at the regular job centers, hopefully with better language skills and more labor market experience. During most of the period we study, the ULSI also cooperated with a public sector employment program provided by the job centers, the Youth Employment Program.¹⁴ Even if the Youth Employment Program was not part of the OLT program, the cooperation implied that some participants received a subsidized employment in their workplace after having finished the on-the-job training. The OLT program could thus potentially also work as a stepping stone to other ALMPs closer to regular employment.

3 Data and sample

We combine administrative records from several different sources. The city of Stockholm provides data on all registrations at the local job centers from January 2010 until June 2019. These data include the date of registration and participation in activities at the job center. We also have access to data from Statistics Sweden covering background characteristics (such as sex, age and family situation); migration history; educational attainment and monthly labor earnings.¹⁵ Importantly, to capture language skills and native language, we rely on data on enrollment, level and grades at SFI since 1993, as well as the registered mother tongue (available from the same source since 2004). We have information about the reason for being granted a residency permit (e.g. as a refugee) and when it was granted, from the Swedish Migration Agency. The data from the PES include all registrations and participation in programs run by the PES since 1991, and the National Board of Health and Welfare (NBHW) provides data on monthly social assistance payments, medical prescriptions, and hospitalizations.

We limit our estimation sample to immigrants between the ages 18 and 61 who registered at a local job center between January 1, 2010 and December 31, 2017. To better reflect who participates

¹⁴Participants in this program (all aged below 30) are employed for six months at different municipal workplaces such as childcare centers, administration, and care for the elderly. Mörk et al. (2022) find that the program increases participants' employment by 10 percentage points up to three years after starting the program.

¹⁵During the early years of our follow-up period, we have access to annual earnings as well as employment indicators for the first and last month of each employment spell. To estimate monthly earnings, we assume that earnings are evenly distributed between these months. From 2019 onward, we have access to monthly earnings data.

in the program, we furthermore restrict the sample to immigrants born outside Nordic and Western European countries who have been in Sweden no longer than 15 years at the time of enrollment at the job center.¹⁶ This sample consist of 11,820 registrations and 9,353 unique individuals. We define program participation as starting the OLT program within 18 months after registration, no later than December 31, 2018.¹⁷

To measure labor market success, we examine employment (defined as having received any earnings in a given quarter of a year) as well as total earnings in a given quarter. In addition, we divide employments into regular and subsidized employment.¹⁸ We also study SA receipt, since SA is the most common income support received by individuals enrolled at the job centers. These data exist until 2021, and we may therefore follow all individuals in our sample at least three years after entering the program. We study how the outcome variables evolve each quarter, starting one year before entering the OLT program (to assess pre-trends) and ending three years thereafter. To quantify the total effects of the program, we also analyze total earnings and SA received before, during and after the program. Finally, to capture language skills or progression in the formal language training system, we study the likelihood of passing an SFI course during the same time intervals.

3.1 Descriptive statistics

Table 1 describes the mean characteristics of our sample at the time of registration at the job center. Column 1 describes the participants, and Column 2 the non-participants. Some notable differences between the two groups are that women and married are over-represented among participants, who also have obtained less formal education than non-participants and are almost twice as likely to have arrived in Sweden within the past two years (44 vs. 23 percent).¹⁹ In terms of region of origin, individuals born in Sub-Saharan Africa are somewhat over-represented among participants. It is also clear that a large share of the individuals in our sample, especially among the non-participants, received their residency permit as a refugee or as relatives of someone already residing in Sweden. While more than 70 percent of the individuals in our sample had entered an SFI course before

¹⁶Countries excluded are all EU-countries, Norway, Iceland, Great Britain and Switzerland.

¹⁷We however take program participation later in an job center spell into account when computing the inverse probability weights and when censoring outcomes for non-participants, as explained in greater detail in Section 4.

¹⁸Subsidized employment is defined as being registered with an employment subsidy at the PES, while simultaneously having positive earnings registered in Statistics Sweden's registers. We consider employments with no registered subsidy, or with more than one employer in a given quarter, as regular employments.

¹⁹One may worry that we will not have overlap in terms of this important variable but, as Figure A1 in the Appendix shows, there is also a large number of recent immigrants who never participate in the OLT program.

registering at the job center, we see indications that participants do indeed have weaker formal Swedish language skills than other immigrants enrolled at the job center: They are 12 percentage points less likely to have passed an SFI course, and 16 percentage points less likely to have started one of the higher level SFI courses (C or D) before registering.

The table also includes information about individuals' native language, which covers 98 percent of participants and 76 percent of non-participants. Arabic and Somali are the largest language groups among participants, each accounting for approximately one-third, and they are also relatively common among non-participants. Participants are overrepresented in all language groups spoken by the bilingual caseworkers compared to non-participants, with the exception of Dari.²⁰

²⁰This likely reflects the fact that the program started to provide Dari-speaking caseworkers from 2018.

Table 1: Mean characteristics at the time of job center registration 2010–2017

	(1)		(2)	
	Participants		Non-participants	
	mean	sd	mean	sd
Age, years	34.04	9.77	33.56	10.16
Female	0.69	0.46	0.52	0.50
Married	0.63	0.48	0.44	0.50
Child in household	0.52	0.50	0.47	0.50
Compulsory schooling	0.55	0.50	0.46	0.50
High school	0.21	0.41	0.29	0.45
University	0.19	0.39	0.24	0.43
Education unknown	0.05	0.21	0.02	0.12
Number of quarters registered PES	2.89	5.13	4.03	6.18
SA previous month	0.45	0.50	0.43	0.50
0–2 years since immigration	0.44	0.50	0.23	0.42
3–5 years since immigration	0.32	0.47	0.27	0.45
6–15 yrs since immigration	0.24	0.43	0.50	0.50
Born in Asia, excl. Middle East	0.08	0.27	0.15	0.36
Born in Sub-Saharan Africa	0.51	0.50	0.36	0.48
Born in Middle East or North Africa	0.37	0.48	0.37	0.48
Other country of birth	0.04	0.20	0.12	0.32
Refugee	0.39	0.49	0.48	0.50
Relative of refugee	0.15	0.36	0.14	0.35
Relative of other immigrant	0.22	0.41	0.26	0.44
Started SFI course	0.73	0.44	0.70	0.46
Passed SFI course	0.30	0.46	0.42	0.49
Started SFI course, level C–D	0.30	0.46	0.46	0.50
Registered native language	0.98	0.14	0.76	0.43
Arabic	0.34	0.47	0.22	0.41
Somali	0.33	0.47	0.13	0.33
Dari	0.03	0.18	0.06	0.23
Tigrinya	0.13	0.33	0.08	0.27
Other native language	0.15	0.36	0.28	0.45
Observations	1053		10767	

Note: Variables are measured at the time of registration at the job center. Unless otherwise stated, the means refer to the share. Refugee is defined as immigrants who received their residency permits as a refugees. SFI refers to Swedish language classes for immigrants, where courses C and D represent the highest levels. The sample is limited to individuals born outside Nordic and Western European countries (see footnote 16), with at most 15 years since immigration.

3.2 Timing and duration of the program

One important aspect of the OLT program is at what point in time participants are assigned to the program (Figure 1a). Many participants enter the program early, within the first two months of their job-center spell. However, there continues to be an inflow of participants into the program each month. Because very few participants enter the program more than 18 months after registering at the job center, we limit our study to participants entering within the first 18 months. The variation in the timing of the program start that we see in Figure 1a may, for instance, be explained by individual circumstances, like one's family situation or a temporary health issue, or by different priorities made by the local caseworker or job center.²¹ We also observe some bunching on program start dates in calendar time, likely explained by the fact that the first part of the introductory course is done in a group/class, implying that it will begin when the number of participants speaking a certain language is large enough, see Figure A2 in the Appendix.

Finally, Figure 1b shows the duration of the OLT program spells in our data. Duration is defined as the time from starting the introductory course until ending the on-the-job training. The introductory course lasts for four weeks and we allow for at most a two-month long gap between the introduction course and the on-the-job training (the maximum time gap to find a suitable workplace allowed by the ULSI). Figure 1b reveals that some participants exit the program very early on. This may be due to a failure of the matching caseworker to find a suitable workplace, or that the participant is unable to continue with the program for other reasons, like having found a job.²² But, as expected, many participants are enrolled in the program for three to six months, and longer durations are uncommon.

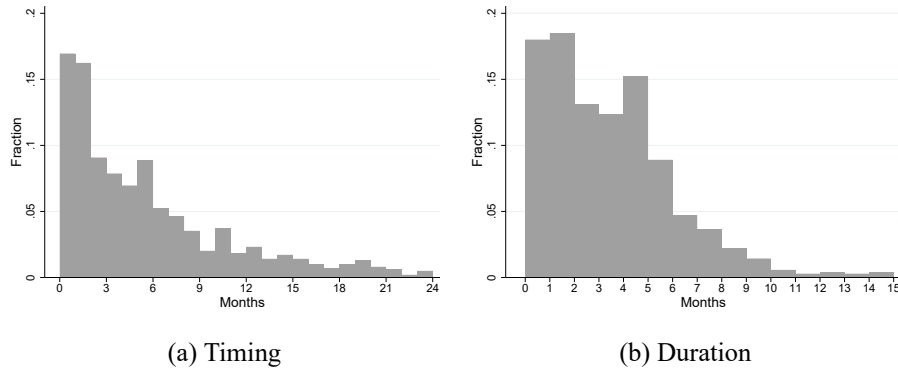
4 Empirical strategy

To evaluate the OLT program, we are interested in the average treatment effect on the treated (ATET). Since we are unable to observe what would have happened to the participants had they not taken part in the program, we use our rich set of covariates to compare participants with observably similar non-participants. If we are able to condition on all factors that determine assignment to the program and the outcome variables, the conditional independence assumption (CIA) is fulfilled and the difference can be given a causal interpretation. Section 4.1 discusses the fulfillment of the

²¹We do not have access to caseworker IDs in our data.

²²Because we do not want to condition on a successful match, we also include individuals where we can only observe an introductory course (and no on-the-job training) in the data.

Figure 1: Timing and duration of On-the-Job Language Training (months)



Note: In (a), time is measured between the date of registration at the job center and the start date of the introductory course. 62 participants start later than after 24 months. In (b), time is measured between the start date of the introductory course and the end date of the program. One program duration exceeds 15 months.

CIA in our setting.

Another problem we need to handle is that individuals can be assigned to the program at any point in time when registered at the job center. If individuals with poor language skills remain at the job center long enough, everyone may eventually be assigned to the OLT program. This implies that individuals with short spells at the job center will be over-represented among non-participants. Not taking this dynamic nature of the selection to the program into account may bias our results (Fredriksson and Johansson, 2008). In Section 4.2, we describe how applying the dynamic IPW proposed by van den Berg and Vikström (2022) addresses this problem.

In Section 4.3, we describe the implementation of the dynamic IPW approach, and in Section 4.4 show how the empirical strategy improves the balance in observables between participants and non-participants, and discuss potential remaining bias.

4.1 Controlling for confounding factors

To estimate the causal effect of the OLT program, we need to have access to all potential confounders affecting both program assignment and future outcomes.²³ One important caveat in our setting is that the OLT program participants have spent a relatively short time in Sweden, and hence, there is less information about them in the administrative data in terms of, for instance, labor market history. This makes it more difficult to fulfill the CIA assumption, which is important to bear in mind. However, the fact that our data still provide us with a number of potentially

²³Due to the dynamic setting, the sequence of potential outcomes has to be independent of assignment to the program at a given point time, given our observable characteristics at that time (dynamic CIA).

important controls may still make it possible for us to find non-participants similar to the participants in most critical aspects. Perhaps most importantly, we have access to information about individuals' full SFI course history before registering at the job center. This allows us to, at least indirectly, account for language skills, which is an important selection criteria into the program for the caseworkers at the local job centers. Since immigrants with better language skills are likely to have an easier time finding a job, we may underestimate the program's potential positive effects by not taking language skills into account. In addition, the data include a rich set of individual and household characteristics, native language, as well as migration, labor market, SA, PES, and job center history. These factors are likely known to the caseworker and may also influence his or her decision to assign a given client to the program. Since we also have information about individuals' previous medical drug prescriptions and hospitalizations, we may also be able to capture how the caseworker perceived the client's health situation, which could also affect selection to the program. Finally, we know at which local job center the assignment to the program takes place, which may capture variation in caseworkers' and job centers' propensity to assign their clients to the program.

Because matching on a large set of covariates is very demanding, we apply propensity score matching as suggested by Rosenbaum and Rubin (1983). However, since including unnecessary covariates in the propensity score leads to loss of efficiency, we limit the number of variables we include. The variable selection process is described in detail in Section 4.3.

4.2 Dynamic inverse probability weighting (IPW)

We apply dynamic IPW (van den Berg and Vikström, 2022) to take into account that individuals who are registered at the job center can be assigned to the OLT program at any point in time. In so doing, we compare individuals who enter the program after a certain time since registering at the job center, to observably similar non-participants who are still registered at the job center at this time, and have not entered the program when the outcome of interest is measured.

