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From welfare to work

Financial incentives, active labor market policies, and integration programs

Lillit Ottosson

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From Welfare to Work

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* Public economics
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* Microeconometrics
* Environmental economics
* Housing and urban economics

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From Welfare to Work

Financial Incentives, Active Labor Market Policies, and Integration Programs

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Dissertation presented at Uppsala University to be publicly examined in Hörsal 2, Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Friday, 9 September 2022 at 10:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Professor Alexander Willén (Department of Economics, Norwegian School of Economics).

Abstract
Ottosson, L. 2022. From Welfare to Work. Financial Incentives, Active Labor Market Policies, and Integration Programs. *Economic studies* 204. 220 pp. Uppsala: Department of Economics. ISBN 978-91-506-2959-0.

Essay I: I study the effects of increased social assistance (SA) generosity by exploiting exogenous variation induced by a ruling in the Swedish Supreme Administrative Court in 1993, mandating local governments to provide a minimum level of untied SA payments. The new rule forced some local governments to increase their SA generosity, while others were unaffected as they already complied with the stricter standards. I find that a 1 percent increase in SA generosity caused an increase in SA recipiency by 1.3 percent and a decrease in employment by 0.2 percent, among individuals with a high risk of receiving SA. For individuals who were already recipients of SA, the increase in SA payments was not offset by lower labor earnings, resulting in increased disposable income.

Essay II (with Eva Mörk and Ulrika Vikman): We evaluate a temporary public sector employment program targeted at individuals with weak labor market attachment. Using dynamic inverse probability weighting to account for non-random dynamic assignment into the program, we show that the program is successful in increasing employment and reducing social assistance. The positive employment effect is driven by individuals at a regular workplace; for participants with temporary employment at a constructed workplace, we find negative employment effects. The decrease in social assistance is partially countered by an increase in the share that receive unemployment insurance benefits. This indicates that municipalities are able to shift costs from the local to the central budget.

Essay III (with Cristina Bratu and Linna Martén): This paper studies a 2010 reform in Sweden that transferred responsibility for a refugee integration program from municipalities to the Public Employment Service (PES). Aiming to increase female participation in the program, the reform strengthened economic incentives for the secondary earner in the household to participate. We show that the program improved women's earnings and employment, and that these effects emerge 2–3 years after program participation. The strengthened economic incentives increased participation in the program for women, but this does not drive the labor market effects. Instead, the increased labor market focus brought on by transferring the program to the PES seems to be the main mechanism behind our findings.

Essay IV (with Ulrika Vikman): In this paper, we evaluate an active labor market program (ALMP) targeted toward immigrants with very limited language skills. The program combines support in the participant's native language with an ALMP in a regular workplace. We apply dynamic inverse probability weighting to account for dynamic selection and compare participants with observably similar non-participants. We find a positive 10 percentage point employment effect, mainly explained by the participants obtaining subsidized employment as part of the program. In the medium term, these positive effects disappear. Participation in the program also leads to improved Swedish language skills.

Keywords: Social assistance, Welfare, Labor supply, Public sector employment programs, Cost-shifting, Dynamic inverse probability weighting, Refugees, Integration, Active labor market policies, Language support

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Till farfar Kea

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I had barely heard about the PhD program, let alone considered it, until I started the master's program in Uppsala eight years ago. The friendly environment at the Department of Economics, and encouraging words from Daniel, Tomas, Adrian, Niklas, and Erica are important reasons for why I applied to the PhD program. Today, I am very grateful that I did, so thank you, and keep up the great work! Throughout my time as a PhD student, I have continued to benefit from the friendly and supportive environment at the Department of Economics. In addition, I spent the last years of the program at IFAU, another great and nourishing place to be. Thanks to all friends and colleagues for so generously sharing your knowledge and time – and for lifting my spirits in the fika room when I needed it. I am especially grateful to Anders, Johan, Olof and the organizers of the Uppsala Labor Group for your comments and advice. I also want to thank researchers and PhD students at UIL and UCLS. In addition, I am grateful for having been part of Kommungruppen. Anders, Ulrika, Ellen, Rikard, Ingeborg, and Mattias: I have really enjoyed our workshops and study visits, and I have learned so much about local active labor market policy from you. In my third year, I got the opportunity to visit Stanford University, thanks to the Jan Wallander och Tom Hedelius foundation and Petra Persson, who was kind to invite me. It was a wonderful experience that enriched my time as a PhD student greatly.

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Contents

Introduction

While most individuals who lose their job find new employment relatively fast, large groups of individuals struggle to (re)enter the labor market. Long-term unemployment comes at high fiscal and social costs for society and also has large detrimental effects on affected individuals and their families. How to reduce the poverty and the costs associated with long-term unemployment, as well as its duration, are policy questions of utmost importance. The aim of this thesis is to contribute to our understanding of these issues.

Individuals who have been unemployed a long time, or who lack previous labor market experience, are typically not entitled to receive unemployment insurance benefits (Immervoll and Knotz 2018).[1] In many welfare states, the final safety net and income support program available to them is means-tested social assistance. The aim of social assistance is to enable poor households to maintain a decent standard of living and as such, it is a crucial tool for reducing poverty. In Sweden (the setting of the four chapters of this thesis), social assistance is administered and financed by the local governments in the municipalities. In many regards, long-term unemployment and efforts to reduce poverty are thus burdens on the budgets of the local governments (who also lose out on tax revenues).

How can policy help individuals with no other means to support themselves, to go from welfare to work and self-sufficiency? In this thesis, I investigate some of the tools that are available to policymakers. As the subtitle of the thesis suggests, the central questions examined are how financial incentives (for individuals in chapters 1 and 3 and for local governments in Chapter 2), active labor market policies (chapters 2–4), and integration programs for immigrants (chapters 3–4) affect labor market, SA, and health outcomes of individuals with a weak labor market attachment. Another theme that reoccurs in most chapters, is the local governments as the provider of these programs.

To increase social assistance generosity, and hence the income of households with very limited means, can have several beneficial effects. Giving children access to (more generous) income support programs has been shown to improve test scores and mental health, as well as health and self-sufficiency later in life (Akee et al. 2018; Dahl and Lochner 2012; Hoynes et al. 2016; Milligan and Stabile 2011). In addition, poverty has in itself been found to cause poverty traps, as the stress and malnutrition it may cause can harm the

[1] Eligibility to the social insurance programs are e.g. often conditional on having a minimum employment or contribution record.

cognitive function and ability to make good decisions (see, for instance, Mani et al. (2013), Schilbach et al. (2016), and Shah et al. (2012)). However, there is also a potential downside to the provision of more generous income support programs. A high level of income support makes unemployment more attractive, which can create work disincentivizes (Bargain and Doorley 2011; Bargain and Jonassen 2022; Hoynes et al. 2016; Lemieux and Milligan 2008; Meyer and Rosenbaum 2001). If the latter force is strong, more generous benefits may be more harmful than helpful in the long-run, trapping individuals in social assistance recipiency and unemployment. The disincentive effect of social assistance has been documented for specific subgroups, but our knowledge of whether this is a general phenomena or not, is still limited.

In the first chapter, titled **Social assistance generosity and labor market outcomes**, I study a ruling in the Swedish Supreme Administrative Court in 1993, which forced some local governments to increase social assistance generosity. The Court mandated local governments to provide a minimum level of untied social assistance payments, set by the national government (the national norm). Before this, the national norm was used as a guideline for how local governments should set their local norms. The local norm both specified the social assistance eligibility threshold – the level of other incomes under which households were eligible to receive social assistance – and the size of the cash transfer they were entitled to receive. Before the 1993 court ruling, there was ample variation in local norms across municipalities, and as many as 51% of all local governments applied a local norm that was less generous than the national norm. After the 1993 court ruling, these less generous municipalities thus had to raise their local norms. This both caused an upward shift in the social assistance eligibility threshold and an increase in the total sum of social assistance payments a given household was eligible to receive.

Individuals who lived in municipalities that were initially less generous than the minimum level were affected by the 1993 court ruling, while individuals who lived in municipalities already complying with it were not. By comparing the outcomes of individuals in these two municipality groups before and after the 1993 court ruling, I study the effects of social assistance generosity in a more general population than in previous papers.

I show that an increase of social assistance generosity increased social assistance recipiency, which crowded out labor earnings, leaving disposable income for individuals at risk of receiving social assistance unchanged. In my setting, I can also study the effects on previous social assistance recipients separately. In so doing, I find that for this group, the increase in social assistance is not offset by a decrease in earnings, resulting in a higher disposable income. This implies that the negative effect on employment for individuals at risk of receiving social assistance is primarily explained by an increased inflow into unemployment and social assistance recepiency, rather than a decreased outflow. My results also confirm that there is a trade-off between improving the economic situation of social assistance recipients and creating work disincentives for individuals on the margin of receiving SA.

One way in which policymakers can counteract the negative effects on employment caused by providing generous benefits, is by conditioning benefit receipt on participation in active labor market policies. Such activation policies became common practice in the US and other parts of Europe in the 1990s, and the local governments in Sweden were no exception (Thorén 2008).[2] These policy measures could include job search activities, on-the-job and vocational training, and work placements. The idea that social assistance recipients had to deserve their welfare by participating in activation programs, was common during the 1990s (Thorén 2008). Today, many programs also focus on improving social assistance recipients chances of finding employment, e.g. through learning new skills, obtaining labor market experience and networks. The knowledge of what programs work for social assistance recipients, who tend to have a much weaker attachment to the labor market than the UI recipients that have been the focus of most previous literature, is very limited. The second (and fourth) chapter of the thesis aim to bridge this gap in the literature.

In **To work or not to work? Effects of temporary public employment on future employment and benefits**, co-authored with Eva Mörk and Ulrika Vikman, we evaluate a commonly used local active labor market programs: a temporary public sector employment program. This type of program, when targeted at UI recipients, does generally not come out well in evaluations (Card et al. 2010, 2018). However, as social assistance recipients tend to be less attached to the labor market, they may be expected to benefit more from this program which aims to provide networks and labor market experience. They may also be expected to be harmed less by negative lock-in effects.

The program under study is a temporary employment program provided by the city of Stockholm lasting for 6–12 months. We study three different type of employments, allowing us to investigate some of the underlying mechanisms. Who participates in the program is not random, and we therefore compare the outcomes of participants with observably similar non-participants, to evaluate the effects of participating in the program. We show that the employments that are placed at regular workplaces (e.g. childcare centers and schools) are successful in increasing participants' employment and reducing recipiency of social assistance up to 3 years after entering the programs. However, for participants that had their temporary employment at a constructed workplace (e.g. doing outdoor cleaning), we instead find negative employment effects. Together with the finding that many participants who find employment after the program continue in the same workplace or sector, it seems crucial that the temporary employment provides participants with networks and labor market experience applicable to sectors in which there is a demand for labor. Our

[2] Although formally, Swedish active labor market policy is the responsibility of the central government and the Public Employment Service.

analysis also reveals that participation in the temporary employment has positive effects on health. Finally, our findings show that the decrease in social assistance is to some extent countered by an increase in the share receiving UI benefits. This indicates that in providing temporary employments to social assistance recipients, municipalities are able to shift costs from the local to the central budget.

More than half of the participants in the temporary employment program we study are immigrants. This group is also heavily over-represented among the long-term unemployed, as well as among social assistance recipients. In 2019, foreign-born individuals accounted for more than 60 percent of the long-term unemployed in the ages of 16–64, compared to 30 percent in the population in the same age group (PES 2021), and 60 percent of households receiving social assistance had at least one foreign-born adult family member (NBHW 2022). In addition to a weak labor market attachment, immigrants face language barriers to enter the labor market. Integration programs, which aim to support the integration of immigrants into their host country, may thus require a different set-up compared to regular active labor market programs. In the last two chapters of the thesis, I focus on programs targeted at this group.

In the thesis' third chapter, **Integrating Refugee Women**, co-authored with Cristina Bratu and Linna Martén, we focus on a group whose attachment to the labor market is especially weak – refugee women. We study the effects of a large reform of the refugee integration program in Sweden 2010, which aimed to speed up the integration of refugees and to increase women's participation in the program. The first cornerstone of the reform was to transfer the responsibility for the integration program from municipalities to the Public Employment Service. This was supposed to increase the labor market focus of the program and make access to active labor market programs more equal across municipalities and genders. The second cornerstone was to strengthen the financial incentives for secondary earners to participate, aiming to increase women's participation. Before the reform, the financial benefits associated with participating in the integration program were set and means-tested at the household level. This implied that, if one person in the household found employment, the benefits, and hence the financial incentives to participate in the program for unemployed household members, would decrease. After the reform, the benefits depended on the individual's own participation in the program, regardless of the employment status of other household members. We exploit that eligibility to the reformed program was determined by the date individuals received their residency permit, and compare the outcomes of individuals who were granted a permit slightly before October 31 2010, to those who were granted a permit slightly after.

Our finding show that the program improved women's earnings and employment, and that these effects emerge 2–3 years after the program has ended. However, we find no effects on labor market outcomes on men. Exploring potential mechanisms, we find that participation in the program increased for married women but not for singles, which implies that the strengthened economic incentives caused the increased program participation. However, as the labor market effects are not explained by married women, this mechanism is not likely to be behind the improved labor market integration caused by the reform. We also find that the reform increased women's registration with, and participation in active labor market programs at, the Public Employment Service. The patterns we discover are in line with the hypothesis that the increased labor market focus brought on by transferring the program to the Public Employment Service was a crucial factor behind the positive effects on labor market outcomes.

The findings in the latter chapter point out that, even if financial incentives can be successful in increasing program participation, this does not guarantee that a reform will be successful in improving the outcomes in the group who is incentivized to start the program. It is also likely crucial that the content is tailored to the needs of the participants. In the fourth and final chapter of this thesis, **Supporting labor market integration by lowering language barriers**, co-authored with Ulrika Vikman, we evaluate a program in which the contents is tailored to fit the needs of immigrants with very limited language skills. The program is provided by the city of Stockholm, and is unique in the sense that it combines an active labor market program in a regular workplace (an internship or a temporary public sector employment) with support in the participant's native language, provided by bilingual caseworkers. The aim is to learn about the Swedish labor market and work-life, and practice Swedish. Having bilingual caseworkers convey information, motivate and help solve potential communication problems in the workplace, may be a way to lower the language barriers and facilitate participation in more work-related activities. To evaluate the effects of participating in the program, we use the rich set of covariates that we have access to, to find observably similar non-participants.

We find that participation in the program increases employment by 10 percentage points in the short term. However, this effect is mainly explained by the participants obtaining subsidized employment as part of the program. In the medium term, the positive effects on employment disappear, but for women and individuals with more than compulsory school education, we find indications of a positive effect by the end of our 2 year follow-up period. Investigating if the program affects language skills, we also show that participation seems to have a positive effect on language acquisition in the medium term.

To sum up, this thesis has shown that while increasing the generosity of social assistance may increase the inflow to social assistance recipiency and unemployment, active labor market policies – in particular temporary public sector employment in regular workplaces – can be used to counteract this by improving unemployed individuals' chances to find employment.[3] The thesis

[3]One successful example of combining generous (non-means tested) benefits with tailored active labor market programs to social assistance recipients is evaluated in Markussen and Røed

also shows that, while financial incentives can be important tools to increase participation in integration programs, this is not necessarily enough to improve labor market integration. While some benefit from getting access to the active labor market programs and services provided by the Public Employment Service, others may need more tailored programs.[4]

(2016). They show that the program had large positive effects on employment for social assistance recipients in Norway.

[4]Dahlberg et al. (2020) and Helgesson et al. (2020) study programs in Sweden tailored to the needs of immigrants with low education and refugee women, respectively, and find large positive effects of these programs.

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1. Social assistance generosity and labor market outcomes

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1.1 Introduction

Means-tested social assistance (SA) is the safety net of last resort for households in financial distress in most welfare states. The aim of SA is to enable poor households to maintain a decent standard of living and as such, it is a crucial tool for reducing poverty. However, a report issued by the European Commission (Frazer and Marlier 2016) concludes that 30 of 35 European countries provide inadequate levels of SA to fulfill these aims. Sweden, which is the country under study in this paper, is one of these countries. The report also assesses that the level of SA in most EU countries is not sufficiently high to lift people above the at-risk-of-poverty threshold[1]. It has been shown that alleviating poverty is crucial for child development (e.g. Akee et al. (2018), Dahl and Lochner (2012), Hoynes et al. (2016), and Milligan and Stabile (2011)), but as poverty and financial stress have also been found to impede the cognitive function and the ability to make decisions (Mani et al. 2013; Shah et al. 2012), inadequate levels of SA may in fact be an obstacle to leaving benefit recipiency. However, there is also a potential downside to providing generous SA; it makes unemployment more attractive and may, hence, create work disincentives.[2] The disincentive effect of SA generosity has been documented for specific groups (Bargain and Doorley 2011, 2017; Bargain and Jonassen 2022; Hoynes 1996; Lemieux and Milligan 2008; Meyer and Rosenbaum 2001).[3] Nevertheless, our understanding of whether the disincentive effect is a general phenomena, or restricted to specific groups, is still limited.

The aim of this paper is to study the causal effect of SA generosity on employment, SA recipiency and disposable income in a more general population of individuals at risk of receiving SA, compared to previous studies. Furthermore, by studying the effects of SA generosity for different subgroups and contexts, I explore mechanisms and whether the effects are heterogeneous across local economic conditions. In particular, in separately studying the effect on previous SA recipients, I can examine whether the economic conditions for this group are in deed improved, and whether policy makers are in fact facing the trade-off between employment disincentives and improved economic conditions for SA recipients.

In order to achieve this, I exploit a ruling in the Swedish Supreme Administrative Court 1993, which induced plausibly exogenous changes in SA generosity across local governments. The ruling implied that the national norm,

[1] Defined as 60 % of the median of the total national household equivalized income.

[2] There are also qualitative and a few quantitative studies which find a relationship between SA recipiency and poor mental health (e.g. Dackehag et al. (2020) and Huber et al. (2011)) and feelings of shame and social exclusion (see, for instance Angelin (2009) for evidence from Sweden).

[3] In the US, welfare cash benefits are only available for families with children (see, for instance, Hoynes (1996) and Meyer and Rosenbaum (2001)), and previous studies from Europe and Canada exploit age thresholds in generosity of benefits for 25–30 year-olds without children to estimate the causal effects of SA generosity using a regression discontinuity design.

set by the central government, was imposed as a minimum level of untied SA payments. The new rule only affected individuals living in municipalities in which the previous local norm was less generous than the national norm, while individuals living in municipalities already complying with the national norm before the ruling were unaffected.[4] The proposed approach addresses two important challenges to estimating the causal effects of SA generosity. First, local SA policy is likely to be endogenous. For example, if increasing unemployment rates and SA recipiency cause policy makers to decrease SA generosity, then estimated effects of SA generosity on employment will be biased. Second, changes in SA generosity are often implemented in tandem with other welfare reforms, making it difficult to isolate the effects of changes in SA generosity (see, for instance, Moffitt (2002) for a discussion). The large welfare reforms that were implemented e.g. in the UK and the US in the 1990s are examples of this.

My study broadens the understanding of how changes in SA generosity affect individuals' behavior and economic situation. First, providing evidence from different countries is important, and I am the first to study this question using Swedish data. Second, because the studied increase in SA generosity applied to everyone, I can study the effects of SA generosity for a relatively broad population, compared to previous work.[5] Third, the richness of data also allows me to explore mechanisms by studying the effect on previous SA recipients, and to study the groups that have been the focus of many previous studies, within a common setting.[6]

The analysis is based on administrative data for the entire Swedish population 1990-1995. The data include a rich set of individual characteristics, SA and labor market histories, educational attainment and migration background. The means-testing, and the fact that most workers are eligible for other social insurance benefits, like unemployment and disability insurance, imply that SA policy affects a limited part of the population. I therefore define a sample of individuals at risk of receiving SA. Previous studies have typically specified the at risk group as those eligible to receive SA (e.g. single mothers in the US) and high school drop-outs (e.g. Bargain and Doorley (2011) and Lemieux and Milligan (2008)). The richness of data allows me to use a large set of predetermined characteristics to identify a sample of individuals who are likely

[4]The local norm determines the eligibility threshold and the untied cash benefits a household is entitled to, and depends on household size and composition. The national norm had been issued by the Swedish National Agency for Health and Welfare since 1985 to guide municipalities in determining the local norm and to decrease differences across municipalities. 51 percent of local governments were however still applying local norms below the national norm in 1992.

[5]In Sweden, all households with incomes below a certain threshold are eligible to apply for SA, given that they have depleted all assets and other means of supporting themselves.

[6]Bargain and Jonassen (2022) also separate between the inflow to and outflow from SA recipiency and find that 2/3 of the disincentive effect they estimate for 25 year old, unmarried, and childless individuals with a low level of education in Denmark is attributed transitions from work to social assistance, and 1/3 to a reduced labor market entry.

to be affected by SA policy changes.[7] I apply an event study approach to investigate the reduced form effect of the 1993 court ruling over time, making it possible to estimate time-varying treatment effects and to assess pre-trends. In order to estimate the causal effect of SA generosity, I also apply an instrumental variable approach, in which I use the 1993 court ruling as an instrument for the local norm.[8]

I find that SA generosity on average increased by 7 percent, or SEK 3,500[9] (approximately EUR 350), in municipalities which were the least generous before the 1993 court ruling, compared to unaffected municipalities. I also find that these changes in the local norm affected individuals' actual SA receipts which on average increased by SEK 1100 (22 percent), and that the likelihood of receiving SA the year after the court ruling increased by 2 percentage points (9.1 percent) for the at risk group. In part, increased SA payments and recipiency are a mechanical effect of the more generous SA policy, which raised the SA eligibility threshold. To examine if increased SA generosity also discentivizes work, I study individual labor market outcomes and find that individuals who are predicted at risk of SA recipiency lower their labor supply by 1 percentage point (1.5 percent) as a response to increased SA generosity. Taken together, I find that increased SA generosity does not lead to an increase in total disposable income for this group. For previous SA recipients, the increase in SA generosity generated by the 1993 court ruling increased the likelihood of still receiving SA by 3.7 percentage points (6.9 percent), but, as the increase in SA payments was not offset by a decrease in labor earnings in this group, increased SA generosity lead to a increase in disposable income of 1.4 percent. My findings thus confirm that there is a trade-off between improving the economic conditions for SA recipients and creating work disincentives among individuals on the margin of receiving SA.

Compared to previous studies of the effect of SA generosity on labor supply, the estimated negative effect on employment for the group predicted at risk of receiving SA is relatively large: The implied elasticity of labor supply with respect to SA of -0.2 is somewhat larger than what has been found for single mothers (Meyer and Rosenbaum 2001) and significantly larger than previous estimates for young singles with low educational attainment (Bargain and Doorley 2011, 2017; Bargain and Jonassen 2022; Lemieux and Milligan 2008). I examine whether this is driven by the fact that the samples are different, and find indications that the effects of SA generosity on labor market outcomes are larger for the at risk group I study, especially compared to single men with low educational attainment, also when studied within a common

[7]I estimate the relationship between SA recipiency and a number of individual and household characteristics (in years before the 1993 court ruling), and then predict the likelihood of SA recipiency for all years given these pre-determined characteristics.

[8]The approach is similar to Fiva (2009), who exploits a similar policy change in Norway to study the effects of increased local norms on welfare migration.

[9]Unless otherwise stated, all amounts in SEK in the paper have been deflated to 2019 levels.

(Swedish) setting. Another factor that is different compared to previous work, is that Sweden was hit by a very severe economic crisis in the early 1990s. I exploit differences across municipalities in the changes in local employment rates caused by the crisis, and find that the negative effects on labor market outcomes for the at risk group are driven by individuals living in municipalities which were the most exposed to the crisis. These results suggest that local economic conditions matter, and that the negative effect on employment may rather reflect limited opportunities to find and maintain employment.

The remainder of the paper is structured as follows. Section 1.2 provides a theoretical discussion of individuals' responses to changes in SA generosity. In Section 1.3, I describe SA in Sweden in the 1980s and 1990s, and present the details of the 1993 court ruling exploited in the main analysis. Next, I examine how local governments responded to the 1993 court ruling. The empirical strategy is described in Section 1.5, and the data and samples studied are presented in Section 1.6. In Section 1.7, the main results are presented along with several robustness tests. Finally, Section 1.8 concludes.

1.2 How are individuals expected to be affected by increased SA generosity?

This section provides a theoretical discussion of how different groups of individuals are expected to be affected by, and respond to, changes in the level of social assistance, based e.g. on the discussion in Moffitt (2002).

First, it is important to recognize that because eligibility to SA is based on means-testing, increased SA generosity shifts the threshold or level of income for which a household becomes ineligible to receive SA upward. This shift leads to a mechanical increase in the number of households eligible for SA, and increases the amount received for a given SA recipient, also in the absence of behavioral responses.

SA generosity may also distort individuals' behavior. For non-recipients, more generous benefits creates incentives to reduce income to locate below the SA eligibility threshold. As benefits become more generous and hence more attractive to receive, non-recipients can lower their labor supply in order to become eligible for SA, which leads to an increase in the inflow to SA. Non-recipients who are unemployed but receive other time-limited transfers, like unemployment insurance and sickness insurance benefits, may also be less inclined to search for a job if SA benefits, which they can apply for once the social insurance benefits run out, are more generous. Non-recipients who are entitled to SA and choose not apply, may find it worth while to apply if SA is generous enough. In the latter case, SA recipiency increases without necessarily reducing labor supply. In total, labor supply among non-recipients is expected to decrease as a response to more generous benefits.

Policy makers can however impose costs of receiving SA in order to avoid this type of behavioral effects. In the case of SA, means-testing makes receiving benefits less attractive, as assets like savings, housing etc. must be depleted to qualify for SA. There is also a fixed cost to applying for SA on a monthly basis, if individuals need to go to the social welfare office each month to have their needs and incomes scrutinized. There can also be other psychic costs of applying for and receiving SA, like social stigma (Moffitt 1983).

For SA recipients, the predicted effects on labor supply are more ambiguous. As SA recipients already fulfill the means-testing criteria, more generous SA leads to higher disposable income, all other things equal. This creates a stronger disincentive to search for and take up employment, as it makes it more attractive to remain unemployed and raises the reservation wage an individual is willing to accept to leave unemployment, thus leading to lock-in effects and decreased outflow from SA. As SA recipients have already depleted their assets, are used to visits at the welfare office and may be selected in terms of low stigma, the costs of receiving SA are potentially lower compared to non-recipients and the disincentive effect thus potentially stronger. However, increased disposable income could also give this very vulnerable group the means to leave unemployment and poverty. Having to worry about how to put food on the table the coming days, can impede cognitive function and lead individuals to making bad financial decisions (see e.g. Mani et al. (2013), Schilbach et al. (2016), and Shah et al. (2012) for discussion and empirical evidence). If this is the case, an increase in SA generosity may lead to increased outflow from SA and into employment. The total effect of increased generosity on labor supply among SA recipients is theoretically ambiguous.

Labor demand also likely plays an important role for the effects of SA generosity on individual labor market outcomes. In economic downturns, or at locations with worse labor market conditions, fewer jobs are available and it becomes more difficult to find employment. As more effort is needed to avoid or leave SA recipiency, we may expect larger negative employment effects of increased SA generosity during economic downturns. This may also potentially be reinforced by workers' conditions worsening, and the social stigma of receiving SA becoming less prominent if the number of recipients increases.

1.3 Institutional setting

1.3.1 Social assistance in Sweden

Households with insufficient means to support themselves can apply for SA, which is administered and financed at the municipality level. The right to apply for SA is universal, but only households with sufficiently low total incomes from other sources and without wealth, are qualified to receive SA.[10] This implies that households must deplete all assets, e.g. use up their savings, to be eligible. The means-testing is done on a monthly basis by social workers at the Social welfare office, who are guided by an eligibility threshold.

The eligibility threshold is determined by a local norm, set by the local policy in the municipality. The local norm specifies the *untied cash benefits*, net of other income, that eligible households receive as a monthly lump-sum. The untied cash benefits are fixed, given the composition of the household; the number of adults and number and age of the children. It is a template of all monthly expenses that are assumed to be the same across households, e.g. expenses for food and clothes.[11] The fixed amount simplifies the work of the social worker and avoids scrutiny of each post. It also gives the household greater independence since they can use the money however they see fit. Eligible households also receive *tied cash benefits*. These are supposed to cover needs, like housing, that vary across households and over time.[12] SA is not taxed, and the means-testing and lack of an earnings disregard implies that every 1 SEK earned from other sources leads to a 1 SEK reduction in total SA payments, and hence a marginal tax rate of 100 percent.[13]

Development of SA in the 1980s and 1990s

In 1982, a new Social Services Act was implemented with the aim to guarantee that all individuals who were unable to provide for themselves would be able to maintain a ”reasonable living standard”, regardless of where in the country they lived. The law was however not specific about the minimum level of SA needed to ensure this reasonable living standard. Thus, it was up to each local government to set the level and specify a local norm (Bergmark 2013).

To give some more guidance to the local governments, the National Agency for Health and Welfare (NAHW) issued the first national norm in 1985, tied to the consumer price index, to protect its real value.[14] The national norm was however not binding, and even after it had been issued, there was ample variation in local norms.

[10]The other sources can e.g. include earnings, housing allowance, unemployment (UI) or sickness insurance (SI) benefits. However, SA recipients often need SA, because they are not qualified to receive UI or SI benefits, e.g. due to lack of previous labor market experience or exhausted time limits.

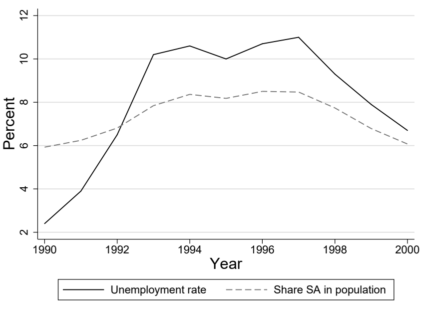
[11]The national norm between 1985 and 1996 covered needs for food, clothes and shoes, play and spare time activities, furniture, consumption goods, health and toiletry articles, phone, newspaper and TV fees, medical and dental fees. The local norms included most or all of these posts.

[12]Tied cash benefits can e.g. also include childcare and union fees, large medical and dental expenses. The SA granted to cover these needs must be accounted for each month with receipts, rent contracts etc.

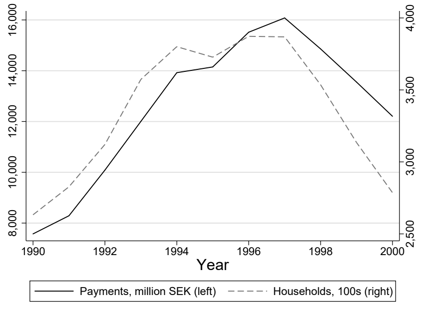
[13]The earnings disregard was first introduced in Sweden in 2013.

[14]In practice the national norm was tied to the “basic amount”. It is an amount of money set by the central government which determines the level of social insurance payments.

Figure 1.1. Social assistance and unemployment 1990–2000



(a) Share with SA & unemployment rate



(b) SA expenditure and number of households with SA

Note: Amounts are in 2019 SEK. In panel (b) Payments in million SEK are at the left y-axis, while the number of households in 100s are to the right.

In the early 1990's, Sweden was hit by a severe economic crisis. Unemployment rose from low, stable, rates of around 2 percent, to as much as 10 percent. Before the crisis, the costs for SA had not been a major concern for municipal finances, but as unemployment rose, so did the costs for SA (see Figure 1.1). The share receiving SA in the adult population had been stable around 6 percent during the 1980s until 1990, and reached it's maximum at 8.2 percent 1996. The expenditures for SA (corrected for inflation) increased by 33 percent between 1990–1992.[15] The number of households and share receiving SA in the population remained high throughout the 1990s. Although the numbers had returned to similar levels as before the crisis by 2000, long-term SA recipiency continued to increase, and the expenditures for SA remained higher than in 1990.

1.3.2 The 1993 court ruling

Despite the use of the local norm, there is substantial discretion for the social workers, both in determining eligibility to receive benefits and setting the level of the benefit (Stranz (2007)). The local norm is used as a tool, but the final decision of the social worker is based on an individual assessment of the needs to receive SA based on if the total income of the applying household falls below the eligibility threshold and whether applicants have no other way of supporting themselves. Individuals who are dissatisfied with the decision made by the social worker, can make an appeal to an Administrative Court.

In the early 1990's, some of these appeals were examined in the Supreme Administrative Court. In an important court ruling in April 1993, the Supreme

[15] As a share of total municipal expenditure, SA increased from 1.7 to 2 percent.

Administrative Court ruled in favor of the SA recipient, and declared that the national norm should determine what a ”reasonable standard of living” is, and that local norms below the national norm were insufficient in the eyes of the court.[16] The ruling thus implied that the national norm became the minimum level of SA generosity.

After the 1993 court ruling, there were a couple of other important events causing changes in SA generosity later in the 1990s. The development 1994–1998 in practice lead to a complete convergence between the local and national norms, as the national norm successively became less generous and was finally fixed as the minimum level in the new Social Service Act in 1998.[17] This implied that the increased generosity caused by the 1993 court ruling was temporary in many municipalities.

1.4 Local governments' response to the 1993 court ruling

The 1993 court ruling is expected to have made local SA policy more generous on average, as it forced less generous municipalities (51 percent of all municipalities) to raise the local norm, while it did not affect municipalities that were already following the national norm. There is also a possibility that municipalities responded by shifting need posts between the local norm and *tied cash benefits*: If forced to increase the local norm, a municipality could potentially become less generous in terms of approving other expenses, outside the local norm. But as these expenses mainly included rent (Aguilar and Gustafsson 1993), there was not much scope for less generous municipalities to manipulate SA generosity. There were also some municipalities that were more generous than the national norm (34 percent), and it is not clear whether these municipalities would respond to the ruling, as it did not address more generous local norm. In this section, I will study empirically how local governments responded to the 1993 court ruling.

To characterize local SA generosity, I exploit data on the local norms[18] 1988–1996 available in Statistics Sweden's ”Statistical reports” (Statistiska meddelanden) and from surveys made by Statistics Sweden on behalf of the National Board of Health and Welfare.[19] The data set includes the local norm

[16]The ruling included 2 appeals based on SA payments made in 1992, where two different municipalities had gone to court against individual SA recipients (Regeringsrätten 1993).

[17]Four expense posts were removed from the national norm – two (medical costs and furniture) in 1996 and two (electricity and home insurance) in 1998. These expenses were shifted to the *tied cash benefits*, but since these were only approved on the occasion the welfare office judged them as strictly necessary, the overall generosity decreased.

[18]The local norms have previously been used to measure SA generosity, for instance, by Fiva (2009) in Norway and by Dahlberg and Edmark (2008) in Sweden.

[19]The data covers the years 1988, 1991–1992, 1994 and 1996.

by household composition. Because norms for single and cohabiting households are highly correlated, I focus on the local norm levels for single adults as a measure of local norms in the remainder of the paper.[20]

Figure A.4 shows how the local norms were distributed across the Swedish municipalities in 1992. Even if the least generous municipalities tend to be located in the central southern parts of Sweden, and northern Sweden was on average more generous than the south, there was still ample geographic variation in terms of local norms.

1.4.1 Evolution in the local norms

Figure 1.2a shows how the local norms evolved over time 1988–1996. Municipalities have been divided into four groups based on the level of the local norm in 1992, prior to the 1993 court ruling. Municipalities on (Q3) or above (Q4) the national norm comprise the two first groups. Municipalities applying local norms below the national norm are divided into two groups: municipalities deviating the least (Q2) and the most (Q1) (henceforth called the least generous) below the national norm. As is shown in Figure 1.2a, the average local norms in the four groups were relatively stable 1988–1992. Between 1992 and 1994 the average local norms converged toward the national norm after the 1993 court ruling. This was true both among municipalities initially applying more and less generous local norms compared to the national norm. However, due to policy changes in the following years (which lead to less generous SA across the board), the increased SA generosity ended up being temporary in many municipalities. Figure A.2 provides additional description of the evolution of the local norms.

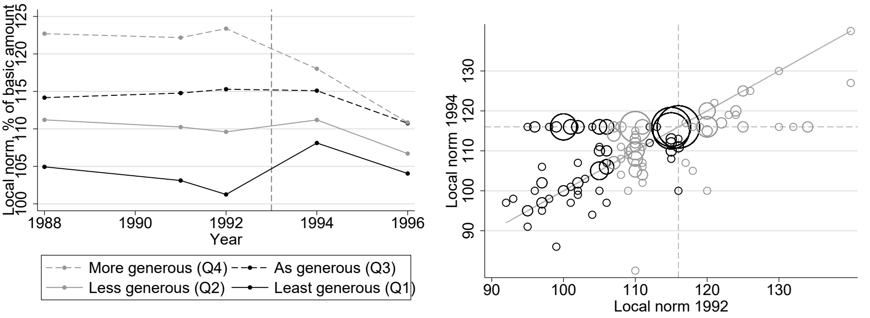
Figure 1.2b provides a more detailed illustration of how municipalities adjusted their local norms around the time of the 1993 court ruling. It plots the relationship between the local norms in 1992 and 1994 for each municipality. The main takeaway from this figure is that many, but not all, municipalities adhered to the 1993 court ruling. Among the least generous municipalities (black circles), 62 % increased their local norm (of which 73 % adjusted fully to the national norm), 27 % remained at the same level and 11 % decreased their local norm.[21] To deal with the fact that not all of the least generous municipalities responded to the 1993 court ruling, I will also use an IV approach and estimate the causal effect of an increase in the local norm.

A threat to identification would be if local economic conditions affected SA generosity. If we see that low local norms are strongly associated with low employment rates, this is likely to be the case. To examine the relationship

[20] According to aggregated data provided by Statistics Sweden, 81 % of households receiving SA in 1992 were single households.

[21] As is shown in Figure A.3, municipalities who decreased the local norm despite being affected by the 1993 court ruling were already on a declining trend 1991–1992, which suggests that they did not lower the local norm as a response to the 1993 court ruling.

Figure 1.2. Evolution in the local norms



(a) Average norm by distance to national norm 1992

(b) Changes 1992–1994

Note: In 1.2b, the size of the circle indicates how many municipalities are located in a given cell. The dashed line indicates the level of the national norm. Black circles indicate the least generous municipalities and municipalities complying with the national norm.

between local conditions and SA generosity, I run OLS-regressions with local employment on the left hand side and different definitions of the generosity of the local norm on the right hand side, controlling for year and municipality fixed effects. There appears to be no relationship between the local employment rate and the local norm, which indicates that this is not a major concern (see Table A.1).

1.4.2 Municipality level responses to the court ruling

The previous section confirms that local governments on average responded to the 1993 court ruling by adjusting the local norms toward the national norm. But if the social workers do not update their practice according to the new norm, individuals will not be affected. This section therefore investigate whether the court ruling lead to changes in aggregated actual SA payments. To do this, I apply an event-study difference-in-differences approach. I divide municipalities into four groups, based on the distance between the local and national norm (as in the previous section). Municipalities already complying with the national norm are used as a control group. Other municipalities are differentially treated depending on how much they deviate from the national norm, and are divided into three treated groups. The event study regression is specified:

$$Y_{mt} = \sum_{t=1990}^{1995} \sum_{q=1,q\neq 3}^{4} (\tau_{qt} NormQq_{1992,m} 1[year = t]) + \gamma_m + \gamma_t + \epsilon_{mt} \quad (1.1)$$

where $NormQq_{1992,m}$ indicates to which treatment group municipality m belongs, based on the local norm in 1992: $NormQ1_{1992,m} = 1$ for the least generous municipalities, $NormQ2_{1992,m} = 1$ for municipalities deviating less below the national norm, and $NormQ4_{1992,m} = 1$ for municipalities applying local norms more generous than the national norm. The third quarter consists of municipalities complying with the national norm, and comprises the comparison group. τ_{qt} give the estimated treatment effects for each year.

Table 1.1. *Difference-in-difference results using aggregated municipality data*

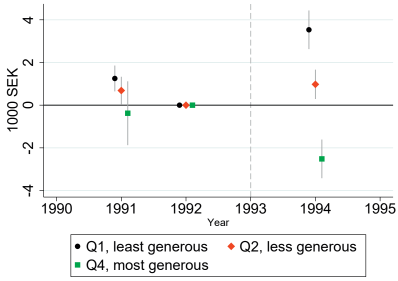
	(1) Local norm	(2) SA/inhabitant	(3) SA/rec.	(4) SA months/hh	(5) SA recipiency
Q1	3.5265***	0.2567***	1.8256***	0.3410***	0.0037
	(0.4620)	(0.0656)	(0.4299)	(0.1247)	(0.0027)
Q2	0.9639***	0.1990***	1.8168***	0.1450	0.0028
	(0.3505)	(0.0546)	(0.4440)	(0.1242)	(0.0025)
Q4	-2.5151***	0.0711	-0.0357	0.0545	0.0013
	(0.4622)	(0.0720)	(0.5287)	(0.1572)	(0.0033)
	(0.0861)	(0.0156)	(0.1147)	(0.0333)	(0.0007)
N	566	572	572	562	572

Note: Years 1992 and 1994. Amounts in SEK 1000. Standard errors clustered by municipality. Year and municipality fixed effects are included in all specifications. *, **, *** represent the 10%, 5% and 1% significance levels. I have aggregated data on SA/inhabitant and fraction with SA from the sources presented in Section 1.6, while SA months per household have been aggregated by Statistics Sweden.