Let T_u denote the duration of enrollment at the job center, the *initial state*, and T_s the duration until assignment to the OLT program, the *treatment*. An individual with $T_u < T_s$ leaves the initial state before being treated. Let t_s be the time when an individual enters treatment. We denote the potential time in the initial state given treatment assignment at t_s , $T_u(t_s)$, the outcome of interest Y , and the potential outcome if the individual is assigned to treatment at time t_s , $Y(t_s)$. If an individual is assigned to never being treated, we let $T_u(\infty)$ and $Y(\infty)$ capture the potential duration and outcome, respectively. If the CIA is fulfilled, the average treatment effect of the

treated (ATET) is given by

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

The estimator of $ATE T(t_s)$, proposed by van den Berg and Vikström, gives greater weight to never-treated individuals who have been in the initial state for a long (rather than short) time. It is unbiased under the assumptions of sequential CIA, "no anticipation" (Abbring and van den Berg, 2003), common support and the Stable Unit Treatment Value Assumption (SUTVA). We use their short-run estimator since our outcomes could be measured before the individuals have left the initial state (the job center); that is, in period $t_s + \tau$ (i.e. τ periods after treatment starts). This estimator is given by:

$$\widehat{ATE T}(t_s) = \frac{1}{\pi(t_s)N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \frac{\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i}}{\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) + \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w_\tau^{t_s}(T_{u,i}, X_i)} \quad (2)$$

where $\pi(t_s)$ is the share treated of N_{t_s} , the number of never-treated individuals still in the initial state at the beginning of t_s . The weights w^{t_s} and $w_\tau^{t_s}$ are given by

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

$$w_\tau^{t_s}(X) = \frac{p(t_s, X)}{\prod_{m=t_s}^{t_s+\tau} (1 - p(m, X))} \quad (4)$$

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

The first line of Equation 2 gives the mean for the treated and is observed. The second line

gives the estimated outcome under no treatment, i.e. the weighted outcome of interest for non-treated individuals. Non-treated individuals who have left the initial state when the outcome is measured are given the weights in Equation 3, while non-treated individuals still in the initial state at τ (when the outcome is measured) are given the weights in Equation 4. $p(t, X)$ is the propensity to be treated in period t (Equation 5), where X represents the included covariates measured at t . The denominators of the weight equations imply that greater weight is given to individuals with longer time spent in the initial state.

Instead of reporting ATET for all different assignment periods, t_s , we present an aggregated ATET, which is obtained by averaging over the distribution of T_s :

$$\widehat{ATE}T = \frac{\sum_{t_s=1}^{T_u^{max}} \widehat{ATE}T(t_s) \pi(t_s) N_{t_s}}{\sum_{t_s=1}^{T_u^{max}} \pi(t_s) N_{t_s}} \quad (6)$$

In practice, the weights will be replaced by estimated weights based on estimated propensity scores for each t_u that the non-treated individuals are still at the job center. In our main specification, we want to capture how the ATET evolves over time, and hence, we estimate the propensity scores and calculate weights and ATET for each τ since having entered treatment (the OLT program). For example, when studying employment in quarter four, we calculate weights for non-treated individuals based on their estimated propensity to receive treatment during the periods they remain at the job center (up until quarter four). These weights are used to calculate employment under no treatment in the fourth quarter. This procedure is repeated for each follow-up time.

4.3 Implementation

Because only a limited number of individuals participate in the OLT program, entering on different days since enrollment at the job center, we have to aggregate over larger time intervals to estimate the dynamic IPW. We choose the length of our assignment periods (t_s) based on when participants enter the OLT program (see Figure 1a), trading off having a sufficient number of participants in a given assignment period and losing variation in the data. Table 2 displays the length and number of participants of each assignment period. Since most individuals enter the program early in their job-center spell, the first two assignment periods are one-month long, after which we gradually increase the length to two, three and finally nine months.²⁴

²⁴The last period is only considered when estimating the weights. In this period we aggregate all participants who start the OLT program after more than 18 months.

Table 2: Participants entering the On-the-Job Language Training (OLT) program per period

	Participants	Non-participants
	freq	freq
Month 1	168	11652
Month 2	161	10836
Month 3–4	168	9607
Month 5–6	157	7462
Month 7–9	133	5886
Month 10–18	160	4196
Month >18	106	1683
Total	1053	51322

Note: Participants starting the OLT program in the last period (Month > 18) are only used to estimate the weights used in the dynamic IPW, but are not included when estimating the treatment effects.

The next step entails choosing the variables to be included in the propensity score from the original pool of potential covariates (Table B1 in the Appendix), using LASSO (Tibshirani, 1996).²⁵ The variables chosen are shown in Table B2 in the Appendix. As expected, information about SFI course participation, time since immigration, native language and variables capturing unemployment and employment history are selected.

When we have chosen the set of confounders, we estimate the propensity scores using logistic regression models for each assignment period (t_s) and calculate the weights. To avoid extreme values of the weights among non-participants, we trim our sample by excluding individuals with weights exceeding one percent of the sum of weights for the non-participants (Huber et al., 2013). For more information about the estimated propensity scores and weights, see Table B3 in the Appendix.

For participating individuals, the follow-up period is defined in relation to the date they enter the program. However, there is naturally no such date for non-participants. For this group, we must hence impute program start dates. We do this by (within each assignment period) drawing a date with replacement from the pool of actual start dates for the participants. In the analysis, we exclude non-participants who, in a given period, have left the job center before their simulated start date, following Lechner (2002). We test the sensitivity of our results to the latter exercise in

²⁵To choose variables we pool our periods and estimate the propensity to be treated in any period (after partialling out the effect of the periods) using the *cvtlasso* command in STATA (Ahrens et al., 2019, 2020). A common approach in the literature is to use cross-validation to find the λ that is within one standard deviation from the λ_{opt} , λ_{1se} (Trevor Hastie et al., 2009). Due to the small number of participants, the λ_{1se} in our case excludes all variables. We choose a lambda that results in a reasonable number of covariates (21), $\lambda = 0.2 * \lambda_{1se}$, given that the logit in the propensity score estimation does not converge if we use too many covariates. However, we test how sensitive our results are to using different values of lambda in the sensitivity analysis.

Section 5.2.

Finally, to obtain standard errors, we use bootstrapping with 999 replications. For each bootstrap replication, we repeat the propensity score estimation and calculation of weights, giving us a distribution of potential differences between the participants and similar non-participants.

4.4 Balance

One way of evaluating if our empirical strategy is successful in finding comparable participants and non-participants, is to look at the normalized differences across the two groups. Normalized differences provide a way to assess balance that is independent of scale and sample size.²⁶ Table 3 summarizes the normalized differences before and after weighting (see Table B4 for more details). The mean normalized difference among the selected covariates (included in the propensity score) decreases from 0.55 to 0.012 after weighting. After weighting, none of the variables have a normalized difference above 0.05, and no difference in means is statistically significant at the five percent level. Our approach thus seems successful in achieving balance for the included variables. In Table B5 we also check the balance for the whole pool of covariates (163 variables). While our model heavily reduces the number of unbalanced variables even for this wide set of covariates, some imbalances remain.

Table 3: Summary of the normalized differences in means before and after dynamic IPW

	Normalized difference			P-values from t-test			
	Mean	Max	> 0.1	Mean	Min	< 0.05	N
Before	0.550	1.373	20	0.034	0.000	20	21
After	0.012	0.045	0	0.688	0.317	0	21

Note: > 0.1 shows the number of variables with a normalized difference above 0.1, < 0.05 shows number of variables with a p-value below 0.05 from a t-test of equal means and N the number of covariates included. The weights used are based on information 36 months after the program started. See Table B4 for more details.

If we fail to include all important confounders in the propensity score, our results will likely be biased. It is not clear to us if this bias will lead us to over- or underestimate the true effects of the OLT program. If the weighted non-participants are not in the program because they for instance have social or health-related issues, we will likely overestimate our results. However, since limited language skills are a prerequisite to participate, and individuals with better language skills can be

²⁶It is the absolute difference between the means between participants and non-participants, normalized by the standard deviation among participants.

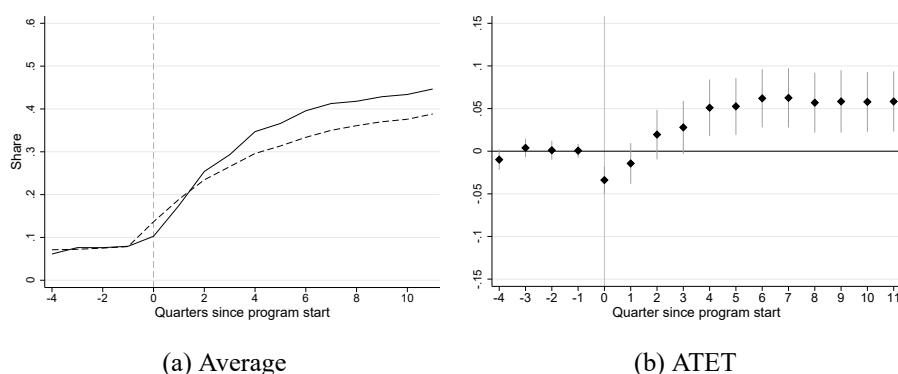
expected to do better in the labor market, we may also underestimate our results if our controls do not perfectly capture this factor. Limited language skills (conditional on time in Sweden) may potentially also reflect a more limited ability to learn in general, which can also be expected to lead to a downward bias in our estimates. There are thus also reasons to believe that our analysis would underestimate the results.

As another way to evaluate the robustness of our empirical strategy and set of confounders, we show estimated placebo effects of program participation for the pre-program year in the results section.

5 Results

We are interested in how participation in the OLT program affects the labor market integration of immigrants. Figure 2 shows the effect of participating in the program on employment, comparing participants and observably similar non-participants. Figure 2a describes how the share employed among participants and the weighted non-participants evolves one year before the program start until three years thereafter. Figure 2b shows the difference (ATETs) between these two lines, with 95 percent confidence intervals (CIs). Before we discuss the results, it is important to note that the analysis does not estimate the effect of participating in the program compared to inactivity, but rather compared to “business as usual”, or, taking part in other activities at the local job center.²⁷

Figure 2: Results, employment



Note: Employment is defined as receiving any earnings in a given quarter. The solid line in Figure 2a shows the share employed among participants, and the dashed line among the weighted non-participants. The squares in Figure 2b mark the difference between the two, and the vertical lines the 95 percent CIs based on 99 bootstrap replications. Point estimates, standard errors and weighted average for non-participants are shown in Table B6.

²⁷We discuss these counterfactual activities further in Section 6.

The lack of pre-program employment effects indicates that our approach is successful in finding non-participants that are good matches to the participants in terms of employment history. Both groups follow the same trend in employment (Figure 2a) and the ATETs are very close to zero (Figure 2b).²⁸

The first quarter of the program (quarter zero) we see a clear negative effect on employment, which we interpret as a lock-in effect while participants are still enrolled in the program. After this, there is a gradual increase in employment which is steeper for the participants compared to the weighted non-participants. From the fourth quarter, the employment rates are 5–6 percentage points higher among participants, and these positive employment effects persist throughout our three-year follow-up period. The positive effect in the last quarter corresponds to an increase in employment of 15 percent compared to the weighted controls.

Are these positive labor market effects explained by subsidized employment, for instance, through the cooperation between the ULSI and the Youth Employment Program (see Section 2)? In Figure A3 in the Appendix, we divide the employment variable into regular and subsidized employment. The figure shows that the program leads to a positive ATET for both these employment types, which indicates that the program works as a stepping-stone to more labor market oriented activities for some, while it helps others enter the regular labor market directly. A year after program start, subsidized employment makes up about 50 percent of total employment among participants, while this share decreases to about 25 percent after three years. This furthermore suggests that, while subsidized employments are relatively important in the short-run, its relative importance decreases over time as more participants are able to find regular employment.

We are also interested in quantifying the total economic gains or losses from participating in the program. We therefore study total labor income and income from SA. In Table 4, we show the estimated effects for (i) the year prior to program start (to assess pre-trends), (ii) the six-month period when most individuals are still enrolled in the program, and (iii) the following 2.5 years when most individuals have left the program.

The first panel shows that, in terms of earnings (Column 1), the group we study is doing very poorly; in the year prior to the (fictitious) program start, weighted non-participants earn SEK 3,500. Column 2 shows that SA is a much more important source of income for this group in the year prior to entering the program – the weighted non-participants on average receive SEK 37,000. The

²⁸It is however important to note that only a very low share of the individuals we study are employed during the pre-period, and the variable hence contains limited information.

Table 4: Effect on total earnings, social assistance and SFI course completion

	(1) Earnings	(2) Social assistance	(3) Pass SFI course
<i>Pre-program effects, month -12 to -1</i>			
ATET	-269	599	0.007
St err	362	1,329	0.012
Mean	3,459	37,289	0.192
<i>During program effects, month 0–6</i>			
ATET	-1,599	1,946	0.021
St err	720	811	0.015
Mean	9,416	25,957	0.214
<i>Post-program effects, month 7–36</i>			
ATET	26,090	-7,390	0.014
St err	6,846	3,404	0.018
Mean	136,803	92,214	0.400

Note: Earnings and social assistance are measured in 2019 SEK. Pass SFI course is measured as the likelihood to pass an SFI course within the specified time interval. Mean is the average among the weighted non-participants and St err refers to standard errors obtained through bootstrapping with 999 replications.

differences between the participants and the weighted non-participants in the pre-period are small and not statistically significant.

During the first six months following program start, we see indications of a lock-in effect for participants (as in Figure 2) – they earn less and rely more on SA payments. However, after the program is expected to have ended, participants experience a SEK 26,100 increase in earnings over the following 2.5 years, compared to weighted non-participants. In relative terms, this corresponds to an increase of 19 percent compared to the mean among non-participants. There is also a small decrease in SA reciprocity of approximately eight percent.²⁹ Combining these two estimates, participants' total post-program income increases by SEK 18,700 or eight percent.

5.1 Swedish language skills

One of the explicit goals of the OLT program is to improve the participants' language skills. Even if we cannot measure language skills directly, we can study how individuals progress in the formal language training system, SFI. We focus on an indicator variable for passing any additional SFI course within a given time span in relation to starting the OLT program.

The pre-program effect on the likelihood of receiving a passing grade in SFI before starting the

²⁹We also show how the effects on earnings and SA reciprocity evolves over time in figures A4–A5 in the Appendix.

program in the third column of Table 4 is reassuringly close to zero. We are thus not comparing individuals with very different recent SFI course history when they (fictitiously) enter the program, even if part of the selection is based on language skills.

In the second panel in Table 4, we see a positive ATET on passing SFI during the program corresponding to 10 percent. This point estimate is however very imprecisely estimated. For the 2.5 years following the end of the program, the corresponding ATET is positive, but noisy and close to zero.

Whereas these imprecise estimates do not allow us to conclude that the OLT program has a positive effect on the likelihood to pass an additional SFI course, they at least allow us to rule out any sizable negative effects, hence indicating that the OLT program does not crowd out language acquisition. This result contrasts previous findings in Arendt (2022) and Arendt and Bolvig (2023).³⁰

5.2 Sensitivity analysis

In this section, we test how sensitive our main results are to the choices we have made in implementing the analysis.

First, to be able to give our estimates a causal interpretation, the CIA must hold. The covariates we include in the propensity score must hence capture all variables that affect both assignment to the OLT program and the outcomes we study. While we have access to a large number of potential confounders, there is still a risk that relevant variables are missing, which is important to bear in mind when interpreting our results. The data-driven LASSO approach we use to select which variables to include in the propensity score estimation is appealing as we do not choose which variables to include ourselves (from the pool of available covariates). However, we need to choose the value of λ for the cross-validation. To test the sensitivity of this choice we vary the value of λ and thereby the number of covariates included in the propensity score.³¹ If there is large variation depending on the specification, this may also indicate that we have issues fulfilling the CIA. While the interpretation of the point estimates does not change, and they for the most part remain sizable, we see in Figure A6 that adding more covariates to the propensity score decreases the size of the point estimates somewhat. This indicates that we should interpret the absolute size of our point

³⁰Arendt (2022) shows that a similar Danish work-first program decreased participation in language training, and the study by Arendt and Bolvig (2023) confirms that this did indeed lead to fewer language course grades passed and lower test scores in a language test.

³¹Table B2 lists the variables that are included for different values of λ .

estimates with some caution.

Second, we test how sensitive our results are to the fact that we remove non-participants who exit the job center before their fictitious program start date. As Figure A7 shows, this only marginally affects our point estimates. In Figure A8, we also show that the estimated effects on the likelihood of passing an SFI class are robust to these tests, especially during the post-program period. Including non-participants who have left the job center in the analysis even leads to larger estimated effects.

Finally, when we study the pre-program effects, we need to use weights estimated when individuals have already entered the program. One potential concern is that the absence of pre-program effects is only valid for the weights we use (from quarter zero). To make sure that this is not the case, we show the estimated effects during the pre-program year using weights calculated with information for different follow-up periods. As seen in Figure A9, the pre-program effects are still close to zero.

5.3 Effect heterogeneity

We also examine whether the effects of the OLT program mask differences across sex and level of education.

Even if labor market integration is often a long process for both immigrant women and men, this is especially true for women. They have, on average, reached a lower level of formal education and obtained less work experience in their country of origin (Albrecht et al., 2021). With this in mind, it is worrying that they are also less likely to participate in introduction programs and ALMPs than men (Albrecht et al., 2021; Arendt, 2022; Bratu et al., 2025), and when they do, tend to get less access to labor market oriented ALMPs (Andersson Joona, 2020; Cheung, 2018). Furthermore, several previous papers (e.g. Andersson Joona and Nekby (2012); Arendt (2022); Dahlberg et al. (2024)) fail to find positive effects of ALMPs for immigrant women. The OLT program may thus benefit women and men differently.³²

We also examine whether the results differ based on participants' educational attainment.³³ Individuals with lower levels of formal education may face higher frictions in the labor market,

³²Note that, as women are over-represented among participants, dividing the sample by sex gives two unevenly sized groups (69 percent of participants are women, see Table 1). Estimate precision is thus higher for women.

³³We divide the sample based on the highest obtained education and define individuals with a low level of formal education as those with at most compulsory schooling, and those with at least high school education as highly educated. This gives us two fairly evenly sized groups with 55 percent of participants having obtained the lower level of formal education, see Table 1.

for instance because they have fewer formal qualifications. They have also been found to benefit less from formal language training than individuals with higher educational attainment, potentially due to lower learning efficiency (Lochmann et al., 2019). The latter may lead to more limited local language skills. Taken together, this implies that individuals with different levels of formal education may also benefit differently from different types of ALMPs. For instance, Battisti et al. (2019) find that job-matching support for recently arrived refugees in particular increases employment for refugees with low levels of educational attainment. The finding that formal language training does not benefit individuals with low levels of educational attainment to the same extent (Lochmann et al., 2019), also implies that a program like the OLT program may be used to promote the language skills of individuals who struggle to learn a language in the classroom.

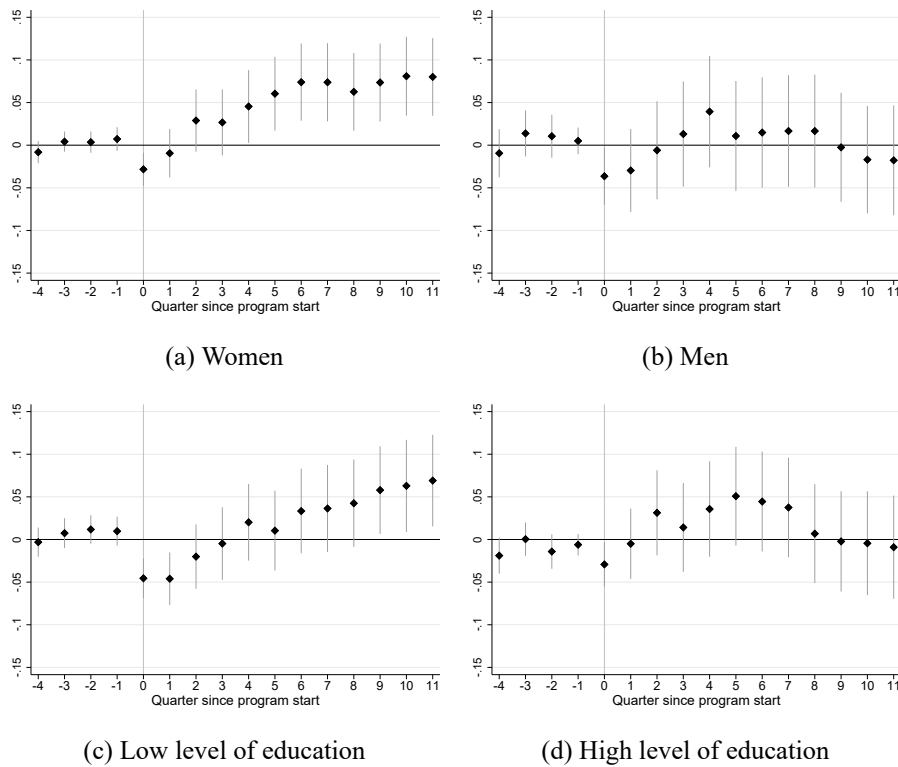
We start by studying the effects on employment, and how these evolve over time.³⁴ As expected, we note that employment rates are lower among women than among men before the start of the program (Figure A10 in the Appendix). Just before starting the program, the employment share is about twice as high for men compared to women. The difference by educational attainment is smaller, but individuals with higher educational attainment do slightly better.

Figure 3 shows that, as in the main analysis, there is evidence of an initial lock-in effect of participating in the program across all groups. However, while there are clear positive post-program effects for women (Figure 3a) reaching approximately eight percentage points and seemingly persisting over time, the effects are very close to zero for men (Figure 3b). As we divide employment by whether it is subsidized or not, we see that women's employment increases in both these categories, while it is unaffected for men (see Figure A11 in the Appendix).

As we split the sample by educational attainment (figures 3c–3d), we also see quite different patterns by educational level. For individuals with a low level of education, the initial lock-in effect lasts longer, but from the sixth quarter, participants are 3.5–7 percentage points more likely than weighed non-participants to be employed. For individuals with higher levels of formal education, participation in the OLT program is associated with positive employment effects in quarters 2–7. However, for the last year of our follow-up period, the point estimates are close to zero. Figure A12 in the Appendix reveals that this more temporary increase in employment for individuals with high levels of education may in part be explained by a relative decrease in the likelihood of having

³⁴We select which set of covariates to include in the propensity score using LASSO for each of the subsamples used separately. There is some but limited variation in which variables are chosen for the different subsamples, see Table B7. Table B8 displays the number of participants for each group and period. As in the main analysis, we have overlap (Table B9) and balance in covariates between non-participants and participants after weighting (Table B10) Examining the balance using our full set of potential confounders, Table B11 shows that we obtain reasonable balance for all groups.

Figure 3: Employment results by sex and educational attainment



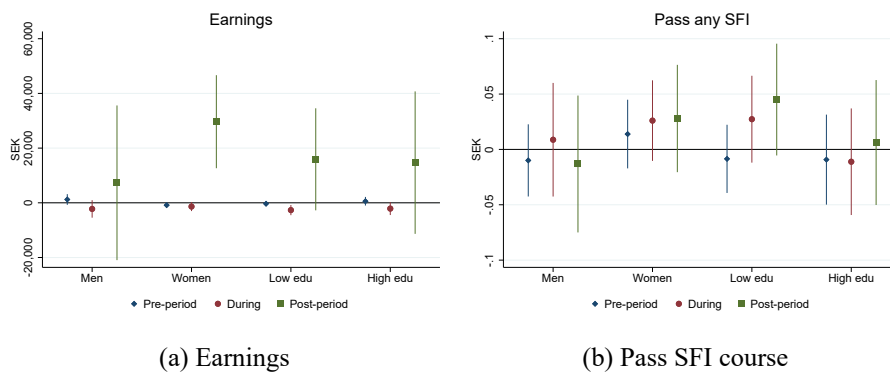
Note: The squares shows the ATET and the vertical lines the 95 percent CIs based on 999 bootstrap replications. Low level of education refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. Point estimates and standard errors are shown in tables B12–B13.

a subsidized employment in the final year of our follow-up period. One way to interpret this is that the program gives them earlier access to subsidized employments. However, also regular employment is lower during the final year. Individuals with lower levels of education seem to get access to subsidized employment later (compared to individuals with higher levels of education). The effects in the last three quarters of our follow-up period are around five percentage points and statistically significant at the five percent level. For regular employment, the effects are very modest and not statistically different from zero.

We can also observe in what type of workplace individuals find employment after the OLT program. To see if this varies by sex and educational attainment, we estimate the program's effect on the probability to work at a municipal and non-municipal workplace by subgroup, see Figure A13. It is clear that the OLT program especially affects the probability to work at a municipal workplace: the point estimates are positive and sizable for all groups, even if the 95 percent confidence interval does not exclude a zero effect for men. For women, municipal employment increases by

31 percent. Participating women are also three percentage points more likely to work in a non-municipal workplace, while the point estimate for men and those with higher levels of educational attainment are negative. These results imply that the program provides a pathway to future municipal employment, but may crowd out non-municipal employment for some groups (for whom non-municipal workplaces are relatively common). This could explain the zero results for men's total employment.

Figure 4: Earnings and SFI results by sex and educational attainment



Note: The markers show difference between participants and weighted non-participants in a given time period, and the vertical lines the 95 percent CIs based on 999 bootstrap replications. Low edu refers to having studied at most compulsory schooling, and High edu to having studied more than compulsory schooling. Point estimates, standard errors and weighted average for non-participants are shown in Table B14 and Table B15.

We also examine whether the employment effects translate into earnings (Figure 4a). For women, there is a positive earnings effect of SEK 30,000 or 27 percent, over 2.5 years after the program, while there is no relative increase in earnings for participating men. As we split the sample by educational attainment, the point estimates indicate sizable positive post-program earnings effects of around SEK 15,000. In relative terms, earnings increase by 12 percent for individuals with low levels of educational attainment, and nine percent for those with higher levels of formal education (see Table B14).

Finally, we investigate the effects of the OLT program on SFI-course completion by subgroup in Figure 4b. Based on these imprecisely estimated results, we cannot conclude that there are any substantial differences between women and men in terms of how the program affects language skills. However, there is a relatively large (but imprecise) effect for individuals with low formal educational attainment. For this group, the probability to pass a course increases by 2.7 percentage points (14 percent) during the program, and by 4.5 percentage points (12 percent) in the post-program period. For individuals with high educational attainment we see no effect. These results

may explain the fact that the positive effects on employment for individuals with less formal education emerge later (see Figure 3): if the OLT program helps some participants finish their formal Swedish language training, this may in turn prepare them to eventually enter the labor market. As previous research has shown that language training in the classroom particularly benefits highly educated immigrants (Lochmann et al., 2019), the OLT program may work as a complement for those with less prior formal education who tend to do less well in the classroom.

6 Mechanisms

What is it about the OLT program that leads to increased employment? Is it having a bilingual caseworker, the early labor market experience the on-the-job training provides, or the focus on finding workplaces that are suitable to practice Swedish? Ideally, we would like to be able to separate between these three components. Since this is unfortunately not fully possible, the following section discusses different indirect pieces of evidence that may help us learn something about the mechanisms.

One way to examine the importance of the bilingual coaches is to study the effect of the program by whether or not you had access to a bilingual coach speaking your native language, or had a coach who spoke “simple Swedish”. However, because only 15 percent of the participants speak other languages than Arabic, Somali, Tigrinya and Dari, it is not possible to split the sample by access to a coach speaking ones’ native language. Instead, to indicate if the results are markedly different depending on bilingual coach access, we do a subgroup analysis using only individuals who have Arabic, Somali or Tigrinya registered as their native language.³⁵ In Figure A14, we compare the earnings effect from the main analysis to the effect for the sample restricted to participants who had access to bilingual coaches. If the results were explained only by having had access to a bilingual coach, we would expect to see larger post-program earnings effects for the group where individuals without this have been removed. The ATET is in fact approximately SEK 10,000 higher for the full sample than for the sample restricted to participants with access to bilingual coaches (but the confidence intervals overlap). This could indicate that the bilingual caseworker access is not the main explanation for the positive effects we find in the main analysis, or at least not compared to having a caseworker speaking “simple Swedish”. However, this analysis is not a direct test of the bilingual coach mechanism, and the groups may be different with regard to other important aspects

³⁵For simplicity, we also remove Dari speakers from this analysis as bilingual coaches only became available to them in 2018 (they make up three percent of the participants in our sample).

that we do not capture with our estimation strategy. Furthermore, the results do not rule out that the additional support the caseworkers provide to participants and supervisors in the workplace are important.