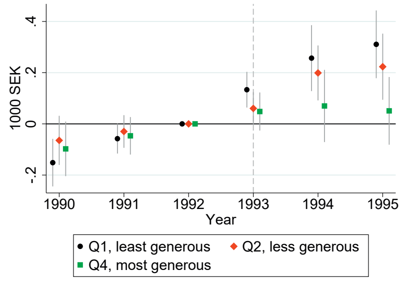
The results from the event-study approach are displayed in Figure 1.3, and Table 1.1 shows the results for 1994. First, Figure 1.3a and Column 1 in Table 1.1 show how local norms were affected by the court ruling. While the local norms on average increased by approximately SEK 3500 and SEK 1000 in least and less generous municipalities respectively, they decreased by 2500 in more generous municipalities. Even if more generous local norms were not addressed in the 1993 court ruling, generous municipalities also seem to have responded by adjusting the local norm.

Figures 1.3b–1.3e and columns (2)–(5) in Table 1.1 show how actual SA payments changed in response to the court ruling. I measure actual SA payments as (i) the amount SA paid out per inhabitant and recipient, (ii) the number of months with SA per receiving household, and (iii) the share of the adult population receiving any SA payments in a given year. Among more generous municipalities (Q4), the decrease in local norms did not affect actual SA payments, on average. Compared to less generous municipalities (Q2) – for which I also find positive effects of the 1993 court ruling on SA payments – the effect were larger in municipalities that were the least generous before the ruling (Q1), which is in line with these municipalities having raised the local norms the most.

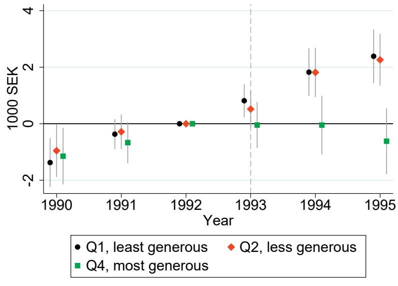
Figure 1.3. Aggregated effects of the court ruling, using municipality level data



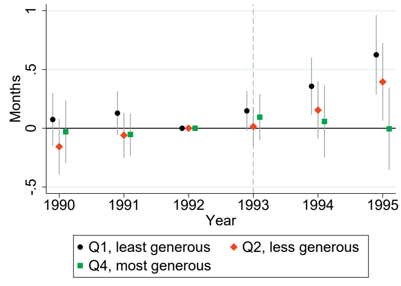
(a) Local norm, SEK 1000



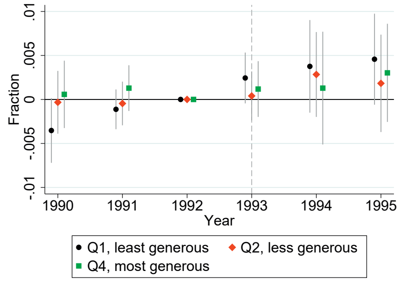
(b) SA/inhabitant, SEK 1000



(c) SA/recipient



(d) SA months/household



(e) Fraction with SA

Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Year and municipality fixed effects are included in all specifications. I have aggregated data on SA/inhabitant and fraction with SA from the sources presented in Section 1.6, while SA months per household have been aggregated by Statistics Sweden.

There are some trends in SA payments before 1993. To control for local employment does not change this, which indicates that they are not explained by changes in the local labor market (see Figure A.6). To discover trends in local SA payments is perhaps not surprising given that municipalities are divided into treatment and control groups based on the generosity of the SA policy. The year during the pre-period that stands out as being different from zero is 1990, which is likely different from the other years as the crisis had not yet become a major factor. Given that the pre-effects for 1991 are close to zero and that there is a compelling divergence between municipalities that were deviating below versus above the national norm after the court ruling, indicates that we are picking up an effect of the 1993 court ruling between 1992 and the post reform years 1993–1995.

Due to the findings that (i) actual payments in more generous municipalities were not affected, and (ii) individuals living in less generous municipalities did on average not experience a substantial change in the local norm, I will exclude these two municipality groups from the analyses in the remainder of the paper. I will thus exploit the change in the local norm in the least generous municipalities (Q1), compared to the unaffected municipalities (Q3), as the identifying variation in the remainder of the paper.[22] The empirical strategies I apply to do this will be presented next.

1.5 Identifying effects of SA generosity on individual outcomes

I apply two empirical strategies in this paper. I first apply an event difference-in-differences approach, to analyze how individuals' labor market outcomes, SA and disposable income were affected by the 1993 court ruling:

$$Y_{imt} = \sum_{t=1990}^{1995} \tau_t NormBelow_{1992,m} 1[year = t] + \gamma_m + \gamma_t + \beta X_{imt} + \epsilon_{imt} \tag{1.2}$$

where Y_{imt} is SA, earnings, employment or disposable income in year t for individual i living in municipality m. $NormBelow_{1992,m}$ is an indicator variable which is equal to one if an individual's municipality of residence was among the least generous municipalities in 1992. Municipalities adhering to the national norm in 1992 are not affected by the court ruling and are used as the control group, $NormBelow_{1992,m} = 0$. $\tau_{1990-1991}$ (1992 is used as baseline) assesses the pre-trends, and $\tau_{1993-1995}$ the treatment effect that can

[22]The lack of effect on actual SA payments for more generous municipalities could potentially be explained by shifting posts between the tied and untied benefits as a means to keeping the level of SA constant constant. While this is also an interesting question to study, it is outside the scope of this paper.

vary by year. γ_m captures time-invariant municipality-specific factors, and γ_t captures a common time effect. X_{imt} includes individual characteristics; sex, marital status, age, age squared, children, educational attainment, migration background (time in Sweden, region of origin).[23]

The validity of the difference-in-differences strategy relies on parallel trends in the outcome variables in the absence of the court ruling. $\tau_{1,1990-1991}$ tests whether there are pre-treatment trends for these years, which indicates whether this is a reasonable assumption to make.

Second, I estimate the causal effect of SA generosity on labor market outcomes, exploiting the interaction between an indicator variable for living in a least generous municipality, and an indicator variable for the year after the 1993 court ruling, as an instrument for the local norm level, in the following first stage equation:

$$Norm_{mt} = \alpha_1 NormBelow_{1992,m} * Post_t + \gamma_m + \gamma_t + \beta X_{imt} + \upsilon_{imt} \quad (1.3)$$

where $Post_t$ equals 1 for the year 1994 and 0 for 1992. $NormBelow_{1992,m} = 1$ for the least generous municipalities (treated) and $NormBelow_{1992,m} = 0$ for the municipalities that already conformed with the national norm (controls).[24]

The second stage regression is given by:

$$Y_{imt} = \tau_1 \hat{Norm}_{mt} + \gamma_m + \gamma_t + \beta X_{imt} + \epsilon_{imt} \quad (1.4)$$

where τ_1 captures the effect of changes in the local norm for individuals who live in municipalities that respond to the court ruling by adjusting the local norm, the Local Average Treatment Effect (LATE). Within a complying municipality (a municipality raising the local norm), all individual are treated by the subsequent change in the local norm.

The identifying assumptions of the instrumental variable analysis is that the instrument is relevant and independent of all other variables that determine Y_{imt}. The relevance of the instrument can be assessed by studying the results from the first stage regression. I run several robustness and placebo tests in Section 1.7.3 to support the claim that the instrument is independent of e.g. labor market outcomes.

[23] As municipality covariates like the unemployment rate and population composition could in themselves be outcomes, i.e. bad controls (Angrist and Pischke 2008), I choose not to include them in the main analysis. In Section 1.7.3, I however test the robustness of my results to controlling for local employment and immigrant inflow. I also perform the analysis using plausibly unaffected individuals, to make sure that the estimates are not merely picking up overall changes in the local labor market.

[24] I have also used the actual distance between the local and national norm to measure exposure to the court ruling, but this does not lead to a stronger first stage effect compared to the indicator variable for living in a least generous municipality.

Another identifying assumption of the instrumental variable approach is monotonicity. This assumption is violated if there exists municipalities which, as a response to a court ruling forcing them to raise SA levels, decrease generosity to defy the court ruling. In the sample of municipalities initially below the national norm, there are 9 municipalities which lower the local norm 1992–1994. In Figure A.3, I show that the average local norm level was already decreasing in these municipalities before 1992, which indicates that they were continuing with ”business as usual”, rather than defying the Supreme Administrative Court.

In the main analysis, I study the effects on individuals at risk of receiving SA. To learn something about the underlying mechanisms, I also study the effects on SA recipients separately. In studying the effects on employment in the former group, I capture both the inflow and outflow from SA recepiency into employment, while studying the latter allows me to isolate effect on the outflow from SA. I also study the effects separately by the level of exposure to the economic crisis, to investigate the importance of macroeconomic conditions. In the following section, I describe the data and how these different groups are defined.

1.6 Data

The analysis exploits annual data from administrative population, tax and educational records, for all 16–65 year-olds in the Swedish population 1990–1995, provided by Statistics Sweden. The data includes information on age, gender, marital status, children, municipality of residence, migration history, educational attainment and income sources.

The outcome variables of interest are labor earnings, employment (defined as earning at least one monthly minimum wage in a given year), the amount SA payments received, SA recipiency (defined as receiving any SA in a given year) and disposable income.[25] Disposable income is used as a means of assessing whether an individual is made better off by the 1993 court ruling and the subsequent increase in SA generosity.[26]

In the event analysis, I follow individuals 1990–1995. Since data on my measure of SA generosity, the local norms, are not available for 1993 (nor 1995), the IV analysis is performed using data from 1992 and 1994, the years

[25]I follow Kramarz and Skans (2014) who use the full-time monthly wage for janitors as a proxy for the minimum wage. For SA outcomes, data is missing in 12 municipalities (Burlöv, Arvika, Täby, Stenungsund, Lilla Edet, Alingsås, Mariestad, Hudiksvall, Trosa and Örnsköldsvik), which are excluded from the analysis.

[26]Disposable income includes all incomes from e.g. employment, self-employment, capital and transfers, net of taxes. It is first measured at the household level, and then individualized using consumption weights.

before and after the court ruling, respectively.[27] Local norms in 1992 and 1994 were measured in February each year, which ensures that the 1994 local norm levels include the changes caused by the court ruling. Treatment status is decided by the local norm 1992 in the individual's municipality of residence.

In the heterogeneity analysis in Section 1.7.5, I study whether the effects are larger or smaller depending on how exposed a given municipality was to the economic crisis. I follow the approach taken by Engdahl and Nybom (2021), and define exposure to the economic crisis in Sweden in the 1990s as the change in employment in a given municipality between 1990 and 1992. I then divide municipalities into two groups based on whether or not they experienced a decrease in employment that was below or above the median decrease.

1.6.1 Description of affected and unaffected municipalities

Columns 1–2 in Table 1.2 describe the adult population and municipality characteristics in 1992 in the two municipality groups included in the analysis, respectively. In addition, Column 3 shows the descriptive statistics for all municipalities, which allows me to assess how representative the municipality groups under study are.

Local norms are on average SEK 7,000 lower in the least generous municipalities compared to the municipalities adhering to the national norm, and SEK 5,000 lower than the average. Despite this, the share receiving some SA is very similar across groups, approximately 7 percent. One of the main differences to notice between the municipality groups is that the least generous municipalities are considerably less likely to be governed by a left-wing government, both compared to the comparison group (Column 2) and all municipalities (Column 3). There is also a difference in the proportion of municipalities that are metropolitan areas, where the least generous municipalities consist of 20 percent metropolitan areas compared to 11 percent for complying municipalities, and 16 percent overall.[28] The share immigrants and share with only compulsory school education are also slightly higher in the least generous municipalities, but close to the average.

Compared to the median exposure to the economic crisis (change in employment 1990–1992), municipalities who were following the national norm in 1992 were less likely to experience above median crisis exposure, and the least generous municipalities were more likely, compared to the average municipality. However, disposable income, employment and the change in unemployment between 1990 and 1992 are, on average, comparable across the

[27] Given that the increase in local norms 1992–1994 on average ended up being temporary (see Section 1.3.2), only studying short to medium run effects seem like a reasonable choice.

[28] Among Sweden's three largest metropolitan areas, Gothenburg is the only municipality included in the analysis (among the least generous municipalities), while Stockholm is found among the most generous municipalities (Q4) and Malmö among the less generous municipalities (Q2).

Table 1.2. *Municipality level summary statistics, means in 1992*

	(1) Least generous		(2) At national norm		(3) All	
	mean	sd	mean	sd	mean	sd
Local norm	491	18	559	2	539	38
SA > 0	0.071	0.019	0.070	0.016	0.072	0.018
Immigrant	0.103	0.058	0.082	0.052	0.091	0.057
0–5 yrs since immigr.	0.018	0.010	0.013	0.007	0.015	0.009
Compulsory school	0.319	0.058	0.291	0.051	0.299	0.056
Disposable income	1378	87	1382	104	1387	109
Earnings> 0	0.892	0.019	0.890	0.021	0.890	0.021
Employment	0.845	0.021	0.841	0.024	0.842	0.024
Δ Employment 90–92	-0.073	0.012	-0.072	0.014	-0.073	0.013
High crisis exposure	0.532	0.502	0.460	0.501	0.500	0.501
Left-wing government	0.257	0.440	0.534	0.502	0.454	0.499
Metropolitan area	0.195	0.399	0.110	0.314	0.157	0.365
City	0.377	0.488	0.400	0.492	0.367	0.483
Small city or countryside	0.429	0.498	0.490	0.502	0.476	0.500
Observations	77		100		286	

Note: Amounts are in 100 SEK (2019 base year). Municipalities equally weighted. High crisis exposure refers to above median decline in local employment rate 1990–1992. Employment is defined as the proportion earning at least one monthly minimum wage.

three groups. Municipalities that were affected by the court ruling are thus not strikingly different in terms of local labor market conditions, judging by the the average labor market outcomes of the individuals living there.

1.6.2 Definition of studied samples

In my analysis, I restrict the sample to individuals aged 18–58 and the period to 1990–1995.[29] The sample is, as previously mentioned, also restricted to individuals living in the least generous municipalities and municipalities initially complying with the court ruling. In the following section, I define the sample of individuals at risk of receiving SA and the sample of previous SA recipients.

At risk group

For most individuals, the probability of being in need of SA is very close to zero. Only 7 percent of all households receive some SA in 1992 and, due to the presence of UI or SI benefits, the requirement to deplete all assets, and the monthly means-testing of SA benefits, individuals who temporarily loose their job are not likely to apply. In combination with the relative modesty of

[29] I set the upper age limit at 58, as individuals born before 1932 are generally not covered in the register (flergenerationsregistret) from which I extract country of birth.

the changes in SA generosity, it is unlikely that I can detect potential effects on labor market outcomes studying the full adult population. Instead I focus on a sample at risk of receiving SA, which in previous literature has been defined as high school dropouts (Bargain and Doorley 2011; Lemieux and Milligan 2008). Given the rich data I have access to, I can improve the accuracy of the prediction of being at risk of receiving SA, by utilizing additional information about individuals. I predict the likelihood of receiving SA based on demographic characteristics; age, sex, immigration status, being born in a non-Western country, number of children, having small children (6 years old or younger), being a single parent, educational attainment and interactions of sex and immigrant status with each other covariate included. I run a logit regression for the pre-court ruling years, 1990–1992, and then predict the propensity to receive SA 1990–1995.[30] The individuals who are in the top quintile in a given year are included in the sample of individuals at risk of receiving SA.

The sample of individuals at risk of receiving SA, of course, also includes some actual SA recipients, and can therefore both capture effects on the entry and exit from SA and unemployment. The share of the at risk sample who received some SA in 1992 is 21 percent (see Table 1.3), which is much larger than 7 percent in the population at large, but the at risk sample mainly consists of non-recipients.

Previous SA recipients

In order to isolate the effects of increased generosity on exit from SA and unemployment, I also perform an analysis where I study previous SA recipients separately.

The fact that SA recipients in treated and control municipalities are different in terms of labor market trajectories due to the differences in SA eligibility thresholds before the reform, forces me to define the sample in a way that holds these differences constant. Since I cannot define the sample based on SA recipiency after the 1993 court ruling, and I only have data going back to 1990, I define this group as individuals who received SA two years prior to a given year, in $t-2$. One limitation with this definition is that I do not perfectly capture previous SA recipients. In 1992, 53 percent of the individuals in this sample (defined as those receiving SA two years prior), received some SA. Furthermore, it only allows me to perform the IV analysis, as I do not have data going back far enough to assess pre-trends, necessary for the reduced form analysis.

Description of the studied samples

Table 1.3 describes the characteristics of the two samples presented in the previous sections. Columns 1–2 describe individuals who are at risk of receiving

[30] This is similar to Markussen and Røed (2016) and Hernæs et al. (2017), who also use predetermined characteristics to estimate the propensity to be affected by a policy change.

SA in 1992, and divide them by whether they live in the least generous municipalities (treatment group, Column 1) or in municipalities complying with the national norm (control group, Column 2). Columns 3–4 describe previous SA recipients in the two municipality groups respectively.

Table 1.3. *Individual level summary statistics, means in 1992*

	(1)	(2)	(3)	(4)
	At risk group		Previous SA recipient	
	Least generous	At national norm	Least generous	At national norm
Female	0.508	0.524	0.485	0.497
Age	32.990	32.948	34.124	34.231
Married	0.296	0.235	0.307	0.282
Parent	0.670	0.708	0.583	0.613
Nr of children	1.362	1.403	1.225	1.252
Single parent	0.343	0.418	0.223	0.240
Immigrant	0.494	0.365	0.375	0.246
Non-western immigrant	0.219	0.129	0.230	0.135
0-5 yrs since immigration	0.111	0.075	0.143	0.089
Compulsory school	0.540	0.538	0.443	0.413
High school	0.370	0.386	0.469	0.516
Post high school	0.090	0.076	0.087	0.072
Employment	0.674	0.704	0.532	0.575
Earnings, 100 SEK	1132	1191	736	803
Disposable income, 100 SEK	1175	1216	1122	1150
SA > 0	0.223	0.204	0.538	0.513
SA amount, 100 SEK	51	41	130	101
Observations	295,596	216,500	89,338	69,786

Note: Amounts are in 100 SEK, 2019 used as base year. Disposable income and SA payments are individualized from household level measures. Earnings are measured for each individual.

Comparing the at risk group in treated and control municipalities (columns 1 and 2), individuals in treated municipalities are more likely to be married and less likely to be (single) parents, and the share immigrants is 13 percentage points higher in the treatment group. In terms of gender, age, and educational attainment, individuals are very similar across the two groups. Individuals in the least generous municipalities are somewhat worse off in terms of labor market outcomes and SA recipiency.

Also among previous SA recipients (columns 3–4), married individuals and immigrants are more common in treated municipalities. Previous SA recipients in the least generous municipalities also have lower educational attainment. Another noteworthy pattern is that individuals in treated municipalities are more negatively selected in terms of labor market outcomes, compared to the control group. This makes sense, as the local norm, i.e. the eligibility threshold, is lower in less generous municipalities. The share receiving some SA among previous SA recipients is 51–53 percent. As explained in Section 1.6.2, being part of this group in 1992 is conditioned on receiving some SA in 1990, and only around half thus still received some SA two years later. Note also that individuals who live in the least generous municipalities receive higher SA payments per person. This could be explained by the fact that this group is more negatively selected due to the lower eligibility threshold, or that the proportion receiving some SA is 2.5 percentage points higher.

1.7 Results: effects of SA generosity on individual outcomes

The results from the analysis on individual outcomes based on the event-study analysis are presented in Section 1.7.1 and the findings from the IV analysis in Section 1.7.2. In Section 1.7.3, I perform several robustness checks. Sections 1.7.4–1.7.5 present results for previous SA recipients and by exposure to the economic crisis.

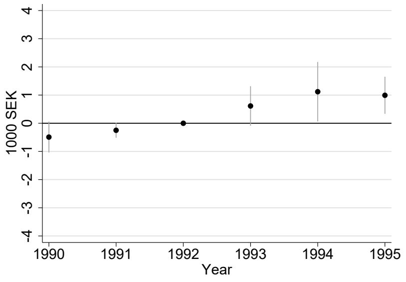
1.7.1 Reduced form: the effect of the court ruling

Figure 1.4 shows the estimated reduced form effects of the court ruling on SA payments, labor market outcomes and disposable income for the group at risk of receiving SA. All regressions include controls for individual characteristics, year and municipality fixed effects, see Equation 1.2.

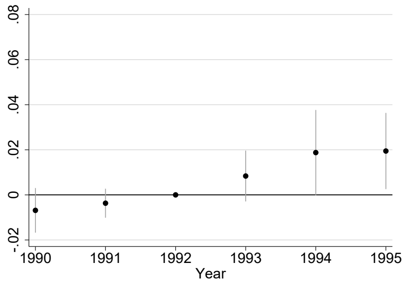
After the 1993 court ruling, there was an immediate increase in both the amount SA payments and recipiency in 1993. In 1994–1995, when effects had stabilized, the court ruling had lead to an average increase in annual SA amounts of approximately 1,000 SEK. Compared to the pre-court ruling mean of the dependent variables in the treated group, SEK 5,100, this implies an increase by 19.6 percent. On the extensive margin, the likelihood of receiving some SA increased by 2 percentage points 1994–1995, which corresponds to an increase of 9 percent compared to the average before the 1993 court ruling (22.3 percent). There are some indications of pre-trends in SA recipiency and payments, which implies that the size of the effects should be interpreted with some caution. It is likely explained by the fact that treatment status is determined based on the municipalities' SA policy prior to the 1993 court ruling. The point estimates are however not statistically different from zero at the 5 percent level and the change after the 1993 court ruling is much larger, which suggests that the analysis is actually picking up an effect of the ruling. Furthermore, adding controls for municipality characteristics has no effect on this pattern (see Appendix B).

The increased SA recipiency could simply be a mechanical effect of raising the local norm, which works as the SA eligibility threshold. To investigate if

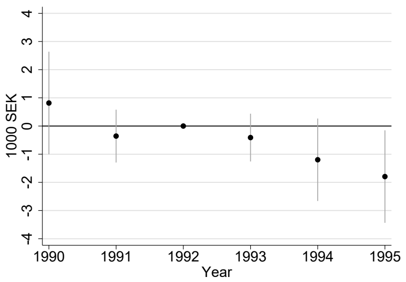
Figure 1.4. The effect of the 1993 court ruling on SA, labor market outcomes and disposable income over time.



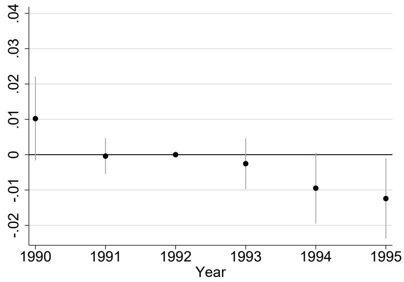
(a) SA amount, at risk



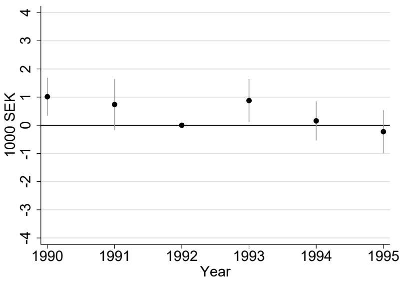
(b) SA recipiency, at risk



(c) Earnings, at risk



(d) Employment, at risk



(e) Disposable income, at risk

Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and person controls: sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent.

the more generous SA benefits also lead to changes in behavior, Figure 1.4c–1.4d show the effects on employment and earnings. The results show that, by 1994–1995, annual earnings had decreased by SEK 1,000–2,000, and the probability of being employed by 1 percentage point, or 1.5 percent, compared to the pre-1993 court ruling average in treated municipalities (67.4 percent). The increase in SA payments and the decrease in earnings are of similar magnitude, and the estimated effects on disposable income do not suggest that the court ruling increased disposable income in the sample. The results thus indicate that more generous SA and increased SA recipiency crowded out labor earnings among individuals who were at risk of receiving SA.

The estimates for 1990 stand out from 1991 and 1992 (the baseline) for all outcomes of interest. This is potentially explained by the economic crisis taking of after 1990. In Section 1.7.3, I do several robustness checks in order to rule out that the economic crisis is driving the results.

1.7.2 IV: the effect of SA generosity on labor market outcomes

This section presents the results from the instrumental variable analysis. These results are informative of how individuals' SA recipiency, labor market outcomes and disposable income are affected by increased SA generosity (induced by the court ruling).

The main results are presented in Table 1.4. All regressions include year and municipality fixed effects, see Equation 1.4. Columns (2), (4), and (6) present estimates including controls for individual characteristics. In general, adding individual characteristics to the specification does not affect the point estimates much, but it improves precision. Panel A shows the result from the first stage, and panel B–C the results for the IV regressions on SA payments, SA recipiency, earnings, employment and disposable income.

The court ruling on average lead to an increase in the local norms of approximately SEK 3,400 (approximately EUR 340). Compared to the average local norm in the least generous municipalities in 1992, SEK 49,400, this represents a 6.9 percent increase in the local norm. The F statistic is 21, which implies that the instrument is relevant and strong.

The IV estimates should be interpreted in the following way: A SEK 1,000 (approximately EUR 100) increase in SA generosity (which corresponds to an increase of 2 percent) increases the probability of receiving SA with 0.6 percentage points, which is an increase of 2.7 percent compared to the mean before the court ruling. The estimated effect on the SA amount received is SEK 330 (6.5 percent).

Studying the effect on employment, the point estimates suggest that experiencing a SEK 1,000 (2 percent) increase in the local norm decreases employment by 0.3 percentage points, or 0.4 percent. This indicates that there is a negative effect on employment of increased SA generosity for individuals at

Table 1.4. *Effect of SA generosity on SA, labor supply and disposable income – First stage and 2SLS results. At risk group.*

	(1)	(2)	(3)	(4)	(5)	(6)
First stage results	Local norm					
Panel A: first stage						
Below*Post	3.388***	3.388***				
	(0.738)	(0.738)				
N	1,003,683	1,003,683				
F stat	21	21				
Mean	49.4	49.4				
IV results	SA amount		SA rec.			
Panel B: SA outcomes						
Local Norm	0.347***	0.332***	0.006**	0.006**		
	(0.124)	(0.115)	(0.002)	(0.002)		
N	1,003,683	1,003,683	1,003,683	1,003,683		
Mean	5.1	5.1	0.223	0.223		
IV results	Earnings		Employm.		Disp. inc.	
Panel C: labor market outcomes						
Local Norm	-0.446*	-0.362*	-0.003**	-0.003**	0.033	0.056
	(0.231)	(0.205)	(0.001)	(0.001)	(0.110)	(0.104)
N	1,024,760	1,024,760	1,024,760	1,024,760	1,024,760	1,024,760
Mean	113.2	113.2	0.674	0.674	117.5	117.5
Individual char.		Yes		Yes		Yes

Note: Amounts in SEK 1000. Standard errors clustered by municipality. Mean refers to the mean of the dependent variable in the least generous municipalities before the court ruling. Year and municipality fixed effects are included in all specifications. Controls for individual characteristics are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

risk of receiving SA. Turning to labor earnings, a SEK 1,000 increase in SA generosity lead to a SEK 360 decrease in earnings, which is very similar in size to the increase in SA payments. As a result, the estimated effect on disposable income is very close to zero.

The effect on employment implies an elasticity of labor supply with respect to SA payments of -0.2. This is a very large response compared to the range -0.03 to -0.06 found for (mainly male) single, childless, 25–30 year-olds (Bargain and Doorley 2011, 2017; Bargain and Jonassen 2022; Lemieux and Milligan 2008), and more in line with the elasticity of -0.15 found for single mothers in Meyer and Rosenbaum (2001). In my sample, as many as 69 percent are parents and 37 percent are single parents. The high proportion parents may be one explanation for how my results relate to previous findings. I test if the relatively large effects I find on labor market outcomes can be explained by how I define the the at risk group, by performing the analysis separately for single mothers and young childless men with low educational attainment, which have been the focus of previous research. The results are displayed in Table A.4 and Figure A.7, and give some indications that the same pattern across subgroups (very small effects for childless, young, men; larger effects for single mothers; the largest for the at risk group) is found within my data and setting, through the estimates for childless men are very imprecise and the evidence not conclusive. In Section 1.7.5, I instead explore if the setting, i.e., the poor economic conditions caused by the crisis, could be an explanation for deviations from previous findings.

1.7.3 Robustness checks

To verify that the results are not sensitive to specification choices and that they are not driven by different trends in the local labor market situation, I perform several robustness and placebo analyses in Appendix 1.7.5.

First, as a check of the validity of my sample definition of individuals at risk of receiving SA, I have performed the event analysis for each risk quintile separately. The results are displayed in Figure B.1, and show that the effects on SA recipiency are stronger the more likely an individual is to receive SA (and thus the more likely to be affected by changes in SA policy), and that the employment and earnings effects are mainly prevalent for the most likely quintile. The lack of effects on employment for all other risk groups also suggests that there was not a general negative trend among individuals living in treated compared to control municipalities. In Table B.1, I also show the results using a stricter definition of being at risk of receiving SA, namely individuals belonging to the most likely decile to receive SA. The effects are stronger for the most likely decile, as expected.[31]

Next, I perform two placebo tests in which I study subsamples of individuals who are not expected to be affected by the 1993 court ruling and the subsequent increase in the local norm. If I find effects on the outcomes of interest in these supposedly unaffected groups, it is possible that the main results are driven by some trend in the local labor market situation. To define the first placebo group, I sample individuals in the at risk group who have a fellow household member who earns above median labor earnings in a given year. Since SA is means-tested at the household level, these individuals are not be eligible for SA even if they reduce their own labor supply. Furthermore, as they have similar individual and household characteristics and educational attainment as the affected individuals in the at risk group, they are likely to operate in the same part of the labor market and consequently be affected by the same local labor demand trends. The second placebo group is the group of individuals who were predicted the 20 percent the least at risk of receiving SA. The absence of effects for this group helps me rule out that the results are driven by local trends that are common to everyone in a given municipality. For both placebo groups, estimated effects in Figure B.2 and Table B.2 are very close to zero. The absence of any effects for these plausibly unaffected groups, who either have very similar or very different pre-determined characteristics as the at risk group, indicates that municipality specific trends are not driving the results, as this would likely show up in at least one of these placebo samples.

Another way of ruling out that local trends are driving my results, is to add municipality level controls to the main specifications in Equations 1.2 and 1.4. In Table B.3, I compare the main results in Column 1, to specifications where I add municipality controls for the share of immigrants in the population who arrived past 0–2 and 3–5 years[32] (Column 2), the employment rate in time t for individuals in the quintile with the lowest predicted probability of receiving SA (Column 3), and the local employment rate in t–2 (Column 4).[33] Figure B.3 shows the corresponding robustness check for the event specification. Adding municipality controls to the event and IV specifications does in general not change the results. However, as local employment in t–2 is added, the point estimates on employment and earnings become somewhat smaller, and are no longer statistically significant at the five percent level in the event specification.

[31] My results are also robust to changes in how the at risk predictions are made, e.g. including information about hospitalizations, and to instead predict the risk of receiving SA at least as high as the 25th percentile of annual SA payments (as opposed to receiving any SA).

[32] Dahlberg and Edmark (2008) show that municipalities decrease generosity in response to an inflow of refugees, which in turn causes neighboring municipalities to decrease SA generosity. I control for the foreign born share of the population to see that this is not behind the results.

[33] The latter is measured in t–2 to avoid that the employment rate could have been affected by the 1993 court ruling.

Table 1.5. *Effect of SA generosity on SA, labor supply and disposable income – 2SLS results. Previous SA recipients.*

	(1)	(2)	(3)	(4)	(5)	(6)
IV results	SA amount		SA recipiency			
Panel A: SA outcomes						
Local Norm	0.633***	0.664***	0.009***	0.010***		
	(0.190)	(0.192)	(0.003)	(0.003)		
N	352,545	352,545	352,545	352,545		
Mean dep. variable	130	130	0.538	0.538		
IV results	Earnings		Employment		Disp. income	
Panel B: labor market outcomes						
Local Norm	-0.095	-0.181	-0.001	-0.002	0.482***	0.426***
	(0.234)	(0.220)	(0.001)	(0.001)	(0.143)	(0.139)
N	358,800	358,800	358,800	358,800	358,800	358,800
Mean dep. variable	736	736	0.532	0.532	1122	1122
Individual char.		Yes		Yes		Yes

Note: Standard errors clustered by municipality. Individual characteristics, year and municipality fixed effects are included in all specifications. Individual characteristics are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.* First stage results are presented in Table A.3.

Finally, I perform a placebo test where 1992 is treated as the post court ruling year, and 1991 as the year prior to it. The results of the reduced form analysis are displayed in Table B.4. I do not find that there is an effect of the placebo court ruling on the outcomes of interest. The coefficient on the amount SA paid is positive and statistically significant at the 10 percent level, but as the results from the event analysis in Figure 1.4 already indicated, there were some trends in SA payments prior to the court-ruling. The first stage results has a F statistic of 9, which is much weaker than the main analysis, and the coefficient is negative.

1.7.4 Results for previous SA recipients

A group of particular interest is previous SA recipients. They are an economically vulnerable group in need of SA to make ends meet. Increased SA generosity is meant to help them financially. Furthermore, studying this group allows me to isolate the effect of increased SA generosity on the outflow from SA and unemployment. As previously discussed 1.6.2, I define previous SA recipients as individuals who received SA in t–2, in order not to define the sample based on an outcome of the 1993 court ruling, and to hold differences across treated and control municipalities constant. Since I only have access to individual data from 1990, this prevents me from implementing the event-study specification for this sample.

The result from the IV analysis is presented in Table 1.5. The positive effect on SA payments is about twice as large for the group of previous recipients compared to the at risk group. A SEK 1,000 increase in the local norm leads to an average increase in SA payments of SEK 660. One reason whythe increase is not 1-to-1 is that not all in the sample were still receiving SA at the time of the court ruling and subsequent changes in the local norm. The increase in the local norm increases the likelihood of receiving SA by 1 percentage point, or 1.9 percent compared to the mean in the least generous municipalities in 1992.

The estimated coefficients on labor market outcomes are not statistically significant at the ten percent level, and do not imply that more generous SA disincentivizes work for this group. The point estimate is however not small enough to exclude the possibility that there is an economically meaningful negative effect on employment. In combination with the large positive effect on SA payments, increased SA generosity leads to an increase in disposable income for previous SA recipients of SEK 430 for each SEK 1,000 increase in the local norm. Previous SA recipients are hence made better off economically by the increase in local norms.

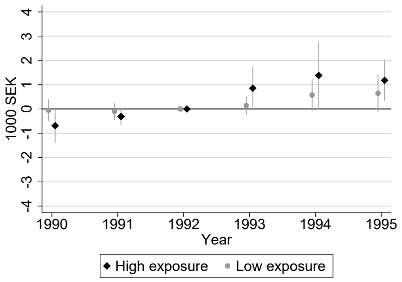
1.7.5 Results by exposure to the economic crisis

To examine the importance of macroeconomic conditions for the effect of SA generosity, I divide the samples into two groups based on whether individuals live in municipalities which were above or below the median exposure to the economic crisis, following Engdahl and Nybom (2021). I thus compare individuals living in the least generous municipalities, who experienced high exposure to the recession, to individuals in municipalities initially following the national norm, who also experienced a high exposure to the recession. I do the same exercise for individuals living in municipalities who experienced relatively low exposure to the crisis. Less exposed municipalities also experienced substantial decreases in local employment rates 1990–1992, ranging between 3 and 7 percentage points, while the corresponding figures for more exposed municipalities were 7–12 percentage points. The results for less exposed municipalities should thus not be interpreted as results in the absence of an economic downturn.[34]

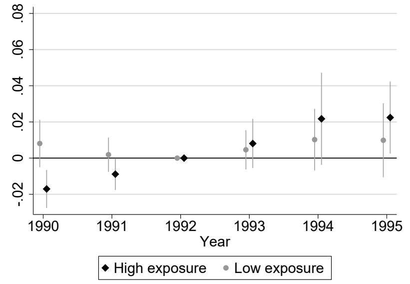
Figure 1.5 displays the results for the event analysis by crisis exposure. Whilst I cannot rule out that the effects are the same across crisis exposure groups, the point estimates suggest that the negative effects on employment and earnings are driven by municipalities that were heavily exposed to the cri-

[34] In fact, the drop in employment during the 1990s economic crisis was unprecedented. Both the drop in employment during the financial crisis and the Covid-19 crises have been in the lower range of the "less exposed" municipalities 1990–1992.

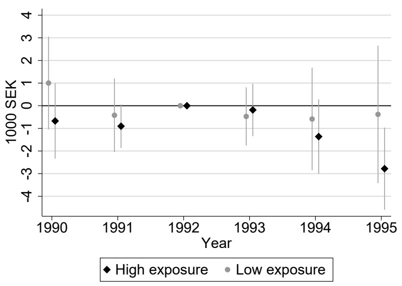
Figure 1.5. The effects of the 1993 court ruling over time by crisis exposure



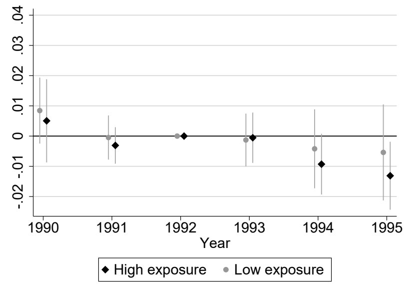
(a) SA amount



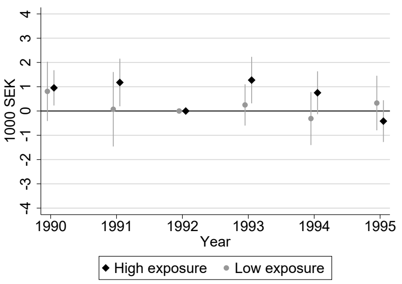
(b) SA recipiency



(c) Earnings



(d) Employment



(e) Disposable income

Notes: High exposure refers to municipalities that experienced above median decrease in employment 1990–1992. Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and person controls: sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent.

sis. In less exposed municipalities, I do not find support for a negative effect on labor market outcomes, and the effects on SA outcomes are smaller.

Table 1.6. *Analysis by exposure to the crisis – 2SLS results*

	SA amount	SA rec.	Earnings	Employm.	Disp. inc.
At risk					
Panel A: High exposure					
Local Norm	0.437***	0.007**	-0.445*	-0.003**	0.254*
	(0.152)	(0.003)	(0.250)	(0.001)	(0.137)
N	577,608	577,608	586,541	586,541	586,541
Panel B: Low exposure					
Local Norm	0.169**	0.003	-0.179	-0.001	-0.087
	(0.082)	(0.002)	(0.342)	(0.002)	(0.164)
N	425,425	425,425	437,569	437,569	437,569
Previous SA recipients					
Panel C: High exposure					
Local Norm	0.873***	0.012**	-0.612**	-0.004***	0.489**
	(0.259)	(0.005)	(0.278)	(0.001)	(0.201)
N	208,306	208,306	211,970	211,970	211,970
Panel D: Low exposure					
Local Norm	0.323*	0.005	0.232	0.002	0.297*
	(0.164)	(0.004)	(0.403)	(0.002)	(0.171)
N	144,115	144,115	146,706	146,706	146,706

Note: Standard errors clustered by municipality. Individual characteristics, year and municipality fixed effects are included in all specifications. Individual characteristics are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. *$p<0.1$, **$p<0.05$, ***$p<0.01$. The first stage results are presented in Table A.3.

Table 1.6 displays the results from the IV analysis for both individuals at risk of receiving SA and previous SA recipients. For previous SA recipients living in municipalities the most exposed to the crisis, there is in fact a rather strong negative effect on employment, in contrast to the overall effects presented in the previous section. As increased SA payments compensate for the loss of earnings, disposable income still increases for this group. Individuals living in less exposed municipalities, do not seem to change their employment behavior in response to the increased SA generosity.

The results indicate that macroeconomic conditions are important for how individuals are affected by increased SA generosity, and that labor demand likely plays an important role for effects on employment.

1.8 Conclusions

I estimate the effects of increased SA generosity on individual labor market outcomes, exploiting a court ruling in the Swedish Supreme Administrative Court 1993, which implied that a minimum level of SA generosity was implemented. The court ruling affected individuals living in less generous municipalities, while individuals in municipalities who were already complying with the national norm were unaffected. I find that affected municipalities on average responded by increasing the annual local norm by 7 percent, or approximately SEK 3,500 (EUR 350) per year, compared to unaffected municipalities. Studying individuals predicted to be at risk of receiving SA (based on pre-determined characteristics) I show that the increase in SA generosity lead to increased SA recipiency and amount of SA received, and that employment and earnings are crowded out. A 1 percent increase in SA generosity increased SA recipiency by 0.3 percentage points (1.4 percent) and lead to a decrease in employment of 0.15 percentage points (0.2 percent), leaving disposable income unchanged.