Studying participants' counterfactual activity participation may also tell us something about the mechanisms. Figure A15 in the Appendix plots the share of weighted non-participants who are enrolled in other types of activities (like job search, courses or regular on-the-job training) during the approximate expected duration of the OLT program. If we interpret this as the program content participants would have gotten access to had they not enrolled in the OLT program, we can note that whereas at least 50–60 percent get access to “job search, coaching and guidance” – which is also essentially part of the OLT program – few would have come into closer contact with the labor market during these six months. Only around 20 percent participate in on-the-job training and less than five percent in public sector employment. This indicates that getting access to labor market oriented activities at least to some extent sets participants apart from non-participants. Even if the differences in overall activity participation across subgroups are in general small, we see that women, who explain the positive employment effects (see Section 5.3), are even less likely to get access to regular on-the-job training programs in the absence of the OLT program than men (in line with findings in Andersson Joona (2020), Cheung (2018) and Arendt (2022)). This may be one reason why the weighted non-participating women do worse than the corresponding men in terms of employment (see Figure A10).

However, something that may speak against the early labor market experience as being the sole channel behind the positive employment effects is the lack of medium-run employment effects found for the on-the-job-training program targeting recent immigrants in Denmark (Arendt and Bolvig, 2023). In contrast to the Danish program, the OLT program does not seem to crowd out language training (see Table 4). The matching of participants to workplaces that are suitable for practicing Swedish may be one explanation for this difference compared to the results in Arendt and Bolvig (2023). Furthermore, the finding that participation in the OLT program increases the likelihood to pass another SFI course for individuals with low educational attainment may indicate that the program can even work as a complement to formal language training (see Section 5.3). As a further indication of this, Figure A15 in the Appendix suggests that individuals with lower educational attainment are less likely to enroll in activities closer to labor market entry and more likely to participate in preparatory measures, which may indicate that they need to improve their human capital and/or Swedish skills further before being ready to enter the labor market. The

combination of a workplace-based policy with non-formal language training may thus be key for the success of the OLT program.

Finally, one way to analyze the importance of the language training component is to analyze how the workplace's worker composition differs between participants and non-participants who are employed after the program. If participants are more likely to be employed in workplaces with a higher share native Swedes, this may imply that they have obtained skills that are in demand in workplaces where speaking Swedish is likely more important. We see no clear evidence of this in Table B16, but the estimates are quite imprecise. In the selected sample of employed individuals, the share Swedish-born coworker is very similar for participants and weighted non participants. The only group where the Swedish-to-foreign-born worker composition increases (by 15 percent) is for individuals with higher levels of formal education. This may indicate that those with higher human capital, who are likely closer to finding employment, benefit from the program by getting access to new types of workplaces, potentially because they did their on-the-job training there.³⁶

7 Conclusions

Immigrants' labor market integration takes time in many Western countries, and policies aiming to promote this process are still of great public interest. In this paper, we contribute to these policy discussions through studying a new public policy initiative in Sweden, an active labor market program targeted at immigrants with limited host country language skills. In particular, the program consists of a three-month on-the-job training program in a regular workplace, which is facilitated by support and coaching by a bilingual caseworker speaking the participant's native language. In the workplace, participants are not only supposed to gain valuable labor market experience, but also get the opportunity to improve their Swedish language skills through communicating with coworkers.

We compare and follow participants and observably similar non-participants for up to three years after entering the program using rich administrative data and a dynamic IPW approach (van den Berg and Vikström, 2022). After an initial lock-in effect, we find that program participation increases employment by around 5–6 percentage points: effects that persist throughout our follow-up period.³⁷ By the end of the period, this corresponds to a 15 percent increase in em-

³⁶However, we do not know in what firm they did their on-the-job training.

³⁷While our sensitivity analyses indicate that our results are robust, it is important to bear in mind that there is a risk that we do not include all relevant variables in the weighting. The results should hence be interpreted with some caution.

ployment compared to the weighted non-participants. Furthermore, these effects are explained by both subsidized and regular employments, and imply an increase in total earnings by 19 percent during the 2.5 years following the end of the program.

As we divide the sample by sex, we find that the positive labor market effects are explained by women, whereas we find no increase in employment or earnings for men. One explanation for this may be that the program leads to an increase in subsidized employment for women, which, together with their participation in the OLT program – implies that they get access to more labor market oriented activities than in the absence of the program. In general, immigrant women are less likely than men to get access to this type of programs (Andersson Joona, 2020; Cheung, 2018), something that we also observe in our sample of similar non-participants.

We also divide the sample by level of education, and find that, while both groups experience an increase in post-program earnings compared to their weighted non-participants, the employment effect is clearly dominated by subsidized employments for individuals with less formal education. These employment effects furthermore emerge relatively late in the follow-up period. One explanation for this may be that this group needs further human capital investments to be ready to enter the labor market. One finding that supports this interpretation is that program participation increases the likelihood to complete a Swedish language training course with 12–14 percent for individuals with a lower level of education. Since this group could be expected to do less well in formal language training (Lochmann et al., 2019; Pont-Grau et al., 2023), the OLT program may serve them as a complement to classroom training, improving their Swedish skills, and thereby helping them to advance also with their formal language training.

Overall, our results are more encouraging compared to previous work by Arendt (2022) and Arendt and Bolvig (2023). They find that participating in an on-the-job training within the first year since arrival in Denmark crowds out time spend in language class, lowers the number of classes passed as well as language test scores. Furthermore, they find no lasting effects on employment. The support and access to a bilingual caseworker, as well as the emphasis put on non-formal language training in the workplace, may explain why our results are less discouraging. On the aggregate, we find small albeit imprecise positive effects on language training completion, which at least allow us to rule out any sizable negative effects.³⁸

³⁸To evaluate the effectiveness of the program we would ideally like to weight the cost of the program against its benefits. The administration at the Labor market unit in the City of Stockholm estimates that the caseworker-to-participant ratio is similar for the OLT program compared to the local job centers, but are not able to estimate if total costs are higher or lower for the OLT program compared to the counterfactual program participation.

There is a risk that the program leads to displacement effects, that is, that the participants find employment at the expense of other unemployed individuals and that the program hence does not generate any net employment gains (Crépon et al., 2013). While this is inherently difficult to measure, the fact that we find most employment effects to come from the municipal sector – a sector with a large and growing shortage of staff – may limit these concerns (SKR, 2022).

To summarize, our results indicate that programs like the On-the-Job Language Training program may work (i) as a complement to formal language training for individuals with less formal education, and (ii) to give immigrant women access to more labor market oriented activities early on in the integration process (eventually helping them obtain employment). Both findings are important as immigrant women and immigrants with less formal education are groups that struggle to integrate in the labor market, and evidence identifying effective integration activities for these group have been scarce.

References

- Abbring, J. H. and van den Berg, G. J. (2003). The Nonparametric Identification of Treatment Effects in Duration Models. *Econometrica*, 71(5):1491–1517.
- Ahrens, A., Hansen, C. B., and Schaffer, M. E. (2019). lassopack: Model selection and prediction with regularized regression in Stata. *The Stata Journal*, 20(1):176–235.
- Ahrens, A., Hansen, C. B., and Schaffer, M. E. (2020). LASSOPACK: Stata module for lasso, square-root lasso, elastic net, ridge, adaptive lasso estimation and cross-validation. Statistical Software Components.
- Albrecht, C., Pérez, M. H., and Stitteneder, T. (2021). The integration challenges of female refugees and migrants: Where do we stand? *CESifo Forum*, 22(2):39–46.
- Andersson Joona, P. (2020). Flykting- och anhöriginvandrares väg till arbete. SNS-Rapport.
- Andersson Joona, P., Lanninger, A. W., and Sundström, M. (2016). Reforming the Integration of Refugees: The Swedish Experience. IZA Discussion Papers 10307.
- Andersson Joona, P. and Nekby, L. (2012). Intensive Coaching of New Immigrants: An Evaluation Based on Random Program Assignment. *The Scandinavian Journal of Economics*, 114(2):575–600.
- Arendt, J. N. (2022). Labor market effects of a work-first policy for refugees. *Journal of Population Economics*, 35(1):169–196.
- Arendt, J. N. and Bolvig, I. (2023). Trade-offs between work-first and language-first strategies for refugees. *Economics of Education Review*, 92:102353.
- Attar, Z., Blom, E., and Le Pichon, E. (2020). Towards more multilingual practices in the mathematics assessment of young refugee students: effects of testing language and validity of parental assessment. *International Journal of Bilingual Education and Bilingualism*, 25(4):1–16.
- Battisti, M., Giesing, Y., and Laurensyeva, N. (2019). Can job search assistance improve the labour market integration of refugees? Evidence from a field experiment. *Labour Economics*, 61:101745.

- Bratu, C., Martén, L., and Ottosson, L. (2025). Individualized benefits and access to active labor market programs boost refugee women's economic integration. *The Review of Economics and Statistics*, Accepted.
- Brell, C., Dustmann, C., and Preston, I. (2020). The Labor Market Integration of Refugee Migrants in High-Income Countries. *Journal of Economic Perspectives*, 34(1):94–121.
- Butschek, S. and Walter, T. (2014). What active labour market programmes work for immigrants in Europe? A meta-analysis of the evaluation literature. *IZA Journal of Migration*, 3(1):48.
- Cheung, M. (2018). Hur skapar vi en mer jämställd arbetsmarknadsetablering? Arbetsföremdlings Working Paper 2018:2.
- Chin, A., Daysal, N. M., and Imberman, S. A. (2013). Impact of bilingual education programs on limited English proficient students and their peers: Regression discontinuity evidence from Texas. *Journal of Public Economics*, 107:63–78.
- Chiswick, B. R. and Miller, P. W. (1995). The Endogeneity between Language and Earnings: International Analyses. *Journal of Labor Economics*, 13(2):246–288.
- Cortes, K. (2004). Are refugees different from economic immigrants? Some empirical evidence on the heterogeneity of immigrant groups in the United States. *Review of Economics and Statistics*, 86(2):465–480.
- Crépon, B., Duflo, E., Gurgand, M., Rathelot, R., and Zamora, P. (2013). Do labor market policies have displacement effects? evidence from a clustered randomized experiment. *The Quarterly Journal of Economics*, 128(2):531–580.
- Dahlberg, M., Egebark, J., Vikman, U., and Özcan, G. (2024). Labor market integration of refugees: RCT evidence from an early intervention program in Sweden. *Journal of Economic Behavior & Organization*, 217:614–630.
- Dressman, M. and Sadler, R. W. (2020). *The handbook of informal language learning*. John Wiley & Sons.
- Foged, M., Hasager, L., Peri, G., Arendt, J. N., and Bolvig, I. (2022). Language Training and Refugees' Integration. *The Review of Economics and Statistics*, pages 1–41.

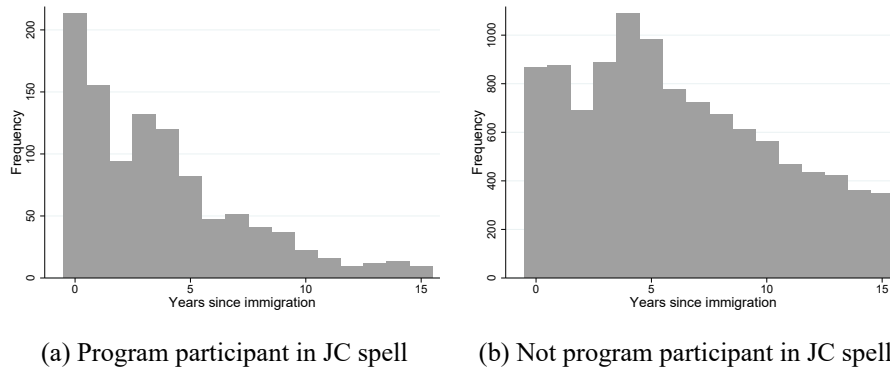
- Fredriksson, P. and Johansson, P. (2008). Dynamic Treatment Assignment. *Journal of Business & Economic Statistics*, 26(4):435–445.
- Helgesson, P., Jönsson, E., Ornstein, P., Rödin, M., and Westin, U. (2020). Equal Entry – can job search assistance increase employment for newly arrived immigrant women? *Arbetsföremödingen analys* 2020:10.
- Heller, B. H. and Mumma, K. S. (2023). Immigrant Integration in the United States: The Role of Adult English Language Training. *American Economic Journal: Economic Policy*, 15(3):407–437.
- Huber, M., Lechner, M., and Wunsch, C. (2013). The performance of estimators based on the propensity score. *Journal of Econometrics*, 175(1):1–21.
- Lechner, M. (2002). Some Practical Issues in the Evaluation of Heterogeneous Labour Market Programmes by Matching Methods. *Journal of the Royal Statistical Society Series A: Statistics in Society*, 165(1):59–82.
- Lochmann, A., Rapoport, H., and Speciale, B. (2019). The effect of language training on immigrants’ economic integration: Empirical evidence from France. *European Economic Review*, 113:265–296.
- Mörk, E., Ottosson, L., and Vikman, U. (2022). To work or not to work? Effects of temporary public employment on future employment and benefits. IZA discussion Paper No. 15071.
- Pont-Grau, A., Lei, Y.-H., Lim, J. Z. E., and Xia, X. (2023). The effect of language training on immigrants’ integration: Does the duration of training matter? *Journal of Economic Behavior & Organization*, 212:160–198.
- Rosenbaum, P. R. and Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1):41–55.
- Sarvimäki, M. and Hämäläinen, K. (2016). Integrating Immigrants: The Impact of Restructuring Active Labor Market Programs. *Journal of Labor Economics*, 34(2):479–508.
- Sianesi, B. (2004). An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s. *The Review of Economics and Statistics*, 86(1):133–155.

- Sianesi, B. (2008). Differential effects of active labour market programs for the unemployed. *Labour Economics*, 15(3):370–399.
- SKR (2022). Valfärdens kompetensförsörjning personalprognos 2021–2031 och hur välfärden kan möta kompetensutmaningen. Sveriges Kommuner och Regioner.
- Tibshirani, R. (1996). Regression Shrinkage and Selection Via the Lasso. *Journal of the Royal Statistical Society: Series B (Methodological)*, 58(1):267–288.
- Trevor Hastie, Robert Tibshirani, and Jerome Friedman (2009). *The Elements of Statistical Learning Data Mining, Inference, and Prediction*. Springer Series in Statistics. Springer, New York.
- van den Berg, G. J. and Vikström, J. (2022). Long-Run Effects of Dynamically Assigned Treatments: A New Methodology and an Evaluation of Training Effects on Earnings. *Econometrica*, 90(3):1337–1354.
- Vikman, U. and Westerberg, A. (2017). Arbetar kommunerna på samma sätt? Om kommunal variation inom arbetsmarknadspolitiken. Rapport 2017:7, IFAU - Institute for Evaluation of Labour Market and Education Policy.

Appendix

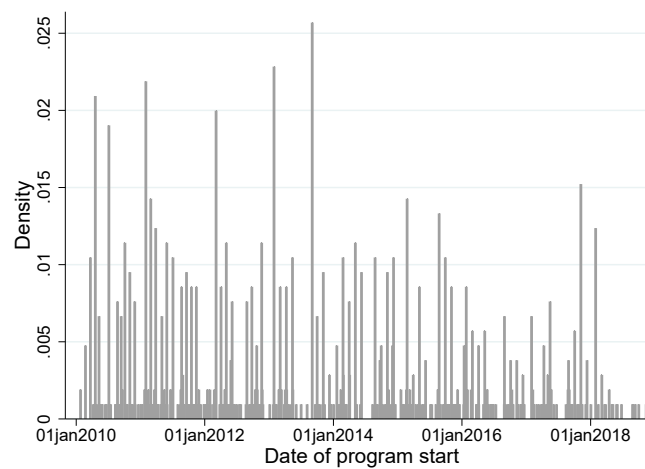
A Additional figures

Figure A1: Years since immigration at the time of JC registration



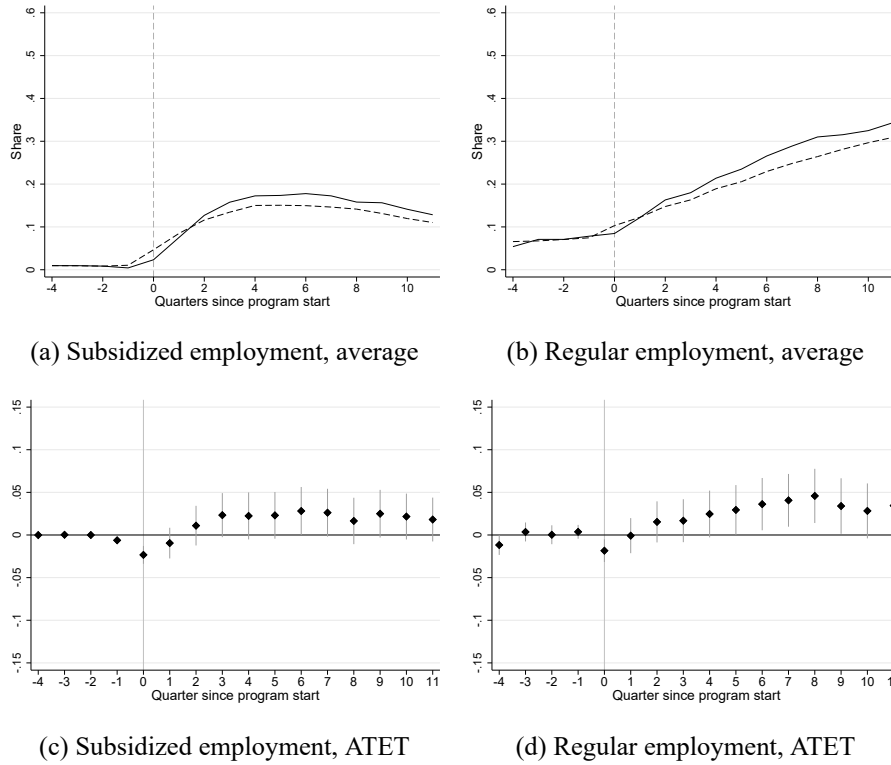
Note: JC refers to job center. (a) includes individuals who participate in the OLT program at some point during their job center spell, and (b) those who do not.

Figure A2: OLT program start dates



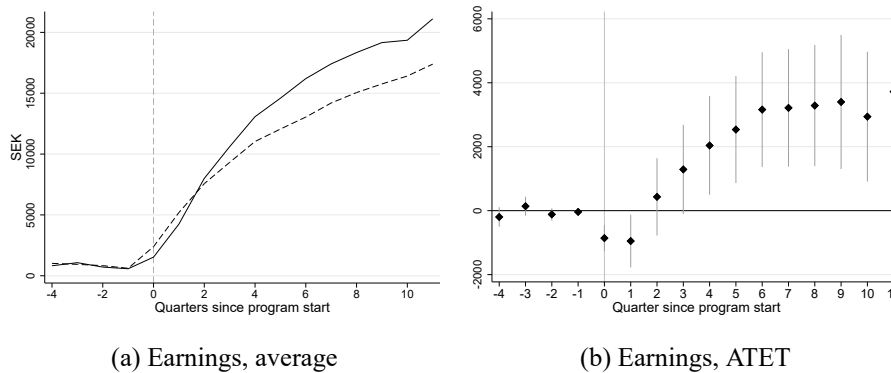
Note: The figure shows the distribution of participants at different start dates in calendar time.

Figure A3: Results, subsidized and regular employment



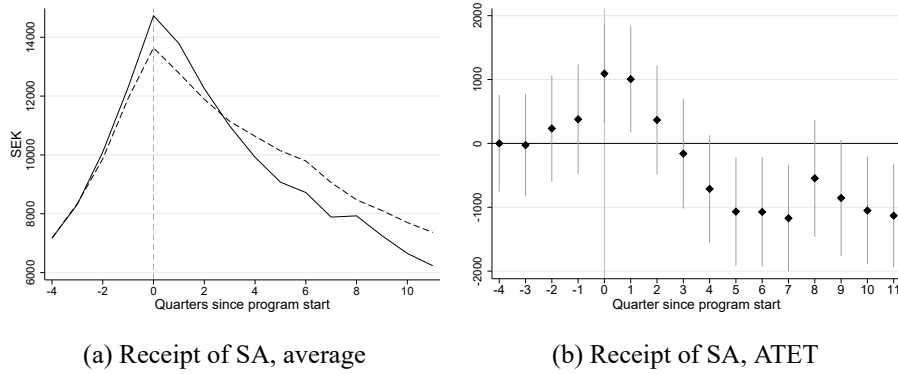
Note: The solid line in Figures A3a and A3b shows the share for participants, and the dashed line for the weighted non-participants. The squares in Figures A3c and A3d mark the difference between the two, and the vertical lines the 95-percent CIs based on 999 bootstrap replications. Point estimates, standard errors, and weighted averages for non-participants are shown in Table B6.

Figure A4: Results, earnings (SEK)



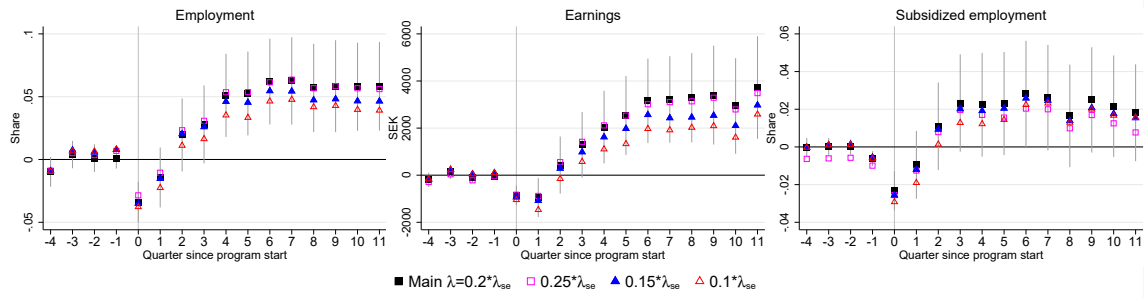
Note: Earnings is measured as the sum of earnings (in 2019 SEK) in a given quarter. The solid line in Figure A4a shows the average earnings for participants, and the dashed line for the weighted non-participants. The squares in Figure A4b mark the difference between the two, and the vertical lines the 95-percent CIs based on 999 bootstrap replications. Point estimates, standard errors, and weighted averages for non-participants are shown in Table B6.

Figure A5: Results, social assistance amount (SEK)



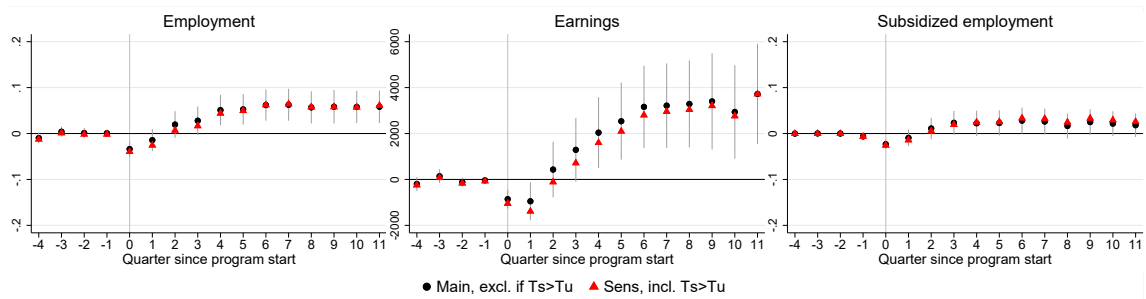
Note: The solid line in Figure A5a shows the share with SA for participants, and the dashed line for the weighted non-participants. The squares in Figure A5b mark the difference between the two and the vertical lines the 95- percent CIs based on 999 bootstrap replications. Point estimates, standard errors, and weighted averages for non-participants are shown in Table B6.

Figure A6: Sensitivity, different set of confounders



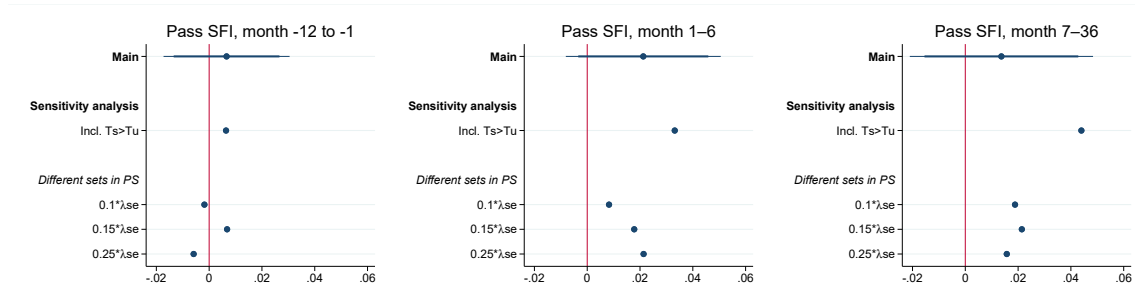
Note: The different sets of confounders included depending on the value of lambda are specified in Table B2. The 95-percent CIs shown are estimated through bootstrapping with 999 replications using the main specification.

Figure A7: Sensitivity, including non-participants with simulated treatment date after leaving job center



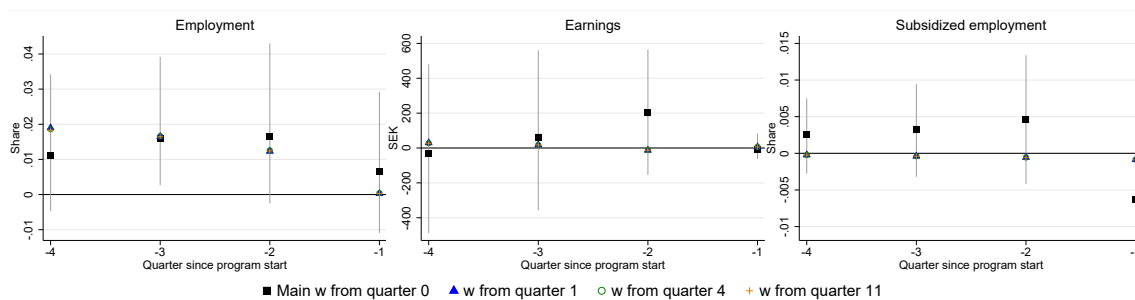
Note: The black circles represent the main results, where non-participants who have left the job center before their simulated start date are excluded, and the red triangles the results when we include them. The 95-percent CIs shown are estimated through bootstrapping with 999 replications using the main specification.