I also study the effects of increased SA generosity for previous SA recipients, and find that, for them, the increase in income created by an increase in SA amounts received was not offset by an decrease in labor earnings, on average. The increase in SA generosity therefore resulted in an increase in disposable income for this financially vulnerable group: For every SEK 1,000 increase in the local norm, disposable income increased by SEK 430. This improvement in the financial situation of SA recipients likely made the group better off, also in other dimensions, if it improved financial decision-making and hence potentially increases employment in the long-run (Mani et al. 2013; Schilbach et al. 2016; Shah et al. 2012). My findings suggest that there is a trade-off between creating work disincentives and improving the economic conditions for individuals in need. The results for the at risk group and previous SA recipients also suggest that the decrease in employment is primarily driven by an increased inflow from employment to SA recipiency for individuals at the margin of receiving SA, as opposed to a decrease in the outflow from unemployment for previous SA recipients.

When studying the effects by degree of exposure to the 1990's economic crisis, I find that the negative effects on employment and earnings among individuals at risk of receiving SA were driven by individuals in municipalities that were heavily exposed to the economic crisis. For individuals living in less exposed municipalities, I find no evidence of a disincentive effect. For previous SA recipients, the analysis reveals a similar pattern, i.e. while I find no effect on labor market outcomes for previous SA recipients living in less exposed municipalities, there is a negative effect of increased SA generosity on employment and earnings for previous SA recipients living in highly exposed municipalities. Hence, negative effects of increased SA generosity on labor market outcomes seem to be more prominent when employment is hard to find than when the labor market is strong. This also points to the importance of understanding the role of labor demand when analyzing the incentive effects of changing SA generosity.

Compared to previous studies of the effects on SA generosity on the labor supply of young single childless individuals and married couples with children, the estimated overall effects in the at risk group are large (Bargain and Doorley 2011, 2017; Bargain and Jonassen 2022; Hoynes 1996; Lemieux and Milligan 2008) and more alone the lines of the results for single mothers (Meyer and Rosenbaum 2001). In addition to studying a different sample, differences in the macroeconomic conditions is likely an important explanatory factor for these differences, as I find that the negative effects on employment and earnings are driven by individuals living in municipalities that were highly exposed to the economic crisis.

Since the increase in SA generosity caused by the 1993 court ruling, SA cash benefits in Sweden have become less generous. Adjusted for inflation, the national norm in 2019 was at the same level as the local norms in the least generous municipalities before the 1993 court ruling. If the initial level of SA matters for the behavioral effects of increased SA generosity, we may expect similar results today, all things equal.[35] Today, SA generosity in Sweden is below the OECD median (OECD 2017), implying that most OECD countries today have more generous SA cash benefits than Sweden in 1992.[36]

Finally, the 1993 court ruling took place during a severe economic recession. This is indeed a particular setting. Yet, discussions about strengthening the social safety net often surface in times of economic downturns, like the Covid-19 crisis (e.g. Bitler et al. (2020)). As the results of my study imply, understanding how the effects of SA generosity are affected by macroeconomic conditions are important for predicting how individuals will respond to increased SA generosity under such circumstances.

To conclude, this study has shown that increased SA levels improve the economic conditions of SA recipients, at the same time as it has negative effects on labor market outcomes of individuals at risk of receiving SA. Policymakers must weigh these factors against each other. It is however also central that they consider the effects of SA generosity, or lack thereof, on children and health. These are important topics for future research.

[35]However, compared to the situation in many countries today, activation requirements were not yet common. If participation in active labor market programs help improve recipients skills, or if being forced to participate in them makes SA recipiency less attractive, then the disincentive effect of a corresponding increase in SA is expected to be smaller today, all things equal. For example, Markussen and Røed (2016) study the effects of a program in Norway where SA recipients receive more generous, non-means tested, benefits conditional on participating in a combination of activities. They find that this had a large positive effect on employment.

[36]The figures compare SA generosity as a share of the median disposable income in a given country. In 2014 the national norm covered 18 percent, compared with the OECD median of 22 percent. In 1992, the least generous municipalities' local norms covered 36 percent, implying that, relative to the incomes in the population, SA generosity has become less generous since.

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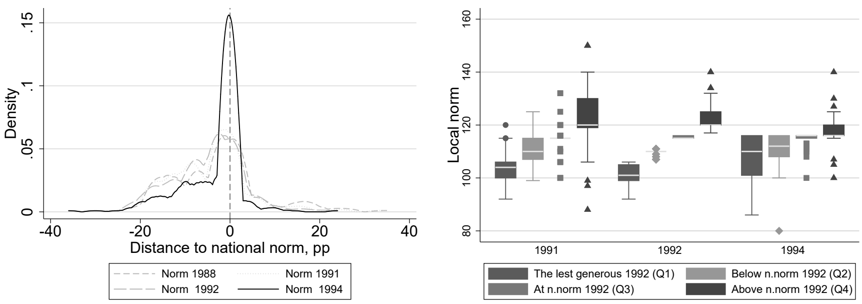
Appendix A: Additional description and results

Figure A.1. Timeline, policy and court ruling induced changes in local and national norms

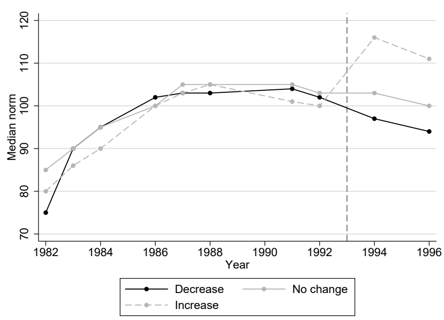
Jan '85	**Apr '93**	Jul '94	Sep '96	Jan '98
National norm (NN)	**Court: NN min. lev.**	Court: ok to shift posts	NN re-duced	Law: NN min. lev. & reduced

Figure A.2. Local norms, additional figures



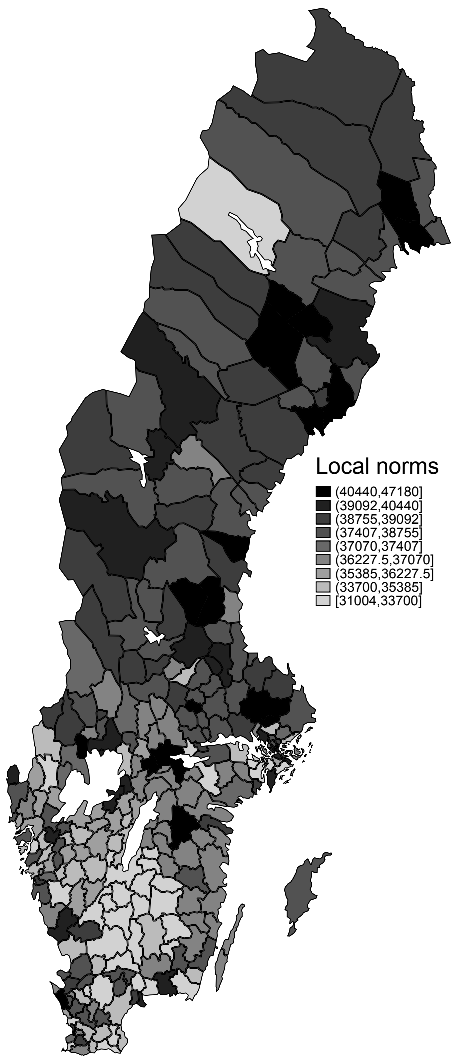
(a) Distribution of local norms 1988–1994 *(b)* Boxplot by 1992 level

Figure A.3. The median norm in the least generous municipalities, by direction of change 1992–1994.



(a) Note: The norm is decreased in 9 municipalities, increased in 49 and kept constant in remaining municipalities.

Figure A.4. Geographical distribution of local norms in 1992



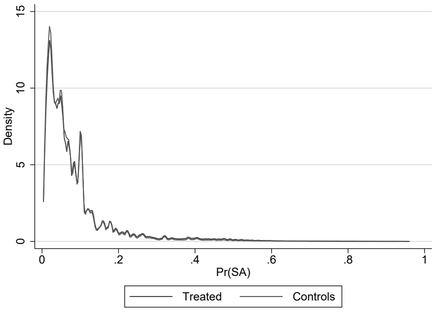
Note: Each area represents a municipality. The darker the gray color is, the higher the local norm in 1992.

Table A.1. *Relationship between SA generosity and local employment rate, years 1991, 1992, 1994 – OLS*

	(1)	(2)	(3)	(4)	(5)	(6)
Local norm	-0.0001	-0.0000				
	(0.0002)	(0.0003)				
Below N.N.			0.0013	0.0003		
			(0.0014)	(0.0021)		
Least generous					-0.0008	-0.0002
					(0.0015)	(0.0019)
Municipalities	All	Sample	All	Sample	All	Sample

Note: Local norms are measured in 1000 SEK. N.N. refers to the national norm and sample to the least generous municipalities and municipalities complying with the national norm in 1992. Year and municipality fixed effects are included in all specifications. Standard errors clustered by municipality. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Figure A.5. The propensity to receive SA in sample 1992, by treatment status



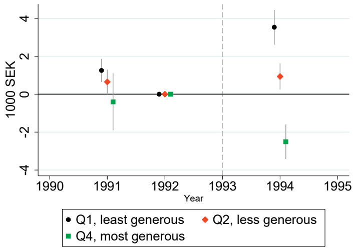
Note: Treated refers to individuals living in the least generous municipalities, and controls to those living in municipalities already complying with the norm

Table A.2. *Propensity to receive SA estimation, 1990–1992*

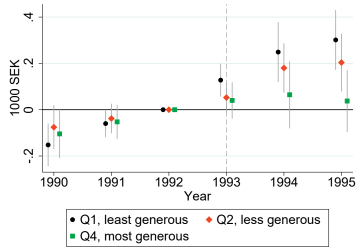
	(1) SA recipiency
Female	-0.371***
	(0.0293)
Age	0.0250***
	(0.00116)
Age^2	-0.000623***
	(0.0000146)
Age above 25	0.166***
	(0.00527)
Immigrant	4.234***
	(0.0338)
Born in Non-western country	1.042***
	(0.00386)
Unknown origin	0.563***
	(0.0663)
Children	0.271***
	(0.000965)
Child age 0–6	0.114***
	(0.00339)
Not married, no kids	1.336***
	(0.00418)
Single parent	1.699***
	(0.00462)
Couple with kids	0.182***
	(0.00445)
Primary school	1.356***
	(0.00379)
High school	0.859***
	(0.00365)
Female*age	0.0139***
	(0.00152)
Female*age^2	-0.0000496***
	(0.0000190)
Female*Child aged 0–6	0.389***
	(0.00386)
Female*Married	-0.256***
	(0.00304)
Female*non-western CoB	-0.0733***
	(0.00560)
Female*age below 25	0.219***
	(0.00692)
Female*Primary school	-0.183***
	(0.00333)
Female*University	-0.142***
	(0.00449)
Immigrant*age	-0.115***
	(0.00171)
Immigrant*age^2	0.00129***
	(0.0000212)
Immigrant*Female	-0.174***
	(0.00430)
Immigrant*child age 0–6	-0.0823***
	(0.00415)
Immigrant*married	0.816***
	(0.00359)
Immigrant*young	-0.220***
	(0.00777)
Immigrant*primary school	-1.422***
	(0.00462)
immigrant*high school	-1.098***
	(0.00441)
Constant	-4.764***
	(0.0230)
N	27722373
R^2	0.150

Note: Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

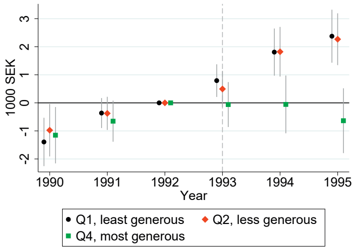
Figure A.6. Aggregated effects of the court ruling, using municipality level data including control for employment



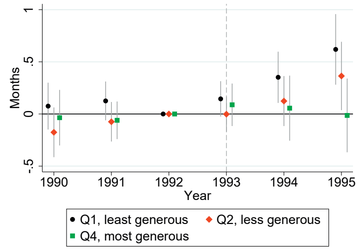
(a) Local norm, controls



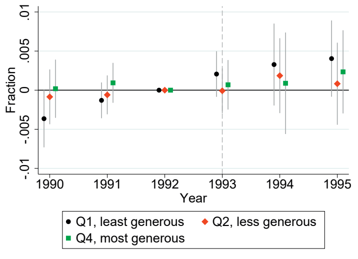
(b) SA/inhabitant, controls



(c) SA/recipient, controls



(d) SA months/household, controls



(e) Fraction with SA, controls

Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Year, municipality fixed effects and controls for employment rate (in t-2) are included in all specifications. Amounts are in 1000 SEK. I have aggregated data on SA amounts and fraction with SA from the administrative data sources presented in Section 1.6, while SA months per household and local employment have been aggregated by Statistics Sweden.

Table A.3. *First stage results – effect of the court ruling on the local norm.*

	(1)	(2)	(3)	(4)
	By crisis exposure		All	
	High ex.	Low ex.		
Panel A:At risk				
BelowNorm*Post	3.163***	3.415***	3.388***	3.388***
	(0.997)	(0.799)	(0.738)	(0.738)
N	586,541	437,569	1,003,683	1,003,683
F stat	10	18	21	21
Panel B: Previous SA recipients				
BelowNorm*Post	3.568***	3.611***	3.698***	3.698***
	(0.955)	(0.805)	(0.732)	(0.732)
N	211,970	146,706	352,545	352,545
F stat	14	20	26	26
Individual char.	Yes	Yes	No	Yes

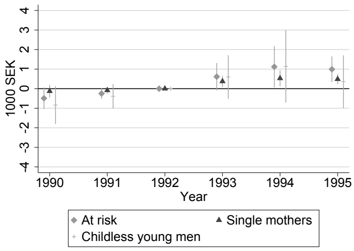
Note: High ex. refers to above median exposure to the economic crisis, and low ex. to below median. Standard errors clustered by municipality. Individual characteristics, year and municipality fixed effects are included in all specifications. Individual characteristics are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Table A.4. *2SLS results using (A) single mothers and (B) childless men*

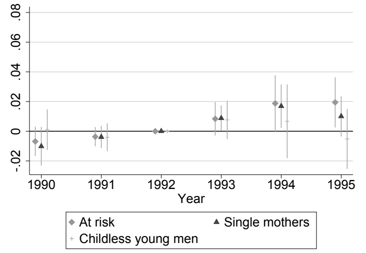
	(1)	(2)	(3)	(4)	(5)
	SA amount	SA rec.	Earnings	Employm.	Disp. inc.
Panel A: At risk (baseline)					
Local Norm	0.3321***	0.0055**	-0.3621*	-0.0028**	0.0564
	(0.1150)	(0.0022)	(0.2054)	(0.0012)	(0.1040)
N	1,003,683	1,003,683	1,024,760	1,024,760	1,024,760
Panel B: Single mothers					
Local Norm	0.1537***	0.0049**	-0.1640	-0.0022**	0.1673
	(0.0465)	(0.0020)	(0.2119)	(0.0010)	(0.1408)
N	306,917	306,917	314,969	314,969	314,969
Panel C: Men w. low education					
Local Norm	0.3234	0.0017	0.1894	-0.0027	0.4965**
	(0.2307)	(0.0034)	(0.5217)	(0.0021)	(0.2295)
N	86,290	86,290	87,592	87,592	87,592

Note: Men with low education is defined as men aged at most 35 with compulsory school education and no children. Standard errors clustered by municipality. Individual characteristics, year and municipality fixed effects are included in all specifications. Individual characteristics are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Figure A.7. Effects for groups studied in previous literature

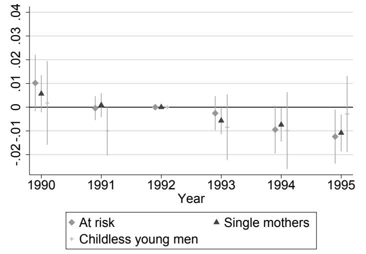


(a) SA amount



(b) SA recipiency

(c) Earnings



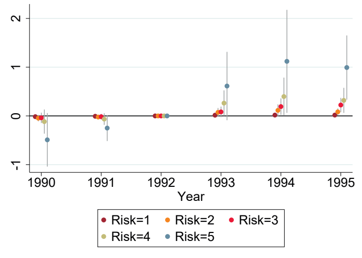
(d) Employment

(e) Disposable income

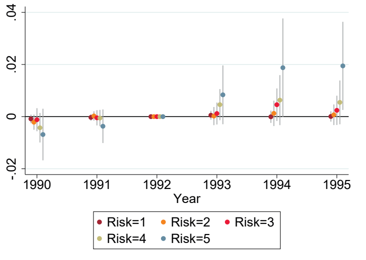
Notes: The subsample men with low education is defined as men aged 35 and below with compulsory school education and no children. Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and person controls: sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent.

Appendix B: Robustness checks

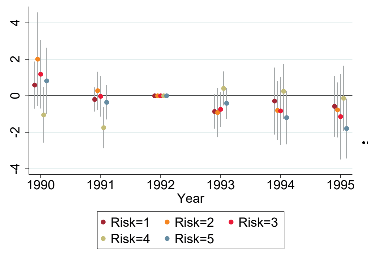
Figure B.1. Robustness check: the effect of the 1993 court ruling by risk group



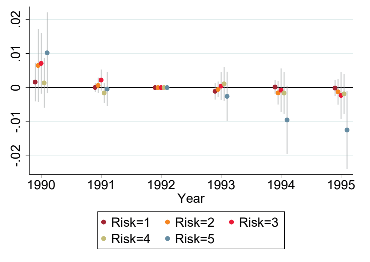
(a) SA amount



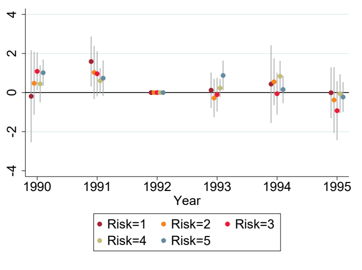
(b) SA recipiency



(c) Earnings



(d) Employment



(e) Disposable income

Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and person controls: sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent.

Table B.1. *Robustness check: 2SLS results using the most likely (A) decile and (B) quintile (as in main sample) to receive SA*

	SA amount	SA rec.	Earnings	Employm.	Disp. inc.
Panel A: Risk decile					
Local Norm	0.4362***	0.0068**	-0.6482**	-0.0027	0.1986
	(0.1388)	(0.0027)	(0.2550)	(0.0016)	(0.1358)
N	490,784	490,784	500,775	500,775	500,775
Panel B: Risk quintile					
Local Norm	0.3321***	0.0055**	-0.3621*	-0.0028**	0.0564
	(0.1150)	(0.0022)	(0.2054)	(0.0012)	(0.1040)
N	1,003,683	1,003,683	1,024,760	1,024,760	1,024,760

Note: Standard errors clustered by municipality. Person controls, year and municipality fixed effects are included in all specifications. Person controls are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

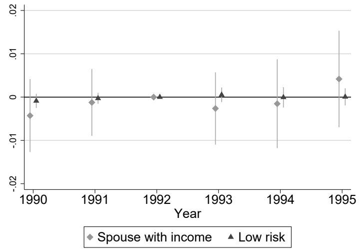
Table B.2. *Robustness check: 2SLS results using (A) at risk group with above median earning spouses and (B) individuals predicted least at risk*

	(1) SA amount	(2) SA rec.	(3) Earnings	(4) Employm.	(5) Disp. inc.
Panel A: Working spouse					
Local Norm	0.0057	0.0000	0.3411	0.0014	-0.2018
	(0.0142)	(0.0016)	(0.2291)	(0.0016)	(0.1647)
N	122,920	122,920	126,051	126,051	126,051
Panel B: Low risk					
Local Norm	0.0053	0.0001	-0.1262	0.0000	0.1319
	(0.0052)	(0.0003)	(0.3079)	(0.0003)	(0.2862)
N	1,024,582	1,024,582	1,054,701	1,054,701	1,054,701

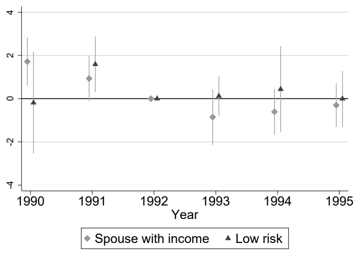
Note: Standard errors clustered by municipality. Person controls, year and municipality fixed effects are included in all specifications. Person controls are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Figure B.2. Robustness check: effects over time for (i) at risk group with spouses with high incomes and (ii) group least at risk.

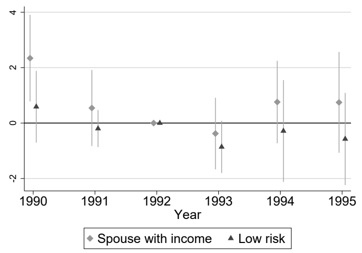
(a) SA amount



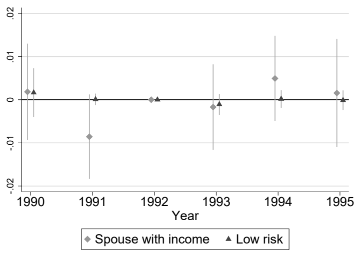
(b) SA recepiency



(c) Disposable income



(d) Earnings



(e) Employment

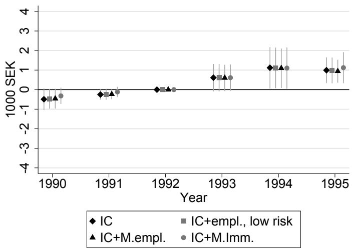
Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and person controls: sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent.

Table B.3. *Robustness check: 2SLS results for baseline with (1) individual controls, adding municipality controls for (2) recent immigrants, (3) local employment rate in t-2 and (4) employment rate in the low risk group.*

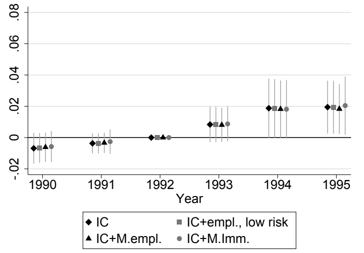
	(1)	(2)	(3)	(4)
Panel A: First stage, local norm				
BelowNorm*Post	3.3770***	3.2740***	3.4816***	3.3980***
	(0.7366)	(0.7423)	(0.7260)	(0.7420)
N	1,024,760	1,024,760	1,023,063	1,024,760
F stat	21	19	23	21
Panel B: SA recipiency				
Local Norm	0.0055**	0.0052**	0.0054**	0.0051**
	(0.0022)	(0.0022)	(0.0022)	(0.0021)
N	1,003,683	1,003,683	1,003,683	1,001,986
Panel C: SA amount				
Local Norm	0.3321***	0.3214***	0.3254***	0.3161***
	(0.1150)	(0.1177)	(0.1130)	(0.1085)
N	1,003,683	1,003,683	1,003,683	1,001,986
Panel D: Employment				
Local Norm	-0.0028**	-0.0023*	-0.0026**	-0.0027**
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
N	1,024,760	1,024,760	1,024,760	1,023,063
Panel E: Earnings				
Local Norm	-0.3621*	-0.3118	-0.2985	-0.2708
	(0.2054)	(0.2185)	(0.1939)	(0.2002)
N	1,024,760	1,024,760	1,024,760	1,023,063
Panel F: Disposable income				
Local Norm	0.0564	0.0908	0.0609	0.1217
	(0.1040)	(0.1017)	(0.1047)	(0.0985)
N	1,024,760	1,024,760	1,024,760	1,023,063
Immigration controls		Yes		
Employment rate, t–2			Yes	
Employment rate, low risk				Yes

Note: Standard errors clustered by municipality. Person controls, year and municipality fixed effects are included in all specifications. Person controls are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. Immigration controls control for the proportion immigrants who arrived the past 0–2 and 3–5 years. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

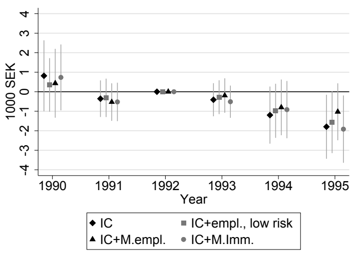
Figure B.3. Robustness check: adding municipality controls for employment rate and share recent immigrants



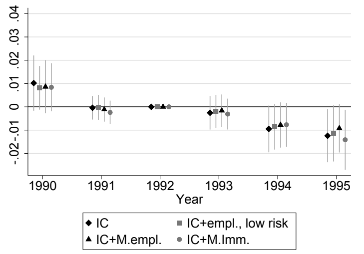
(a) SA amount



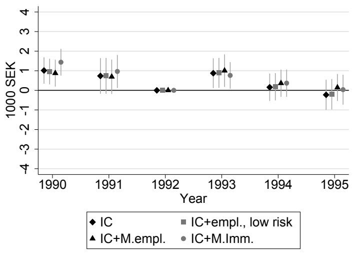
(b) SA recepiency



(c) Earnings



(d) Employment



(e) Disposable income

Notes: Standard errors clustered by municipality, 95 % confidence intervals are displayed. Regressions include year and municipality fixed effects and individual characteristics (IC): sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent. *Empl., low risk* is a control for the employment rate of the least likely quintile to receive SA in the municipality, and *M.empl.* the employment rate in the municipality in t–2 (aggregated data from Statistics Sweden). *Imm.* refers to controls for the proportion immigrants who arrived the past 0–2 and 3–5 years.

Table B.4. *Placebo test: Reduced form (A) and First stage (B) results, 1992 as post- and 1991 as pre-treatment year*

	(1) SA amount	(2) SA rec.	(3) Earnings	(4) Employm.	(5) Disp. inc.
Panel A: Reduced form					
BelowNorm*Post	0.2524*	0.0039	0.3715	0.0005	-0.7332
	(0.1334)	(0.0032)	(0.4733)	(0.0025)	(0.4549)
N	997,171	997,171	1,020,245	1,020,245	1,020,245
Panel B: First stage					
BelowNorm*Post	-1.561***				
	(0.511)				
N	1,020,699				
F stat	9				

Note: Standard errors clustered by municipality. Person controls, year and municipality fixed effects are included in all specifications. Person controls are sex, age, age^2, number of children, an indicator for having a child below age of 6, education (3 categories), indicator for foreign born, born in non-western country, immigrated up to 5 years ago, indicator for being a single parent * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

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

Co-authored with Eva Mörk and Ulrika Vikman

Acknowledgments: We are grateful for comments and suggestions from seminar participants at IFAU, Ratio, The Department of Economics and the Urban Lab at Uppsala University, as well from conference participants at the virtual EALE 2021 conference and the CESifo Area Conference on Labor Economics 2022. The comments from Martin Lundin, Simen Markussen, Lukas Mergele, Johan Vikström, Michael Rosholm and Olof Åslund are also highly appreciated. We also thank Ellen Parsland for sharing insights from interviews conducted with program participants and the Labor market unit in Stockholm for their helpful provision of information about the program. Ottosson and Vikman thank FORTE (Reg. No. 2016-07123) for financial support.

2.1 Introduction

How to best help individuals with a weak labor market attachment find employment is high up on the agenda for policy makers all over the world. In this paper, we investigate whether temporary public employment in the form of a public sector employment program (PSEP) is a way forward. Given that PSEPs provide participants with networks and labor market experiences, this program can be expected to be well suited for marginalized groups with otherwise poor labor market prospects.[1] However, in contexts where different levels of the government are responsible for financing unemployed with and without unemployment insurance (UI) benefits, there are incentives for caseworkers to use PSEPs as a means of providing participants with eligibility to UI benefits and thereby shift costs to the other level, rather than targeting individuals most likely to benefit from the program.[2] Although there is anecdotal evidence that such cost-shifting does occur, empirical evidence is scarce.

In this paper, we ask whether having a temporary municipal employment serves as a stepping stone to future employment or whether it mostly works as a means for the welfare office to transfer individuals from SA to UI benefits. Our focus is Sweden, where municipalities finance and activate unemployed SA recipients, whereas UI benefits are paid out by central UI funds. More specifically, we evaluate a PSEP in the city of Stockholm, targeted at unemployed SA recipients and other individuals at risk of becoming long-term unemployed. Our paper thus adds to our limited knowledge of what works for this particular group (see, for instance, Bolvig et al. (2003); Cockx and Ridder (2001); Heinesen et al. (2013); Markussen and Røed (2016); Thomsen and Walter (2010)), as well as broadens our understanding of the role played by institutional setups in terms of determining how individuals are moved between different benefit schemes (see, for instance, Bonoli and Trein (2016); Schmidt and Sevak (2004)). A specific feature of the program that we study is that we can distinguish between participation at regular and non-regular workplaces. The findings of this paper can therefore improve our understanding in how important networks and experiences from a regular workplace are as opposed to just having any previous labor market experience.

Earlier evidence on PSEPs for SA recipients is mixed. Whereas Danish evidence concerning subsidized public employment programs shows positive effects for SA recipients overall and non-Western immigrants in particular (Bolvig et al. 2003; Heinesen et al. 2013), results from Germany and Belgium

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

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

are less promising: no effects are found for Social employments in Belgium (Cockx and Ridder 2001), nor for Temporary extra jobs in Germany (Thomsen and Walter 2010). In general, very few programs have turned out to be successful for this particular group. An exception is the Norwegian qualification program that combines full-time (voluntary) activation with a generous non-means-tested benefit (Markussen and Røed 2016).

Previous evidence regarding to what extent PSEPs are used to provide participants with eligibility to UI benefits is very scarce.[3] What we do know is that decentralized job centers tend to prioritize local objectives. For example, Mergele and Weber (2020) find that decentralized job centers in Germany adjust labor market policies towards programs that are financed by the federal government and potentially generate local public goods, rather than favoring the reemployment prospects of the program participants. A similar conclusion is reached by Lundin and Skedinger (2006) who, by studying a Swedish pilot program, show that decentralization increased the targeting of individuals with a relatively high level of dependence on SA, which is what we should expect if local governments use their increased influence to improve municipal budgets at the expense of the central government.[4]

The temporary employment program we study is called Stockholm jobs and consists of a 6–12 months long employment in the municipal sector. We evaluate three different types of Stockholm jobs. In two (Youth employments and Other municipal employments), participants work at a regular workplace performing quality-enhancing activities that would otherwise not have been undertaken. In the third (Stockholm hosts), participants are employed at a workplace created especially for this purpose where they are engaged in outdoor cleaning. The aim of the temporary employment is to strengthen the participants' position in the labor market and thereby increase their chances of finding employment or moving on to further education. Through the employment, individuals become eligible for UI benefits, financed by central UI funds, which typically provide individuals with a higher disposable income compared to SA. Hence, in the longer run, having a Stockholm job is financially beneficial both for the individual and the municipality, even if it does not lead to regular employment. Caseworkers thus face several, potentially conflicting, objectives, similar to what is discussed in, for instance, Schmieder and Trenkle (2020).

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

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

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

In order to address the fact that treatment assignment is not random and that participants can enter the program at any time after enrollment at the job center, we apply the dynamic inverse probability weighting (IPW) approach suggested by Van den Berg and Vikström (2022). Earlier studies relying on matching strategies mostly follow Sianesi (2004, 2008) and apply dynamic propensity score matching, thus estimating the effect of being assigned to a program at a specific time as opposed to potentially being assigned at a later time.[5] In the dynamic IPW, the group of potential controls is made up of individuals who never take part in the program. The estimand is thus the effect of taking part in the program or not doing so, which is arguable the relevant question for policy makers. The method accounts for the fact that individuals with short durations at the job center will be over-represented in the potential control group of never-treated by giving greater weights to never-treated individuals who have been registered at the job center for a long time.

We find that the employment prospect for individuals placed at regular workplaces are improved thanks to the program. The effect is especially pronounced and lasting for Youth employments, where former participants are around 10 percentage points more likely to be employed up to two years after the end of their temporary employment. However, the type of workplace is important; for participants in Stockholm hosts, we find negative employment effects up to two years after the program. Those participants that do find an employment after program are to a large extent employed at the same workplace or in the same sector as they were during their Stockholm job, indicating that it is crucial that the program provides participants with experiences and networks that are relevant in sectors where there is a demand for labor. We further find that having any type of Stockholm job reduces the likelihood of receiving SA with more than 50 percent during the two years following the employment. To some extent, this is counteracted by an increase in UI, in particular for Stockholm hosts, for which more than 60 percent receive UI after the program ended. In addition, we find that individuals' health outcomes improve once they start their temporary employment and that these positive effects to some extent pertain once the program ends.

[5] Heinesen et al. (2013) instead use the timing-of-events method suggested by Abbring and Berg (2003).

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

The outline of the paper is as follows. In the next section, we present the institutional setting and the program under study. Section 2.3 describes the data, how we select our sample, and gives some descriptive statistics. In Section 2.4, we present the empirical strategy, which we apply to deal with dynamic selection into the program, and how we implement the strategy. Section 2.5 presents the results as well as sensitivity analyses whereas Section 2.6 discusses potential explanations to the results found. Finally, Section 2.7 concludes by discussing our findings.

2.2 Institutional setting

Like many other welfare states, Sweden combines relatively generous (earnings-related) UI benefits with mandatory active labor market programs (ALMPs).[6] The formal responsibility for providing ALMPs is placed on the Swedish Public Employment Service (PES), a central governmental agency. Unemployed individuals who do not qualify for UI benefits (or with very low levels of UI benefits or whose UI benefits have been exhausted) can apply for social assistance (SA) at the local welfare office. To be eligible, all other means, including savings and valuable assets, must be exhausted. The means-testing is performed at the household level, implying that an individual with a spouse with high earnings is not entitled to SA. The (centrally) stipulated benefit level depends on the number and age of dependent children as well as the number of adults in the household.[7]

Unemployed SA recipients are required to actively look for work, be registered at the PES and take part in ALMPs offered by the PES. If the PES cannot

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

[7]The stipulated benefit level in 2010, excluding housing costs, was SEK 3,680 (EUR 360) per month for a single person without children and SEK 10,770 (EUR 1,060) for a couple with two children aged 5 and 13. In 2019, the corresponding numbers were SEK 4,080 and SEK 12,960. The municipalities are allowed to deviate both upwards and downwards from the stipulated benefit level if they can motivate these deviations.

offer a suitable program, municipalities have the right to condition benefits on taking part in activation programs organized by the municipalities. This right is used by most municipalities (Forslund et al. (2019)).

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

The program that we analyze in this paper is called Stockholm jobs and was introduced in 2010 as one of the activation programs provided by the job centers in the city of Stockholm. The main component of the program is temporary (often subsidized) employment in the municipal sector lasting 6–12 months. Wages are paid out by the municipality, implying the workplace where the individual is employed faces no salary costs. The purpose of the program is to, by providing labor market experience and networks, strengthen the participants' position in the labor market and thereby increase their chances to find employment or to go on to further education.

We focus on three types of Stockholm jobs that differ with respect to target group, type of workplace and employment duration. Table 2.1 summarizes the main characteristics of these three program types. Youth employments target individuals aged 16–29 in need of extra support to find and maintain employment. Participants are employed at regular workplaces such as childcare centers, schools, nursing homes or the municipal administration. The employment lasts for six months, but may be prolonged for an additional six months if it is deemed beneficial for the individual. Other municipal employments, introduced in 2012, are in many aspects similar to the Youth employments, except for the target group (SA recipients in general) and the length of the program (typically 12 months). Stockholm hosts differ from the other two in that the temporary employments do not take place at regular workplaces. Instead, participants work outdoors, together in teams with other participants and supervisors. Their work tasks include picking litter, clearing snow and assisting tourists with directions. The employment lasts for 6 (2010–2011)/12 months (2012–2016). The program is targeted at individuals who are 25 years or older with children to care for, or individuals who have been registered at the job center for at least 6 months, or are considered at great risk of remaining at the job center for a long time.

Before being directed to the workplace, most participants take part in an introduction consisting of general information about UI benefits, unions, how to behave at a workplace and the program itself. The introduction can also contain a 4–8 weeks long internship aiming at ensuring a good match between the

Table 2.1. *Description of different types of Stockholm jobs*

	Youth employments	Other municipal employments	Stockholm hosts
Target group	16–29 years w. poor labor market prospects	SA recipients	SA recipients $\geq$25 years w. children or at risk of long-term unemployment
Workplace	Regular workplace in municipal sector	Regular workplace in municipal sector	Outdoor cleaning
Employment length	6+6 months	12 months	6 months (2010–11) 12 months (2012–16)

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

participants and the workplace.[8] During this introduction, participants keep the benefits they received prior to the program (typically SA). Once at the workplace, the participants are provided with a supervisor and perform quality-enhancing activities outside the scope of the regular tasks. This may include playing with the children in a childcare facility (but not engaging in pedagogical work), taking residents for a walk in homes for the elderly, or helping elderly individuals with simple IT-related questions in a library. They may also perform regular tasks under supervision.[9] When employed, participants above the age of 19 receive a salary of at least SEK 19,000 (approximately EUR 1,800) per month (SEK 18,000 until 2015). During the temporary employment, caseworkers at the job center help participants plan what to do once the Stockholm job ends. This may entail going to the job center one afternoon a week to search for jobs or enrolling in education. Since 2016, all participants are offered additional assistance for three months after the end of their employment.

As opposed to the other activation programs at the job center, which are mandatory for unemployed SA recipients if referred to by the caseworkers,

[8]For the period we study, introductory internships have mainly been used paired with Youth employments, where the share that had an internship before entering their workplace is 89 percent. For the other two programs, the corresponding shares are 1 and 13 percent, and for these two programs, internships have mainly been used for those starting a Stockholm job after 2014.
[9]Since 2015, participants are allowed to study half-time simultaneously with their employment. Initially, this opportunity only applied to participants in some types of Stockholm jobs and for some types of educational choices.

taking up a Stockholm job is voluntary.[10] As it is uncommon that an individual declines an offer to take up a Stockholm job, selection into the program is mainly driven by the priorities made by the caseworkers. These vary somewhat across job centers and type of Stockholm job. As a general rule, caseworkers prioritize individuals with dependent children, clients that are judged to be in need of additional assistance before they can enter the regular labor market and long-term recipients of SA. For Youth employments, motivation plays an important role, and for Stockholm hosts participants must, e.g., be able to walk long distances.

Taking up a Stockholm job is financially beneficial for participants. The salary received is higher than the stipulated SA level and is not means-tested at the household level. In addition, having a job with a salary, even if it is subsidized, may offer a sense of pride and purpose for the participant. This view was for example expressed by several participants when we visited their workplace. If an individual does not accept an offered Stockholm job, he/she is likely to be placed in a mandatory activation program.

When the Stockholm job ends, participants returning to unemployment fulfill the work requirement for receiving UI benefits and are entitled earnings-related UI benefits if they have been members of a UI-fund for at least one year. In addition, they no longer need to apply for SA and undergo the means-testing and the scrutiny this implies, nor are they required to visit the job center, although households with many children might still need to top up with SA. Instead, the PES will be responsible for directing them to ALMPs. Participants who find employment will continue to receive a salary.

Most Stockholm jobs are financed via a subsidy from the government.[11] Hence, the municipality will not bear the full wage cost. Given that participants are expected to perform quality-enhancing activities at the workplace, the municipality can reap the benefits of better municipal services. In the long run, it is clearly financially beneficial for the municipality to place individuals in Stockholm jobs as they either become employed or eligible for UI benefits. In both cases, costs for SA will go down and the municipality no longer needs to finance and activate the former recipients at the job center and welfare office.

Caseworkers at the job center face a potential conflict of interest. On the one hand, they might want to prioritize individuals who are the most likely to benefit from the program in terms of future employment prospects. On the other hand, they may be tempted to instead prioritize clients who are hard to

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

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

place with the intention of getting them off their desk. In addition, as mentioned above, this is likely to also benefit the client. However, the intention of the job center to only send motivated individuals to the workplaces can be expected to counteract these incentives.

2.3 Data and sample selection

We combine administrative data from several different sources: the city of Stockholm, Statistics Sweden, the Public Employment Service (PES), the Unemployment Insurance Board (IAF) and the National Board of Health and Welfare (NBHW). The data from the city of Stockholm covers the period from January 2010 to June 2019 and includes information about the start and end date of each spell of enrollment at the job center, as well as the name, type, start and, in most cases, the end date of each activity an individual has participated in (but not the identity of the caseworker). In addition, the data includes information regarding whether the individual him-/herself took the initiative to enroll at the job center. The data from Statistics Sweden covers the years 2008–2019 and includes yearly socio-demographic background characteristics such as age, gender, number and age of children and marital status, region of origin, year of immigration as well as information about the highest attained education level. We also have monthly information about earnings, workplace and sector. The PES data includes information about enrollments at PES and program participation for the period 1991–2019. The data from IAF includes all UI payments between 2008 and 2019. From NBHW, we have access to (monthly) information about medical prescriptions, hospitalizations and SA payments for the period 2008–2019.

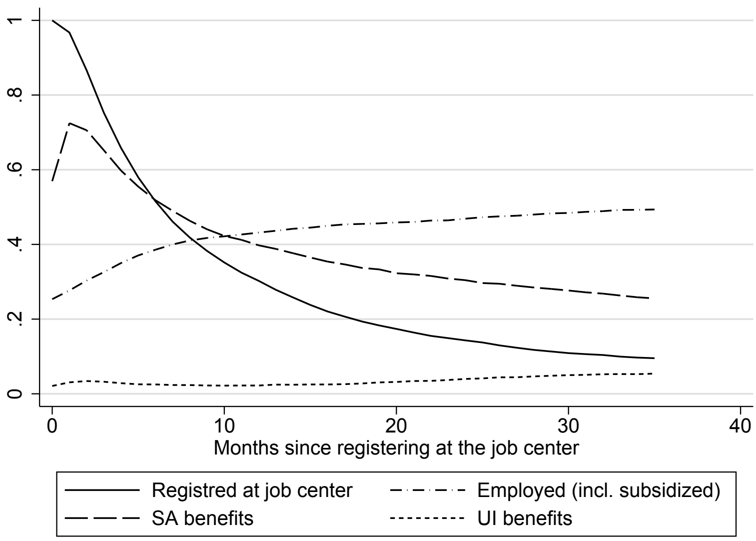
We define our study population as all individuals who enroll at a job center in Stockholm at some point between January 1, 2010, and December 31, 2015, and aged 18–61 at the time of enrollment.[12] This gives us 17,647 individuals who enter a new enrollment at the job center in Stockholm 21,996 times to be included in our analysis. Since the different types of Stockholm jobs have different target groups, we also restrict our estimation samples accordingly. This implies that when estimating the effects for Youth employments, the sample is restricted to those younger than 30. When it comes to Other municipal employments, the sample is restricted to those with a start date in May 2012 or later (since this is the first month that this type of Stockholm job was used). Finally, for Stockholm hosts, we exclude individuals younger than 25.

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

We define treatment as the first participation in a Stockholm job within two years after registration at the job center or in December 2016 at the latest.[13] We define the start date of the Stockholm job as the day when the individual starts her/his employment, that is after the introduction and any internship.

We analyze how employment, SA and UI benefit receipt status evolve month by month up to 36 months after program start, as well as the total number of months in, and amounts received from, employment, with SA and UI benefits in the short (1 year after the end of the program) and medium (year 2 after the end of the program) run. We define an individual as employed in month *m* if he/she has positive earnings during that month. We are thus able to examine whether individuals return to SA after their UI benefits expire after 14 months. In addition, we analyze three health outcomes (medical prescriptions for pain relief, psychiatric drugs and hospitalization for any cause) in order to capture effects on participants' well-being.

Figure 2.1. Share at the job center, in employment (incl. subsidized), with SA and UI benefits since time of enrollment at the job center



2.3.1 Descriptives

Figure 2.1 shows how enrollment at the job center (we consider an individual as having exited the job center when he/she starts a Stockholm job), the share

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

Table 2.2. *Description of job center clients and participants in Stockholm jobs at enrollment at the job center*

	All	Youth employments	Other municipal employments	Stockholm hosts
Age	32.96	20.99	41.50	41.16
Female	0.47	0.43	0.61	0.27
Married	0.26	0.16	0.31	0.42
Child in household	0.38	0.38	0.51	0.30
Some college education	0.18	0.05	0.23	0.11
No college education	0.77	0.86	0.75	0.82
Education unknown	0.05	0.08	0.02	0.07
Foreign born	0.62	0.51	0.79	0.78
0-2 yrs since immigration	0.14	0.15	0.03	0.21
3-5 yrs since immigration	0.13	0.13	0.15	0.16
Born in Nordics or W. Europe	0.05	0.02	0.05	0.03
Born in E. Europe or C. Asia	0.03	0.01	0.02	0.02
Born in W. Asia or N. Africa	0.19	0.14	0.22	0.09
Born in Africa , excl. N. Africa	0.21	0.24	0.36	0.54
Other country of birth	0.15	0.09	0.14	0.11
Own initiative to enroll at the JC	0.05	0.18	0.13	0.01
Ith quarter at PES at JC reg.	3.63	1.86	13.48	7.76
Earnings t-24, 1000 SEK	50.71	25.52	26.69	35.53
SA, nr of months t-24	6.15	5.18	15.61	8.59
Psychotropic drug prescribed t-12	0.20	0.13	0.16	0.12
Pain rel. drug prescribed t-12	0.16	0.09	0.26	0.14
Hospital visit t-12	0.10	0.08	0.08	0.09
Observations	21,996	970	396	196

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

of employed individuals, and the share receiving positive SA and UI benefits evolve since time of enrollment at the job center. As is clear from the figure, the share enrolled at the job center goes down relatively fast over time, and at the end of our follow-up period, only 10 percent are registered at the job center (they may have left and re-entered). Also, the share receiving any SA goes down over time, expect for a small increase in the first month, but not as much as the share enrolled; reaching 25 percent at the end of our period.[14] When first enrolling at the job center, 25 percent are employed (subsidized or non-subsidized). However, their earnings are generally low (see Figure A.1 in the Appendix), implying that they may need SA to top up. The share of employed individuals increases over time, reaching 50 percent after three years. The share receiving UI benefits is very low throughout the follow-up period, never reaching more than 5 percent.

Table 2.2 presents a description of our study population. Column 1 describes the average client at the job center, while columns 2–4 divide the participants into the three different types of Stockholm jobs we study. Focusing first on participants in Youth employments, this group consists of equally many males and females, and the participants are also equally likely to be born in or outside Sweden. Compared to the average client at the job center, they are younger, have shorter spells of unemployment and SA, as expected, as well as better health outcomes. Participants in Other municipal employments and Stockholm hosts are instead older than the average client, and to a larger degree born outside Sweden; the share foreign born is almost 80 percent. The participants in Stockholm hosts have been in Sweden for a shorter time and have somewhat lower education than participant in Other municipal employments. More women than men participate in Other municipal employments (60 percent females), whereas Stockholm hosts are dominated by male participants (70 percent males). Compared to the average client at the job center, participants in these two types of Stockholm jobs have longer unemployment and SA-spells. Participants in Other municipal employments stand out with respect to the participants' previous labor market history being considerably worse and having a longer history of receiving SA and also exhibit worse health, with more drugs prescribed the previous year. On the other hand, almost 13 percent took the initiative to enroll at the job center themselves, rather than being directed by the caseworker at the welfare office. The corresponding share for the Stockholm hosts is only 1 percent, and for Youth employments, it is 18 percent.

Table 2.3 shows in which sector participants in Youth employments and Other municipal employments worked during the temporary employment (Stockholm hosts all work in the same sector). The most common sectors are "Education", for Youth employments, and "Human health and social work activities" (in which care for elderly is included), for Other municipal employments.

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

Table 2.3. *Sector, Stockholm jobs (percent)*

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

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

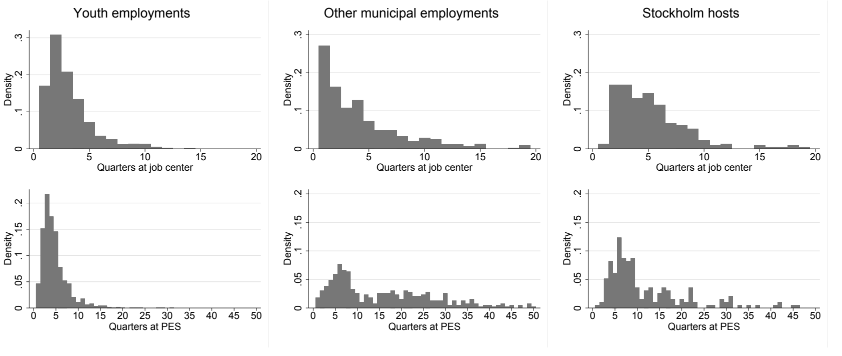
Participants in Other municipal employments also work within ”Public administration” to a large extent, whereas participants in Youth employments work within ”Human health and social work activities” as well as in ”Arts, entertainment and recreation”.

Figure 2.2 shows how long individuals have been enrolled at the job center (upper graphs) and at the PES (lower graphs) when starting a Stockholm job. Participants in Youth employments and Other municipal employments typically enter the program quite early on in their job center spell, whereas participants in Stockholm hosts enter somewhat later. Most participants enter during their first year at the job center. However, many participants have been registered as unemployed at PES for a long time when they are assigned to a Stockholm job; unemployment spells longer than two years are not unusual (an exception is Youth employments for obvious reasons).

Figure 2.3 shows how long participants remain in a Stockholm job.[15] Most participants stay for the whole planned duration of the program (6 months for Youth employments and 12 months for the other programs – at least since 2012) but some end earlier, whereas some employments are prolonged for over a year. The majority of the Youth employments are not prolonged for the possible additional 6 months.

[15] Historically, starting the PSEP as part of the Stockholm jobs program was registered as leaving the job center, which implies that very few end dates were registered before 2014. During this period, the duration of Stockholm hosts was six months. Since 2012, when most end dates in Figure 2.3 were registered, the program lasts 12 months.

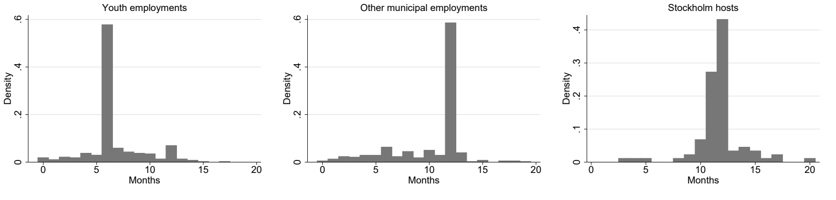
Figure 2.2. Time registered at the job center/Public employment service (PSE) before program start



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

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

Figure 2.3. Duration of Stockholm jobs by program type



Note: Displayed for observations where end date is registered, which was rare before 2014. Two observations (Stockholm hosts) that lasted longer than 20 months are censored in the figure.

2.4 Empirical strategy

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

As in many evaluations of ALMPs, individuals can be assigned to treatment at any point in time during their unemployment spell. This causes a dynamic selection problem as one might expect that all individuals will be assigned to treatment eventually, given that they remain at the job center long enough. If we do not take this into account, a static evaluation will lead to biased estimates, since the choice of the control group relies on future outcomes (Fredriksson and Johansson 2008b).

In the rest of this section, we first argue that the extensive set of individual-specific covariates available in our data makes it likely that we are able to take all potential confounders into account. Thereafter, we describe how we address the dynamic treatment assignment by applying the dynamic IPW suggested by Van den Berg and Vikström (2022). Finally, we provide the details of how the empirical strategy is implemented.

2.4.1 Selection on observables

Since the CIA can not be tested, it is crucial that we have access to all potential confounders.[16] As discussed in Section 2.3, our data includes a rich set of individual background characteristics such as sex, age, family situation, time since migration and education. In addition, tax registers give us information on previous earnings. We also have information on previous SA uptake, UI benefits and prior participation in ALMPs at PES. This information is very similar to the information available to the caseworker at the job center. However, when meeting the client, the caseworker also forms an opinion about the client's health situation as well as her/his intrinsic motivation. In our data, we have access to information about the client's previous drug prescriptions and hospitalizations, which we include in order to control for potential health problems. Our data also includes information on whether the individual him-/herself took the initiative to enroll at the job center. We use this information as a proxy for motivation. Since we also know at which job center an individual is registered, we can control for in which part of Stockholm he/she lives.

Taken together, the rich set of individual specific characteristics, including information on individual background, previous labor market history, SA and UI history, health and motivation, makes it likely that CIA is fulfilled in our

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

setting.[17] Still, there might be additional important variables that we do not observe in our data. As a way to evaluate our set of confounders, we estimate effects for the period before the participants enter into the program (and also prior to the period for which we include pre-treatment outcomes in the conditioning set). We interpret the absence of such pre-effects as suggestive evidence that our empirical strategy is successful.

2.4.2 Dynamic inverse probability weighting (IPW)

To account for the fact that individuals are assigned to treatment at different points in time, we apply the dynamic IPW-strategy proposed by Van den Berg and Vikström (2022). The dynamic IPW estimates the effects of being treated at a certain elapsed duration compared to never being treated at any subsequent time.

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

Let T_u denote duration at the initial state and T_s the duration until treatment. If $T_u < T_s$, the individual leaves the initial state before treatment. Let the potential time at the initial state, if the individual is assigned to treatment at t_s, be denoted by $T_u(t_s)$. Further, let Y denote the outcome of interest and $Y(t_s)$ the potential outcome if the individual is assigned to treatment at time t_s. $T_u(\infty)$ and $Y(\infty)$ capture the potential duration and the potential outcome if the individual is assigned to "never treated". In practice, infinity will be replaced by some upper bound. The average treatment effect of the treated ($ATET$), when assigned to treatment at t_s compared to never being treated is then given by

$$ATET(t_s) \quad = \quad E(Y(t_s) - Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s) \qquad (2.1)$$

Since we do not observe the outcome under "never treatment" for treated individuals, we need to compute this outcome from those who were never treated. However, the potential control group of never-treated will, in general, be a selective sample since individuals with relatively short durations at the job center will be over-represented in that group. The solution, proposed

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

by Van den Berg and Vikström, is to give greater weights to never-treated individuals who have been at the initial state (at the job center) for a long time. Van den Berg and Vikström show that under the assumptions of sequential unconfoundness, "no anticipation" (Abbring and Berg 2003), common support and SUTVA, an unbiased estimator of $ATET(t_s)$ is given by

$$\widehat{ATET}(t_s) = \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_i - \frac{1}{\sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i)} \sum_{i \in T_{s,i} > T_{u,i} \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_i \quad (2.2)$$

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

$$w^{t_s}(t_u, X) = \frac{p(t_s, X)}{\rho_{t_s}(1 - p(t_s, X))} \frac{1}{\prod_{m=t_s+1}^{t_u}(1 - p(m, X))} \quad (2.3)$$

$$p(t, X) = Pr(T_s = t | T_s \geq t, T_u \geq t, X) \quad (2.4)$$

$$\rho_t = Pr(T_s = t | T_s \geq t, T_u \geq t) \quad (2.5)$$

The first part of Equation (4.3) corresponds to the weights from the static IPW, where $p(t, X)$ is the propensity to be treated in period t, given by Equation (4.5). The second part takes the duration at the job center (for never-treated individuals) into account by including the propensity to be treated for each following period, if still at the job center, in the denominator. In practice, the weights will be replaced by estimated weights based on estimated propensity scores for each period the never-treated individuals are still at the job center.

Equation (4.1) is formulated for the effects on outcomes realized after all individuals have left the initial state. We are mainly interested in measuring shorter run outcomes and thus need to take into account that there are individuals who, at the time when outcomes are measured, are still in the initial state. Let Y_t denote the observed outcome in period t and $Y_t(t_s)$ the corresponding potential outcome. The estimand of interest is the ATET of treatment at t_s on the outcome in period $t_s + \tau$ (i.e. τ periods after treatment start). Van den Berg and Vikström show that under no-anticipation (short-run) and unconfoundness (short-run) assumptions, an unbiased estimator of $ATET(t_s)$ is given by

$$
\begin{aligned}
\widehat{ATET}(t_s) &= \frac{1}{\rho_{t_s} N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \\
& \frac{1}{\rho_{t_s} N_{t_s}} \left[\sum_{i \in T_{s,i} > T_{u,i}, t_s+\tau \geq T_{u,i}) \geq t_s} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \right. \\
& \left. \sum_{i \in T_{s,i} > t_s+\tau, T_{u,i} > t_s+\tau} w^{t_s}_{\tau}(T_{u,i}, X_i) Y_{t_s+\tau,i} \right] \qquad (2.6)
\end{aligned}
$$

where w^{t_s} is given by Equation (4.3) and

$$
w^{t_s}_{\tau}(X) = \frac{p(t_s, X)}{\prod_{m=t_s}^{t_s+\tau}(1-p(m,X))} \qquad (2.7)
$$

The weights in Equation (4.4) are applied to non-treated individuals who are still in the initial state when the outcome is measured (at τ). Since $t_s + \tau < t_u$ for these individuals, only information available at τ is used when estimating these weights.

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

2.4.3 Implementation

Even though we observe the exact day of assignment, we have to aggregate over larger time intervals in order to estimate the dynamic IPW because of the limited number of individuals entering the program each day (see for instance Biewen et al. (2014); Fitzenberger et al. (2008) for a similar approach). When doing so, we face a trade-off between having enough treated individuals in each assignment period and losing important variation in the data when aggregating over too long time intervals. As guidance, we base our decision on the number of participants in each type of Stockholm job and when they typically enter the program. As is clear from the top panel in Figure 2.2, most individuals who enter a Youth employment do so during the first year enrolled at the job center. This is also the program type with the most participants. We thus define $t_s = [1, 4]$ as quarters of a year, and $t_s = [5, 6]$ as six-month periods when evaluating this program. For Other municipal employments, there are fewer participants and most of them enter already in their first quarter at the job center. We thus we define $t_s = [1]$ as quarter of a year, $t_s = [2, 3]$ as six-month periods and the last period $t_s = [4]$ as the remaining 9 months. Stockholm hosts is the program type with the smallest number of participants, and where very few enter during the first quarter. We thus define $t_s = [1, 4]$ as six-month periods. In Table A.1 in the Appendix we displays the number of treated individuals for each program and assignment period.[18]

The next step is to estimate the propensity scores in Equation (4.5) and the weights in Equations (4.3) and (4.4). As is clear from Section 2.4.1, we have access to an extensive set of potential confounders. However, in the main analysis, we limit the set of covariates to the following set of confounders: age, schooling, own initiative to register at the job center, previous labor market experiences and SA recipiency.[19] Propensity scores are estimated using logistic regression models for each type of Stockholm job and for each assignment period (t_s). Since IPW has been shown to be sensitive to extreme values of the propensity score, we trim our sample following the suggestion by Huber et al. 2013, excluding individuals with weights larger than 1 percent of the sum of weights for the controls.[20] Appendix B shows balancing before and after weighing, as well as summary statistics over propensity scores and weights.

Finally, in order to be able to compare outcomes for the participants with those of their weighted controls, we need to impute fictitious start dates for individuals in the control group. We do this by, for each type of Stockholm job and time of assignment, drawing a date with replacement from the pool of start dates for the treated individuals. See Figures A.2–A.4 in the Appendix for distributions of actual and simulated start dates.[21] For each follow-up month, observations that are later treated are excluded. Observations with a (simulated) treatment date after 2016 are also excluded once the weights have been calculated.

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

[19] The reason for applying a limited set of confounders is that the bootstrap procedure that we apply to estimate standard errors did not always converges with a larger set. The limited set of confounders is chosen to achieve similar patterns for participants and their weighted controls in the outcomes of interest before participants entered the program. See Tables D.1–D.3 in the Appendix for a list of the variables included. In Section 2.4.1, we test for the robustness of including more extensive sets of confounders.

[20] It turns out that this constraint is only binding for Other municipal employments, where at most 27 treated and 12 controls are excluded.

[21] For each assignment period, we consider all individuals who are still registered at the job center at the beginning of that period as our pool of potential controls. Since we, in the estimations, aggregate over assignment periods and do not condition on non-participants to remain at the job center for the full length of the assignment period, there will be some individuals who have left the initial state before their imputed start date. This, in turn, implies that the estimates for the months closest to the program start might be different from zero for mechanical reasons.

2.5 Results

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

2.5.1 Youth employments

Youth employments typically last for six months, take place at regular workplaces and are targeted at individuals aged 16–29, who may or may not take up SA. The upper panel in Figure 2.4 shows how the likelihood of employment (having positive earnings), receiving any SA and receiving any UI benefits evolve before, during and after the participants enter the program, as well as the corresponding evolution for their weighted controls. The lower panel shows the ATET in each month relative to program start as well as 95-percent confidence intervals (CIs).

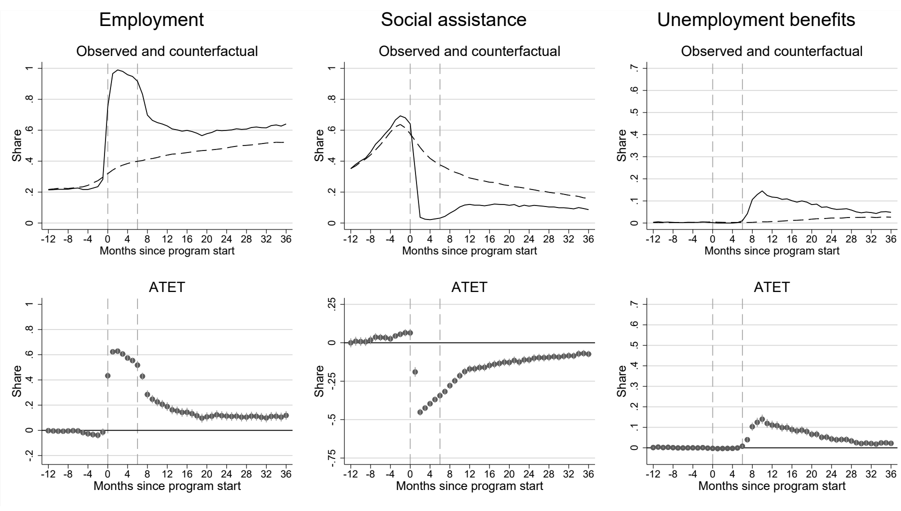
In the year preceding the program (i.e., at months -12 to -1), the differences between participants and their weighted controls are small, implying that our empirical strategy is successful.[22] Once the temporary employment starts, the share of employed individuals (left panel) in the treatment group mechanically increases to 1. During the six months that a Youth employment last, employment rates are constantly higher for the treatment group than for the control group, even though employment increases gradually for the latter group. Once most Youth employments have come to an end, the share employed goes down, but remains higher than the corresponding share in the control group (and is considerable higher than before the program started). As a result, there is a statistically significantly ATET of the program during the full follow-up period that stabilizes at about 10 percentage points one year after the temporary employment as ended.

There is a corresponding mechanical sharp drop in the share receiving SA the first two months after individuals enter the program. After the six months that the Youth employments typically last, the share receiving SA among the former participants increases somewhat, but remains considerably lower than the corresponding share before program start as well as the share in the control

[22] A small decrease in the share employed just before the participants start their employment can be detected, which could be explained by participation in a pre-program internship. The positive pre-effect present for SA the months just before program start may be a consequence of that some individuals in the control group already have left the job center at the time of their simulated start date, as mentioned in footnote 21. This interpretation is reinforced by our sensitivity analysis, where we show that these positive pre-effects disappear when we aggregate over shorter assignment periods, see Section 2.5.4.

group, even though the latter decreases over time. Hence, there is a negative effect on SA recipiency for the full follow-up period, reaching around 7.5 percentage points three years after program start.

Figure 2.4. Outcomes and ATET by month since program start: Youth employments



Note: Solid line indicates treated group, dashed line indicates weighted control group. 95% CIs based on 997 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in Appendix C.

The likelihood of receiving UI benefits (right panel) increases sharply in the treatment group in month 6, when most Youth employments have come to an end. The effect is at it's largest 10 months after program start when it amounts to around 15 percentage points. The effect then diminishes, but three years after program start, the share receiving any UI benefit is still 2 percentage points higher among former participants than among non-participants.

In order to get a better impression of how large the estimated effects are, Table 2.4 shows the cumulative effects on the number of months employed, receiving any SA and UI benefits, as well as on earnings and amounts received from SA and UI benefits respectively, the year before program start, the six months that the program typically lasts, and in the short (first post-program year) and medium (the second year after the program has ended) run. As a comparison, the table also provides the means for the weighted controls.

Reassuringly, the pre-effects are all small, lending support to our identification strategy. It is also evident that employment and earnings go up during the six months that participants are employed and that SA-recipiency goes down during this period. The more interesting thing is what happens once the temporary employment has ended. From the table, we see that having had a Youth employment increases employment in the short run by approximately

Table 2.4. *Cumulative ATET: Youth employments*

	Employment (months)	SA (months)	UI benefits (months)
Months 12–0 before program start			
ATET	-.189	.308	.00633
St err	.102	.0915	.0169
Mean	2.8	5.79	.0339
During program (month 1-6)			
ATET	3.49	-2.17	-.00579
St err	.0562	.0575	.00506
Mean	2.27	2.66	.0182
Short run outcomes, 1 year after program			
ATET	2.41	-2.29	1.2
St err	.174	.121	.0804
Mean	5.27	3.52	.0937
Medium run outcomes, 1-2 year after program			
ATET	1.31	-1.26	.555
St err	.186	.131	.0731
Mean	5.88	2.58	.264

	Earnings (SEK)	SA (SEK)	UI benefits (SEK)
Months 12–0 before program start			
ATET	-1,178	1,226	-13
St err	841	793	43.2
Mean	12,959	34,035	103
During program (month 1-6)			
ATET	66,539	-12,760	-37.2
St err	1,119	388	14.9
Mean	21,788	15,418	58.7
Short run outcomes, 1 year after program			
ATET	36,379	-13,659	3,638
St err	3,347	835	306
Mean	66,479	20,587	484
Medium run outcomes, 1-2 year after program			
ATET	17,648	-7,886	1,961
St err	4,109	840	401
Mean	86,215	15,148	1,809

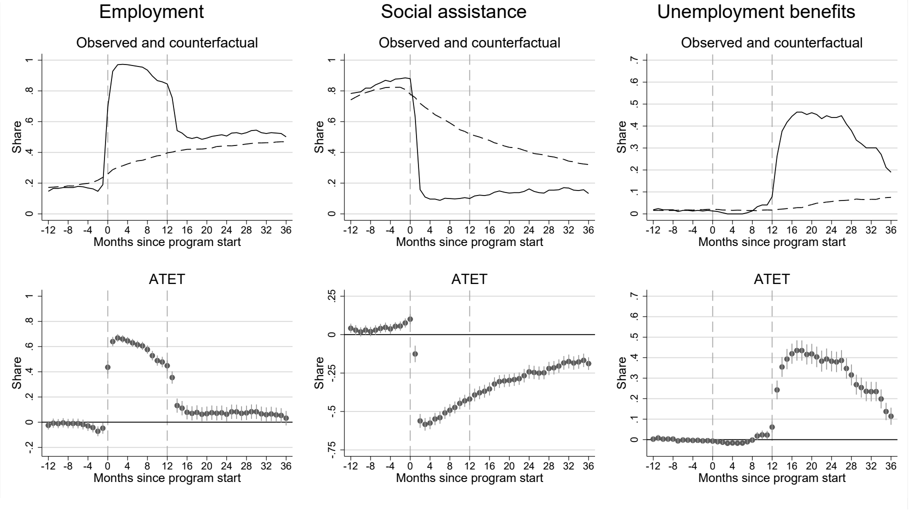
Note: Short (medium) run outcomes are measured months 7–18 (19–30) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 997 replications.

2.4 months and increases earnings by SEK 36,400 during the same period. These effects correspond to a 45–55-percent increase in employment and earnings compared to the averages in the weighted controls. In the medium run, the effects are smaller (approximately 50 percent of the short run-effects), but still economically (and statistically) significant, corresponding to increased employment and earnings with around 20 percent. The program further reduces the number of months with any SA by 2.3/1.25 months (65 and 50 percent) in the short/medium run, and increases the number of months with any UI benefits with 1.2/0.5 months respectively. The amount received in SA decreases by SEK 13,500/7,900 whereas the amount in UI benefits increases by SEK 3,600/2,000. Comparing the amounts gained in earnings and UI benefits with the amount lost in SA, we conclude that taking part in the Youth employment program results in SEK 38,000 higher income on average over two years after the program ended.

2.5.2 Other municipal employments

Other municipal employments last for twelve months, take place at regular workplaces and are targeted at SA recipients. Figure 2.5 shows the evolution of outcomes and estimated effects for this employment type.

Figure 2.5. Outcomes and ATET by month since program start: Other municipal employments



Note: Solid line indicates treated group, dashed line indicates weighted control group. 95% CIs based on 995 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in Appendix C.

Once the temporary employment starts, the share employed goes up, whereas the share receiving SA goes down, as expected. When the program ends, after one year, there is a distinct drop in the share employed among former participants, but not to the level of the weighted controls. Hence, there is a positive employment effect of around 5–10 percentage points for the two years that follow after the temporary employment has ended. The share receiving SA increases only marginally once the program ends and remains at a lower level compared to the share among the weighted controls, with a treatment effect of just below 20 percentage points at the end of our follow-up period. Turning to the share receiving UI benefits, there are indications of a small negative effect during the period when the employment lasts, which is partly mechanical given that employed individuals are not entitled to UI benefits. Once the employment ends, there is a sharp increase among former participants, that is not present among the weighted controls, implying a positive ATET of around 25 percentage points. The effect increases the following months, reaching a maximum of just above 40 percentage points. Two years after the temporary municipal employment has ended, the share among former participants is around 10 percentage points higher compared to had they not taken part in the program.

Table 2.5 shows the cumulative effects on number of months (top panel) and amounts (bottom panel). By participating in the program, individuals gain 1.2/0.8 months in employment and SEK 19,700/10,200 in earnings in the short/medium run. These effects correspond to increases of around 20/10 percent compared to those in the control group. The number of months with SA decreases by 3.7/2.4, corresponding to a decrease of around 70/75 percent, whereas the amount received decreases by 47,000 during these two years. The increase in the number of months with any UI is 4.7/3.1 months and the corresponding amount is 33,000. Whereas the increase in the number of months receiving UI benefits is larger than the corresponding decrease in the number of months receiving SA, the amount gained in UI benefits is smaller than the amount lost in SA. Also taking into account the increase in earnings, participating in Other municipal employments results in SEK 15,900 more in income in the short and medium run. All pre-program effects are economically insignificant.

Table 2.5. *Cumulative ATET: Other municipal employments.*

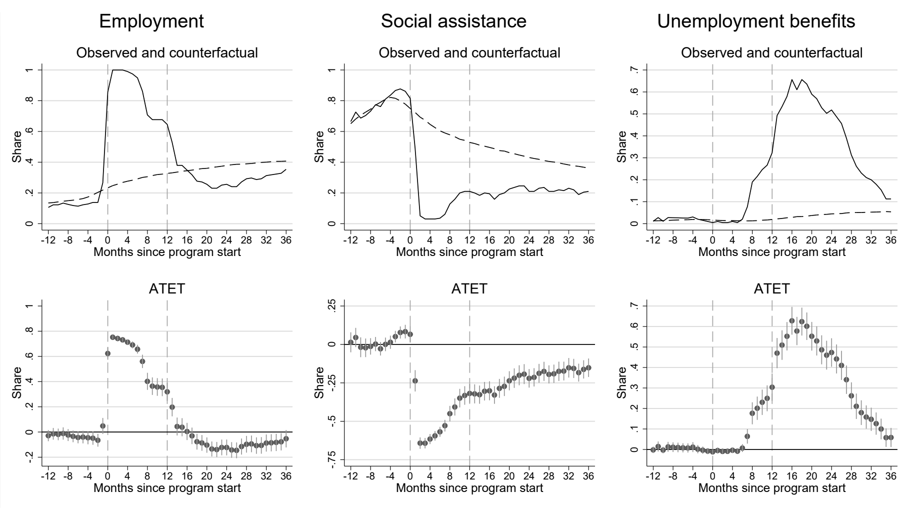
	Employment (months)	SA (months)	UI benefits (months)
Months 12–0 before program start			
ATET	-.293	.477	-.00756
St err	.19	.102	.0684
Mean	2.31	9.59	.208
During program (month 1-12)			
ATET	6.99	-5.73	.0212
St err	.173	.2	.0486
Mean	4.13	7.49	.206
Short run outcomes, 1 year after program			
ATET	1.17	-3.72	4.67
St err	.299	.224	.243
Mean	5.16	5.36	.441
Medium run outcomes, 1-2 year after program			
ATET	.767	-2.38	3.1
St err	.313	.24	.231
Mean	5.55	4.2	.809
	Earnings (SEK)	SA (SEK)	UI benefits (SEK)
Months 12–0 before program start			
ATET	-2,750	-3,845	-131
St err	2,386	1,627	246
Mean	17,250	64,611	807
During program (month 1-12)			
ATET	148,148	-42,528	-322
St err	3,918	1,439	182
Mean	56,154	51,207	983
Short run outcomes, 1 year after program			
ATET	19,726	-27,897	22,404
St err	6,283	1,553	1,417
Mean	83,414	36,280	2,917
Medium run outcomes, 1-2 year after program			
ATET	10,158	-19,114	10,598
St err	7,242	1,761	1,095
Mean	98,222	29,539	5,455

Note: Short (medium) run outcomes are measured months 13–24 (25–36) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 995 replications.

2.5.3 Stockholm hosts

Stockholm hosts differ from the other two types of Stockholm jobs in that participants are not employed at a regular workplace, but at a workplace created especially for program participants. The program is targeted at SA recipients older than 25 or other individuals at risk of becoming long-term unemployed. The length of the program has been either six or 12 months. The results for this program are shown in Figure 2.6.

Figure 2.6. Outcomes and ATET by month since program start: Stockholm hosts



Note: Solid line indicates treated group, dashed line indicates weighted control group. 95% CIs based on 997 bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). A regression table corresponding to the lower panel is available in Appendix C.

That the program length varied over time is evident from the graphs: for all three outcomes, there is a drop/increase in the share employed/receiving SA or UI benefits after six months and a corresponding change after twelve months.[23] As opposed to the findings for the other two types of Stockholm jobs that we analyze, the share of employed individuals among former program participants drops to a level below the corresponding share for non-participants one year after the employment begins (and when the majority of temporary employments have come to an end). The negative employment effect is the largest two years after program start, reaching almost 15 percentage points. The negative effect decreases over time, and towards the end of our follow-up period, we cannot reject that it is zero (at the five-percent significance level).

[23] Figure C.1 in the Appendix show the ATET when excluding participants entering the program before 2012, i.e. when the employment was shorter. In Section 2.6 we further discuss how the length of the program may matter for the effects.

The share receiving any SA hovers around 20 percent once the program has ended. Compared to the corresponding share among the weighted controls, this is considerably lower, and the ATET amounts to around 15 percentage points. For the share receiving any UI benefits, there is a positive effect already after six months, when the temporary employments in 2010 and 2011 had come to an end, and an additional increase after one year. The effect is at its largest shortly thereafter, amounting to around 60 percentage points, and then diminishes over time. At the end of our following up period former participants are around 5 percentage points more likely to receive any UI benefits than their controls.

The negative employment effect is also visible in Table 2.6, which shows the cumulative effects. In the short/medium run, former participants are employed 0.5/1.2 fewer months (a 10/20 percent decrease) and earn SEK 15,300/24,400 less (a 20/30 percent decrease) compared to non-participants. The negative employment effect is thus larger in the medium run than in the short run. Participating in the program reduces the number of months receiving SA by 3.2/2 (45–55 percent) and the amount received by SEK 23,500/18,800 (60–65 percent). The time receiving UI benefits increases by 6.5/2.5 months and the amount received by SEK 31,800/10,900. Taken together, income is SEK 39,400 lower for participants compared to non-participants during these two years. However, this loss in disposable income is lower than the increase in disposable income while being in the program (SEK 87,000).

2.5.4 Sensitivity analyses

As mentioned in Section 2.4.3, we limit the number of confounders in the main analysis due to issues with the bootstrap procedure. The fact that the pre-effects are all very close to zero indicates that this limited set does the job. To further test whether we miss any important underlying differences between the two groups, we include additional individual characteristics, indicators for the different job centers, year effects, additional health indicators, as well as additional controls for labor market history, one by one and jointly, in addition to applying the algorithm suggested by Luna et al. 2011 for covariate selection (see Tables D.1–D.5 in the Appendix for information on the variables included). As is clear from Figure D.1 in the Appendix, the estimated ATETs are more or less identical for all these different sets of confounders.

Another way to allow for a larger set of confounders and still be able obtain bootstrapped standard errors is to pool over assignment periods when estimating the propensity scores in Equation 4.5, but adding assignment periods dummies. Doing this we can include all variables mentioned above but the downside is that we restrict the parameters to be the same for all assignment periods. As seen in Figure D.2 in the Appendix, we find very similar estimates when using this alternative way to estimate the propensity scores.

Table 2.6. *Cumulative ATET: Stockholm hosts*

	Employment (months)	SA (months)	UI benefits (months)
Months 12–0 before program start			
ATET	-.359	.0692	.053
St err	.224	.174	.0981
Mean	1.94	8.92	.198
During program (month 1-12)			
ATET	6.64	-5.67	1.2
St err	.22	.199	.144
Mean	3.51	7.31	.175
Short run outcomes, 1 year after program			
ATET	-.542	-3.19	6.46
St err	.326	.323	.323
Mean	4.27	5.68	.409
Medium run outcomes, 1-2 year after program			
ATET	-1.2	-2.09	2.49
St err	.347	.325	.257
Mean	4.76	4.68	.615
	Earnings (SEK)	SA (SEK)	UI benefits (SEK)
Months 12–0 before program start			
ATET	-3,729	733	443
St err	1,890	2,096	539
Mean	14,846	56,862	676
During program (month 1-12)			
ATET	122,552	-37,761	2,394
St err	4,786	1,577	403
Mean	48,221	46,602	656
Short run outcomes, 1 year after program			
ATET	-15,346	-23,498	31,772
St err	5,629	2,068	2,029
Mean	67,590	36,328	2,498
Medium run outcomes, 1-2 year after program			
ATET	-24,441	-18,818	10,936
St err	6,782	2,089	1,524
Mean	81,471	31,344	4,214

Note: Short (medium) run outcomes are measured months 13–24 (25–36) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 997 replications.

The limited number of program participants forces us to aggregate over several months when defining assignment periods. To investigate whether our results are sensitive to the way in which we aggregate, we have shortened the time periods somewhat (see Table A.2 in the Appendix), which comes at the cost of having fewer participants entering the program at each assignment period. When doing this, the positive pre-effects that were detected in the likelihood of receiving any SA that were present in Figures 2.4–2.6 are considerably smaller, which we take as evidence that the former were a consequence of the aggregation over assignment periods rather than ”true” pre-effects. Apart from this the results are insensitive to the length of the time periods, see Figure D.3 in the Appendix.

When estimating ATET for the period before participants enter the program (months -12 to -1), we need to weight the non-participants to make them comparable with the participants. However, the weights in Equation (4.4) are only estimated for periods when participants have already entered the program. In the main analysis, we apply the weights from month 1 for the pre-program period. As a consequence, we might worry that the pre-period is less relevant when it comes to evaluating the balance for participants who enter later in their job center spell. Instead using weights from months 12, 24 and 36 respectively does not change the ATET for the pre-period, see Figure D.4 in the Appendix.

2.5.5 Health outcomes

Participating in the program may also affect participants' health and general well-being.[24] In addition, for those participants whose disposable income increases, there are opportunities to invest in their health, and potentially a reduced negative stress associated with living with limited resources.

Tables 2.7–2.9 show the ATET for the likelihood of having any drug prescribed/any hospitalization for the year before the individual enters the program (months -12 to 0), while they take part in the program, as well as in the short and medium run for the three types of Stockholm jobs.[25]

The results in Table 2.7 show indications of positive health effects for participants in Youth employments. Compared to their weighted controls, they are 25 percent less likely to be hospitalized and almost 40 percent less likely

[24] Having a job with a salary, even if it is subsidized, may offer a sense of pride and purpose for the participant. When asked in interviews, participants respond that they do tell their family and friends about acquiring a Stockholm job. We interpret this as evidence of pride. E.g., Ivanov et al. 2020 find that job creating schemes improve the social integration and well-being of long-term unemployed individuals in the German setting.

[25] When estimating the effects on these outcomes we use the same covariates as in the main analysis except that we also condition on whether the individual received any pain relief the year before registering at the job center, whether he/she received any psychiatric drugs and whether he/she was hospitalized during the same period. See table B.3 and B.4 in the Appendix for the results on overlap and balance.

to be prescribed any psychiatric drugs while upholding their Stockholm job. The latter effect pertains in the short run, corresponding to a reduced likelihood of almost 15 percent. Turning next to participants in Other municipal employments in Table 2.8, we conclude that also for this group, getting a Stockholm job reduces the likelihood of being prescribed any psychiatric drugs, both while in the program (with around 35 percent) and in the short and medium run (with around 30 percent). When it comes to hospitalization, there are however indications that those that take part in the program are somewhat less likely to be hospitalized already before starting their Stockholm job. Table 2.9 finally, shows the corresponding results for Stockholm hosts. Having this type of Stockholm job reduces the likelihood of receiving any pain relief with almost 25 percent while in the program and with 30/20 percent in the short/medium run. One explanation to these positive effects may be the very active nature of the employment where participants spend the day outdoor walking long distances.

Table 2.7. *Cumulative ATET: Health outcomes Youth employments*

	Prescription:		Hospitalization
	Pain relief	Psychiatric	
Months 12–0 before program start			
ATET	-.0102	-.00973	-.00841
St err	.00548	.00469	.00434
Mean	.104	.134	.0713
During program (month 1-6)			
ATET	-.00619	-.0262	-.0252
St err	.00836	.00829	.00734
Mean	.0722	.104	.0674
Short run outcomes, 1 year after program			
ATET	-.0127	-.0212	-.000523
St err	.0111	.0119	.0115
Mean	.116	.153	.112
Medium run outcomes, 1-2 year after program			
ATET	-.0127	.00155	-.00982
St err	.0108	.0128	.0113
Mean	.117	.159	.116

Note: Short (medium) run outcomes are measured months 7–18 (19–30) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 999 replications.