Figure A8: Sensitivity, passing an SFI course



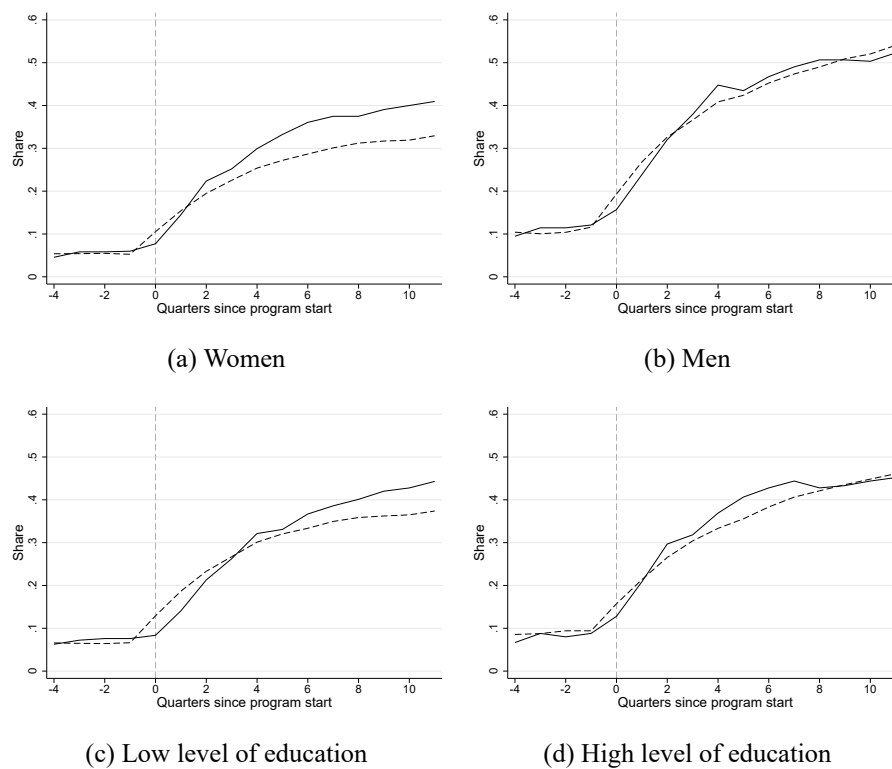
Note: The first panel shows the ATET from the main analysis, with 90- and 95-percent CIs. The second panel shows the ATET when including non-participants who have left the the job center before their simulated start date. The third panel shows the ATETs when using different values of lambda. The different sets of confounders included depending on the value of lambda are specified in Table B2.

Figure A9: Sensitivity, different weights



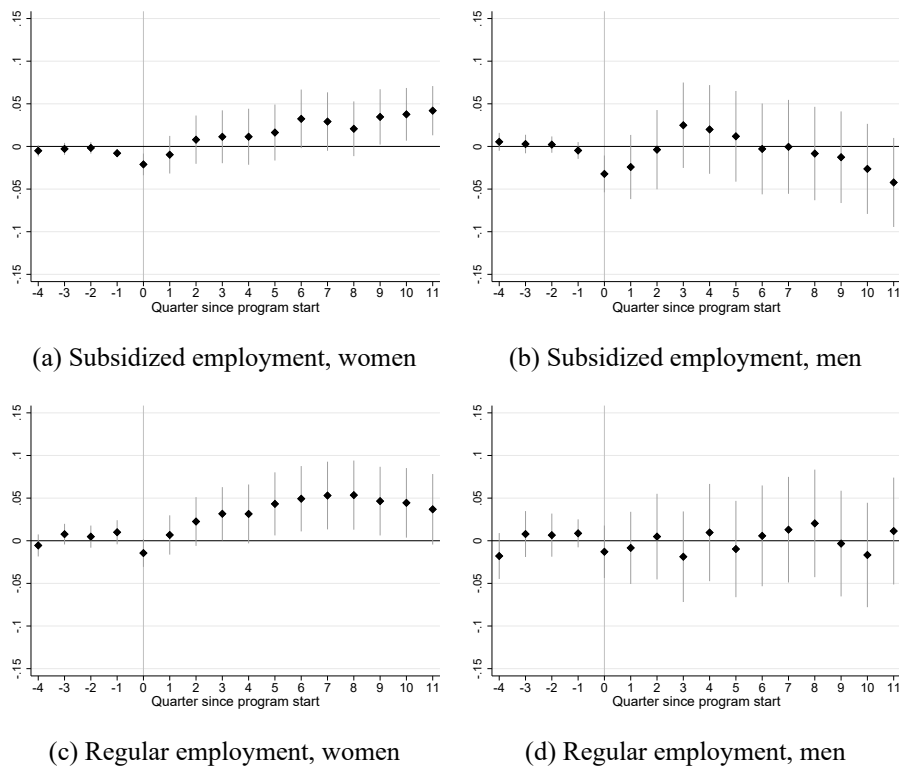
Note: The 95 percent CIs shown are estimated through bootstrapping with 999 replications using the main specification.

Figure A10: Results, average employment by sex and education level



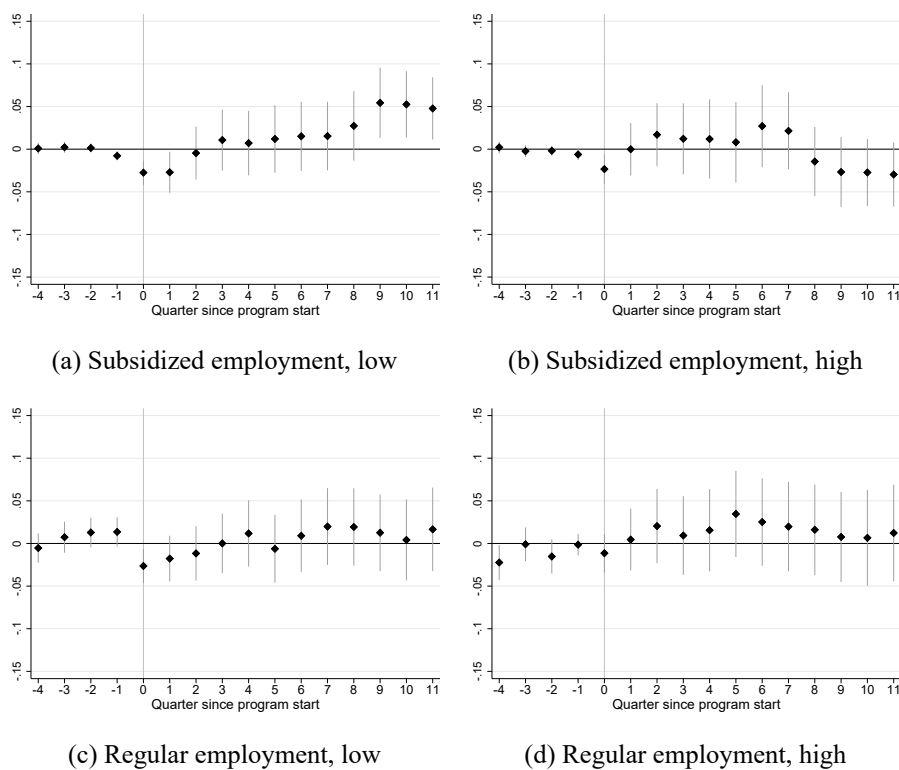
Note: The solid lines show the share for participants, and the dashed lines for the weighted non-participants. Low level of education refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. Weighted averages for non-participants are shown in Tables B12–B13.

Figure A11: Results, subsidized and regular employment by sex



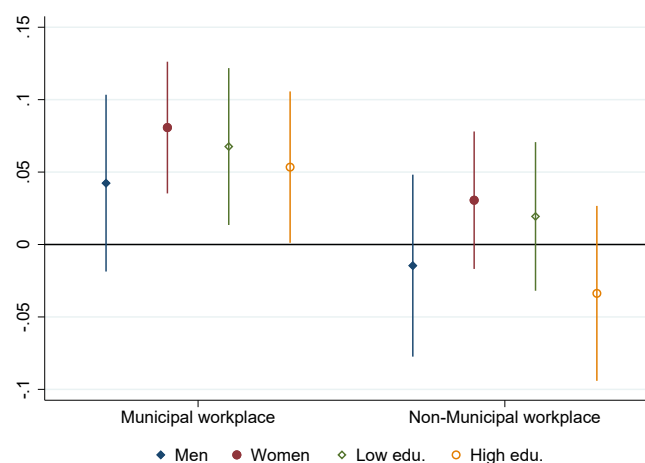
Note: The squares mark the difference between participants and the weighted non-participants, and the vertical lines the 95-percent CIs based on 999 bootstrap replications. Point estimates and standard errors are shown in Table B12.

Figure A12: Results, subsidized and regular employment by education level



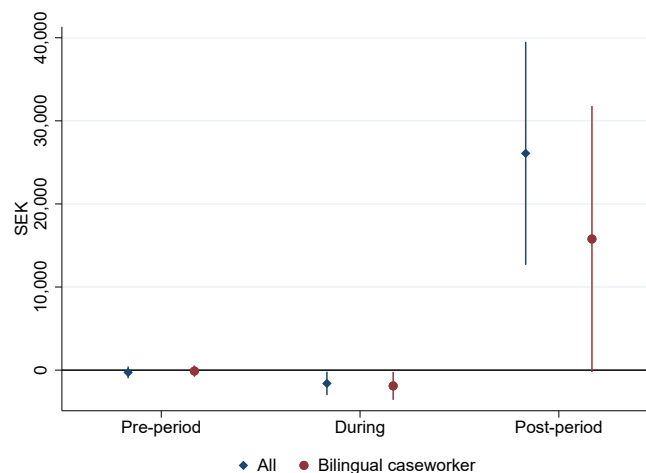
Note: The squares mark the difference between participants and the weighted non-participants, and the vertical lines the 95-percent CIs based on 999 bootstrap replications. Low refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. Point estimates and standard errors are shown in Table B13.

Figure A13: Results, sector of work



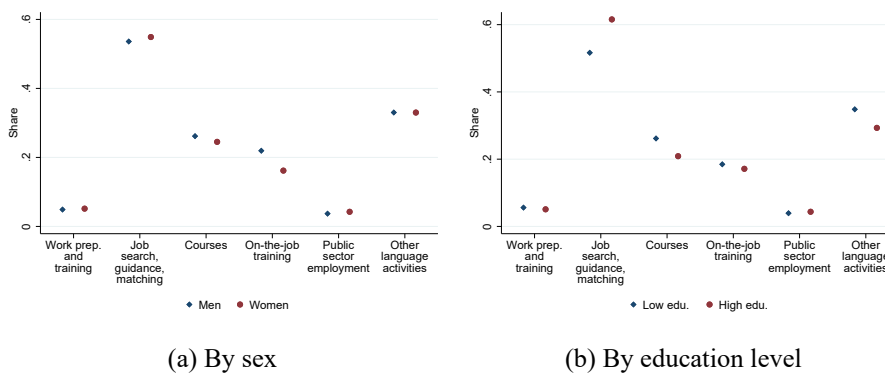
Note: The outcomes are measured 6–35 months after program start. 95-percent CI are based on bootstraps with 999 replications. Low level of education refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. Point estimates and standard errors are shown in Table B16.

Figure A14: Results, earnings with bilingual caseworkers (SEK)



Note: The diamond markers represent the main results, and the circles the subsample with access to a bilingual coach speaking their native language. 95-percent CIs are based on bootstraps with 999 replications. Point estimates and standard errors are shown in Table B14.

Figure A15: Activities at job center for weighted non-participants



Note: Activities are measured over the six months following the simulated start date for the non-participants.

B Additional tables

Table B1: Original pool of covariates

Variables included		
<i>Individual and family characteristics</i>		
Age, years	Youngest child 4–6	6–10 years since immigration
Age ²	Youngest child 7–10	>10 years since immigration
Age 18–24	Youngest child 11–15	Born in Eastern Europe or Central Asia
Age 25–29	University, more than 2 yrs	Born in North America
Age 30–39	University, less than 2 yrs	Born in South America
Age 40–49	High school	Born in Asia, excl. Middle East
Female	Compulsory schooling	Born in Sub-Saharan Africa
Married	Education unknown	Born in Middle East or North Africa
Child in household	0–2 years since immigration	Born in other or unknown
Youngest child 0–3	3–5 years since immigration	
<i>JC and PES characteristics</i>		
Own initiative to be registered	JC registration in 2012	JC registration in Q4
JC Vällingby	JC registration in 2013	Ever PES at JC registration
JC unga Globen	JC registration in 2014	Spells at PES at JC registration
JC Skärholmen	JC registration in 2015	Cumulated quarters at PES
JC Kista	JC registration in 2016	0 quarters at PES at JC registration
JC Farsta	JC registration in 2017	1–2 quarters at PES at JC registration
JC City	JC registration in Q1	3–8 quarters at PES at JC registration
JC registration in 2010	JC registration in Q2	>8 quarters at PES at JC registration
JC registration in 2011	JC registration in Q3	Not work ready
<i>Employment history</i>		
Employed ts-1	Months w. wage > PBB t-12	Employed t0-24
Employed ts-2	Months w. wage > PBB t-24	Months employed t0-6
Employed ts-3	Subsidized empl in t-6	Months employed t0-12
Earnings ts-1	Subsidized empl in t-12	Months employed t0-24
Earnings ts-2	Subsidized empl in t-24	Months in subsidized employment t0-6
Earnings ts-3	Months in sub. employment t-6	Months in subsidized employment t0-12
Employed t-6	Months in sub. employment t-12	Months in subsidized employment t0-24
Employed t-12	Months in sub. employment t-24	Subsidized employment in t0-6
Employed t-24	Log earnings t-6, 1000 SEK	Subsidized employment in t0-12
Months employed t-6	Log earnings t-12, 1000 SEK	Subsidized employment in t0-24
Months employed t-12	Log earnings t-24, 1000 SEK	Log earnings t0-6, 1000 SEK
Months employed t-24	Employed t0-6	Log earnings t0-12, 1000 SEK
Months w. wage > PBB t-6	Employed t0-12	Log earnings t0-24, 1000 SEK
<i>Social assistance (SA) history</i>		
SA t-6	1–12 months with SA, t-24	Months with SA t0-12
SA t-12	13–24 months with SA, t-24	Months with SA t0-24
SA t-24	Log SA t-6, 1000 SEK	Log SA t0-6, 1000 SEK
Months with SA t-6	Log SA t-12, 1000 SEK	Log SA t0-12, 1000 SEK
Months with SA t-12	Log SA t-24, 1000 SEK	Log SA t0-24, 1000 SEK
Months with SA t-24	SA t0-6	Reason for SA, unemployment
0 months with SA, t-24	SA t0-12	Reason for SA, other
1–6 months with SA, t-24	SA t0-24	Introduction Allowance t-12
7–12 months with SA, t-24	Months with SA t0-6	Introduction Program t-12

Continued on next page

Table B1 – continued from previous page

Variables included		
<i>Health history</i>		
Pain rel. drug prescr. t-6	Other drug prescr. t-6	
Pain rel. drug prescr. t-12	Other drug prescr. t-12	
Psychotropic drug prescr. t-6	Hospital visit t-12	
Psychotropic drug prescr. t-12	Hospital visit t-6	
<i>Immigrant status and SFI history</i>		
Refugee	Started SFI a before t	Started SFI d 13–24 months before t
Relative of refugee	Started SFI b before t	Passed SFI before t
Relative of other immigrant	Started SFI c before t	Passed SFI 12 months before t
Enter SFI at a level	Started SFI d before t	Passed SFI 13–24 months before t
Enter SFI at b level	Started SFI a 12 months before t	Passed SFI > 24 months before t
Enter SFI at c level	Started SFI b 12 months before t	Arabic native language
Enter SFI at d level	Started SFI c 12 months before t	Somali native language
Started SFI before t	Started SFI d 12 months before t	Dari native language
Started SFI 12 months before t	Started SFI a 13–24 months before t	Tigrinja native language
Started SFI 13–24 months before t	Started SFI b 13–24 months before t	Other native language
Started SFI > 24 months before t	Started SFI c 13–24 months before t	Native language unknown

Note: SA denotes social assistance, JC job center and PES the Public employment service. $t0 - x$ refers to x months prior to enrollment at the job center, $t - y$ refers to y months prior to the start of the assignment period and, $ts - z$ refers to z months prior to the (simulated) start date.

Table B2: Different set of confounders chosen using different λ

	(1) $0.1 * \lambda_{se}$	(2) $0.15 * \lambda_{se}$	(3) $0.2 * \lambda_{se}$	(4) $0.25 * \lambda_{se}$
Age, years	1			
Age 18–24	1	1	1	
Female	1	1	1	
Married	1	1		
0–2 years since immigration	1	1	1	1
6–10 years since immigration	1			
>10 years since immigration	1	1	1	
Born in Asia, excl. Middle East	1	1	1	
Born in Sub-Saharan Africa	1	1	1	
JC unga Globen	1	1		
JC Farsta	1	1		
JC City	1	1		
JC registration in 2017	1			
JC registration in 2015	1			
JC registration in 2011	1	1		
JC registration in 2010	1	1		
JC registration in Q3	1			
JC registration in Q2	1			
Spells at PES at JC registration	1	1	1	1
Cumulated quarters at PES	1	1	1	1
Employed $ts-1$	1	1	1	
Subsidized employment in $t-12$	1	1	1	
Log earnings $t-12$, 1000 SEK	1	1	1	1
Log earnings $t-24$, 1000 SEK	1	1	1	1
SA $t0-6$	1			
Months with SA $t0-24$	1			
Earnings $ts-1$	1	1	1	1
Earnings $ts-3$	1	1		
Log SA $t0-24$, 1000 SEK	1	1		
Psychotropic drug prescr. $t-12$	1			
Hospital visit $t-12$	1			
Started SFI d before t	1	1	1	1
Started SFI c before t	1	1	1	
Started SFI 12 months before t	1	1	1	1
Started SFI b 12 months before t	1	1	1	1
Started SFI a 13–24 months before t	1			
Passed SFI > 24 months before t	1			
Arabic native language	1	1	1	1
Somali native language	1	1	1	1
Dari native language	1	1		
Other native language	1	1	1	1
Native language unknown	1	1	1	1
Number of variables	42	30	21	13

Note: Column 3 represents the variables chosen in the main specification. JC denotes job center, SA social assistance, PES the Public employment service, and SFI refers to Swedish language classes. SFI a–d represent different SFI levels. $t0$ refers to enrollment at the job center, t to the start of the assignment period and ts to the (simulated) start date.

Table B3: Summary statistics of the estimated propensity scores and weights

	Propensity Scores				Dynamic IPWs				
	Mean	Min	Max	Obs	Mean	Min	Max	Obs	Trim
Participants	0.062	0.000	0.281	947	1.000	1.000	1.000	945	0
Non-participants	0.020	0.000	0.244	45482	0.024	0.000	0.456	29462	0

Note: The weights used are based on information from 36 months after the start of the program. Obs refers to the number of observations and Trim to the number of observations excluded because their weights exceed one percent of the sum of weights for the non-participants.

Table B4: Normalized differences (ND) before and after dynamic IPW

	Before weighting				After weighting			
	Part.	Non-part.	ND	P-value	Part.	Non-part.	ND	P-value
Age 18–24	0.22	0.19	0.06	0.70	0.22	0.20	0.04	0.88
Female	0.68	0.54	0.28	0.00	0.68	0.68	0.00	0.44
0–2 years since immigration	0.44	0.24	0.40	0.00	0.44	0.44	0.01	0.71
>10 years since immigration	0.05	0.19	0.60	0.00	0.05	0.06	0.01	0.99
Born in Asia, excl. Middle East	0.07	0.15	0.30	0.00	0.07	0.08	0.01	0.87
Born in Sub-Saharan Africa	0.52	0.37	0.29	0.00	0.52	0.52	0.01	0.70
Spells at PES at JC registration	1.46	2.11	0.65	0.00	1.46	1.42	0.04	0.44
Cumulated quarters at PES	4.87	8.54	0.52	0.00	4.87	4.88	0.00	0.55
Employed ts-1	0.07	0.25	0.75	0.00	0.07	0.06	0.02	0.91
Subsidized employment in t-12	0.02	0.09	0.50	0.00	0.02	0.02	0.01	0.95
Log earnings t-12, 1000 SEK	1.26	3.91	0.79	0.00	1.26	1.19	0.02	0.80
Log earnings t-24, 1000 SEK	1.75	4.79	0.77	0.00	1.75	1.66	0.02	0.59
Earnings ts-1	188	2043	1.37	0.00	188	167	0.02	0.72
Started SFI d before t	0.11	0.28	0.54	0.00	0.11	0.11	0.01	0.50
Started SFI c before t	0.36	0.50	0.29	0.00	0.36	0.36	0.01	0.32
Started SFI 12 months before t	0.70	0.40	0.65	0.00	0.70	0.70	0.00	0.60
Started SFI b 12 months before t	0.46	0.20	0.52	0.00	0.46	0.46	0.00	0.43
Arabic native language	0.34	0.23	0.23	0.00	0.34	0.34	0.01	0.88
Somali native language	0.34	0.15	0.40	0.00	0.34	0.34	0.00	0.55
Other native language	0.15	0.28	0.38	0.00	0.15	0.15	0.00	0.79
Native language unknown	0.02	0.20	1.25	0.00	0.02	0.02	0.01	0.83

Note: JC denotes job center, SA social assistance, PES the Public employment service, and SFI refers to Swedish language classes. SFI a–d represent different SFI levels. t_0 refers to enrollment at the job center, t to the start of the assignment period and ts to the (simulated) start date. The weights used are based on information from 36 months after the start of the program.

Table B5: Summary of the normalized differences before and after dynamic IPW, full set of potential confounders

	Normalized difference			P-values from t-test			
	Mean	Max	> 0.1	Mean	Min	< 0.05	N
Before	0.270	1.373	105	0.111	0.000	118	163
After	0.041	0.277	14	0.405	0.000	26	163

Note: See Table B1 for the included covariates. > 0.1 shows the number of variables with a normalized difference above 0.1, < 0.05 shows number of variables with a p-value below 0.05 from a t-test of equal means, and N the number of covariates included. The weights used are based on information 36 months after the program started.

Table B6: Estimation results for labor market outcomes by quarter

Quarter	Employment			Sub. Employment			Reg. Employment		
	ATET	St err	Mean	ATET	St err	Mean	ATET	St err	Mean
q-4	-0.010	0.006	0.071	-0.000	0.002	0.010	-0.012	0.006	0.066
q-3	0.004	0.006	0.072	0.000	0.002	0.009	0.004	0.006	0.067
q-2	0.001	0.006	0.075	0.000	0.002	0.008	0.000	0.006	0.071
q-1	0.001	0.004	0.079	-0.006	0.002	0.010	0.004	0.004	0.075
q+0	-0.034	0.008	0.137	-0.023	0.005	0.047	-0.018	0.007	0.103
q+1	-0.014	0.012	0.189	-0.010	0.009	0.085	-0.001	0.010	0.122
q+2	0.020	0.015	0.234	0.011	0.012	0.116	0.015	0.012	0.148
q+3	0.028	0.016	0.265	0.023	0.013	0.134	0.017	0.013	0.163
q+4	0.051	0.017	0.296	0.022	0.014	0.150	0.025	0.014	0.189
q+5	0.053	0.017	0.314	0.023	0.014	0.151	0.029	0.015	0.206
q+6	0.062	0.017	0.334	0.028	0.014	0.150	0.036	0.016	0.229
q+7	0.063	0.018	0.350	0.026	0.014	0.146	0.041	0.016	0.248
q+8	0.057	0.018	0.361	0.016	0.014	0.142	0.046	0.016	0.264
q+9	0.058	0.019	0.370	0.025	0.014	0.131	0.034	0.017	0.281
q+10	0.058	0.018	0.376	0.022	0.014	0.120	0.028	0.016	0.297
q+11	0.058	0.018	0.388	0.018	0.013	0.110	0.034	0.017	0.309

Quarter	Earnings			Social assistance		
	ATET	St err	Mean	ATET	St err	Mean
q-4	-197	155	1,019	-.228	386	7,171
q-3	139	152	937	-27.4	408	8,348
q-2	-117	98	832	234	424	9,867
q-1	-38.6	53.1	614	378	438	11,919
q+0	-859	208	2,390	1,093	393	13,640
q+1	-950	420	5,195	1,006	426	12,782
q+2	431	618	7,568	366	434	11,892
q+3	1,290	711	9,319	-160	438	11,144
q+4	2,039	787	11,034	-713	428	10,636
q+5	2,536	854	12,056	-1,068	432	10,144
q+6	3,161	915	13,042	-1,073	438	9,796
q+7	3,215	937	14,197	-1,171	426	9,059
q+8	3,287	967	15,056	-546	467	8,475
q+9	3,400	1,070	15,762	-855	464	8,111
q+10	2,940	1,036	16,414	-1,051	429	7,703
q+11	3,723	1,111	17,391	-1,132	412	7,367

Note: St err refers to standard errors estimated using bootstrap with 999 replications. Mean shows the average among the weighted non-participants.

Table B7: Set of selected confounders by subsample

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Women	Men	Low	High	Bilingual
Age 18–24	1	1		1		1
Female	1			1		1
0–2 years since immigration	1	1	1	1		
>10 years since immigration	1	1		1		1
Born in Asia, excl. Middle East	1	1		1		
Born in Sub-Saharan Africa	1			1		
Spells at PES at JC registration	1		1		1	1
Cumulated quarters at PES	1		1		1	1
Employed t_{s-1}	1		1		1	1
Subsidized employment in $t-12$	1		1	1	1	1
Log earnings $t-12$, 1000 SEK	1	1			1	1
Log earnings $t-24$, 1000 SEK	1	1	1	1	1	1
Earnings t_{s-1}	1	1	1	1	1	1
Started SFI d before t	1	1	1	1	1	1
Started SFI c before t	1		1	1		1
Started SFI 12 months before t	1	1	1	1	1	1
Started SFI b 12 months before t	1	1	1	1	1	1
Arabic native language	1	1	1		1	
Somali native language	1	1	1	1	1	1
Other native language	1	1		1		
Native language unknown	1	1	1	1	1	
Age, years		1				1
Married		1			1	
JC unga Globen		1			1	1
JC Farsta		1		1		1
JC registration in 2010		1			1	1
Started SFI a 13–24 months before t		1				
Dari native language		1		1		
Education unknown			1			
Employed t_{s-3}			1			
Months w. wage > PBB $t-12$			1		1	
Log SA t_0-24 , 1000 SEK			1	1		
Passed SFI before t			1			
Tigrinja native language			1			
Earnings t_{s-2}				1		
Earnings t_{s-3}				1		1
Passed SFI > 24 months before t				1		
Compulsory schooling					1	
Refugee					1	
JC registration in 2011					1	
Introduction Program $t-12$					1	
Entered SFI at d-level					1	
Started SFI b before t					1	
JC registration in 2015						1
JC registration in 2012						1
SA t_0-6						1
Psychotropic drug prescr. $t-12$						1
Started SFI > 24 months before t						1
Number of variables	21	21	20	22	23	25

Note: Column 1 represents the variables chosen in the main specification. JC denotes job center, SA social assistance, PES the Public employment service, and SFI refers to Swedish language classes. SFI a–d represent different SFI levels. t_0 refers to enrollment at the job center, t to the start of the assignment period and t_s to the (simulated) start date.

Table B8: Participants entering the OLT program per period by subgroup

	By sex		By education level	
	Women	Men	Low	High
Month 1	106	62	100	60
Month 2	95	66	82	76
Month 3-4	116	52	94	66
Month 5-6	114	43	95	55
Month 7-9	86	47	63	64
Month 10-18	122	38	96	59
Month >18	86	20	52	40
Total	725	328	582	420

Note: Low refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. Participants starting the OLT program in the the last period (Month > 18) are only used to estimate the weights utilized in the dynamic IPW, but not included when estimating the treatment effects.