Table 2.8. *Cumulative ATET: Health outcomes, Other municipal employments*

	Prescription: Pain relief	Prescription: Psychiatric	Hospitalization
Months 12–0 before program start			
ATET	-.0126	-.0157	-.0271
St err	.0134	.0084	.00872
Mean	.261	.195	.0806
During program (month 1-12)			
ATET	.00144	-.0795	-.0373
St err	.0248	.0166	.0139
Mean	.262	.219	.0992
Short run outcomes, 1 year after program			
ATET	-.0217	-.0603	-.0139
St err	.023	.0186	.0154
Mean	.251	.214	.0975
Medium run outcomes, 1-2 year after program			
ATET	-.0321	-.0687	-.0166
St err	.0233	.0191	.0149
Mean	.24	.222	.0921

Note: Short (medium) run outcomes are measured months 13–24 (25–36) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 995 replications. Months relate to program start.

Table 2.9. *Cumulative ATET: Health outcomes Stockholm hosts*

	Prescription: Pain relief	Prescription: Psychiatric	Hospitalization
Months 12–0 before program start			
ATET	-.0187	-.0202	.00616
St err	.0168	.0092	.0158
Mean	.173	.164	.122
During program (month 1-12)			
ATET	-.0542	-.0321	.00704
St err	.0254	.0247	.0238
Mean	.229	.191	.126
Short run outcomes, 1 year after program			
ATET	-.0691	-.0304	.0472
St err	.0269	.0229	.0263
Mean	.228	.184	.112
Medium run outcomes, 1-2 year after program			
ATET	-.0451	-.00879	.0188
St err	.027	.0256	.0228
Mean	.23	.188	.0991

Note: Short (medium) run outcomes are measured months 13–24 (25–36) since the start of the program. Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 996 replications. Months relate to program start.

2.6 Mechanisms

One conclusion from our analysis is that the type of workplace seems to matter for the program's success. There are several possible explanations to this finding. One is that working at a regular workplace is a stronger positive signal to future employers than having worked at a constructed workplace, and that the skills acquired are more valuable for employers. In addition working at a regular workplace may provide participants with valuable networks as well as useful references and referrals from the manager. Former participants may even get a regular employment at the same workplace as in which they had their Stockholm job, something that is not possible, or a least to a very limited extent, for former Stockholm hosts.

Table 2.10 shows to what extent former participants are employed at the same workplace and/or in the same sector as they had their temporary employment in/at, sometime between 18–36 months after they enrolled in the program.[26] It turns out that a relative large proportion of those that do have any employment during these two years have it at the same workplace as they had their Stockholm job. This tendency is especially prominent among former participants in Youth employments, where as much as one third work at the same workplace. This finding indicates that employer contacts can be particularly important for young individuals, something which might also explain why we find larger positive employment effects for former participants in Youth employments. It is also evident that many gets their future employment in the same sector; as much as 50 percent of former participants in Youth employments and around 35 percent for the other two employment types.

Table 2.10. *Workplace and sector of employment, 18–36 months after program start (percent)*

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

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

[26] For Stockholm hosts, these figures should be interpreted with some caution. As is shown in Figure 2.3, some temporary employments lasted longer than 18 months and are therefore included in the 20.4 percent working in the same workplace 18–36 months after program start. It is however also possible that a few of them, after finishing the Stockholm hosts program, were employed as supervisors at the same workplace.

A conclusion from the figures in Table 2.10 is that it is important that the temporary employment happens in a sector that are in demand of labor. Whereas many Youth employments and Other municipal employments take place in sectors characterized by shortage of staff, such as childcare and care for elderly, the closest type of job to a Stockholm host is probably a janitor, an occupation that, according to the Swedish PES, is one of those involving the toughest competition among professions with the shortest education.[27] This conclusion is re-inforced by Table 2.11, where we instead explore in which sectors former participants end up three years after the program started. In fact, half of former participants in Youth employments and Other municipal employments work in the education or health sector. Former participants in Stockholm hosts are instead most likely to work with transportation or storage.

Table 2.11. *Sector of employment, 36 months after program start (percent)*

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

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

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

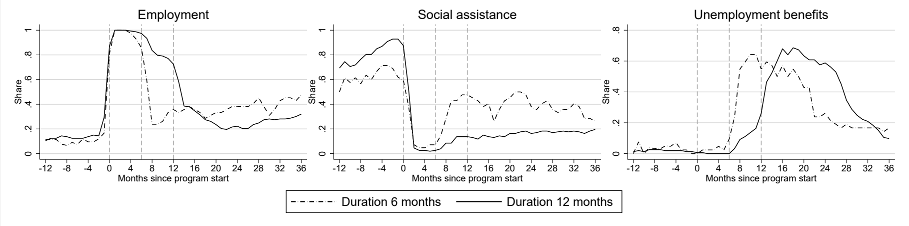
Another potential explanation for the less promising employment effects found for former participants of Stockholm hosts may be selection into the program. The participants in Stockholm hosts are to a larger extent males, have been a shorter time in Sweden and have somewhat lower education than the participants in Other municipal employments. On the other hand, they have somewhat better health status before enrolling at the job center. However, even if the participants in Stockholm hosts were negatively selected, this would not explain the negative employment effects found, since we compare the outcome of those participating in Stockholm hosts, not with participants in the other two employment types but with their weighted controls of never-treated individuals. The differing employments effects could instead be a result of the empirical strategy being differently successful for the three program types. From the estimated pre-effects, there are however no such indications. The negative effects are hence likely due to negative lock-in effects of the program.

A common feature of all three employment types is that participating in the program decreases the likelihood of receiving SA and increases the likelihood of receiving UI benefits once the temporary employment is over. This tendency is less pronounced for Youth employments, whose temporary employments only last six months. To be entitled to earnings related UI benefits, individuals must have worked for at least six months and been a member of a UI fund for at least one year, and it is hence likely that those participating in Youth employments do not fulfill the membership requirement when their Stockholm job finishes. To analyze the importance of the length of the employment, we utilize the fact that the duration of Stockholm hosts was shorter (six months compared to twelve months) during the first two years (2010 and 2011) of our study period.

Figure 2.7 shows observed outcomes by program length.[28] Comparing the employment outcomes for those that took part in the program when it lasted six months and those that took part in the program when its duration was longer, we find that, regardless of the length of the program, the share receiving any UI benefits increases almost to the same extent when the program ends, stabilizing around 20 percent towards the end of our follow up period. However, participants in the shorter program receive SA to a larger extent than those taking part in the longer program, once the temporary employment is finished. A likely explanation is that the former group does not fulfill the membership condition and hence receive lower levels of UI benefits and need to top up with SA. This explanation is supported when comparing the cumulative ATET on the amount UI benefits received including (Table 2.6) and excluding (Table C.1 in the Appendix) participants entering the program before 2012, i.e. when the employment was shorter.

[28]Given the small number of participants we are not able to estimate ATET separately for those entering the program before and after 2012. Results excluding participants that enter the program before 2012 are available in Figure C.1 and Table C.1 in the Appendix.

Figure 2.7. Observed outcomes by program length: Stockholm hosts



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

2.7 Concluding discussion

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

We find positive employment effects of having a Stockholm job taking place at regular workplaces, a result that differs from what previous evaluations of public sector employment programs have found (Card et al. 2010, 2018; Kluve 2010). One explanation is probably that the program we study is targeted at SA recipients and other individuals that to a large extent lack previous labor market experiences, whereas most earlier work focuses on groups with stronger labor market attachment.[29] The conclusion that a temporary employment can act as a stepping stone to future employment for new entrants at the labor market is in line with the findings in e.g. Pallais 2014. But also for this specific group, our results are more promising than the ones found for the German and Belgian evaluations of Temporary extra jobs and Social employments and more in line with the Danish evidence on subsidized employment for SA recipients.

The fact that taking up a Stockholm job is voluntary is potentially one reason for the positive employment effects. In that vein, the program resembles the Norwegian qualification program, which provides tailored activation to hard-to-employ SA recipients in combination with generous non-means-tested ben-

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

efits. This program has been shown to raise employment among participants (Markussen and Røed 2016). Another possible explanation to the relatively good outcome of the program we evaluate is that the job search assistance provided by caseworkers toward the end of the temporary employment is effective. This would be in line with the results in Dahlberg et al. (2020) who evaluate a program for another vulnerable group, low-educated refugees, and find large positive effects on employment. The program in their study included intensive language training, work practice and ended with intensive job search assistance.

However, having the temporary employment at a regular workplace seems to be crucial for future employment prospects. Our findings are thus in line with previous evidence indicating that programs that more resembles regular employment, such as subsidized employment, work better (see e.g. Calmfors et al. (2002)). For Stockholm hosts, who work at a constructed workplace, we instead find negative employment effects. One explanation to the differing results is Youth employments and Other municipal employments often take place at workplaces with a shortage of personnel, whereas Stockholm hosts have their temporary employment at a workplace with very limited possibility of prolonged employment. This conclusion is supported by the fact that several participants get employed at the same workplace as in which they had their temporary employment. This pattern is especially pronounced for young people, a finding that is in line with previous work by Müller (2021), who shows that early employer links account for more than 30 percent of Swedish vocational high school students' first regular employment, and that losing this link before graduation has a long-lasting negative impact on earnings and employment.[30]

A common feature of all three employment types is that participating in the program decreases the likelihood of receiving SA and increases the likelihood of receiving UI benefits once the temporary employment is over. Municipalities are thus able to shift cost from the local budget to the UI funds by placing individuals into Stockholm jobs.[31] However, the extent to which this is possible seems to depend on whether the temporary employment is long enough to make participants fulfill the membership condition for being entitled to earnings related UI benefits.

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

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

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

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

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Appendix A: General description

As seen in Figure 1, the share receiving SA is higher one month after registering at the job center than one month before. This can be explained by the fact unemployed individuals are required to register at the job center and participate in activities in order to qualify for SA. There could also be a measurement error due to employment being registered on a monthly level, where an individual shows up as employed if he/she becomes unemployed at the beginning of the month, and register at the job center directly after.

Below we divide the study population into those who receive SA when registering at the job center and those who do not, and then analyze the second group in more detail. Of those around 9,500 new registrations without SA, 46 percent receive SA the following month. Of the remaining 54 percent, 59 percent are younger than 30.

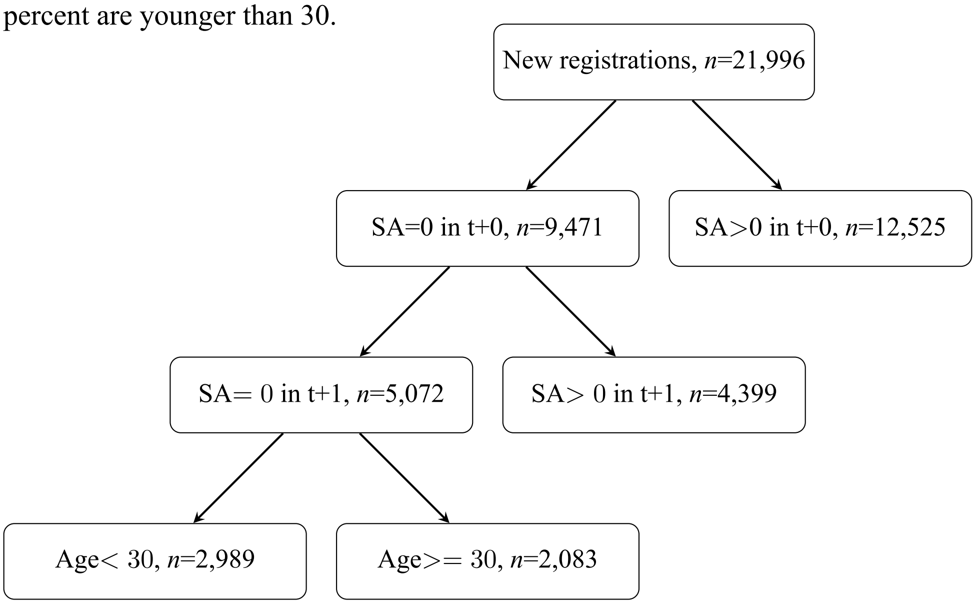
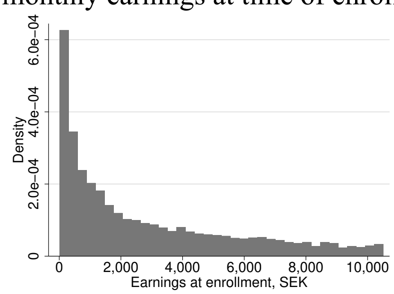


Figure A.1. Average monthly earnings at time of enrollment at the job center



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

Figure A.2. Actual and simulated start dates: Youth employments

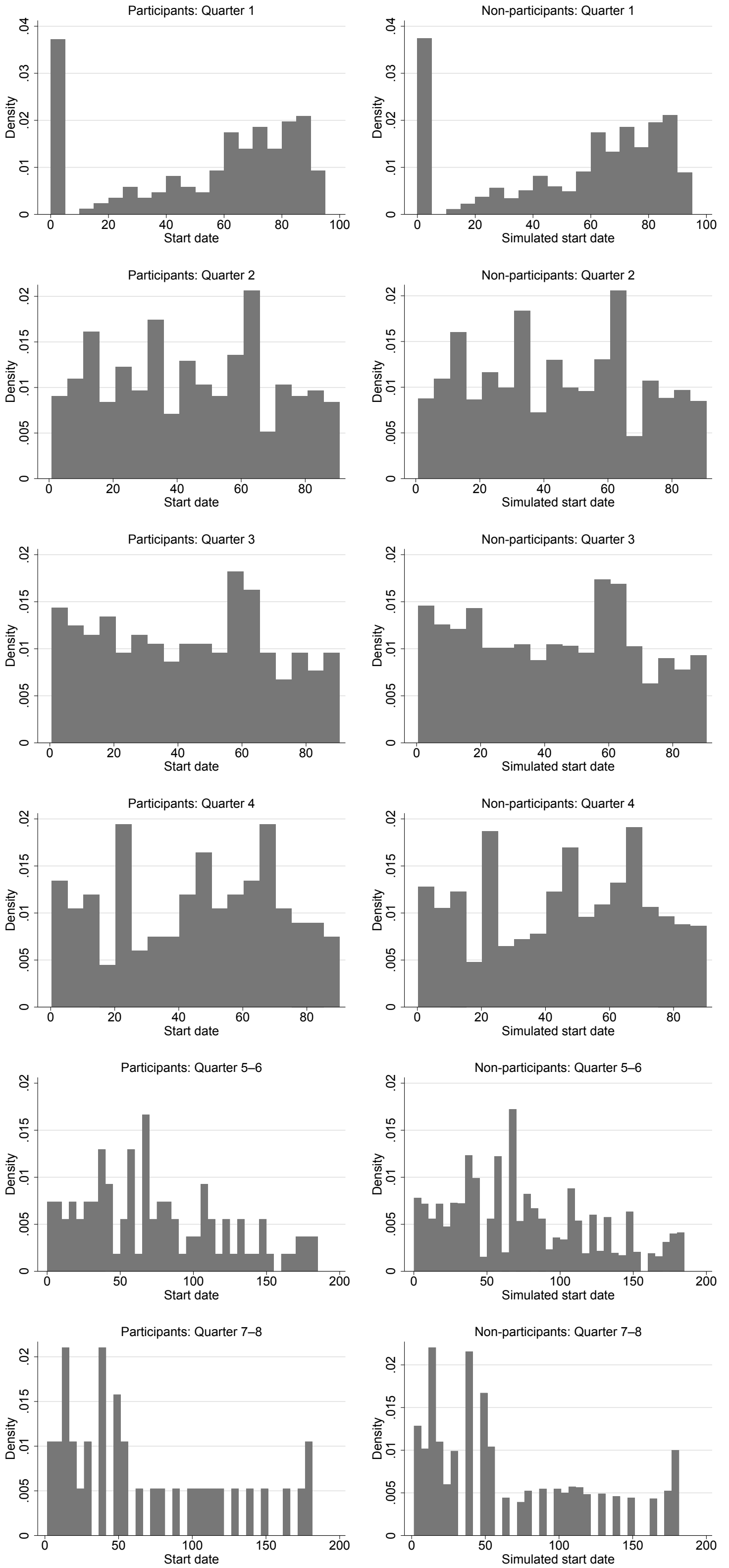


Figure A.3. Actual and simulated start dates: Other municipal employments

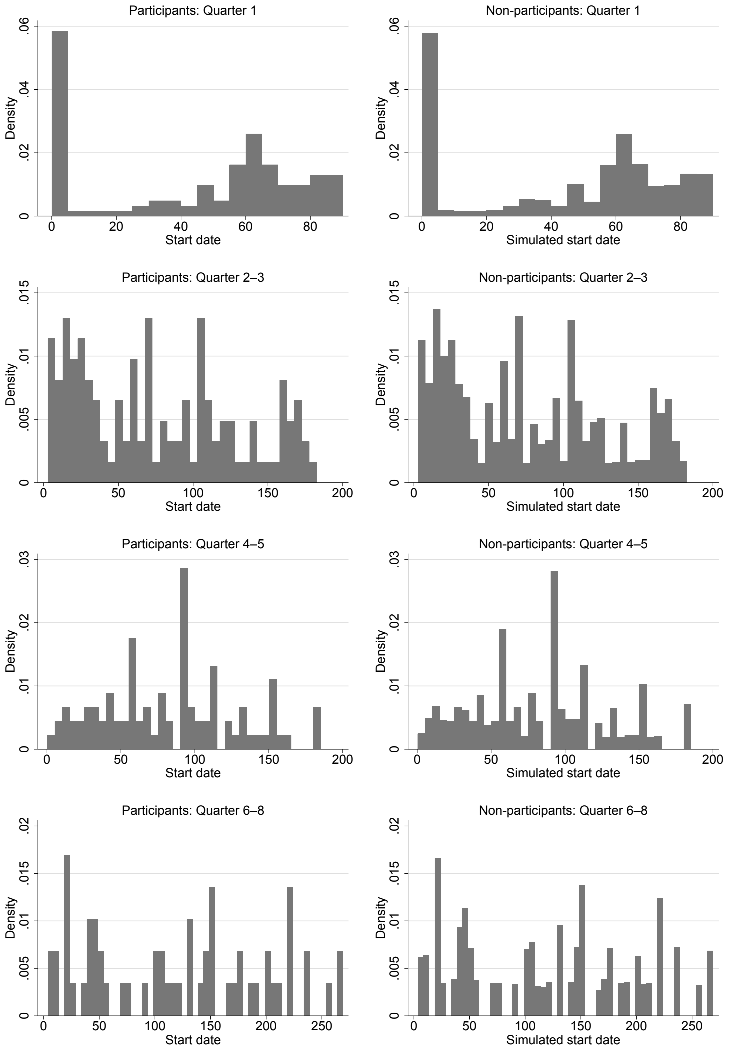


Figure A.4. Actual and simulated start dates: Stockholm hosts

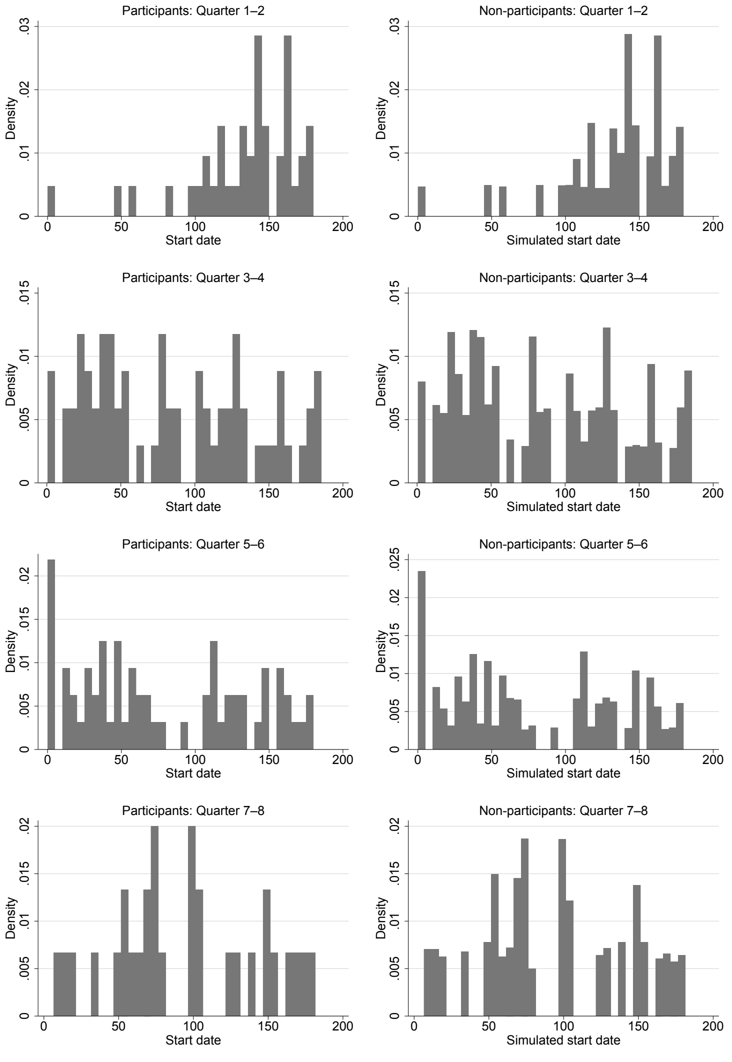


Table A.1. *Program participants, per assignment period: Main analysis*

<table>
<tr><th>Quarter</th><th>Youth employments</th><th>Other municipal employments</th><th>Stockholm hosts</th></tr>
<tr><td>1</td><td>171</td><td>123</td><td rowspan="2">41</td></tr>
<tr><td>2</td><td>310</td><td rowspan="2">123</td></tr>
<tr><td>3</td><td>209</td><td rowspan="2">66</td></tr>
<tr><td>4</td><td>134</td><td rowspan="2">91</td></tr>
<tr><td>5</td><td rowspan="2">108</td><td rowspan="2">61</td></tr>
<tr><td>6</td><td rowspan="3">59</td></tr>
<tr><td>7</td><td rowspan="2">38</td><td rowspan="2">28</td></tr>
<tr><td>8</td></tr>
<tr><td>(> 8)</td><td>35</td><td>57</td><td>31</td></tr>
</table>

Note: Program participants with assignment periods later than quarter 8 are included in the propensity score estimations but not in the estimations of the ATET.

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

<table>
<tr><th>Months</th><th>Youth employments</th><th>Other municipal employments</th><th>Stockholm hosts</th></tr>
<tr><td>1</td><td>44</td><td rowspan="3">123</td><td rowspan="3">3</td></tr>
<tr><td>2</td><td>33</td></tr>
<tr><td>3</td><td>99</td></tr>
<tr><td>4</td><td>110</td><td rowspan="3">74</td><td rowspan="3">38</td></tr>
<tr><td>5</td><td>120</td></tr>
<tr><td>6</td><td>85</td></tr>
<tr><td>7</td><td>76</td><td rowspan="3">49</td><td rowspan="3">38</td></tr>
<tr><td>8</td><td>81</td></tr>
<tr><td>9</td><td>54</td></tr>
<tr><td>10</td><td>43</td><td rowspan="3">46</td><td rowspan="3">29</td></tr>
<tr><td>11</td><td>49</td></tr>
<tr><td>12</td><td>42</td></tr>
<tr><td>13–18</td><td>102</td><td>67</td><td>61</td></tr>
<tr><td>19–24</td><td>37</td><td>37</td><td>28</td></tr>
<tr><td>(> 24)</td><td>35</td><td>57</td><td>31</td></tr>
</table>

Note: Program participants with assignment periods later than month 24 are included in the propensity score estimations but not in the estimations of the ATET.

Appendix B: Evaluating overlap and matching

Table B.1. *Summary statistics of the covaiate balance before and after DIPW.*

	Before DIPW				After DIPW			
	Mean		Balance		Mean		Balance	
	T=1	T=0	ND	p-value	T=1	T=0	ND	p-value
Youth employment								
Age 25-29	.18	.365	.424	1e-48	.18	.187	.018	.593
Less than high school	.526	.507	.0378	.251	.526	.525	.001	.986
Born outside N. & W. Eur.	.484	.508	.05	.13	.484	.479	.009	.776
Own initiative to be reg.	.176	.086	.269	5e-13	.176	.178	.004	.912
0 quarter at PES, at JC reg.	.269	.264	.0117	.722	.269	.269	.0004	.989
Employed in t0-6	.187	.244	.14	9e-06	.187	.192	.0125	.703
SA in t-1	.652	.524	.262	5e-16	.652	.647	.009	.78
Other employment								
Age 18-29	.127	.35	.541	2e-36	.127	.132	.0144	.781
Age 30-39	.268	.274	.013	.799	.268	.288	.044	.401
Age 40-49	.366	.214	.339	2e-09	.366	.35	.032	.542
Employed in t0-6	.038	.197	.51	5e-54	.038	.042	.018	.725
Subsidized empl in t0-6	.602	.135	1.1	1e-73	.602	.577	.051	.332
SA-reason: unemployment	.881	.726	.398	1e-19	.881	.877	.012	.82
SA, nr of months t-24	17.1	10.2	.852	3e-72	17.1	16.6	.062	.231
SA in t-1	.867	.629	.57	6e-40	.867	.865	.006	.913
Own initiative to be reg.	.065	.057	.035	.515	.065	.053	.051	.356
Stockholm host								
Age 50-	.277	.166	.269	.001	.277	.281	.01	.894
Less than high school	.492	.368	.252	.001	.492	.488	.009	.9
Born outside N. & W. Eur.	.749	.666	.181	.009	.749	.746	.006	.939
Own initiative to be reg.	.010	.001	.119	.215	.010	.005	.065	.441
0 quarter at PES, at JC reg.	.113	.207	.258	.00004	.113	.115	.0081	.91
Employed in t0-6	.056	.147	.304	5e-08	.056	.039	.081	.297
SA in t-1	.79	.614	.391	2e-09	.79	.79	.002	.983

Note: Weights used are based on all information given 36 months after program start.

Table B.1. *Summary statistics of estimated PS and associated weights from DIPW*

	Propensity Scores				Dynamic Inverse Prob. weights				
	mean	min	max	obs	mean	min	max	obs	trim
Youth Employment									
Participants	.0762	.0041	.186	970	1	1	1	970	0
Non-participants	.0421	.0016	.304	21274	.0494	.0016	.404	18781	0
Other Employment									
Participants	.112	.0002	.7	396	1	1	1	396	27
Non-participants	.0128	.0001	.679	27370	.013	.0001	2.92	25348	3
Stockholm hosts									
Participants	.0154	.0005	.209	196	1	1	1	195	0
Non-participants	.0066	.0001	.202	29308	.0066	.0001	.253	27472	0

Note: Propensity score is estimated with logistic regression. Weights are based on all information given 36 months after program start. Trimmed observations is those with weights larger than 1 percent of the sum of weights for the controls, following the suggestion by Huber et al. (2013) including treated with the same estimated weights.

Table B.2. *Summary statistics of estimated PS and associated weights from DIPW, Health outcomes*

	Propensity Scores				Dynamic Inverse Prob. weights				
	Mean	min	max	obs	Mean	min	max	obs	trim
Youth Employment									
Participants	.0781	.00418	.215	970	1	1	1	970	0
Non-participants	.042	.00136	.306	21274	.0495	.00152	.441	18670	0
Other Employment									
Participants	.117	.00016	.722	396	1	1	1	396	25
Non-participants	.0128	.00002	.697	27370	.0129	.00002	2.53	25348	3
Stockholm hosts									
Participants	.0188	.0005	.227	196	1	1	1	195	0
Non-participants	.00656	.00002	.201	29308	.00665	.00002	.252	27472	0

Note: Propensity score is estimated with logistic regression. Weights are based on all information given 36 months after program start. Trimmed observations is those with weights larger than 1 percent of the sum of weights for the controls, following the suggestion by Huber et al. (2013) including treated with the same estimated weights.

Table B.4. *All statistics of the covariate balance before and after DIPW, health outcomes.*

	Before DIPW				After DIPW			
	Mean		Balance		Mean		Balance	
	T=1	T=0	ND	p-value	T=1	T=0	ND	p-value
Youth employment								
Age 25-29	.18	.362	.418	2.7e-45	.18	.185	.0115	.726
Less than high school	.526	.506	.0386	.241	.526	.525	.0006	.985
Born outside N. & W. Europe	.484	.508	.049	.137	.484	.478	.0115	.728
Own initiative to be registered	.176	.0869	.267	6.1e-13	.176	.179	.0063	.849
0 quarter at PES, at JC reg	.269	.263	.0139	.674	.269	.268	.0021	.948
Employed in t0-6	.187	.245	.142	6.6e-06	.187	.192	.0149	.648
SA in t-1	.652	.522	.264	2.5e-16	.652	.645	.0127	.699
Psychotropic drug prescr. t-12	.133	.169	.101	.0014	.133	.136	.0094	.774
Pain rel. drug prescr. t-12	.101	.109	.0265	.414	.101	.0993	.0057	.862
Hospital visit t-12	.067	.0858	.0706	.0238	.067	.0652	.0074	.822
Other employment								
Age 18-29	.127	.35	.543	4.9e-37	.127	.132	.016	.758
Age 30-39	.267	.274	.0166	.75	.267	.286	.0433	.403
Age 40-49	.369	.214	.346	7.5e-10	.369	.354	.0321	.542
Employed in t0-6	.0377	.197	.511	1.2e-54	.0377	.0417	.0205	.689
Subsidized empl in t0-6	.604	.135	1.11	7.2e-75	.604	.575	.0578	.267
Reason for SA, unemployment	.879	.726	.392	5.8e-19	.879	.876	.0069	.895
SA, nr of months t-24	17.1	10.2	.847	1.7e-71	17.1	16.6	.0621	.229
SA in t-1	.865	.629	.564	4.5e-39	.865	.864	.0024	.963
Own initiative to be registered	.0701	.0566	.0552	.313	.0701	.052	.0755	.176
Psychotropic drug prescr. t-12	.186	.25	.155	.0017	.186	.187	.0024	.964
Pain rel. drug prescr. t-12	.251	.198	.126	.0204	.251	.248	.0058	.912
Hospital visit t-12	.0728	.103	.107	.0258	.0728	.0776	.0181	.725
Stockholm host								
Age 50-	.277	.166	.269	.0006	.277	.28	.0071	.921
Less than high school	.492	.368	.252	.0006	.492	.488	.0078	.914
Born outside N. & W. Europe	.749	.666	.181	.0085	.749	.747	.0045	.95
Own initiative to be registered	.0103	.0013	.119	.215	.0103	.0047	.064	.446
0 quarter at PES, at JC reg	.113	.207	.258	.00004	.113	.115	.0079	.912
Employed in t0-6	.0564	.147	.304	5.0e-08	.0564	.0391	.081	.298
SA in t-1	.79	.614	.391	2.3e-09	.79	.79	.0007	.992
Psychotropic drug prescr. t-12	.149	.25	.256	.0001	.149	.15	.0043	.952
Pain rel. drug prescr. t-12	.144	.224	.208	.0015	.144	.143	.0008	.991
Hospital visit t-12	.123	.0992	.0761	.312	.123	.125	.0068	.924

Note: Weights used are based on all information given 36 months after program start.

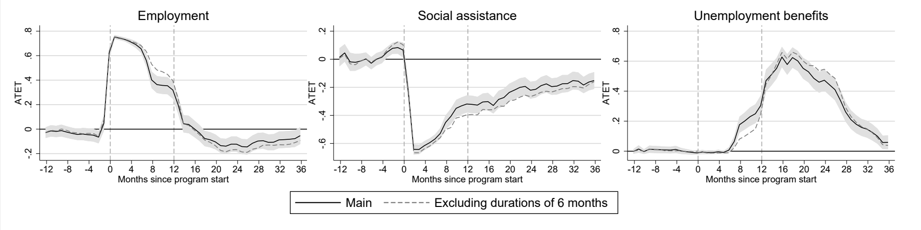
Appendix C: Additional results

Table C.1. *Cumulative ATET: Stockholm hosts excluding participants entering the program before 2012*

	Employment (months)	SA receipt (months)	UI benefits (months)
Months 12–0 before program start			
ATET	-.207	.248	.0456
St err	.27	.178	.109
Mean	1.95	9.46	.185
During program (month 1-12)			
ATET	7.3	-6.15	.658
St err	.232	.227	.127
Mean	3.51	7.44	.151
Short run outcomes, 1 year after program (13–24 months after program start)			
ATET	-.768	-3.89	6.93
St err	.378	.352	.367
Mean	4.33	5.7	.399
Medium run outcomes, 1-2 year after program (25–36 months after program start)			
ATET	-1.68	-2.49	2.72
St err	.395	.362	.283
Mean	4.87	4.65	.623
	Earnings (SEK)	SA receipt (SEK)	UI benefits (SEK)
Months 12–0 before program start			
ATET	-2,744	1,062	580
St err	2,128	2,440	663
Mean	14,785	60,522	671
During program (month 1-12)			
ATET	138,266	-40,704	2,025
St err	5,408	1,880	508
Mean	49,315	47,601	658
Short run outcomes, 1 year after program (13–24 months after program start)			
ATET	-18,818	-27,505	37,511
St err	6,687	2,261	2,327
Mean	70,315	36,726	2,615
Medium run outcomes, 1-2 year after program (25–36 months after program start)			
ATET	-33,738	-21,795	12,046
St err	7,736	2,210	1,715
Mean	85,604	31,727	4,170

Note: Means are calculated for the weighted controls. Standard errors are obtained using bootstrapping with 997 replications.

Figure C.1. ATET by month since program start: Stockholm hosts excluding participants entering the program before 2012



Note: 95% CIs (main analysis) based on 997 bootstrap replications. Weights estimated for time 1 are used for the pre-period. Treated before 2012 excluded.

Table C.2. *Estimation results: Youth employments*

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	-.00267	.0116	-.000248	.015	.00125	.00209
-11	-.00476	.0117	.00989	.0148	.00372	.0027
-10	-.00629	.0119	.00776	.0145	.00135	.00212
-9	-.00702	.0112	.00654	.0142	.00248	.00232
-8	-.005	.0117	.0183	.014	.000915	.00202
-7	-.00318	.012	.0365	.0131	-.000522	.00171
-6	-.00468	.0121	.0346	.0119	-.000505	.00156
-5	-.0178	.0124	.0331	.0121	-.000174	.00194
-4	-.0267	.0131	.0259	.0116	8.03e-06	.00179
-3	-.0336	.0132	.0443	.00918	-.000489	.0018
-2	-.0386	.0137	.0554	.0103	.000877	.0023
-1	-.014	.0146	.0651	.0118z	-.00151	.00183
0	.433	.0159	.0644	.0132	-.00282	.00154
1	.623	.0101	-.19	.0159	-.00385	.000646
2	.629	.00876	-.452	.0113	-.00329	.000567
3	.606	.00946	-.425	.0103	-.00284	.000513
4	.574	.0105	-.396	.0101	-.00264	.000507
5	.554	.0111	-.369	.0104	-.000709	.00156
6	.517	.0126	-.344	.0105	.00765	.0033
7	.429	.0143	-.317	.0107	.0396	.00633
8	.284	.0167	-.279	.0112	.103	.0101
9	.247	.0166	-.247	.0113	.124	.0113
10	.226	.0173	-.214	.0122	.14	.0114
11	.207	.0174	-.187	.0124	.119	.0109
12	.189	.0176	-.171	.0123	.111	.011
13	.163	.0178	-.169	.0126	.108	.0106
14	.155	.0176	-.161	.0125	.0984	.01
15	.143	.0177	-.16	.0127	.0986	.0105
16	.144	.018	-.147	.0131	.0889	.00986
17	.132	.0177	-.14	.0129	.0825	.00947
18	.116	.0178	-.134	.0126	.0861	.00962
19	.0969	.018	-.126	.0128	.0791	.00947
20	.107	.0182	-.128	.0128	.0659	.00891
21	.112	.018	-.115	.0129	.066	.00898
22	.123	.0179	-.125	.0126	.0513	.00832
23	.116	.0179	-.111	.0128	.052	.00852
24	.113	.0178	-.111	.0125	.0432	.00834
25	.11	.0177	-.1	.0129	.0392	.00812
26	.113	.0175	-.097	.0128	.0404	.00805
27	.105	.0175	-.0972	.0125	.0404	.00808
28	.105	.0176	-.0946	.0125	.0334	.00785
29	.112	.0179	-.09	.0121	.025	.00739
30	.112	.0175	-.0931	.0118	.0208	.00697
31	.104	.0178	-.0874	.0118	.0234	.00733
32	.0986	.018	-.0841	.0118	.0209	.00726
33	.111	.0179	-.0851	.0114	.0182	.00673
34	.112	.018	-.0718	.0119	.0239	.00723
35	.105	.0178	-.0692	.0114	.0243	.00725
36	.118	.0177	-.0735	.011	.0219	.00705

Table C.3. *Estimation results: Other municipal employments*

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	-.025	.0171	.0415	.0153	.00267	.0072
-11	-.00813	.0188	.0292	.0166	.00806	.00789
-10	-.0112	.0187	.0179	.0154	.00287	.00734
-9	-.00506	.0189	.0295	.0152	.00304	.00698
-8	-.0111	.019	.0201	.0156	.00297	.00709
-7	-.01	.0197	.0294	.0158	-.00586	.00552
-6	-.0107	.0196	.04	.0153	-.00158	.00684
-5	-.0188	.02	.0472	.0159	-.00211	.00695
-4	-.03	.02	.0373	.0161	-.00381	.00599
-3	-.0428	.0197	.0536	.0138	-.00324	.00666
-2	-.0721	.0199	.0559	.0151	-.00582	.00656
-1	-.0478	.0226	.0759	.0152	-.00476	.00661
0	.436	.0268	.101	.0166	-.00696	.00591
1	.64	.0188	-.126	.0274	-.0097	.00591
2	.669	.0153	-.561	.0231	-.0124	.00445
3	.66	.0152	-.584	.021	-.0174	.0024
4	.646	.0154	-.575	.0204	-.016	.00225
5	.63	.0159	-.549	.0204	-.017	.00226
6	.615	.0159	-.54	.0202	-.0164	.0021
7	.606	.0163	-.509	.0201	-.0101	.00489
8	.577	.0169	-.492	.0204	-.00198	.00691
9	.528	.0191	-.474	.0197	.0179	.00972
10	.489	.0208	-.447	.0196	.0235	.0108
11	.478	.0212	-.431	.0202	.0226	.0104
12	.448	.0217	-.419	.0203	.0613	.0136
13	.354	.025	-.393	.0212	.243	.0233
14	.132	.0288	-.378	.0215	.355	.0248
15	.111	.029	-.368	.0214	.393	.0253
16	.0786	.0289	-.353	.0218	.419	.0251
17	.0701	.0288	-.322	.0224	.435	.0254
18	.0774	.0289	-.305	.0213	.435	.0253
19	.0632	.0287	-.301	.0209	.416	.0251
20	.0676	.0289	-.298	.021	.418	.0251
21	.0754	.0292	-.292	.0211	.404	.0256
22	.0712	.0295	-.286	.0206	.383	.0263
23	.0746	.0294	-.267	.0224	.393	.0262
24	.0634	.0291	-.242	.0229	.383	.0255
25	.0828	.0292	-.246	.0221	.379	.0254
26	.0825	.0289	-.25	.0219	.386	.0257
27	.0702	.0287	-.248	.0216	.347	.0256
28	.0739	.0292	-.22	.022	.316	.0261
29	.0836	.0293	-.216	.0223	.268	.0248
30	.083	.0295	-.205	.0222	.254	.0249
31	.0665	.0293	-.184	.0228	.236	.0246
32	.06	.0295	-.175	.0231	.234	.0243
33	.0654	.0291	-.185	.0223	.235	.024
34	.0582	.0294	-.178	.0228	.199	.0236
35	.0536	.0295	-.167	.0223	.137	.0216
36	.0319	.0294	-.188	.021	.114	.0213

Table C.4. *Estimation results:Stockholm hosts*

	Employment		SA		UI	
Month	ATET	Std err	ATET	Std err	ATET	Std err
-12	-.029	.0224	.0138	.0331	-.00207	.00797
-11	-.0145	.0238	.0451	.032	.0145	.0126
-10	-.0191	.0235	-.0184	.0329	-.00283	.00801
-9	-.012	.0249	-.0219	.0309	.0129	.0123
-8	-.0242	.0242	-.0127	.0273	.0112	.0121
-7	-.0346	.0237	.00241	.0236	.00999	.0119
-6	-.0434	.0236	-.0282	.0224	.00817	.0114
-5	-.0403	.0254	.000526	.0225	.00754	.0115
-4	-.0455	.0265	.0142	.0219	.0115	.0124
-3	-.0491	.0268	.051	.0191	.00163	.0103
-2	-.0655	.026	.0777	.0202	-.00351	.00905
-1	.0479	.0325	.0833	.0226	-.00798	.00752
0	.623	.0252	.0657	.0273	-.0116	.00557
1	.752	.00833	-.237	.0354	-.00513	.00739
2	.742	.00827	-.642	.019	-.00934	.00533
3	.731	.00818	-.643	.0172	-.00879	.00516
4	.713	.0111	-.616	.0167	-.0038	.00724
5	.691	.0139	-.595	.0158	-.00824	.00513
6	.657	.0177	-.568	.0165	.00686	.00982
7	.56	.027	-.529	.0193	.064	.0182
8	.402	.0346	-.45	.0258	.177	.0274
9	.364	.0354	-.407	.0284	.201	.029
10	.357	.0358	-.349	.0308	.23	.0315
11	.355	.0358	-.332	.0308	.25	.0322
12	.319	.0365	-.319	.0312	.304	.034
13	.197	.0372	-.321	.0317	.47	.0376
14	.0437	.0362	-.327	.0302	.509	.0368
15	.038	.0366	-.304	.0307	.552	.0353
16	.00418	.0348	-.302	.0316	.628	.0346
17	-.0306	.0336	-.329	.0292	.578	.0352
18	-.0768	.0321	-.286	.0314	.624	.0348
19	-.0866	.0319	-.273	.0323	.601	.0343
20	-.104	.0311	-.237	.0318	.552	.0357
21	-.135	.0303	-.219	.0324	.53	.0363
22	-.139	.0307	-.201	.0332	.486	.0361
23	-.123	.0318	-.193	.0331	.46	.0359
24	-.123	.032	-.22	.0308	.473	.0355
25	-.142	.0317	-.214	.0304	.442	.0361
26	-.145	.0319	-.188	.0314	.41	.0362
27	-.115	.0326	-.175	.0318	.341	.0355
28	-.0974	.0338	-.195	.0301	.262	.0335
29	-.0944	.0337	-.191	.0306	.211	.0316
30	-.108	.0334	-.175	.032	.18	.0307
31	-.106	.0336	-.173	.0312	.158	.029
32	-.0876	.0347	-.152	.0325	.147	.029
33	-.0855	.0348	-.156	.0317	.126	.0277
34	-.0825	.0349	-.183	.0298	.0999	.0258
35	-.0776	.0349	-.162	.0303	.0577	.0231
36	-.0533	.0352	-.152	.0307	.0586	.0238

Appendix D: Sensitivity Analysis

Table D.1. *Variables included in the propensity score estimations: Youth employments*

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +LM	(7) all	(8) pooled
Age 25–29	X		X	X	X	X		
Less than high school	X		X	X	X	X		
Born outside N. & W. Europe	X	X	X	X	X	X	X	X
Own initiative to enroll at JC	X	X	X	X	X	X	X	X
0 quarter at PES as enr. at JC	X	X	X	X	X			
Employed in t0–6	X	X	X	X	X	X	X	X
SA in t–1	X	X	X	X	X	X	X	X
Age 21–23		X					X	X
Age 24–26		X					X	X
Age 27–29		X					X	X
Female		X					X	X
High school		X					X	X
Some college education		X					X	X
0–2 yrs since immigration		X					X	X
3–5 yrs since immigration		X					X	X
Job center: Globen			X				X	X
Job center: Skärholmen			X				X	X
Job center: Kista			X				X	X
Job center: Farsta			X				X	X
Job center: City			X				X	X
Year 2011				X			X	X
Year 2012				X			X	X
Year 2013				X			X	X
Year 2014				X			X	X
Year 2015				X			X	X
Year 2016				X			X	X
Psychotropic drug prescr. t–12					X		X	X
Pain rel. drug prescr. t–12					X		X	X
1–2 quarter at PES as enr. at JC						X	X	X
3–8 quarter at PES as enr. at JC						X	X	X
$>$ 8 quarter at PES as enr. at JC						X	X	X
Employed in t0-24						X	X	X
1–12 months with SA, t–24						X	X	X
13–24 months with SA, t–24						X	X	X
Period-specific parameters	X	X	X	X	X	X	X	
Indicators for assignm. periods								X

Note: $t0-6$ refers to the six months prior to enrollment at the job center, $t-1$ refers to the month prior to the assignment period, $t-12$ refers to the 12 months prior to the assignment period, and $t-24$ refers to the 24 months prior to the assignment period. LM refers to labor market history.

Table D.2. *Variables included in the propensity score estimations: Other municipal employments*

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +LM	(7) all	(8) pooled
Age 18–29	X		X	X	X	X		
Age 30–39	X		X	X	X	X		
Age 40–49	X	X	X	X	X	X	X	X
Employed in t0–6	X	X	X	X	X	X	X	X
Subsidized empl. in t0–6	X	X	X	X	X	X	X	X
Reason for SA: Unempl.	X	X	X	X	X	X	X	X
SA, no. of months t–24	X	X	X	X	X	X	X	X
SA in t–1	X	X	X	X	X	X	X	X
Own initiative to enroll at JC	X	X	X	X	X	X	X	X
Age 25–29		X					X	X
Age 30–39		X					X	X
Age 50–		X					X	X
Female		X					X	X
Married		X					X	X
Child in household		X					X	X
High school		X					X	X
Some college education		X					X	X
0–2 yrs since immigration		X					X	X
3–5 yrs since immigration		X					X	X
Born outside N. & W. Europe		X					X	X
Job center: Globen			X					
Job center: Skärholmen			X				X	X
Job center: Kista			X				X	X
Job center: Farsta			X				X	X
Job center: City			X				X	X
Year 2014				X			X	X
Year$\geq$2015				X			X	X
Psychotropic drug prescr. t–12					X		X	X
Pain rel. drug prescr. t–12					X		X	X
1–2 quarter at PES as enr. at JC						X	X	X
3–8 quarter at PES as enr. at JC						X	X	X
$>$ 8 quarter at PES as enr. at JC						X	X	X
Employed in t0–24						X	X	X
1–12 months with SA, t–24						X	X	X
13–24 months with SA, t–24						X	X	X
Period-specific parameters	X	X	X	X	X	X	X	
Indicators for assignment periods								X

Note: $t0-6$ refers to the six months prior to enrollment at the job center, $t-1$ refers to the month prior to the assignment period, $t-12$ refers to the 12 months prior to the assignment period, and $t-24$ refers to the 24 months prior to the assignment period. LM refers to labor market history.

Table D.3. *Variables included in the propensity score estimations: Stockholm host*

	(1) basic	(2) +ind	(3) +jc	(4) +time	(5) +health	(6) +LM	(7) all	(8) pooled
Age 50-	X	X	X	X	X	X	X	X
Less than high school	X		X	X	X	X		
Born outside N. & W. Europe	X	X	X	X	X	X	X	X
Own initiative to enroll at JC	X	X	X	X	X	X	X	X
0 quarter at PES as enr. at JC	X	X	X	X	X			
Employed in t0–6	X	X	X	X	X	X	X	X
SA in t–1	X	X	X	X	X	X	X	X
Age 30–39		X					X	X
Age 40–49		X					X	X
Female		X					X	X
High school		X					X	X
Some college education		X					X	X
0–2 yrs since immigration		X					X	X
3–5 yrs since immigration		X					X	X
Job center: Globen			X				X	X
Job center: Skärholmen			X				X	X
Job center: Kista			X				X	X
Job center: Farsta			X				X	X
Job center: City			X				X	X
Year 2011				X			X	X
Year 2012				X			X	X
Year 2013				X			X	X
Year 2014				X			X	X
Year 2015				X			X	X
Year 2016				X			X	X
Psychotropic drug prescr. t–12					X		X	X
Pain rel. drug prescr. t–12					X		X	X
1–2 quarter at PES as enr. at JC						X	X	X
3–8 quarter at PES as enr. at JC						X	X	X
$>$ 8 quarter at PES as enr. at JC						X	X	X
Employed in t0–24						X	X	X
1–12 months with SA, t–24						X	X	X
13–24 months with SA, t–24						X	X	X
Period-specific parameters	X	X	X	X	X	X	X	
Indicators for assignment periods								X

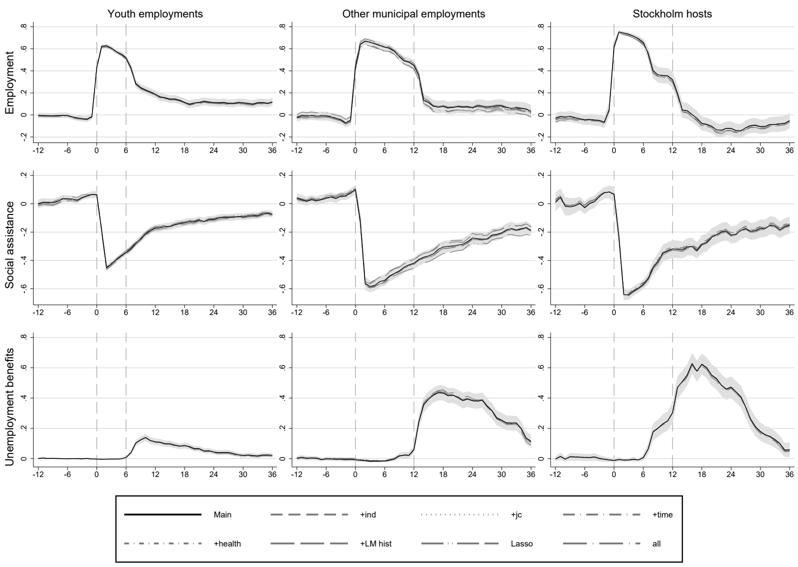
Note: $t0-6$ refers to the six months prior to enrollment at the job center, $t-1$ refers to the month prior to the assignment period, $t-12$ refers to the 12 months prior to the assignment period, and $t-24$ refers to the 24 months prior to the assignment period. LM refers to labor market history.

Table D.4. *Variables selected using the algorithm proposed by De Luna et al. (2011)*

	Youth employments			Other municipal employments		
	Emp	SA	UI	Emp	SA	UI
Age 25–29	X	X	X			
Months employed t0-6	X	X	X			
Months employed t0-24	X	X	X			
Subsidized empl. in t0–6	X		X	X		X
Subsidized empl. in t–24				X		
Ith quarter at PES, at JC reg.				X	X	
SA, nr of months t–24				X	X	
Female					X	
Job center: Kista		X	X		X	
Born in Africa , excl. NA					X	
Enrollment at job center in 2015					X	
Log earnings, t0–24, SEK 1,000				X	X	X
Log earnings, t–24, SEK 1,000				X	X	X

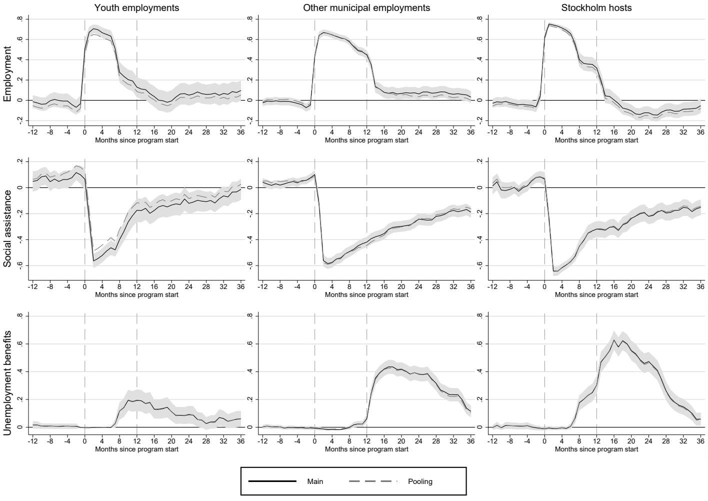
Note: $t0-6$ refers to the six months prior to enrollment at the job center, $t0-24$ refers to the 24 months prior to enrollment at the job center, $t-2$ refers to the two months prior to the assignment period, and $t-24$ refers to the 24 months prior to the assignment period. The algorithm did not select any variables for set for Stockholm hosts.

Figure D.1. ATET by month since program start: Different set of confounders



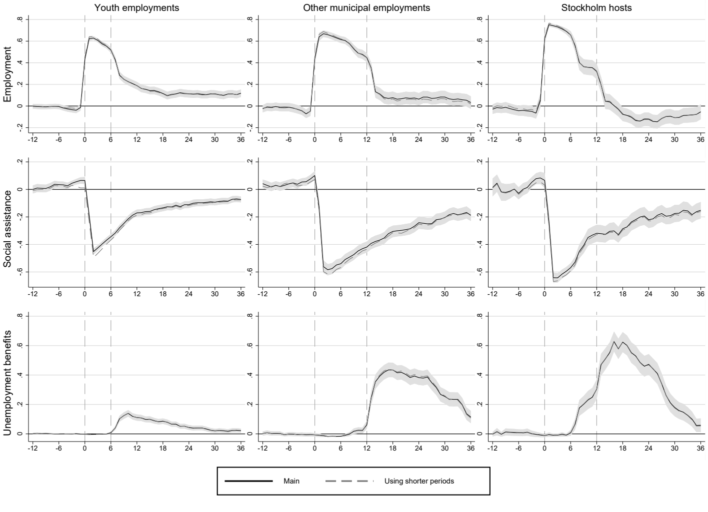
Note: 95% CIs (main analysis) based on 997 (for Youth employment and Stockholm hosts) and 995 (for Other employment) bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). See Tables D.1-D.4 for the confounders included.

Figure D.2. ATET by month since program start: Pooling over assignment periods



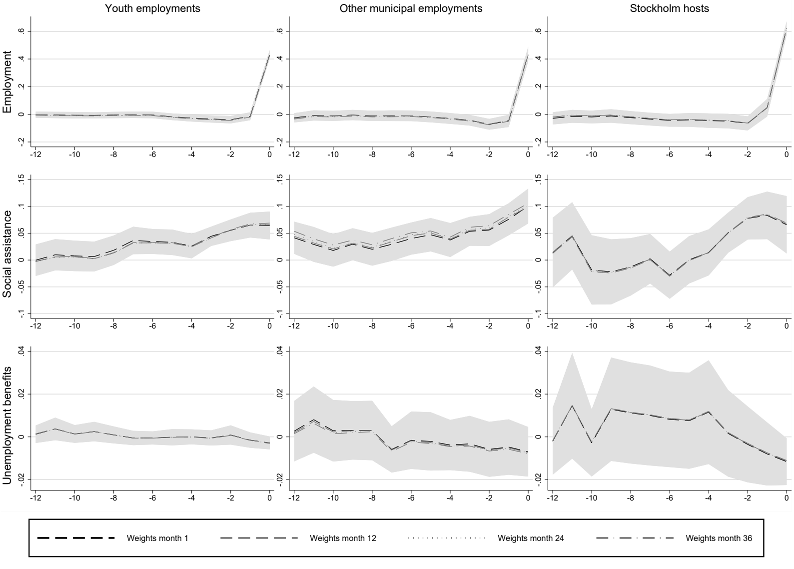
Note: 95% CIs (main analysis) based on 997 (for Youth employment and Stockholm hosts) and 995 (for Other employment) bootstrap replications. Weights estimated for time 1 are used for the pre-period (-12 to 0). See Tables D.1-D.3 for the confounders included.

Figure D.3. ATET by month since program start: Different assignment periods



Note: 95% CIs (main analysis) based on 997 (for Youth employment and Stockholm hosts) and 995 (for Other employment) bootstrap replications. Weights estimated for time 1 are used for the pre-period. See Tables A.1 and A.2 for definition of assignment periods.

Figure D.4. ATET by month before program start: Different weights



Note: 95% CIs (main analysis) based on 997 (for Youth employment and Stockholm hosts) and 995 (for Other employment) bootstrap replications. Weights estimated for time 1 are used for the pre-period.

Appendix E: Cost-benefit for the municipality

In order to conduct a cost-benefit analysis from the local government's perspective, we compare the cost of the temporary employment (salaries and payroll taxes net of employment subsidizes) with the increased tax revenues and decreased cost for social assistance. Assumptions: All Youth employments last six months, all Other municipal employments last 12 months, whereas Stockholm hosts last six months in 2010 and 2011 and 12 months thereafter. While employed, individuals receive a monthly salary of SEK 18,000 in 2010-2014 and SEK 19,000 in 2015. The most common used employment subsidy is Nystartsjobb and we use this to calculate the employment subsidy that the municipality receives. 38.5 percent of all Youth employments are subsidized, 100 percent of Other municipal employments and 90 percent of Stockholm hosts. The subsidy amounts to two payroll taxes, where the payroll tax is 31.42 percent of wages individuals older than X and 15.71 for younger employees. We assume that the higher percent applies to Stockholm hosts and Other municipal employments, whereas the lower percent applies to Youth employments. We calculate the average cost of the program for the years 2010-2015. The average tax rate in Stockholm for the years 2010-2015 was 17.46 percent, whereas it was 17.9 thereafter. We assume no tax deductions. When calculating the change in tax revenues and social assistance payments, we use the ATET from Tables 4-6.

Table E.1. *Cost-benefit for the municipality*

	Youth employments	Other municipal employments	Stockholm hosts
Salary + payroll	125,000	286,000	239,200
Subsidy	13,100	135,700	103,000
Cost of employment	111,900	150,300	136,200
Change in tax revenues	11,600	25,900	21,400
Change in SA-payments	-12,800	-42,500	-37,800
Cost of the program	**87,500**	**81,900**	**77,000**
Change in tax revenues	9,700	5,400	-7,100
Change in SA-payments	21,600	47,000	42,300
Benefit of the program	**31,300**	**52,400**	**35,200**
Cost-benefit	**-56,200**	**-29,500**	**-41,800**

3. Integrating refugee women

Co-authored with Cristina Bratu and Linna Martén

Acknowledgments: We are thankful for useful comments and suggestions from Henrik Andersson, Eva Mörk, Ulrika Vikman, Anna Sjögren and Olof Åslund, as well as seminar participants at Uppsala Immigration Lab (UIL) and Institute for Evaluation of Labor Market and Education Policy (IFAU). We gratefully acknowledge financial support from the Swedish Research Council and Ottosson thanks FORTE (Reg. No. 2016-07123) for financial support.

3.1 Introduction

In recent years, the flows of refugees to countries in Europe have increased substantially, and women make up a large share of these arrivals. While refugees as a group struggle to integrate on the labor market to a larger extent than other types of migrants (Fasani et al. 2022), the situation is even more challenging for refugee women. Across European countries, only 45% of them are employed, compared to 62% of refugee men (EC 2016). Their low employment rates are partly explained by the fact that many refugee women have had limited access to education and labor market experience in their countries of origin. If designed well, integration programs may help refugee women gain the skills needed to facilitate their entry into the labor market. However, refugee women are less likely than men to participate in such programs (Albrecht et al. 2021), potentially because they may be expected to take on most household and family responsibilities. In this paper, we explore if the design of integration programs can increase participation among refugee women, and whether these programs can improve labor market outcomes for refugee women.

We address this question by evaluating a 2010 reform of the Swedish Introduction Program (IP) for refugees.[1] The reform aimed to increase the focus on labor market integration and to tackle the gender inequality in refugees' labor market outcomes. Unlike the previous Introduction Program (Old IP, OIP), which was run by the municipalities, the new Introduction Program (New IP, NIP) was administered by the Public Employment Services (PES). This meant that participants immediately gained access to the active labor market programs (ALMPs) offered by the PES. Moreover, under the new regime, the financial benefits to participants were set at the individual level, instead of at the household level as in the previous regime. This implied that an individual's benefits would no longer be reduced if another household member found a job, as they now only depended on the individuals' own participation. This shift was expected to increase incentives to participate in the program, especially for married women, who are often the secondary earner. Just as before, one of the main components of the program was language training (Swedish for Immigrants, SFI).

Since individuals who participate in the new program are likely different than those who do not, we exploit that program eligibility was determined by the date individuals received their residency permit. We compare female and male refugees who arrived slightly before the eligibility date, to those who arrived slightly after, using a Regression Discontinuity (RD) design. We study their earnings and employment outcomes up to seven years after entering the program and find that the reform had positive effects on labor market outcomes for women, but no effects on men. For earnings, the positive effects emerge

[1] Throughout the paper we use the term "refugees" to refer to individuals who received residency permits based on refugee status (according to the Geneva convention) or being in need of subsidiary protection.

3 years after leaving the program. Looking at employment, we see positive effects already 2 years after the program ended, resulting in a 25% increase in total years with employment over the five post-program years, relative to the baseline.

We explore two main channels for the results we observe. First, the financial incentives to participate increased, in particular for married individuals whose spouses find a job: the secondary earner's benefits did no longer depend on the breadwinner finding employment. If women are more likely to be secondary earners than breadwinners, we expect the highest effect on participation among married women. We find that eligibility to the NIP increased IP participation for women by 6 percentage points, while there was no increase for men. Furthermore, in line with our hypothesis, the effects are indeed larger for married women, whose incentives to participate changed the most compared to the previous regime. These large effects on participation notwithstanding, the effects on employment and earnings are larger among single women than among married women.

Second, we posit that women benefited from connecting with and getting access to the labor market programs offered by the PES. This begs the question, why men did not benefit equally. We observe that men registered with the PES to a much larger extent than women already before the reform, i.e. while they were participating in the municipality programs. We also observe that men who were eligible to participate in the OIP catch up to those who were eligible to the NIP after the program ends. We conclude that the new program incentivized women, who would otherwise not have registered with the PES, to establish a contact with the PES directly upon arrival.

Our study contributes to the small, but growing literature on policies promoting the integration of refugees. Studies from France, Finland and Denmark show that language training and training targeted specifically toward refugees improve their labor market outcomes (Arendt et al. 2020a; Lochmann et al. 2019; Sarvimäki and Hämäläinen 2016). Battisti et al. (2019) provide evidence from Germany that easing matching frictions works in the short-run, particularly for refugees with low levels of education, while Dahlberg et al. (2020) find strong employment effects of a local Swedish program consisting of a bundle of activities (intense language training, supervised work practice, job search assistance) and collaboration between the public and the private sector. In terms of effects of financial support, Arendt et al. (2020a) find no effects on labor market outcomes due to decreasing refugees' welfare benefits. LoPalo (2019) on the other hand, found that higher cash assistance to refugees increased their wages but not employment levels, probably by improving the match between skills and jobs.

Despite the fact that refugee women have much lower employment rates compared to refugee men, few studies examine if integration programs affect men and women differently. Using data from Denmark, Arendt (2022) examines a work-first policy aimed at refugees and finds positive effects for men but not women, which is partly explained by women's lower participation in the program. Helgesson et al. (2020) find positive effects for both men and women when they evaluate a job search assistance program in Sweden that provided direct matches with vacancies and was tailored to the specific circumstances faced by refugee women (lack of previous labor market experience, family responsibilities). We add the important insight that early access to the PES improved outcomes for women, who had a much lower registration rate than men before the reform. Moreover, we show that increasing the financial incentives to participate in the program increased the participation rate for women, without having any positive impact on labor market outcomes. This highlights that while financial incentives may be a useful tool to impact participation, the program content also needs to be adjusted to fit the needs for those who are potentially the least likely to enter the labor force.

We discuss the institutional background in the next section. Section 3.3 introduces our data and sample selection choices. In Section 3.4 we present our empirical strategy. We show and discuss our results in Section 3.5, and Section 3.6 concludes the paper.

3.2 Institutional background

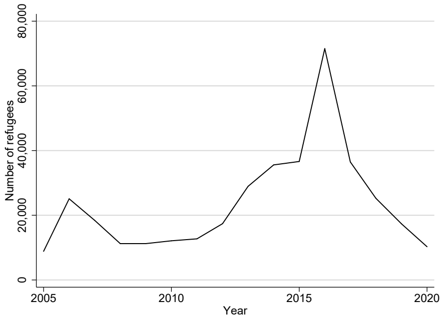
The majority of refugees coming to Sweden apply for asylum upon arrival.[2] The number of refugees who receive asylum each year fluctuates, but during the last 15 years, on average 24,000 individuals were granted a permit to stay each year (see Figure 3.1). After submitting their application, they are offered temporary housing by the Swedish Migration Agency (SMA), but also have the option of finding housing on their own. Once the application has been reviewed and approved, a process that typically takes a year, refugees can enroll in the IP. The program is offered to refugees and their family members (provided they arrive within a certain time span after the refugee). Participation is not mandatory, but is required to receive financial benefits.[3]

Before the reform in 2010, the IP was administered and organized by the 290 municipalities, which were reimbursed by the central government with a fixed amount per refugee. Municipalities are Sweden's lowest administrative level of government, and they are responsible for key policy areas like child care, K-12 education, and local infrastructure. While the central government and the PES are formally responsible for organizing ALMPs in Sweden, municipalities can organize their own activities – financed by the municipalities them-

[2]Sweden also accepts 5,000 UNHCR refugees each year, but they are given a residency permit before arrival.

[3]Participating in the IP is in principle the only way to receive financial support for individuals without work impediments. Refugees can apply for social assistance, but this is only provided as a last resort, i.e. to individuals who can not get assistance from other providers, such as from participating in the IP.

Figure 3.1. Refugees receiving residency permit, 2005–2020



Note: Source: The Swedish Migration Agency.

selves – which are mainly focused on recipients of social assistance. In the old Introduction Program (OIP), a caseworker at the municipality would outline a personal integration plan in consultation with the refugee, which typically lasted for two years (IV 2007). The plan always contained Swedish language courses (Swedish for Immigrants, SFI) and Swedish civic orientation classes, which were provided by the municipality. If possible, it also included an internship, but this varied by municipality (Eriksson 2011). Participants could still register at the PES, and apply for jobs using their services. Most municipalities have their own PES office, but smaller ones can share an office.[4] Participants were paid the OIP benefits, which were set at the household level, and which varied in size across municipalities, although most municipalities paid an amount at or somewhat higher than the national norm for social assistance.[5] Municipalities could decide whether the amount should be reduced if individuals earned additional income, and surveys to the municipalities suggest that almost 90% of individuals who earned additional income also had their OIP benefits reduced (IV 2004).[6]

The OIP was criticised for lacking labor market focus. The quality and content of ALMPs offered to participants also varied substantially depending on municipality of residence. While some municipalities, like Stockholm, had local job centers offering an array of ALMPs similar to the PES, some smaller municipalities did not provide any ALMPs (see e.g. Forslund et al. (2019) and Vikman and Westerberg (2017)). There were also concerns that women were not participating. The fact that the benefits were set at the household level and often decreased with additional income, meant that (married) women had

[4] In 2010 the PES had 232 offices (PES 2010).

[5] The amount typically depended on household size as well as the occurrence of additional income sources (Röttorp 2008).

[6] Some municipalities used social assistance instead of the OIP benefits. While the OIP benefits were always conditional on participating in the program, the requirements for social assistance recipients was determined by each municipality.

lower incentives to participate if their husbands found employment (Röttorp 2008).

A large reform was implemented in 2010 with the aim of speeding up labor market integration, providing equal access to ALMPs and improving gender equality for newly-arrived refugees. It consisted of a number of changes to the OIP. First, the responsibility for the program was centralized, being shifted from the municipalities to the PES. This had the intention to decrease variation in access to ALMPs between municipalities. Second, the new NIP benefits, paid out by the PES, were calculated at the individual level and were no longer means-tested. This implied that if an individual, or another household member, found a job while the individual continued to participate in the program, his/her NIP benefits were not reduced (Eriksson 2011). Third, labor market guides were introduced. These were provided by private actors, and were supposed to help the participants by providing labor market connections and advice. Finally, the PES also took over the responsibility for assisting refugees in finding housing. We return to these aspects when we discuss potential mechanisms in Section 3.5.3.

Other important parts of the integration program did not change due to the reform. After participants registered for the program, caseworkers (now at the PES) would still define a 2-year individualized integration plan, with content tailored to the needs of the refugee.[7] Just as in the previous program, the plan always included SFI and civic orientation classes, which were still provided by the municipalities (Eriksson 2011).

Previous studies of the NIP have estimated fixed effect models which compare the outcomes for refugees who received their permit up to one year before or after the introduction of the NIP. They find positive employment and earnings effects in the short to medium run for both men and women (Andersson Joona et al. 2016; Qi et al. 2021). As described in Section 3.4, we instead employ an RD design to capture the causal effect of the reform.

3.3 Data and descriptive statistics

3.3.1 Data sources and sample selection

We have individual-level administrative data covering the full Swedish population for the years 2009-2017. Our data was compiled by Statistics Sweden and includes information from multiple sources, such as the Swedish Tax Agency,

[7]Data on the content of these individual plans reveal that caseworkers at least to some extent tailor the integration plan to the qualifications of the participant. For example, low-educated men and women are more likely to be assigned to study programs and subsidized employment than high-educated individuals (Table A.1). However, looking at the number of hours spent in a given program per week (conditional on participation), the variation across education groups is small. It thus seems that the tailoring with respect to program content takes place on the extensive rather than the intensive margin.

the Swedish Migration Agency (SMA), and the Swedish Public Employment Service (PES).

Importantly, we can observe which individuals were given a residency permit based on being a recognised refugee or a person in need of subsidiary protection. We use the term refugee to refer to either one of these groups. We also observe if they found housing on their own or if they were assigned to a municipality by the SMA. Furthermore, we know when they submitted their asylum application and when they were granted a residency permit.

We use this information to sample refugees who were given a residency permit within one year of the reform cutoff that applies to them according to the eligibility rules that we described in the next section. Those arriving up to one year before the cutoff are eligible for the OIP, while those arriving up to one year after are eligible for the NIP. We further restrict the sample to individuals who were between 20 and 64 in the year they received a permit. We do not include quota refugees or family members of refugees, who would also be eligible for the program, since the process for arriving to Sweden and obtaining residency permits for these groups is very different from the process other types of refugees go through. We also exclude refugees who receive asylum for exceptionally distressing circumstances (*synnerligen ömmande omständigheter*), including medical reasons, which may prevent them from working. Finally, we exclude a few individuals (2.8% of the sample) who, during their first two years in Sweden lived in a municipality which did not use OIP benefits, since there was no legal requirement for them to participate in the OIP to receive benefits. This leaves us with a sample consisting of 10,700 individuals, of which 46 % are women.

3.3.2 Outcomes

In our main analysis, we study the effect of the 2010 reform on annual earnings and employment. Annual earnings consist of earnings from both labor income and self-employment, are measured at the yearly level and reported in Swedish kronor (SEK) adjusted for inflation using 2014 as a base year. The employment variable takes the value 1 if an individual has positive earnings in a given year, and 0 otherwise. We also construct a variable equal to 1 for regular employments, where we have excluded all forms of subsidized employments from the employment measure. Alternatively, for both employment measures, we also report the number of years an individual has been employed in a given period.

To investigate potential mechanisms behind our main results, we look at some additional outcomes related to program participation and content. We study registration and annual participation in ALMPs at the PES. We also have access to information about enrollment, level and grades in SFI courses. We use indicator variables for having passed any SFI course, and having passed an SFI course at the highest level (D) as proxies for language skills.[8]

Since we have access to start and end dates for each SFI course, and are able to impute the date of registration at the PES,[9] we can use the date of receiving a residency permit as the reference point as we measure these outcomes.

3.3.3 Program participation before and after the reform

Table 3.1. *Benefits and activities by reformed IP eligibility, year 2 in Sweden*

	(1)	(2)	(3)	(4)
	Women		Men	
	Eligible	Not eligible	Eligible	Not eligible
NIP benefits> 0	0.92	0.01	0.93	0.01
OIP benefits> 0	0.08	0.90	0.09	0.92
Total benefits, SEK	690.90	597.45	730.18	655.79
Whereof				
OIP or SA benefits	50.34	592.95	36.69	651.56
NIP benefits	640.56	4.49	693.49	4.29
Disposable income	1132.67	1070.44	1061.77	955.22
SFI enrollment	0.78	0.76	0.83	0.80
Hours SFI	316.69	326.80	326.95	346.30
Pass SFI given enrollment	0.61	0.60	0.66	0.66
Study	0.13	0.11	0.17	0.17
PES registration	0.98	0.67	0.98	0.83
Subsidized employment	0.06	0.06	0.19	0.15
Given PES registration:				
In ALMP (at PES)	0.39	0.22	0.52	0.31
Unemployed with impediment	0.16	0.24	0.04	0.13
Observations	2229	2720	2609	3142

Notes: The table includes all refugees receiving a residency permit one year before or after the reform, and measures benefits and participation 1 year after residency permit, to ensure that those arriving in e.g. December could enter the IP when the annual variables are measured. NIP refers to new IP benefits and OIP to old IP benefits. ”Unemployed with impediment” is used by the PES to label individuals assessed to be impeded from participating in the labor market. Table A.2 provides the same information conditional on benefit receipt.

Depending on when a refugee receives their residency permit, they are eligible to participate either in the OIP or the NIP. We summarize what this implies

[8]SFI is structured in three so-called study paths, each of which offers two courses: SFI 1 (A and B), SFI 2 (B and C), SFI 3 (C and D). D is the most advanced course. A participant starts on the path that is best suited to their educational background and can make their way towards course D on the third study path (for more details, see Åslund and Engdahl (2018)).

[9]We know the number of days per year an individual is registered at the PES, as well as the year an individual first registered.

in terms of benefit receipt and activity participation for each gender, the year after receiving a residency permit, in Table 3.1.

The share receiving the benefits associated with a given IP is high both before and after the reform for both genders. Note that this does not necessarily mean that participation was equally high both before and after the reform. The benefits were calculated at the household level before the reform, so an individual with a non-zero benefit amount before the reform may have lived with a participant without being a participant themselves.

An alternative way of comparing participation in the programs before and after the reform is by looking at SFI enrollment, as it was a central part of both programs. Enrollment in SFI during year two in Sweden increases by 2 and 3 percentage points for women and men, respectively, and men are still 5 percentage points more likely to be enrolled in SFI than women, even after the reform. While the likelihood of passing an SFI course does not change, the number of hours in SFI during a given year decreases. This could indicate that participants pass SFI courses faster.

We also see that the average benefit amount received per person increases, and is SEK 7,000–9,000 higher after the reform. This is also reflected in higher disposable income after the reform, implying that the NIP benefits were higher than the OIP benefits, on average.

One of the aims of shifting the responsibility of the integration program from the municipalities to the PES was to increase the labor market focus. Judging solely by the likelihood of having registered at the PES, this goal seems to have been reached: almost all men (98%) and women (98%) are registered at the PES after the reform. The pre-reform gender gap in PES registration of 16 percentage points gets effectively eliminated after the reform. Table 3.1 also shows that men become 4 percentage points more likely to have a subsidized employment after the reform, and that both women and men are much more likely to participate in any ALMP provided by the PES. The PES also seems to categorize registered refugees differently after the reform. The share of refugees that caseworkers label as ”unemployed with impediment” (thereby likely not assigning them to an ALMP) decreases both for women and for men. However, since ALMP participation and labeling change more for men than for women, inequality with regard to access to ALMPs at the PES seems to persist and even become somewhat larger.[10]

[10]Note that there is no national-level database on ALMPs provided by the municipalities, and we can thus not show how overall ALMP participation changes.

Table 3.2. *Summary statistics by eligibility to the reformed IP and sex*

	(1)		(2)		(3)		(4)	
	Women				Men			
	Eligible		Not eligible		Eligible		Not eligible	
	mean	sd	mean	sd	mean	sd	mean	sd
Age	33.11	10.76	31.77	10.47	32.67	10.06	32.51	9.92
Married	0.63	0.48	0.77	0.42	0.58	0.49	0.76	0.43
Children	1.20	1.65	1.20	1.75	0.97	1.68	1.22	2.03
Child	0.50	0.50	0.47	0.50	0.37	0.48	0.39	0.49
Primary School	0.64	0.48	0.71	0.46	0.50	0.50	0.57	0.50
High School	0.18	0.39	0.14	0.35	0.26	0.44	0.21	0.41
University	0.18	0.38	0.13	0.33	0.24	0.43	0.21	0.41
Somalia	0.36	0.48	0.65	0.48	0.33	0.47	0.64	0.48
Afghanistan	0.08	0.27	0.02	0.15	0.10	0.30	0.03	0.16
Iraq	0.14	0.35	0.11	0.31	0.14	0.34	0.12	0.33
Iran	0.07	0.25	0.03	0.18	0.09	0.29	0.05	0.22
Eritrea	0.19	0.39	0.10	0.30	0.13	0.34	0.06	0.24
Recognized refugee	0.31	0.46	0.20	0.40	0.37	0.48	0.22	0.42
Own housing	0.51	0.50	0.60	0.49	0.54	0.50	0.60	0.49
New IP	0.70	0.46	0.00	0.02	0.78	0.41	0.00	0.03
Ever new IP	0.97	0.18	0.01	0.09	0.97	0.18	0.01	0.08
Observations	2233		2725		2613		3150	

Notes: The sample includes refugees who arrive 365 days before and after the new IP eligibility threshold. New IP refers to the program after the 2010 reform, and ever new IP to being registered as a participant at some point 2010–2017.

3.3.4 Description of the sample

Table 3.2 shows summary statistics by gender and by eligibility to the NIP for refugees in our sample, measured during the first year in Sweden. We start by highlighting a few noteworthy differences between the groups of eligible and non-eligible refugee women, described in columns 1 and 2. There are much fewer married women among the eligible than among the non-eligible (63% compared to 77%), and eligible women have higher levels of education on average.[11] There are also large differences in country of origin composition; for example, there are significantly fewer women from Somalia among the eligible than among the non-eligible group. Large changes in the composition of country of origin is likely explained by the timing of conflicts in the sending countries and, as shown in Figure B.5, is not specific to the months around of

[11] Information on immigrants' education obtained outside of Sweden is often missing for the very first years in Sweden. In Table 3.2, we therefore use the first non-missing value available in the registers. Note that the PES reports education information to Statistics Sweden, and since more individuals eligible for the NIP register with the PES relative to those eligible for the OIP, there are fewer missing data points to begin with for the former group in the registers, i.e. we do less imputing for them (Andersson Joona et al. 2016).

the cutoff date. Eligible women are also more likely to have received asylum as recognized refugees, and less likely to have lived in own (i.e. not SMA-assigned) housing during the asylum process. The groups are otherwise similar in terms of age and number of children. Finally, we can see that while 70% of eligible women are enrolled in the NIP during their first year in Sweden, 97% eventually participate in the program. For men (columns 3 and 4), differences between eligible and non-eligible individuals in our sample roughly follow the same pattern.

3.4 Empirical strategy

3.4.1 Specification

To identify the causal effect of being eligible for the NIP, we exploit the fact that assignment to the program was based on specific cutoff dates, which varied across different groups of refugees. In particular, refugees were eligible if they received their residency permit after October 31, 2010.[12] However, refugees who received their permit before October 31 were also eligible if they still lived at SMA facilities on November 30, i.e. had not found housing and registered in a municipality before this date. For these two groups, we calculate eligibility cutoffs by taking the difference in days (counting only weekdays) between the date of permit receipt and the two corresponding cutoff dates.

We then estimate the following reduced-form equation:

$$y_i = \alpha + \beta D_i + \gamma_1 f(d_i - d_0) + \gamma_2 D_i f(d_i - d_0) + X_i\theta + u_i \qquad (3.1)$$

where y_i is the outcome variable (e.g. earnings, employment) for refugee i; D_i is an indicator variable taking the value one for those receiving a permit after the cutoff date that applies to their respective group; d_i is the date of permit receipt; d_0 is the eligibility cutoff date; $f(d_i - d_0)$ is a local linear polynomial of the running variable; X_i is a vector of covariates measured during the year of arrival and u_i is the error term. We use a triangular kernel to assign larger weights to the observations closer to the cutoff. For each outcome of interest y_i we select the optimal bandwidth according to Calonico et al. (2019). The parameter of interest is β, which measures the jump in the outcome variable at the cutoff, i.e. the effect of being eligible for the NIP on the outcome.

Figure B.1 shows the estimates of β in equation 3.1 with the probability of ever participating in NIP as a dependent variable, separately for women (Panel a) and men (Panel b). The figure conveys the fact that the eligibility thresholds we construct correctly identify participants in the NIP. As discussed above, refugees who received their residency permit before the cutoff dates were eligible to participate in the OIP, hence essentially no one who arrives before those dates participates in the NIP.

[12] Quota refugees or relatives who had a permit upon arrival were eligible if they arrived after November 30, 2010. They are however not included in our sample, see Section 3.3.1.

3.4.2 Validity checks

The empirical strategy outlined above identifies the causal effect of the reform only if certain conditions are fulfilled. The most important requirement is that individuals should not be able to manipulate the date of permit receipt in order to get access to the reformed program. This is unlikely to be a concern for several reasons. First, there was already an integration program in place before the reform and prima facie it is not obvious that the new IP would be better than the old IP. Second, even if the reformed program was perceived to be more desirable than the old regime, it is unlikely that individuals would delay their departure from the host country given that they are refugees fleeing unstable conditions. Delaying submitting an asylum application once in Sweden is also unlikely, given that access to housing and benefits is conditional on having a pending application. Finally, while applicants may decide when to submit an application, they have no control over when a decision is reached, as the time to a decision depends on caseworkers' caseload and the difficulty of the cases being assessed. However, even in the absence of manipulation of the date of permit receipt on the part of applicants, Swedish Migration Agency (SMA) caseworkers may have processed applications in such a way as to select only individuals with certain predetermined characteristics, expected to succeed in the NIP. This is also unlikely to be a concern because the NIP was implemented by the PES, while applications were processed by the SMA and the two authorities are not likely to coordinate with each other in this fashion.