Table B9: Summary statistics of the estimated propensity scores and weights by subgroup

		Propensity Scores				Dynamic IPWs			
	Mean	Min	Max	Obs	Mean	Min	Max	Obs	Trim
<i>Women</i>									
Participants	0.078	0.001	0.608	639	1.000	1.000	1.000	638	3
Non-participants	0.025	0.000	0.408	23950	0.030	0.000	1.678	15962	5
<i>Men</i>									
Participants	0.062	0.001	0.306	308	1.000	1.000	1.000	307	1
Non-participants	0.014	0.000	0.445	20369	0.017	0.000	0.907	13502	11
<i>Low educated</i>									
Participants	0.081	0.001	0.516	530	1.000	1.000	1.000	528	2
Non-participants	0.025	0.000	0.465	19758	0.030	0.000	1.167	13395	8
<i>High educated</i>									
Participants	0.067	0.000	0.957	380	1.000	1.000	1.000	380	6
Non-participants	0.017	0.000	0.533	21434	0.018	0.000	1.141	15446	4

Note: The weights used are based on information from 36 months after the start of the program. Low educated refers to having studied at most compulsory schooling, and high educated to having studied more than compulsory schooling.

Table B10: Summary of normalized differences before and after dynamic IPW by subgroup

	Normalized difference			P-values from t-test			
	Mean	Max	> 0.1	Mean	Min	< 0.05	N
<i>Panel A: Women</i>							
Before	0.484	2.307	18	0.035	0.000	18	21
After	0.013	0.056	0	0.665	0.008	1	21
<i>Panel B: Men</i>							
Before	0.562	1.200	20	0.001	0.000	20	20
After	0.023	0.050	0	0.689	0.223	0	20
<i>Panel C: Low educational level</i>							
Before	0.632	2.013	20	0.014	0.000	21	22
After	0.011	0.028	0	0.756	0.328	0	22
<i>Panel D: High educational level</i>							
Before	0.489	1.160	21	0.010	0.000	21	23
After	0.013	0.044	0	0.647	0.165	0	23

Note: > 0.1 shows the number of variables with a normalized difference above 0.1, and < 0.05 the number with a p-value below 0.05 from a t-test of equal means, and N the number of covariates included in the propensity score. The weights used are based on information from 36 months after the start of the program. Low educational level refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling.

Table B11: Summary of the normalized differences before and after dynamic IPW, full set of potential confounders by subgroup

	Normalized difference			P-values from t-test			
	Mean	Max	> 0.1	Mean	Min	< 0.05	N
<i>Panel A: Women</i>							
Before	0.277	2.307	109	0.129	0.000	115	162
After	0.039	0.249	8	0.460	0.000	12	162
<i>Panel B: Men</i>							
Before	0.279	1.200	106	0.145	0.000	104	162
After	0.050	0.334	12	0.471	0.000	16	162
<i>Panel C: Low educational level</i>							
Before	0.289	2.013	108	0.135	0.000	111	158
After	0.037	0.274	13	0.513	0.000	20	158
<i>Panel D: High educational level</i>							
Before	0.263	1.160	105	0.140	0.000	100	160
After	0.045	0.257	16	0.477	0.000	13	160

Note: See Table B1 for the included covariates. > 0.1 shows the number of variables with a normalized difference above 0.1, < 0.05 the number with a p-value below 0.05 from a t-test of equal means, and N the number of covariates included. The weights used are based on information 36 months after the program started.

Table B12: Estimation results for employment outcomes by quarter and sex

Quarter	Men								
	Employment			Sub. Employment			Reg. Employment		
	ATET	St err	Mean	ATET	St err	Mean	ATET	St err	Mean
q-4	-0.009	0.014	0.104	0.005	0.005	0.014	-0.018	0.014	0.096
q-3	0.014	0.014	0.101	0.003	0.006	0.014	0.008	0.014	0.093
q-2	0.011	0.013	0.104	0.002	0.005	0.011	0.007	0.013	0.098
q-1	0.005	0.008	0.116	-0.005	0.005	0.015	0.009	0.008	0.109
q+0	-0.036	0.017	0.193	-0.032	0.011	0.062	-0.013	0.016	0.150
q+1	-0.030	0.025	0.268	-0.024	0.019	0.122	-0.008	0.022	0.178
q+2	-0.006	0.029	0.326	-0.004	0.024	0.167	0.005	0.026	0.214
q+3	0.013	0.031	0.366	0.025	0.026	0.194	-0.019	0.027	0.234
q+4	0.039	0.033	0.408	0.020	0.027	0.219	0.010	0.029	0.258
q+5	0.011	0.033	0.424	0.012	0.027	0.227	-0.010	0.029	0.271
q+6	0.015	0.033	0.452	-0.003	0.027	0.228	0.006	0.030	0.301
q+7	0.017	0.033	0.474	-0.000	0.028	0.229	0.013	0.032	0.317
q+8	0.017	0.034	0.490	-0.008	0.028	0.221	0.020	0.032	0.342
q+9	-0.003	0.033	0.509	-0.013	0.027	0.214	-0.003	0.032	0.366
q+10	-0.017	0.032	0.520	-0.026	0.027	0.200	-0.017	0.031	0.386
q+11	-0.018	0.033	0.541	-0.042	0.027	0.193	0.011	0.032	0.404

Quarter	Women								
	Employment			Sub. Employment			Reg. Employment		
	ATET	St err	Mean	ATET	St err	Mean	ATET	St err	Mean
q-4	-0.008	0.007	0.054	-0.005	0.003	0.010	-0.006	0.007	0.048
q-3	0.004	0.006	0.054	-0.003	0.004	0.009	0.008	0.006	0.049
q-2	0.004	0.006	0.055	-0.002	0.003	0.008	0.005	0.007	0.050
q-1	0.007	0.007	0.053	-0.008	0.002	0.010	0.010	0.007	0.050
q+0	-0.028	0.010	0.105	-0.021	0.006	0.042	-0.014	0.008	0.074
q+1	-0.009	0.014	0.154	-0.010	0.011	0.074	0.007	0.012	0.092
q+2	0.029	0.019	0.195	0.008	0.014	0.102	0.023	0.015	0.114
q+3	0.027	0.020	0.225	0.011	0.016	0.116	0.032	0.016	0.132
q+4	0.045	0.022	0.254	0.011	0.017	0.129	0.031	0.018	0.158
q+5	0.060	0.022	0.272	0.016	0.017	0.125	0.043	0.019	0.177
q+6	0.074	0.023	0.287	0.032	0.018	0.122	0.049	0.020	0.196
q+7	0.074	0.023	0.301	0.029	0.017	0.116	0.053	0.020	0.216
q+8	0.063	0.023	0.312	0.021	0.016	0.110	0.054	0.021	0.232
q+9	0.074	0.023	0.317	0.035	0.017	0.100	0.046	0.021	0.245
q+10	0.081	0.024	0.319	0.038	0.016	0.087	0.044	0.021	0.258
q+11	0.080	0.023	0.329	0.042	0.015	0.075	0.037	0.021	0.272

Note: The top panel shows the results for men, and the bottom for women. St err refers to standard errors estimated using bootstrap with 999 replications. Mean is the average among the weighted non-participants.

Table B13: Estimation results for employment outcomes by quarter and education level

Quarter	Low educational level								
	Employment			Sub. Employment			Reg. Employment		
	ATET	St err	Mean	ATET	St err	Mean	ATET	St err	Mean
q-4	-0.003	0.009	0.066	0.001	0.003	0.009	-0.005	0.009	0.060
q-3	0.007	0.009	0.065	0.002	0.003	0.007	0.007	0.009	0.061
q-2	0.012	0.009	0.064	0.001	0.003	0.006	0.013	0.009	0.061
q-1	0.010	0.009	0.066	-0.008	0.003	0.010	0.014	0.009	0.062
q+0	-0.045	0.012	0.129	-0.028	0.007	0.047	-0.026	0.010	0.095
q+1	-0.046	0.016	0.187	-0.027	0.012	0.090	-0.018	0.014	0.115
q+2	-0.020	0.019	0.233	-0.005	0.016	0.119	-0.012	0.016	0.139
q+3	-0.005	0.022	0.267	0.011	0.018	0.139	0.000	0.018	0.154
q+4	0.020	0.023	0.301	0.007	0.019	0.154	0.012	0.020	0.186
q+5	0.010	0.024	0.320	0.012	0.020	0.157	-0.006	0.020	0.204
q+6	0.033	0.025	0.334	0.015	0.021	0.154	0.009	0.022	0.221
q+7	0.036	0.026	0.350	0.015	0.020	0.152	0.020	0.023	0.243
q+8	0.042	0.026	0.359	0.027	0.021	0.146	0.019	0.023	0.258
q+9	0.058	0.026	0.362	0.054	0.021	0.133	0.013	0.023	0.273
q+10	0.063	0.028	0.365	0.053	0.020	0.118	0.004	0.024	0.287
q+11	0.069	0.027	0.374	0.048	0.019	0.107	0.017	0.025	0.295

Quarter	High educational level								
	Employment			Sub. Employment			Reg. Employment		
	ATET	St err	Mean	ATET	St err	Mean	ATET	St err	Mean
q-4	-0.019	0.011	0.086	0.002	0.003	0.009	-0.022	0.010	0.081
q-3	0.000	0.010	0.088	-0.002	0.003	0.008	-0.001	0.010	0.084
q-2	-0.014	0.010	0.094	-0.002	0.003	0.007	-0.015	0.010	0.090
q-1	-0.006	0.006	0.094	-0.006	0.003	0.012	-0.001	0.006	0.089
q+0	-0.029	0.013	0.157	-0.023	0.009	0.047	-0.011	0.012	0.123
q+1	-0.005	0.021	0.214	-0.000	0.016	0.083	0.005	0.019	0.150
q+2	0.031	0.025	0.265	0.017	0.019	0.117	0.020	0.022	0.185
q+3	0.014	0.027	0.304	0.012	0.021	0.140	0.009	0.024	0.205
q+4	0.036	0.029	0.333	0.012	0.024	0.159	0.016	0.025	0.225
q+5	0.051	0.030	0.356	0.008	0.024	0.160	0.035	0.026	0.243
q+6	0.044	0.030	0.383	0.027	0.025	0.157	0.025	0.026	0.277
q+7	0.038	0.030	0.406	0.021	0.023	0.150	0.020	0.027	0.304
q+8	0.007	0.030	0.421	-0.014	0.021	0.146	0.016	0.027	0.321
q+9	-0.002	0.030	0.435	-0.027	0.021	0.140	0.008	0.027	0.340
q+10	-0.004	0.031	0.448	-0.027	0.020	0.131	0.007	0.029	0.360
q+11	-0.009	0.031	0.461	-0.030	0.019	0.123	0.012	0.029	0.375

Note: Low educational level refers to having studied at most compulsory schooling (top panel), and high to having studied more than compulsory schooling (bottom panel). St err refers to standard errors estimated using bootstrap with 999 replications. Mean are weighted average of non-participants.

Table B14: Earnings estimation results by subsample

	Sex		Education		Only with Bilingual
	Men	Women	Low	High	
<i>Panel A: Months -12 to -1</i>					
ATET	1,213	-861	-343	578	-110
St err	997	312	345	803	350
Mean	4,827	2,695	2,817	3,776	2,692
<i>Panel B: Months 0–6</i>					
ATET	-2,239	-1,369	-2,668	-2,114	-1,896
St err	1,632	845	945	1,202	859
Mean	13,375	7,635	9,406	10,590	8,891
<i>Panel C: Months 7–36</i>					
ATET	7,318	29,654	15,898	14,691	15,783
St err	14,427	8,679	9,517	13,289	8,168
Mean	205,134	109,727	133,358	166,763	136,690

Note: Low refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. St err refers to standard errors estimated using bootstrap with 999 replications. Mean is the average among the weighted non-participants.

Table B15: SFI estimation results by subsample

	Sex		Education	
	Men	Women	Low	High
<i>Panel A: Months -12 to -1</i>				
ATET	-0.010	0.014	-0.009	-0.009
St err	0.017	0.016	0.016	0.021
Mean	0.157	0.211	0.176	0.236
<i>Panel B: Months 0–6</i>				
ATET	0.009	0.026	0.027	-0.011
St err	0.026	0.019	0.020	0.025
Mean	0.207	0.218	0.193	0.254
<i>Panel C: Months 7–36</i>				
ATET	-0.013	0.028	0.045	0.006
St err	0.032	0.025	0.026	0.029
Mean	0.386	0.407	0.377	0.400

Note: Low refers to having studied at most compulsory schooling, and high to having studied more than compulsory schooling. St err refers to standard errors estimated using bootstrap with 999 replications. Mean is the average among the weighted non-participants.

Table B16: Results, sector of work and share Swedish-born colleagues

	All	Men	Women	Low edu.	High edu.
<i>Municipal workplace</i>					
ATET	0.075	0.042	0.081	0.068	0.053
St err	0.018	0.031	0.023	0.028	0.027
Mean	0.242	0.229	0.259	0.275	0.209
<i>Non-Municipal workplace</i>					
ATET	0.031	-0.015	0.031	0.019	-0.034
St err	0.017	0.032	0.024	0.026	0.031
Mean	0.431	0.636	0.355	0.414	0.531
<i>Share Swedish-born colleagues</i>					
ATET	-0.013	-0.019	-0.024	-0.030	0.059
St err	0.024	0.040	0.028	0.033	0.040
Mean	0.422	0.426	0.434	0.414	0.405

Note: The outcomes in the top and middle panel are measured 6–35 months after program start, and the outcome in the bottom panel after 35 months. St err refers to standard errors estimated using bootstrap with 999 replications. Mean is the average among the weighted non-participants. Low edu. refers to having studied at most compulsory schooling, and high edu. to having studied more than compulsory schooling.