Nonetheless, we perform the density tests proposed by Cattaneo et al. (2020) and McCrary (2008) to more formally check for manipulation. In the absence of manipulation, the density of individuals near the threshold should be continuous. Table A.3 and Figure B.2 show no evidence of a discontinuity in the density of individuals around the cutoff.[13]

Even if the number of applicants is balanced, there could be temporal trends in refugee flows, due to, for instance, ongoing conflicts and weather conditions, which can affect the applicants' characteristics. To test for this more formally, we run equation 3.1 using the baseline characteristics in Table 3.2 as dependent variables.[14] Tables A.4–A.5 show the estimated discontinuities from these regressions for women and men, respectively. The only variables with statistically significant jumps at the cutoff are the indicator variables for being born in Iraq and Eritrea (for women) and Iran (for men). Differences in country of origin composition are likely driven by the timing of conflicts in the sending countries, rather than manipulation. To show that large sudden

[13] Our specification uses time as a running variable, and as noted by Hausman and Rapson (2017), we may run into specific issues given the time series nature of the data. We believe that these issues are minor in our setting, given that we see no signs of anticipation effects and we have high frequency data, and can thereby rely on a narrow bandwidth.

[14] As mentioned in the previous section, the quality of the data on educational attainment likely improved as a consequence of the reform, which is why we do not include education in this exercise.

changes in the number approvals for refugees originating from different countries is not a specific feature of the months around the reform cutoff, Figure B.5 displays trends in the number of approvals and applications by country of origin. The figure confirms that there is much variations at other moments of time as well, and that the number of approvals covaries with the number of applications submitted. Due to these compositional differences in terms of country of origin, we condition on country of birth fixed effects in all specifications. Tables A.4–A.5 also report the estimated discontinuities conditional on country of birth fixed effects, showing that the sample is balanced in terms of individual characteristics at the threshold using this specification. Figures B.3–B.4 visually show the balance test for each variable for the two samples.

Table 3.3. *Post program 5-year cumulative labor market effects (2013–2017)*

	Total earnings		Employment (years)		Regular empl. (years)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: women						
NIP eligibility	642.195	630.872	0.420**	0.382**	0.328**	0.302**
	(401.493)	(389.499)	(0.174)	(0.166)	(0.144)	(0.138)
Baseline	2300.74	2300.74	1.68	1.68	1.14	1.14
N	1389	1389	1574	1574	1850	1850
BW, days	73	73	86	86	102	102
Panel B: men						
NIP eligibility	-276.783	-52.385	-0.038	0.030	-0.085	-0.035
	(515.343)	(487.556)	(0.169)	(0.155)	(0.162)	(0.151)
Baseline	5538.49	5538.49	2.97	2.97	1.77	1.77
N	1494	1494	1851	1851	1851	1851
BW, days	75	75	94	94	93	93
Add. covariates	No	Yes	No	Yes	No	Yes

Notes: Country of birth fixed effects are included in all specifications. Additional covariates include age, age^2, marital status, number of children, has children, and reason to receive permit, all measured the first year in Sweden. Total earnings are measured in hundreds of SEK and employment is measured in number of years. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Figure B.6 visually represents the estimates from columns 1, 3 and 5. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

3.5 Results

3.5.1 Baseline results

This section presents the main results on labor market outcomes: total earnings, the number of years with any employment and the number of years with regular (non-subsidized) employment. We focus on the results for women and contrast them to the results for men.

We start by looking at the cumulative labor market outcomes during a five-year period after the program ended (2013-2017) in Table 3.3. For each outcome, the first column displays the results from the baseline specification, where we include country of birth fixed effects, and the second column the results when we add additional controls for individual and household characteristics. Column 1 in Panel A shows that eligibility to the NIP increased total earnings by 64,200 SEK (around 6,400 EUR) for women.[15] Given that the average cumulative earnings for non-eligible women amounts to 230,100 SEK, this effect represents a sizeable 27.9% increase. However, there is a lack of precision and the estimated effects are not statistically significant. Controlling for additional individual characteristics (Column 2) gives a slightly lower estimated effect on total earnings of 63,100 SEK (27.4%).

Next, we study the effect of eligibility to NIP on the number of years with any employment during the five-year follow up. As In Column 3, we observe a positive effect of 0.42 years, which corresponds to a 25% increase from the baseline. Adding covariates reduces the point estimate to 0.38 years (Column 4), or 22.7%, significant at the 5% level. To investigate whether the employment effects are driven by subsidized employment, we also look at the effect on regular employment only, in columns 5–6. Eligibility to the NIP also has an effect on regular employment, in the range of 0.33 to 0.30 years (compared to the baseline of 1.14), implying that a sizable fraction of the positive effect on employment is driven by regular employment.

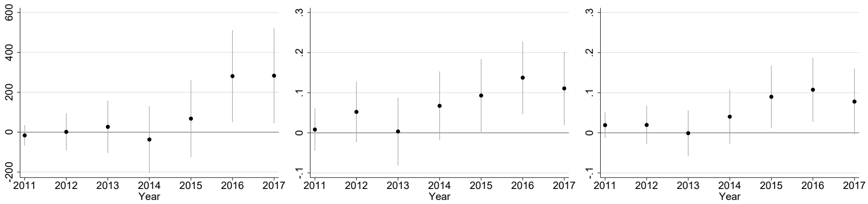
We thus find economically important positive effects of eligibility to the NIP on labor market outcomes of refugee women, in particular on the extensive margin (employment). However, the reform did not have the same positive post-program effects on men's labor market outcomes, as Panel B in Table 3.3 shows. The point estimates tend to be small in magnitude, and statistically insignificant.[16] We note that the average earnings and number of years with employment are roughly twice as high for non-eligible men as compared to non-eligible women, which could be one reason why men's labor market outcomes are not affected by the reform.

A natural follow-up question is when the positive effects for refugee women materialize with respect to the start of the program. In Figures 3.2a–3.2c, we plot the annual effects of the reform and the corresponding 95% confidence

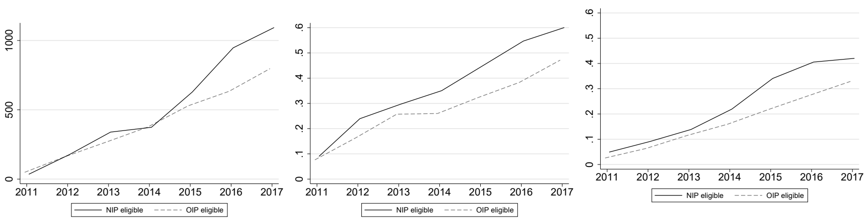
[15] Including all years since residency permit does not affect the results, see Table A.6. Individuals who leave the sample before this 5-year time window ends are excluded from the main analysis (1.3% among eligible women and 2.3% among non-eligible women). If we include them, the results are similar. As shown in Tables A.4–A.5, these is no discontinuity in the likelihood of leaving the sample at the cutoff.

[16] The lack of positive effects on labor market outcomes for men is at odds with previous evaluations of the NIP (Andersson Joona et al. 2016; Qi et al. 2021). The broader time window used in previous evaluations (a year/258 weekdays vs. 75–94 weekdays used in our analysis) is one potential explanation for finding positive effects for men. Figure B.9 shows that the cumulative effects for men become more positive the longer the bandwidth.

Figure 3.2. Yearly labor market effects, women



(a) Earnings *(b)* Employment *(c)* Regular employment



(d) Earnings, averages *(e)* Employment, averages *(f)* Regular employment, averages

Notes: Figures 3.2a–3.2c plot the RD point estimates, conditional on country of birth fixed effects, and the corresponding 95% CIs, and 3.2d–3.2f the average outcomes. Earnings are measured in hundreds SEK, and the employment measures indicate if an individual is employed (any or regular) in a given year. Bandwidths are selected using Calonico et al. (2019) for the cumulative outcomes, see bandwidth in Table 3.3. The sample is restricted to women. Figure B.8 visually represents the estimates for 2015–2017.

intervals (CIs) for each year from 2011 to 2017, for earnings, employment and regular employment, respectively.[17] We see that the effect on earnings is close to zero in the first five years since the reform, after which it gradually increases to almost SEK 30,000 in 2016–2017. For employment, the positive effect emerges in 2014, and continues to increase, with some fluctuations, until 2017 when it has reached 10 percentage points.[18] Figure 3.2c shows that effects on regular employment emerge in 2015, which suggests that the earlier effects on employment are driven by subsidized employment. Together, these results suggest that in the short-run, likely while the program is ongoing, NIP-eligible women are more likely to participate in subsidized employment than OIP-eligible women. In the medium- to long-run, they are more likely to establish themselves on the labor market and obtain regular employment, which materializes in larger earnings over time. In Figures 3.2d-3.2f, which plot the average outcomes over time for eligible and non-eligible individuals, respectively, we see that the positive trend for the eligible group slows down

[17] To keep the estimation window fixed for a given outcome (earnings, employment, regular employment) over all years, we use the same bandwidth that was selected using Calonico et al. (2019) for the cumulative outcomes, see bandwidth in Table 3.3.

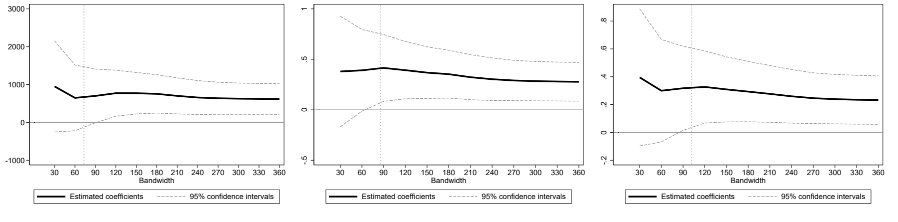
[18] Using a more strict definition of employment, whether annual earnings exceeds the 25th annual earnings percentile, gives somewhat smaller but similar results, see Figure B.10.

in 2017, while the non-eligible group continues to catch up. Comparing the trends in employment and regular employment for the non-eligible group, it seems that the positive trend in the share with a subsidized employment slows down just after the end of the OIP. This could for example indicate that there is a transaction cost in having different actors running the IP (municipalities) and the agency responsible for providing ALMPs after the end of the IP (the PES), as in the old regime, causing a delay in access to ALMPs, like subsidized employments. The yearly effects on earnings and employment for men are shown in Figure B.7 in the Appendix. They fluctuate somewhat over time, but they are close to zero.

3.5.2 Sensitivity checks

We test the robustness of our main results in a number of ways. We start by testing how sensitive our results are to the choice of using the Calonico et al. (2019) optimal bandwidth. In Figure 3.3, we show how the estimated effects and corresponding 95% CIs change as we vary the bandwidth between 30 and 360 days. In general, the point estimates are robust to varying the bandwidth, except for the most narrow window (30 days), for which the estimated effects are larger. Standard errors are also very large close to the eligibility cutoff date. The results for men are somewhat more sensitive to the choice of bandwidth, see Figure B.9, but they are not statistically different from zero at the 5% level for any of the bandwidths.

Figure 3.3. Main results with varied bandwidths, women



(a) Earnings *(b)* Employment *(c)* Regular employment

Notes: Country of birth fixed effects are included in all specifications. The black line displays the point estimates and the gray lines the 95% CIs. Estimation method: local linear regression with triangular kernel with varying bandwidths. The vertical line shows the Calonico et al. (2019) optimal bandwidth.

Figures B.14a–B.14c in the Appendix plot the annual effects using four different sets of bandwidths, and show that these point estimates are also fairly robust to this exercise, even if standard errors are, as expected, larger for smaller bandwidths. Furthermore, Figures B.14d–B.14i confirm that the estimates are not affected by the choice of polynomial (first order) and kernel (triangular) used in the main analysis.

We also perform a placebo analysis, using the corresponding cutoff dates one year after the reform to determine eligibility to the placebo reform. We thus expect to see no effects on earnings and employment, for either women or men. Figures B.14j–B.14l reassuringly confirm the absence of such placebo effects.

3.5.3 Mechanisms

Positive effects of the reform emerge for women, but not for men. In the following section we explore potential explanations for this finding. As described in Section 3.2, a number of features distinguish the NIP from the OIP. With our setup, we compare individuals who arrive soon after the reform to individuals who arrive soon before. Since the latter group is likely exposed to activities within the OIP, we need to first understand the differences and similarities between the two programs and how participation in various integration activities changes as a consequence of the reform.

Strengthened economic incentives

One of the main aims of the IP reform was for the new benefit structure to provide stronger individual incentives to participate in the program than the OIP benefit structure. Ideally, we would study how the reform affected individuals' actual participation in the two programs. Since benefits were calculated at the household level before the reform, we cannot use benefit receipt as an indicator of program participation. However, we know that SFI was part of both the OIP and the NIP, and was also typically one of the first program activities. We can therefore use participation in SFI as a proxy for participation in both the OIP and the NIP and study the effects of the reform on SFI enrollment to understand if participation rates were affected by the reform.

Table 3.4 shows the effect of NIP eligibility on enrollment in SFI at any point during our time period (columns 1–2) and within the first two years since receiving one's residency permit (columns 3–4). The latter is likely a better way to measure participation in the IPs. We see that for women, the likelihood of ever taking a language course does not change, while we see a 6 percentage point increase in the course take-up within the first two years. This indicates that the new individualized benefit structure did encourage more women to participate. For men, however, we see no effect on either participation measures. To examine if women also followed through and finished the language course, columns 5–6 show the effect on having passed an SFI class during the first two years since receiving a residency permit. The results show that they are 11 percentage points more likely to pass a course within this time span. This also suggests that women obtained Swedish language skills earlier as a result

Table 3.4. *Effects on participation in SFI*

	SFI ever		SFI first 2 years		Pass first 2 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Women						
NIP eligibility	0.005	-0.001	0.061**	0.060**	0.107*	0.107**
	(0.022)	(0.020)	(0.026)	(0.026)	(0.056)	(0.053)
Baseline	.95	.95	.9	.9	.61	.61
N	1415	1415	1653	1653	1256	1256
BW, days	75	75	90	90	64	64
Panel B: Men						
NIP eligibility	-0.027	-0.023	-0.014	-0.008	-0.021	-0.008
	(0.026)	(0.026)	(0.028)	(0.028)	(0.041)	(0.040)
Baseline	.96	.96	.96	.96	.74	.74
N	1389	1389	1410	1410	2045	2045
BW, days	67	67	68	68	103	103
Ad. covariates	No	Yes	No	Yes	No	Yes

Notes: Country of birth fixed effects are included in all specifications. Additional covariates include age, age^2, marital status, has children, number of children and reason to receive permit, all measured upon arrival. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Figure B.11 visually represents the estimates from columns 1, 3 and 5 for women. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

of the reform.[19] Furthermore, the fact that the latter effect is larger than the effect on the likelihood of enrolling in SFI courses the first two years implies that also women who would have participated in the IP under the old regime learned Swedish faster.

A possible explanation for the increased participation of women is that the NIP benefits were calculated at the individual, as opposed to the household level. This switch should have affected women in couples disproportionately more. One way to explore this is to divide the sample by civil status, measured during the first year in Sweden.[20] Columns 1 and 3 in Table 3.5 show the results for married women, and columns 2 and 4 for single women. We start by noting that the baseline share participating in and passing SFI is 6 and 8 percentage points lower for married women compared to single women before the reform, which is in line with the relatively weak incentives for married women to participate before the reform. The results in Table 3.5 show that the increased program participation among women is clearly driven by married women, who are 14 percentage points more likely to enter SFI the first two years (compared to 4 percentage points for single women). This indicates that

[19] As shown in Table A.7, the likelihood of passing any or a D-level SFI class during our entire follow-up period is not affected.

[20] More specifically, we can identify individuals living in the same household who are married or who have children together. For simplicity, we refer to all as married. Cohabiting couples who do not have children together are categorized as single.

the new benefits were successful in strengthening the incentives for women in couples to participate. For men, we see no such indications (Table A.8).

Table 3.5. *Effects on participation in SFI, by civil status, women*

	SFI first 2 years		Pass first 2 years	
	Married	Single	Married	Single
	(1)	(2)	(3)	(4)
NIP eligibility	0.140**	0.037	0.277**	0.063
	(0.066)	(0.028)	(0.130)	(0.061)
Baseline	.85	.91	.54	.62
N	304	1349	227	1029
BW, days	90	90	64	64

Notes: Country of birth fixed effects are included in all specifications. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

We next examine if the improved incentives and subsequent increase in participation among married women in particular can explain the positive effects on labor market outcomes. Table 3.6 shows the effects of the reform on earnings and employment by civil status. The results in Table 3.6, showing larger point estimates for single than married women, do not support that this mechanism is behind the improved labor market outcomes, as the estimated coefficients on total earnings and years of employment are larger for single women.

Table 3.6. *Post program labor market effects by civil status, women*

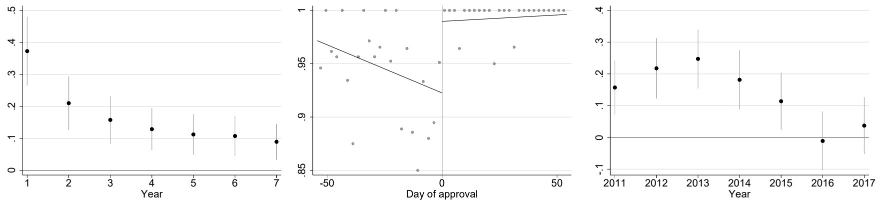
	Total earnings		Employment (years)	
	Married	Single	Married	Single
	(1)	(2)	(3)	(4)
NIP eligibility	261.175	826.132*	0.256	0.505***
	(1004.757)	(439.303)	(0.408)	(0.192)
Baseline	2677.26	2230.7	1.7	1.68
N	254	1135	287	1287
Bandwidth, days	73	73	86	86

Notes: All specifications include country of birth fixed effects. Total earnings are measured in hundreds of SEK, (any and regular) employment measured in number of years. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01*. The corresponding effects for men are presented in Table A.8.

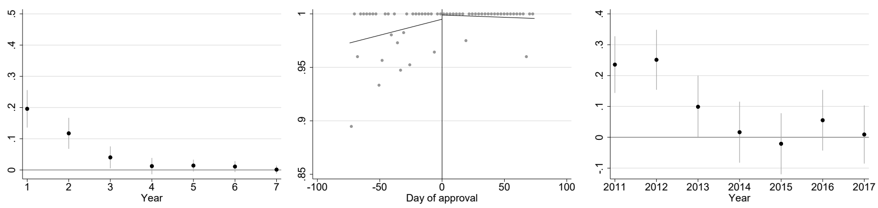
Changed program content

Another potential explanation for why we see positive labor market effects of the 2010 reform for women is that the content of the NIP improved due to the shift of responsibility from the municipalities to the PES, e.g. by making the content more labor market oriented, as was one of the main aims of the reform.

Figure 3.4. Yearly effects on PES enrollment and ALMP participation



(a) Registered at PES, women *(b)* Ever PES, women *(c)* In ALMP, women



(d) Registered at PES, men *(e)* Ever PES, men *(f)* In ALMP, men

Notes: Figures 3.4a and 3.4d are based on monthly information, years represent years since receiving a residency permit. Figures 3.4c and 3.4f represent calendar year, as we only have data on the annual level. They plot the RD point estimates and the corresponding 95% CIs. Calonico et al. (2019) optimal bandwidths are computed the cumulative measures, and kept fixed each year. Country of birth fixed effects are included in all specifications.

We start by exploring how contact with the PES changed as a consequence of the reform in Figure 3.4. We see that both eligible women and men are more likely to register with the PES – which is unsurprising given that the responsibility for managing the program switched from municipalities to the PES (Figures 3.4a and 3.4d).[21] However, the magnitude is almost twice as high for women during the first year in Sweden. Another difference is that, for men, the observed rise in PES registration declines rapidly after the program has ended. For women, eligibility to the NIP increases the likelihood of having registered at the PES by 10 percentage points, also at the end of our time period. Figures 3.4b and 3.4e further show the effects on the likelihood of ever being registered with the PES. These results indicate that the NIP makes some women connect with the PES, who would otherwise have remained outside the labor force, and that it speeds up the time it takes for both women and men to register with the PES. Given that being registered at the PES is an indication of being in the labor force and searching for a job, this is a clear improvement of women's' labor market attachment.[22]

[21] In the OIP, whether registration with the PES was mandatory varied by municipality.

[22] To further explore if access to PES is an important factor, we use the fact that access to PES varied between the municipalities even before the reform. We divide the sample into quartiles

We can also study what individuals do at the PES. Figures 3.4c and 3.4f show how the effect on participation in ALMPs organized by the PES in a given calendar year evolves over time. We see that, for both women and men, the NIP increases the likelihood of being assigned to an ALMP at the PES, which again is unsurprising since the PES is now responsible for the NIP. Just as with PES registration, men eligible for the OIP catch up to those eligible for the NIP after the program has ended, and the two groups are equally likely to participate in ALMPs at the PES after that. For women, we see that those eligible for the OIP do not catch up until year 6. Thus, due to the reform, many women take part in ALMPs that they would otherwise not have had access to. However, this does not necessarily mean that individuals eligible for the OIP did not participate in any ALMPs. As previously mentioned, in the OIP, municipalities should have provided ALMPs if it was possible.

Table 3.7. *Effects on registration at PES and participation in ALMPs, by civil status, women*

	Ever PES registration		Ever ALMP at PES	
	Married	Single	Married	Single
	(1)	(2)	(3)	(4)
NIP eligibility	0.065	0.074***	0.182*	0.264***
	(0.050)	(0.027)	(0.107)	(0.047)
Baseline	.95	.95	.57	.6
N	198	920	271	1215
Bandwidth, days	54	54	80	80

Notes: Country of birth fixed effects are included in all specifications. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. **p<0.1, **p<0.05, ***p<0.01.* The corresponding effects for men are presented in Table A.8.

To examine if the positive effects on PES registration and ALMP participation is only explained by the increased IP participation driven by married women, Table 3.7 shows the effects of eligibility to the reformed IP on the likelihood of ever registering and participating in ALMPs at the PES, by civil status. While the point estimates are positive for both groups, they are larger for single women. This pattern is thus in line with the labor market effects by civil status shown in Table 3.6, and indicates that the early contact with the PES could have caused the positive effects on employment and earnings.

based on the share of refugees who were registered at the PES in the pre-period. The point estimates in Table A.9 imply that the positive effects are driven by individuals living in municipalities in the lowest quartile in terms of registration rate, i.e. locations that are likely to have seen larger increases in PES registration rates following the reform.

Other potential mechanisms

We consider two additional mechanisms that could have caused the improved labor market outcomes for women. First, the responsibility for assigning individuals in need of housing assistance to municipalities shifted from the SMA to the PES when the IP was reformed. If the PES started to assign refugees differently, e.g. focused more on assigning them to a municipality where they had better labor market prospects, this could have improved their outcomes. There is however no obvious reason why this would affect women and men differently, and evaluations have found that the availability of housing and where refugees are willing to accept to move were still the main determinants of where refugees were assigned after the reform (RiR 2014b). In Table A.10 and Figure B.13, we divide the sample into refugees who found housing on their own during the asylum process, and those who lived in asylum centers organized by the SMA and are thus more likely to be assigned to municipalities (by either the SMA or PES) after receiving a residency permit. There are no conclusive differences in labor market effects across housing categories, which speaks against this being the explanation.

Second, the program introduced the use of private guides, who would provide social support and help refugees find employment or enroll in regular education. The refugees could choose what company to use, which was expected to encourage competition and improve outcomes. Sibbmark et al. (2016) evaluated the program and found small differences in performance between the different companies. Interviews revealed that the guides primarily provided social support and focused less on work related activities. The system was abolished in 2015 after being criticized for having an inadequate labor market focus and insufficient control of the private providers (RiR 2014a). Given that the weekly time spent with the guides only reached an hour on average for the refugees in our sample (see table A.1), and the tendency to focus on social support, we think it is unlikely to have produced the labor market improvements we observe.

3.6 Conclusion

The labor market attachment of refugee women tends to be weak in most European countries, and many women never enter the labor market (EC 2016). The slow labor market integration we see for these women is very costly, both from an individual and societal point of view. Given that employment is likely to be beneficial for developing host language skills and meeting natives, it may also harm refugee women's social integration.

We study the reforms to the Swedish Introduction Program (IP) for refugees in 2010. The reform shifted the responsibility for the IP from the municipalities to the Public Employment Service (PES), aiming to speed up the labor market entry by immediately registering refugees as job seekers. It also aimed at increasing gender equality by making benefits individualized. In the previous system the benefit level was means-tested at the household level, and incentives to participate would decrease for all family members as soon as one member found a job.

First, we provide evidence that the reformed IP program increased women's annual employment by 10–15 percentage points, starting 2 years after the program ended. We also observe increases in earnings, although these occur a few years later. We see no effects on men's labor market outcomes. Second, we find that the reform increased the program participation rate for women (but not men) by 6 percentage points. The effect is stronger among married women, and thereby likely to be explained by the increased financial incentives to participate. Yet, we do not observe that the improved labor market outcomes are driven by this group. This may be due to the fact that the increased program participation is driven by women who, for various reasons cannot or do not intend to find employment. This suggests that although the program was successful in activating this group, more is needed to support their transition to employment.

Our leading hypothesis is that one of the main reasons behind the employment gains for women was the fact that the program increased the share of women who got access to ALMPs through the PES. Before the reform, access to such programs varied between municipalities, and the reform thereby equalized refugees' access to ALMPs across the Swedish municipalities. Taken together, our results suggests that it is possible to achieve lasting employment gains for refugee women and that an early connection to the PES, irrespective of individuals' past labor market experience, may be important.

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Appendix A: Additional tables

Table A.1. *Mean program content by sex and educational attainment*

	(1)	(2)	(3)	(4)
	Women		Men	
	High	Low	High	Low
	mean	mean	mean	mean
New IP	1.00	1.00	1.00	1.00
Full time IP	0.97	0.96	1.00	1.00
Labor market program	0.75	0.78	0.85	0.85
SFI	0.91	0.90	0.94	0.95
Guidance	0.90	0.88	0.93	0.91
Work preparation	0.65	0.63	0.66	0.71
Subsidized employment	0.05	0.11	0.17	0.18
Other studies	0.04	0.17	0.09	0.16
Number of hours				
Labor market program	6.08	5.02	6.86	6.35
SFI	13.54	11.41	11.77	10.87
Guidance	0.98	0.94	0.99	0.97
Work preparation	6.05	4.69	5.36	5.45
Civic orientation	0.79	0.89	0.59	0.60
Subsidized employment	16.17	13.95	16.69	16.19
Other studies	11.11	19.75	16.93	19.39
Observations	1311	744	1240	1179

Notes: Only participants are included in this table. *High* refers to participants with at least high school education, *Low* to participants with at most primary school education. Panel B shows the average number of hours given that a given program is part of the integration plan.

Table A.2. *Benefits and activities by benefit type*

	(1)	(2)	(3)	(4)
	Women		Men	
	New IP mean	Old IP mean	New IP mean	Old IP mean
Benefits, SEK	719.10	641.51	765.77	707.76
Whereof:				
OIP or SA benefits	41.38	640.96	30.68	707.26
NIP benefits	677.73	0.55	735.09	0.50
Disposable income	1150.06	1082.12	1066.22	972.17
Enrolled in SFI	0.82	0.80	0.86	0.83
Hours SFI	334.09	350.73	345.55	367.31
Pass SFI given enrolled	0.62	0.60	0.67	0.67
Study	0.13	0.11	0.17	0.18
PES registration	1.00	0.70	1.00	0.85
Subsidized employment	0.06	0.06	0.18	0.15
Given PES registration:				
In ALMP (at PES)	0.41	0.22	0.54	0.29
Unemployed with impediment	0.14	0.23	0.02	0.13
Observations	2061	2312	2422	2696

Notes: The table includes all refugees arriving one year after the reform receiving any NIP benefits or before the reform receiving any OIP benefits. Benefits and participation are measured the year after residency permit, to ensure that those arriving in e.g. December could enter the IP when the annual variables are measured. ”Unemployed with impediment” is used by the PES to label individuals assessed to be impeded from participating in the labor market.

Table A.3. *Density tests*

	Discontinuity	Standard err.	P-value	Bandwidth, L	Bandwidth, R
Women					
McCrary	.0235	.0995	.	77.9	77.9
RD density	.	.0004143	.8664443	57.08578	76.67067
Men					
McCrary	-.1277	.091	.	92.2	92.2
RD density	.	.0003872	.6254219	55.74259	59.77549

Notes: The McCrary test uses the bandwidth selection calculation from McCrary (2008), and the RD density test from Cattaneo et al. (2020).

Table A.4. *Balance in covariates, women*

	estimate	t-statistic	bandwidth	observations
Age	-1.72	-1.28	63	1380
Married	.0394	.65	52	1194
Child	.00124	.0247	85	1732
Children	.217	1.18	68	1474
Somalia	.0178	.284	50	1162
Afghanistan	.00157	.0646	71	1496
Iraq	-.106	-2.97	67	1456
Iran	-.0397	-1.59	57	1306
Eritrea	.155	2.89	52	1192
Statutory refugee	-.0941	-1.89	61	1344
Own housing	-.0684	-1.18	56	1282
Wait time (days)	-52.4	-2.25	47	1104
Leave	-.00481	-.285	106	2114
Conditional on CoB fixed effects:				
Age	-1.49	-1.27	71	1496
Married	.0421	.879	68	1472
Child	.0249	.488	78	1624
Children	.156	.937	81	1676
Statutory refugee	-.0204	-.506	69	1480
Own housing	-.00145	-.0274	55	1268
Wait time (days)	-28.3	-1.45	52	1186
Leave	-.00797	-.467	102	2044

Notes: CoB refers to country of birth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths.

Table A.5. *Balance in covariates, men*

	estimate	t-statistic	bandwidth	observations
Age	.996	1.03	100	2198
Married	-.0265	-.507	69	1594
Child	-.0136	-.264	74	1672
Children	-.0332	-.157	71	1614
Somalia	-.00514	-.0812	53	1296
Afghanistan	.015	.583	71	1608
Iraq	-.0414	-.941	55	1320
Iran	.0499	1.78	68	1576
Eritrea	-.016	-.453	73	1644
Statutory refugee	.0258	.453	53	1302
Own housing	-.0402	-.699	65	1512
Wait time (days)	-16	-.753	61	1426
Leave	-.00455	-.203	83	1842
Conditional on CoB fixed effects:				
Age	1.55	1.35	71	1608
Married	-.00464	-.109	85	1866
Child	.00626	.12	68	1576
Children	.0435	.217	75	1672
Statutory refugee	.0147	.334	63	1470
Own housing	-.0458	-.795	55	1320
Wait time (days)	-19.8	-1.01	58	1388
Leave	-.00324	-.147	78	1750

Notes: CoB refers to country of birth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths.

Table A.6. *Labor market effects (cumulative since time of arrival)*

	Total earnings		Employment (years)		Regular employment (years)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Women						
NIP eligibility	636.67	633.95	0.420**	0.382**	0.328**	0.302**
	(435.56)	(421.87)	(0.174)	(0.166)	(0.144)	(0.138)
Baseline	2542	2542	1.68	1.68	1.14	1.14
N	1374	1374	1574	1574	1850	1850
BW, days	72	72	86	86	102	102
Panel B: Men						
NIP eligibility	-197.4	15.80	-0.038	0.030	-0.085	-0.035
	(539.26)	(512.51)	(0.169)	(0.155)	(0.162)	(0.151)
Baseline	6144	6144	2.97	2.97	1.77	1.77
Add. covariates	No	Yes	No	Yes	No	Yes
N	1600	1600	1851	1851	1851	1851
BW, days	81	81	94	94	93	93

Notes: Country of birth fixed effects are included in all specifications. Additional covariates include age, age^2, marital status, has children, number of children and reason for permit, all measured upon arrival. Total earnings are measured in hundreds of SEK, employment (any and regular) in number of years. Baseline is the average outcome among individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Table A.7. *Effects on SFI grades*

	Pass any SFI course		Pass SFI course D level	
	(1)	(2)	(3)	(4)
Panel A: Women				
NIP eligibility	0.077*	0.060	0.049	0.039
	(0.042)	(0.038)	(0.050)	(0.046)
Baseline	.82	.82	.33	.33
N	1361	1361	1455	1455
BW, days	71	71	77	77
Panel B: Men				
NIP eligibility	-0.036	-0.024	-0.038	-0.024
	(0.036)	(0.035)	(0.050)	(0.049)
Baseline	.84	.84	.39	.39
Ad. covariates	No	Yes	No	Yes
N	1994	1994	1617	1617
BW, days	101	101	81	81

Notes: Country of birth fixed effects are included in all specifications. Additional covariates include age, age^2, marital status, has children, number of children and reason for permit, all measured upon arrival. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Figure B.12 visually represents the estimates from columns 2 and 4.* *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Table A.8. *Effects by civil status, men*

	(1)	(2)	(3)	(4)
	SFI first 2 years		Pass first 2 years	
Panel A	Married	Single	Married	Single
NIP eligibility	-0.001	-0.025	0.049	-0.043
	(0.018)	(0.036)	(0.080)	(0.048)
Baseline	.97	.95	.67	.75
N	327	1062	466	1579
BW, days	68	68	103	103
	Total earnings		Employment (years)	
Panel B	Married	Single	Married	Single
NIP eligibility	-35.451	-340.690	-0.204	0.053
	(1019.299)	(584.412)	(0.320)	(0.201)
Baseline	5632.16	5515.95	2.91	2.98
N	352	1142	431	1420
BW, days	75	75	94	94
	Register PES		ALMP at PES	
Panel C	Married	Single	Married	Single
NIP eligibility	-0.012	0.011	-0.006	0.011
	(0.011)	(0.008)	(0.072)	(0.043)
Baseline	.99	.98	.82	.8
N	352	1142	373	1206
BW, days	74	74	79	79

Notes: Country of birth fixed effects are included in all specifications. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Table A.9. *Post-program labor market effects by municipal PES registration rate, women*

	Total earnings		Employment (years)	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
NIP eligibility	-488.252	745.405	0.065	0.339
	(1193.261)	(913.846)	(0.457)	(0.392)
Baseline	2784.73	1869.73	1.86	1.31
N	223	310	252	353
BW, days	73	73	86	86

Notes: High PES registration rate refers to the quarter of municipalities with the highest registration rate for refugees the 2 first years in Sweden (measured 2009) and low to the quarter with the lowest registration rate. Country of birth fixed effects are included in all specifications. Total earnings are measured in hundreds of SEK, employment in number of years. Baseline outcome is the average among individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Table A.10. *Effects on labor market outcomes, by asylum housing*

	Total earnings		Employment (years)	
	SMA housing	Own housing	SMA housing	Own housing
	(1)	(2)	(3)	(4)
Panel A: Women				
NIP eligibility	489.814	641.394	0.519**	0.242
	(551.182)	(555.539)	(0.234)	(0.254)
Baseline	2483.65	2093.78	1.76	1.59
N	723	666	797	777
BW, days	73	73	86	86
Panel B: Men				
NIP eligibility	-314.670	-486.252	-0.062	-0.057
	(684.546)	(773.589)	(0.232)	(0.245)
Baseline	5397.87	5676.67	2.98	2.95
N	716	778	868	983
BW, days	75	75	94	94

Notes: SMA housing refers to living at asylum centers organized by the Swedish Migration Agency, and own housing to those who organize their own housing. Country of birth fixed effects are included in all specification. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

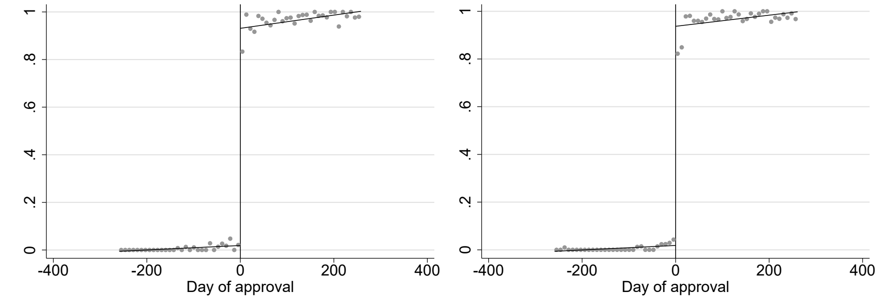
Table A.11. *Sensitivity: Placebo reform 2011, Labor market effects (2014-2017)*

	Total earnings		Employment (years)		Regular employment (years)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Women						
Placebo eligibility	-296.033	-340.515	-0.028	-0.061	-0.174	-0.206
	(342.237)	(335.366)	(0.179)	(0.173)	(0.144)	(0.142)
N	1475	1475	1339	1339	1463	1463
BW, days	80	80	72	72	80	80
Panel B: Men						
Placebo eligibility	4.180	-234.028	-0.009	-0.091	-0.251	-0.343**
	(418.206)	(403.886)	(0.119)	(0.112)	(0.156)	(0.148)
Add. covs	No	Yes	No	Yes	No	Yes
N	1512	1512	2241	2241	1483	1483
BW, days	69	69	102	102	68	68

Notes: Country of birth fixed effects are included in all specifications. Additional covariates include age, age^2, marital status, has children, number of children and reason for permit, all measured upon arrival. Total earnings are measured in hundreds of SEK, employment (any and regular) in number of years. Baseline outcome is the average among untreated individuals left of the cutoff, within the range of bandwidth. Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. * *p<0.1,* ** *p<0.05,* *** *p<0.01.*

Appendix B: Additional figures

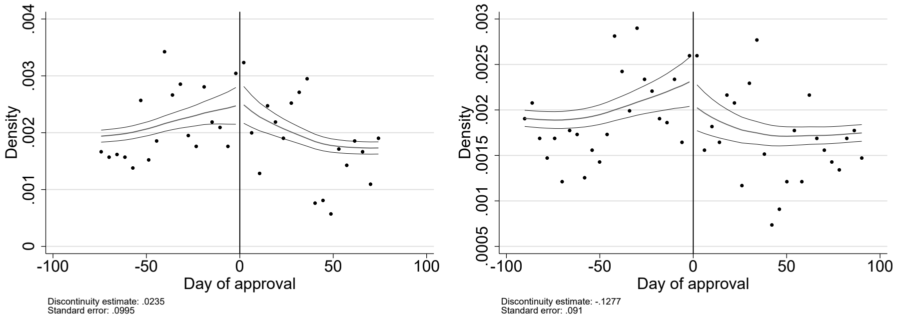
Figure B.1. Participation in NIP by date of residency permit approval



(a) NIP participation, women

(b) NIP participation, men

Figure B.2. McCrary test

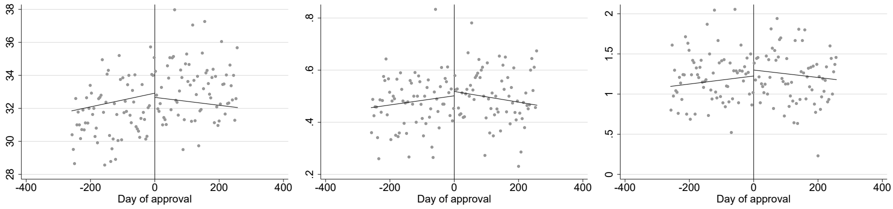


(a) McCrary test, women

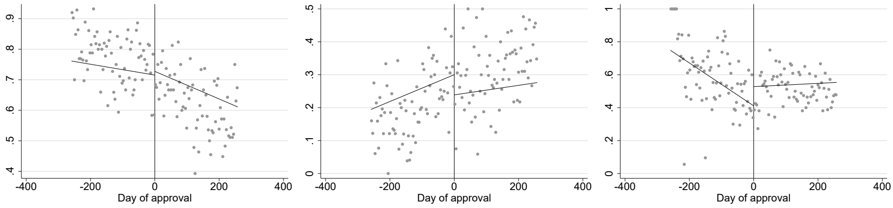
(b) McCrary test, men

Notes: Uses the bandwidth selection calculation from McCrary (2008).

Figure B.3. Balance in covariates conditional on country of birth fixed effects for women



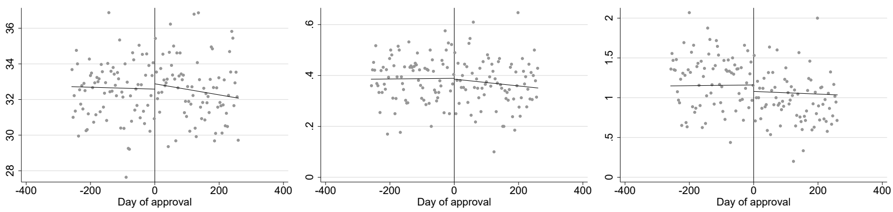
(a) Age *(b)* Has child *(c)* Children



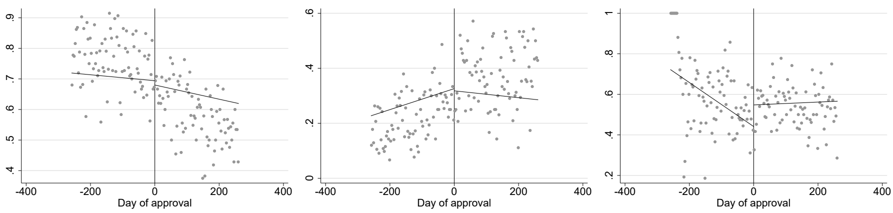
(d) Married *(e)* Statutory refugee *(f)* Own Housing

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths.

Figure B.4. Balance in covariates conditional on country of birth fixed effects for men



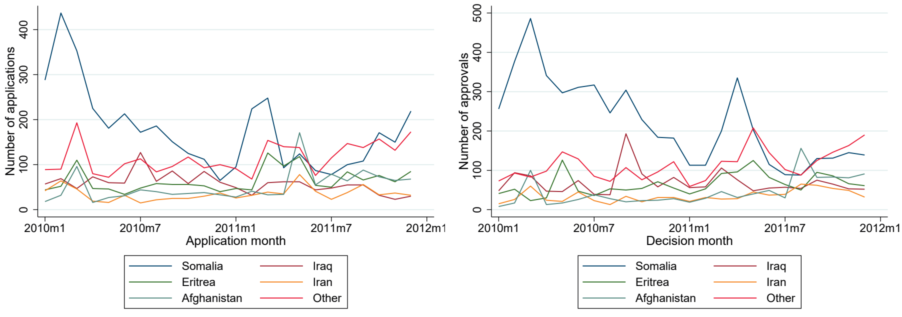
(a) Age *(b)* Has child *(c)* Children



(d) Married *(e)* Statutory refugee *(f)* Own Housing

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths.

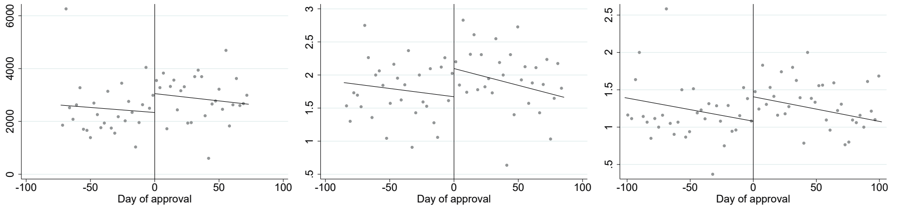
Figure B.5. Applications submitted and permits granted by country of birth, 2010–2011



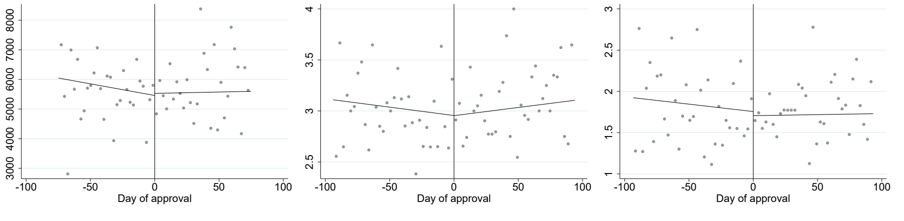
(a) Applications

(b) Permits

Figure B.6. Labor market effects 2013–2017



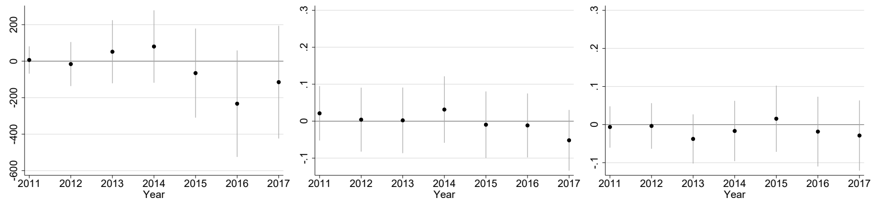
(a) Total earnings, women *(b)* Years employed, women *(c)* Years regular empl., women



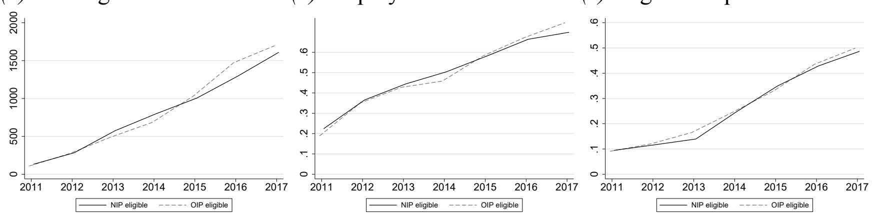
(d) Total earnings, men *(e)* Years employed, men *(f)* Years regular empl., men

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Country of birth fixed effects are included.

Figure B.7. Labor market effects for men



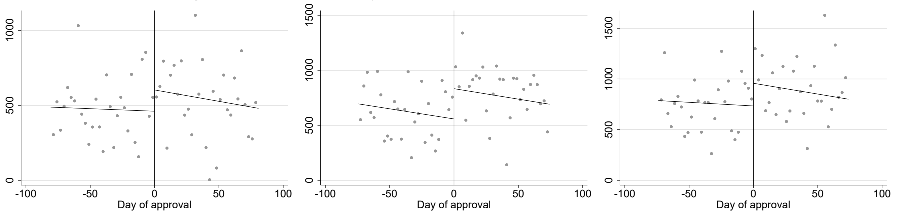
(a) Earnings *(b)* Employment *(c)* Regular empl.



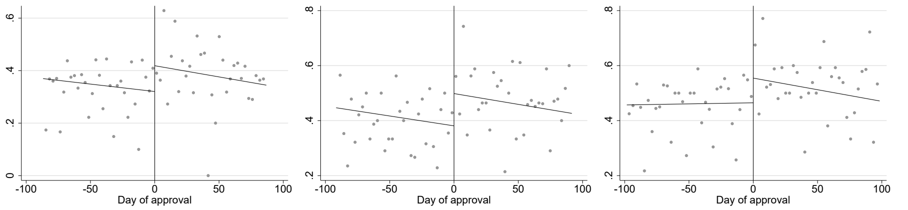
(d) Earnings, averages *(e)* Employment, averages *(f)* Regular empl., averages

Notes: Country of birth fixed effects are included in Figure B.7a–B.7c. Earnings are measured in hundreds SEK, and the employment measures (any and regular) indicate employment in a given year. Bandwidths are selected using Calonico et al. (2019) for the cumulative outcomes, see bandwidth in Table 3.3. The sample is restricted to men.

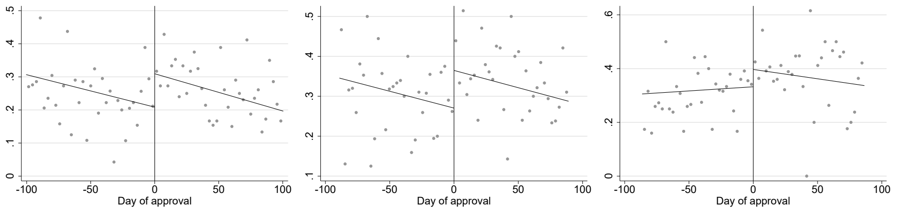
Figure B.8. Yearly labor market effects, women



(a) Earnings, 2015 *(b)* Earnings, 2016 *(c)* Earnings, 2017



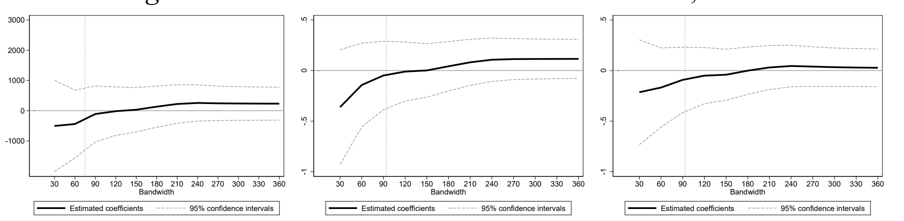
(d) Employment, 2015 *(e)* Employment, 2016 *(f)* Employment, 2017



(g) Regular employment, 2015 *(h)* Regular employment, 2016 *(i)* Regular employment, 2017

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Country of birth fixed effects are included.

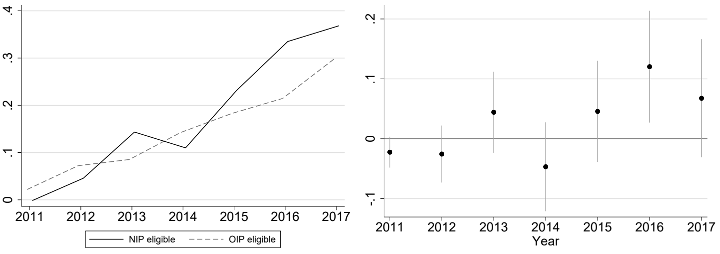
Figure B.9. Main results with varied bandwidths, men



(a) Earnings *(b)* Employment *(c)* Regular employment

Notes: Country of birth fixed effects are included in all specifications. The black line displays the point estimates and the gray lines the 95% CIs. Estimation method: local linear regression with triangular kernel with varying bandwidths. The vertical line shows the Calonico et al. (2019) optimal bandwidth.

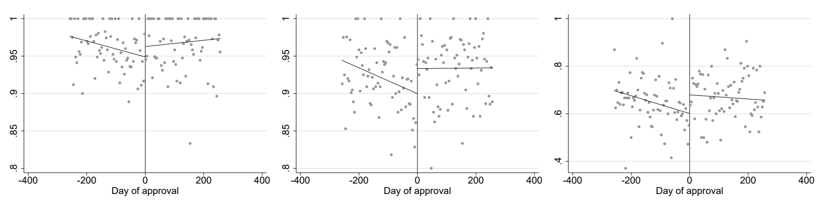
Figure B.10. Stricter employment measure, women



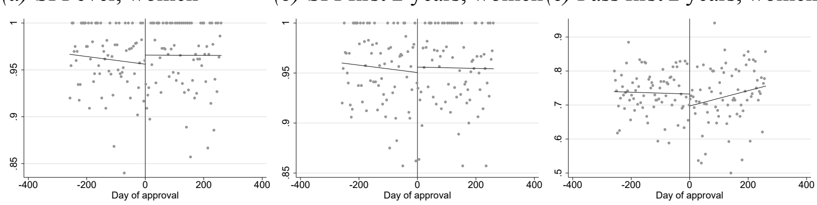
(a) Earnings>25th percentile, means *(b)* Earnings>25th percentile, effects

Notes: Left panel: share among eligible OIP (dashed line) and NIP (solid line) with earnings>25th percentile. Left panel: plot the RD point estimates and the corresponding 95% CIs. Calonico et al. (2019) optimal bandwidths are computed the total number of years with Earnings>25th percentile, and kept fixed each year. Country of birth fixed effects are included.

Figure B.11. Effects on SFI/program participation



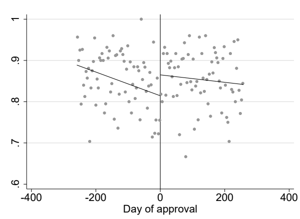
(a) SFI ever, women *(b)* SFI first 2 years, women *(c)* Pass first 2 years, women



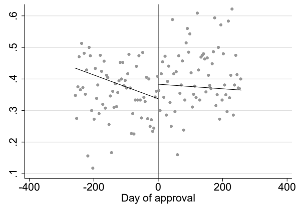
(d) SFI ever, men *(e)* SFI first 2 years, men *(f)* Pass first 2 years, men

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Country of birth fixed effects are included.

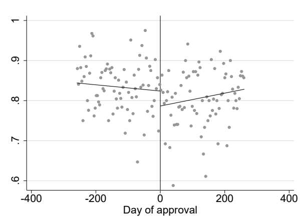
Figure B.12. Effects on SFI grades



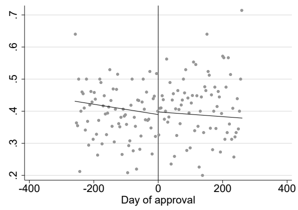
(a) Pass any SFI course, women



(b) Pass D level SFI course, women



(c) Pass any SFI course, men



(d) Pass D level SFI course, men

Notes: Estimation method: local linear regression with triangular kernel and Calonico et al. (2019) optimal bandwidths. Country of birth fixed effects are included.

Figure B.13. Effects on labor market outcomes, by asylum housing, women

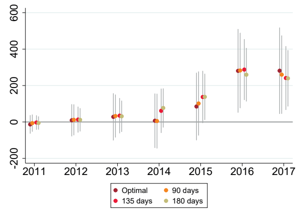
(a) Earnings

(b) Employment

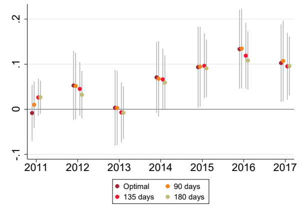
(c) Regular employment

Notes: Country of birth fixed effects are included in all specification. RD point estimates and the corresponding 95% CIs are plotted. Earnings are measured in hundreds SEK, and the employment measures (all and regular) indicate employment in a given year.

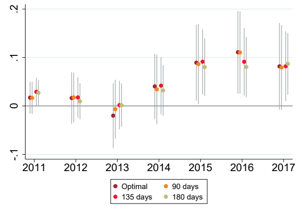
Figure B.14. Sensitivity checks, annual effects



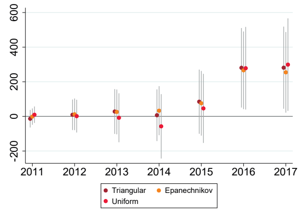
(a) Earnings by bandwidth



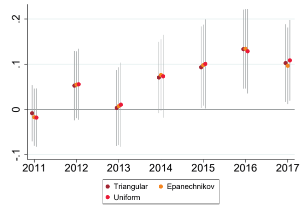
(b) Employment by bandwidth



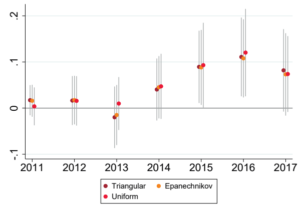
(c) Regular employment by bandwidth



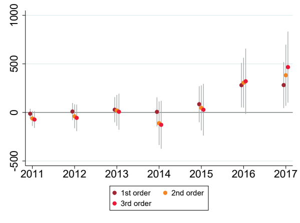
(d) Earnings by kernel



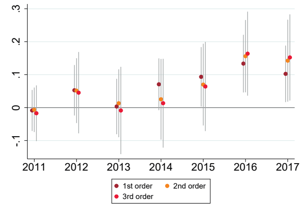
(e) Employment by kernel



(f) Regular employment by kernel

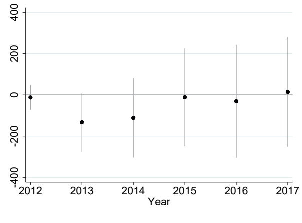


(g) Earnings by polynomial

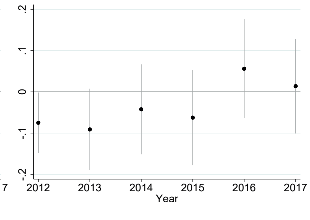


(h) Employment by polynomial

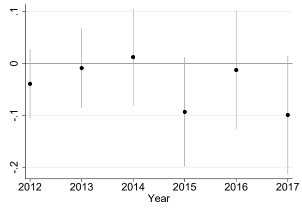
(i) Regular employment by polynomial



(j) Earnings, placebo



(k) Employment, placebo



(l) Regular employment, placebo

Notes: Country of birth fixed effect are included in all specifications. RD point estimates and the corresponding 95% CIs are plotted. Earnings are measured in hundreds SEK, and the employment measures (all and regular) indicate employment in a given year. Placebo studies fictive reform, using corresponding cutoff dates for 2011. We also checked that the density is balanced using the McCrary test.

4. Supporting labor market integration by lowering language barriers

Co-authored with Ulrika Vikman

Acknowledgments: We are grateful for the comments from Henrik Andersson, Olof Åslund, Eva Mörk, Anna Sjögren, Anna Thoresson, Gerard van den Berg and Johan Vikström, as well as the seminar participants at the Institute for the Evaluation of Labor Market and Education Policy (IFAU) and the Uppsala Immigration Lab (UIL). We also thank Dzenana Cenanovic and Samatar Isse for sharing their valuable insight about the program. We thank FORTE (Reg. No. 2016-07123) for financial support.

4.1 Introduction

The slow labor market integration of immigrants, in particular refugees, that we see in most European countries, is costly from both an individual and societal angle. Even after several years in the host country, refugees often have lower employment rates and earnings compared to native-born residents and economic migrants (Brell et al. 2020; Cortes 2004; Ruiz and Vargas-Silva 2018). Finding ways to speed up labor market integration is crucial, both for policymakers and refugees. Language training and subsidized employment can be especially effective at increasing employment for this group (Arendt et al. 2020b; Butschek and Walter 2014; Lochmann et al. 2019). This suggests that it is important to provide new immigrants with both language training and more labor market oriented activities in their new country.

However, these two goals may be conflicting. A work-first approach, where immigrants enter active labor market programs (ALMPs), such as on-the-job training earlier on, risks taking time and effort away from language training. Without sufficient language skills, it might not be possible for immigrants to fully benefit from ALMPs and to gain necessary knowledge about the labor market due to limited ability to communicate with, and understand instructions from, the caseworker. On the other hand, the slow integration of immigrants – especially women and those with a low level of education – may be due to an excessive focus on preparatory measures like language training (Cheung et al. 2019; Dahlberg et al. 2020).

One way to provide early ALMPs to individuals with very poor language skills in their new country is to combine such programs with support and information in the immigrants' native language. We study a program in Sweden that entails an ALMP in a regular workplace coupled with support and information provided by bilingual caseworkers who speak the immigrants' native language. The aim of placing participants in regular workplaces is that they can improve their Swedish communication skills while learning about the Swedish labor market. We investigate if this type of program represents a way forward to increase employment for immigrants with limited language skills.

The program we evaluate, the *Language Support Program*, is run by the city of Stockholm and directed toward unemployed immigrants with very limited Swedish language skills. Caseworkers employed to work with this group are bilingual. For the first month of the program, they provide an extensive introductory course to the Swedish labor market (e.g., how to apply for a job, rules and norms in the workplace) and carry out an in-depth survey of participants' formal and informal skills and preferences, all in their native language. After the first month, participants are matched to a workplace, where they either complete an internship lasting for three months, or a 1-month internship followed by a 6-month public sector employment program (PSEP).[1]

[1] We study the effects of the Language Support Program the period of 2010–2018, during which PSEPs were part of the program in 2017–2018.

In the workplace, participants have supervisors who have received training in communication and intercultural understanding. Participants (and their supervisors) also continue to have access to support from the bilingual caseworker.

Earlier research on the relative effectiveness of a language training versus a work-first approach is limited. One exception is Arendt and Bolvig (2020), who find that while an early on-the-job training program had positive effects on employment in the short term, the effects were temporary. The program also had a large negative impact on language acquisition, which may be costly, as there is a strong correlation between learning the language in one's new country and labor market success (Chiswick and Miller 1995). There is also a growing body of literature on the causal effects of language training, which has revealed its positive influence on labor market outcomes in France (Lochmann et al. 2019) and Denmark (Arendt et al. 2020b). Sarvimäki and Hämäläinen (2016) finds that increased use of language training and integration courses in Finland had large positive effects on earnings. These results indicate that it is vital that early ALMP participation not come at the expense of language acquisition.

There are also studies pointing to the importance of more traditional ALMPs for integrating unemployed immigrants into the labor market. Battisti et al. (2019) show that intensive job search assistance increased short term employment for refugees in Germany, and Dahlberg et al. (2020) find positive effects of a program consisting of increased language training intensity, an internship, and more intensive job search assistance for refugees with a low level of education in Sweden. Finally, in two meta analyses, Butschek and Walter (2014) conclude that wage subsidies work well, and Hangartner et al. (2021) indicate that interventions aiming to improve the match quality between immigrants and training programs are often the most successful.

In studying the Language Support Program, we add to the aforementioned literature on how different ALMPs affect immigrants' labor market integration. To the best of our knowledge, our study is the first to examine whether a program providing information and support in the participant's first language, in combination with a workplace-based ALMP, can accelerate labor market entry among adults. Related studies have investigated bilingual education for students, finding somewhat conflicting evidence. Chin et al. (2013) explore whether bilingual education, compared to English as a second language (ESL), is better for students with limited language skills in Texas, not finding statistically significant effects of bilingual education on test scores. However, refugee children perform better when doing a math test in their first language (Attar et al. 2020), implying that limited language skills can be an obstacle to the acquisition and testing of knowledge. When early integration programs take a work-first approach, weak language skills may prevent the participant from acquiring knowledge of the labor market and successfully completing the program. Providing support in the participant's native language may be a way to overcome these issues. It may also be easier for a caseworker to coach and motivate the participant if they share the same language.

To evaluate the effects of participating in the Language Support Program, we gained access to administrative data covering a rich set of individual characteristics, migration history, as well as labor market and health measures during the immigrants' time spent in Sweden. Importantly, we also acquired information about their prior Swedish language training (SFI) courses and grades. From the city of Stockholm, we obtained access to information about all individuals registered at local job centers: when and in which ALMPs they participate, as well as if they decided to register at the job center themselves, or if they were assigned by a social worker. We use the latter as a proxy for motivation. We follow participants who enrolled in the program between 2010 and 2018 and track them for two years after they entered it. The outcomes we study are employment, subsidized employment, social assistance (SA), earnings and SFI grades.

Assignment to the program is not random, but rather depends on a combination of, for instance, language skills, one's social situation, and caseworkers' awareness about the program.[2] We therefore use our rich set of covariates to find observably similar non-participants. In addition, we deal with a dynamic treatment assignment problem (see e.g., Fredriksson and Johansson (2008a)). Given that individuals registered at a job center can be assigned to the Language Support Program at any time, individuals with short spells at the job center will be over-represented among individuals who are not assigned to the program. To address this, we apply dynamic inverse probability weighting (IPW), as suggested by Berg and Vikström (2022), giving greater weight to non-participants that have been registered at the job center for a long time.

In comparing participants in the Language Support Program with observably similar non-participants, we find that participation increases employment by 10 percentage points soon after entering the program. The positive effect is temporary, lasting 8 months, and is driven by an increase in subsidized employment for participants having a PSEP as part of the program. We thus conclude that the Language Support Program does not increase employment in the medium term. Dividing our sample based on sex and education level respectively, we find that women and highly educated immigrants benefit the most from the program in terms of increased employment, and our results indicate that participants in these groups are more likely than observably similar non-participants to be employed in the medium run. We also study whether participation leads to improved Swedish language skills, and find that it increases the likelihood of passing a Swedish language course at a high difficulty level by 13 percentage points.

[2] We were unable to obtain access to caseworker IDs in our data.

4.2 Institutional setting

There are two cornerstones of introducing new immigrants to Sweden. The first is Swedish for Immigrants (SFI). SFI is available to all immigrants who lack basic Swedish language skills, and courses are run by the local governments at the municipal level. The second cornerstone is participation in ALMPs. Access to ALMPs is somewhat different depending 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 allowed to participate in a 2-year program (the Introduction Program), run by the Public Employment Service (PES).[3] Participants receive benefits (the Introduction allowance) and are required to participate in SFI and other activities specified in an integration plan, which is set up together with a caseworker. SFI and introduction to Swedish society are mandatory components, but other ALMPs should be included. However, refugees, especially women, mostly participate in preparatory activities during the Introduction Program, as opposed to activities closer to the labor market (Andersson Joona 2020).

Other immigrants and refugees who have not found a job within the two years of the Introduction Program can also register with the PES, but are not entitled to receive the Introduction allowance. If they do not have savings or family members who can support them, the final safety net in Sweden is to apply for social assistance (SA) at the social welfare office run and financed by each municipality. Municipalities are allowed to condition the receipt of SA on participation in skill-enhancing activities.

In the city of Stockholm, where the program that we evaluate is run, all unemployed SA recipients are sent to their local job center where a caseworker can assign them to different activities. These activities include work preparation, job searching and coaching, different courses, internships, public sector employment programs, and different language activities. Immigrants may therefore be in contact with, and participate in programs at, both the national PES and the job centers run by the municipalities.

4.2.1 The program

The *Language Support Program* was launched in 2009 at the job centers run by the city of Stockholm. It targets immigrants who arrive at the job center with very limited language skills. The PES and SFI schools can also send their participants to the program, but these groups are not included in our data.

The program is run by a special unit within the municipality, the Unit for Language Support Interventions. In this unit, there are two types of caseworkers: bilingual and matching caseworkers. The presence of the former makes

[3]The 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).

this unit unique. The languages that are spoken by the bilingual caseworkers are Arabic, Somali, Dari, and Tigrinya. These languages are the most common among immigrants with poor language skills and cover a large majority of the participants. For individuals who do not speak one of these four languages, information and support are provided in ”simple” Swedish. The bilingual caseworker first provides an introductory course, after which the matching caseworker matches the participant with a regular workplace. Something that sets this program apart from other ones is that the introductory course, motivational work, and support throughout the program are provided in participants' native language. This helps individuals with an extremely limited knowledge of Swedish to understand the information and instructions provided and to be able to share their previous labor market experience and preferences.

The introductory course lasts for four weeks and involves an introduction to the labor market, including how to write a CV and how to apply for jobs, and learning about rules and norms at work. There is also an in-depth survey of participants' formal and informal skills and preferences conducted by the bilingual caseworkers in the native language of the participant. This enables the caseworker to gather this information, which is then used by matching caseworkers to match participants with employers to a high extent based on participants' interests and requests in the hope that matches with such professions will be more durable in the long run.

The matching process can take up to 60 days. In the workplace, which can be in the private or public sector, the participant has a supervisor who has been trained in communication and intercultural understanding. If any communication issues or conflicts arise, the bilingual caseworker can visit or call the workplace to help resolve the situation.[4] The aim in a regular workplace is to provide labor market experience and to improve the participant's language skills, e.g. by communicating with colleagues. To allow for continued studies at SFI, the program is part-time, but participants must still be at work for parts of the day at least five days a week.

During the time period that we study, 2010–2018, the Language Support Program included two types of placements in regular workplaces. During 2010–2016, assigned individuals participated in the *Language Support Internship* (LSI), which normally lasted for three months. In 2017–2018, a PSEP called *Language Support Employment* (LSE) was added to the toolbox of the caseworkers at the Unit for Language Support Interventions. In practice, everyone who was assigned to the Language Support Program during these two years and was eligible to receive a wage subsidy (”Extratjänster”) participated in the LSE, while those not eligible to receive the wage subsidy participated

[4]The supervisor also participates in networking events with lectures, as well as discussions with supervisors from other workplaces and staff from the Unit for Language Support Interventions. These events are provided to support the supervisors.

in the LSI.[5] Participants in the LSE also started with a month-long internship in the workplace after which the PSEP lasted six months. The two kinds of program share many features. Participants receive support and information in their native language, and are matched to a workplace where they have a supervisor and are only allowed to perform tasks outside the scope of the tasks performed by regular employees (to avoid crowding-out effects). Participants in the LSE, however, remain in the workplace for longer (1+6 months compared to 3 months), and we cannot rule out that they are treated differently in the workplace due to having employment, as opposed to an internship. Another difference is that participants in the LSE receive a monthly salary of SEK 19,000 (approximately EUR 1,800), while the participants in the LSI continue to receive income support.

Also before 2017, there was a chance that participants in the Language Support Program would end up in a PSEP. During this period, the Language Support Program cooperated with another, separate program provided by the job centers, a 6-month PSEP targeting individuals younger than 30, the *Youth Employment Program*.[6] The Youth Employment Program was not part of the Language Support Program, but the cooperation between them implies that some participants received subsidized employment in their workplace after having finished the internship. The Language Support Program could thus have been a stepping stone to other programs closer to regular employment.

4.3 Data and sample

We combine administrative records from many different sources. The city of Stockholm provides data on all registrations at job centers from January 2010 until June 2019. These data include the date of registration and the dates of participating in any activity conducted at the job center. From Statistic Sweden, we gain access to yearly data on background characteristics such as sex, age, and family situation (marital status, number of children); year of immigration and country or region of origin; and education level and labor income. Importantly, to capture language skills, we also obtain data on enrollment and grades at SFI since 2002. We have information about the reason for being granted a residency permit (e.g., as a refugee or as the relative of a refugee) and when it was granted, from the Swedish Migration Agency. The data from the PES include all registrations and 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. For

[5] Eligibility for the wage subsidy is determined by the period of unemployment or time since immigration, which we observe in the data.

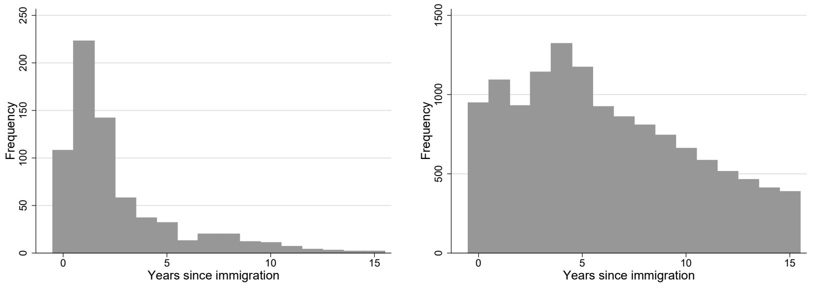
[6] Participants in this program are employed for 6 months at different municipal workplaces such as childcare centers, administration, and elder care. Mörk et al. (2022) find that this increases participants' employment by 10 percentage points up to three years after starting the program.

most background characteristics, we use data from two years prior to registration at the job center. Exceptions are time since immigration, time registered as unemployed, and information regarding SFI, for which we use all available data.

We limit our estimation sample to immigrants born outside Nordic and Western European countries who were registered at the job center between January 1, 2010 and December 31, 2018, between the ages 18 and 61, and had been in Sweden no longer than 15 years at the time of enrollment. This sample consist of 13,665 registrations and 10,365 unique individuals. We define program participation as initial enrollment in the Language Support Program within 12 months of registration, no later than December 31, 2018.[7]

As a direct measure of labor market success we examine employment, defined as having received any earnings in a given month. We also try a stricter definition where we require yearly earnings to exceed the income base amount[8]. In addition, we explore subsidized employment (based on data provided by the PES) as well as SA, since the latter is income support that most individuals enrolled at the job center receive. Having subsidized employment in a given month is defined as being registered as such at the PES while simultaneously having positive earnings registered that month. We study these outcomes for each given month, up to two years after the program started. This allows us to characterize the dynamics of the effects. However, information about monthly labor earnings from Statistics Sweden is only available through September 2020[9] and subsidized employment is only available until December 2019. Finally, we examine total earnings and SA received in the 24 months after the program started, as well as the likelihood of passing an SFI course (at any or C–D level) during the same period.

Figure 4.1. Years since immigration at the time of job center (JC) enrollment



(a) Enter program in JC spell *(b)* Not enter program in JC spell

[7]We take later program participation into account, both when finding IPW weights and when studying outcomes for non-participants. This is explained in greater detail in Section 4.4.
[8]The income base amount tracks general income growth and amounted to SEK 42,400 in 2010 and SEK 46,500 in 2019.
[9]This only allows us to follow some individuals 21 months after program start.

4.3.1 Descriptive statistics

Figure 4.1 shows years since immigration at time of registration at the job center for the individuals in our sample. Figure 4.1a includes individuals who participate in the Language Support Program at some point during the spell at the job center. As expected, most participants have been in Sweden a limited number of years when they register at the job center. A potential concern is that all newly arrived immigrants who register at the job center enter the program. However, as is clear from Figure 4.1b, many recent immigrants never participate in the program.

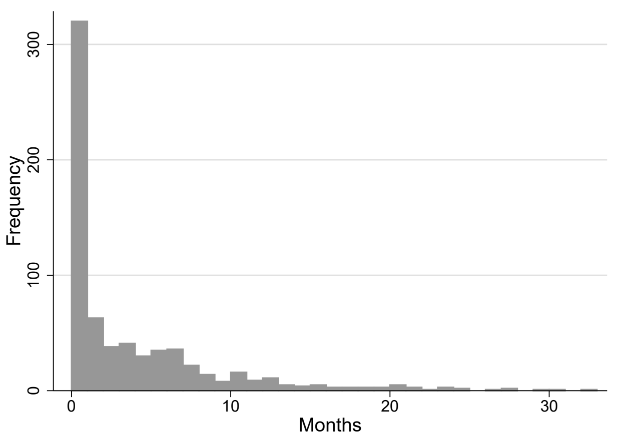
Table 4.1. *Mean characteristics at the time of registration 2010–2018*

	(1) Participants		(2) Non-participants	
	mean	sd	mean	sd
Age	33.89	9.63	33.69	10.30
Female	0.65	0.48	0.53	0.50
Married	0.56	0.50	0.44	0.50
Child in household	0.42	0.49	0.48	0.50
Primary school	0.50	0.50	0.43	0.49
At least high school	0.34	0.47	0.48	0.50
Education unknown	0.16	0.37	0.09	0.29
0-2 years since immigration	0.68	0.47	0.23	0.42
3-5 years since immigration	0.18	0.39	0.28	0.45
6-15 years since immigration	0.14	0.34	0.49	0.50
Born in Asia, excl. West Asia	0.09	0.29	0.15	0.36
Born in Africa , excl. Northern Africa	0.52	0.50	0.38	0.49
Born in West Asia or Northern Africa	0.35	0.48	0.36	0.48
Other country of birth	0.05	0.21	0.11	0.31
Refugee	0.58	0.49	0.49	0.50
Relative	0.22	0.42	0.39	0.49
Started SFI before t	0.88	0.32	0.71	0.45
Passed SFI before t	0.37	0.48	0.42	0.49
Started SFI course C-D	0.39	0.49	0.47	0.50
Observations	694		12971	

Notes: Limited to individuals born outside Nordic and Western European countries; t corresponds to registration at the job center. Refugee is defined as immigrants receiving residency permits as refugees. Relative can be a relative of refugees or another kind of immigrants. SFI is Swedish for Immigrants, the language course for immigrants in Sweden. Courses C and D represent the highest levels of SFI.

Table 4.1 describes the mean characteristics of our sample at the time of registration at the job center. Column (1) describes the participants, and Column (2) non-participants. Both participants and non-participants are on average 34 years old, but the share of women is higher among the participants (65% compared to 53%). Participating individuals are also more likely to be married (56 vs. 44%) and slightly less likely to have children (42 vs. 48%) than non-participants. Participants have less formal education than non-participants, and it is more common for information about their education level to be missing from the Swedish registers. This is probably explained by the fact that a much larger share of the participants arrived in Sweden within the past two years (68% of participants and 23% of non-participants). Individuals born in Africa (excluding Northern Africa) are over-represented among participants compared to non-participants. It is also clear that a large portion of all individuals in our sample, especially among the participants, received their residency permit as refugees (58% of participants and 49% of non-participants). A large number also arrived as relatives, especially among non-participants (39%).[10] Everyone in our sample has the right to study SFI. While most entered an SFI course before registering at the job center, the share among participants is 17 percentage points higher than for non-participants. However, for the proportion that passed an SFI course or started one of the higher level language courses (C or D) before registering, we observe a reverse relationship. This indicates that the Language Support Program is indeed targeted toward individuals with worse Swedish language skills than the average foreign born job center client.

Figure 4.2. Months from job center (JC) registration to entering the Language Support Program



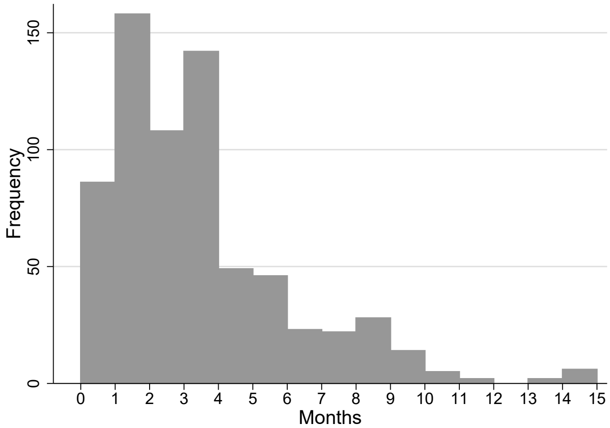
Note: Time is measured between the date of registration at the JC and the start date of the introductory course.

As seen in Figure 4.2, most participants enter the program soon after registering at the job center, even during their first month there. However, it is also clear that this is not always the case. Many participants also enter the program later, potentially due to individual circumstances, like a family situation

[10]Relatives include both relatives of refugees and other relatives of non-EU/EES immigrants.

or health, or priorities made by the caseworkers at the job center. Because very few participants enter the program more than a year after registering at the job center, we limit our study to participants entering within this time frame.

Figure 4.3. Duration of Language Support Program (months)



Note: Time is measured between the start date of the introductory course and the end date of the program. We allow for a gap of up to 60 days between the introductory course and the internship or PSEP. Figure A.1 outlines the program duration by ALMP type.

The introductory course of the program lasts four weeks. Figure 4.3 shows the duration of the program in practice. Duration is defined as the time from starting the introductory course until ending the workplace-based program, which can either be the internship (LSI) or the PSEP (LSE). We allow for up to a 60-day gap between the two activities since the matching can take up to 2 months. Some individuals leave the program the first month. This may be due to a failure of the matching caseworker to find a workplace, the participant finding a job, or being unable to continue with the program for other reasons. Many participants are in the program for around 3–4 months, as expected. In Figure A.1, where we display the same graph for LSI and LSE separately, we can see that the longer durations are mainly explained by the LSE.

4.4 Empirical strategy

To evaluate the Language Support 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, and participants are likely to be different than non-participants, 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. The next section discusses the fulfillment of the CIA in our setting given the large set of covariates we have access to.

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 Language Support Program. This implies that individuals with short spells at the job center will be over-represented among non-participants. As demonstrated by Fredriksson and Johansson (2008a), if we do not take this dynamic selection into account, our estimates will be biased. In Section 4.4.2, we describe how applying the dynamic IPW proposed by Berg and Vikström (2022) addresses this problem.

We follow the implementation of dynamic IPW in Mörk et al. (2022), which we describe, along with how the empirical strategy improves the balance in observables between participants and non-participants, in Section 4.4.3.

4.4.1 Selection on observables

To estimate a causal effect of the Language Support Program, we need to have access to all potential confounders affecting both program assignment and future outcomes.[11] Our participants were chosen due to poor language skills, something that we do not observe directly. Since immigrants with better language skills probably have an easier time finding a job, and could be among the non-participants, we may potentially underestimate the program's effects by not taking language skills into account. Even though we are unable to measure language skills directly, we do have access to information about whether individuals have passed any SFI course and at what level they had studied. The data also include a rich set of individual and household characteristics, as well as migration and labor market history, SA, PES, and job center history. Such factors are likely known to the caseworker and may influence the decision whether to assign a given client to the program. In addition, we have information indicating if individuals themselves decided to register at the job centers, or if they were assigned by their social worker to capture motivation. Since we also have information about individuals' previous drug prescriptions, hospitalizations and Swedish language course participation and results, we are able to capture how the caseworker perceived the client's language status as well as health situation, which are important factors to determine suitability to participate in the program.

The fact that our data provide us with this vast number of potentially important controls makes it possible for us to find non-participants similar to the

[11] Due to the dynamic setting, the sequence of potential outcomes have to been be independent of assignment to the program at a given point time, given our observable characteristics at that time (dynamic CIA).

participants in many critical aspects. However, the short amount of time since immigration to Sweden and limited prior labor market experience among Language Support Program participants makes it more difficult to fulfill the CIA assumption in our setting. The fact that we instead have access to SFI history, is crucial for our matching approach.

Because matching on a large set of covariates is very demanding, we apply propensity score matching as suggested by Rosenbaum and Rubin (1983). However, including unnecessary covariates in the propensity score leads to loss of efficiency. We therefore choose a minimal set of confounders from the original reservoir of covariates using a data-driven algorithm (Luna et al. 2011; Persson et al. 2017). Given that the CIA is fulfilled in the original pool of covariates, the algorithm selects a minimal set such that the CIA is still fulfilled. Let W be the full set of covariates with the CIA fulfilled; that is, $Y_0 \perp\!\!\!\perp T|W$.[12] The algorithm to find the minimal set is done in two steps:

Step 1: Select $W_T \subset W$ such that $T \perp\!\!\!\perp W \backslash W_T | W_T$ holds.

Step 2: Select $X \subseteq W_T$ such that $Y_O \perp\!\!\!\perp W_T \backslash X | X, T = 0$ holds.

That is, in Step 1, we select W_T from the full set of covariates, such that program participation is independent of the remaining set of covariates, given W_T. In Step 2, we select X as a subset of W_T such that the outcome for non-participants is independent of the remaining set of W_T, given X.

As a way to evaluate our set of confounders, we estimate effects for the year before the participants enter the program. We interpret the absence of such pre-effects as suggestive evidence that our empirical strategy is successful.

4.4.2 Dynamic inverse probability weighting (IPW)

We apply dynamic IPW (Berg and Vikström 2022) to take into account that individuals registered at the job center can be assigned to the Language Support 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 and do not enter the program at any later point in time.

Let T_u denote the duration of enrollment at the job center, the *initial state*, and T_s the duration until assignment to the Language Support Program, the *treatment*. An individual with $T_u < T_s$ leaves the initial state before being treated. 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

[12] Since we estimate ATET, it is enough that participation, or treatment (T), is independent of the outcome for the non-participants (Y_0). If estimating the average treatment effect (ATE), T also needs to be independent of the outcome for the participants (Y_1).

assigned to never being treated, we let $T_u(\infty)$ and $Y(\infty)$ capture the potential duration and outcome, respectively. If CIA is fulfilled, the average treatment effect of the treated (ATET) is given by

$$ATET(t_s) \quad = \quad E(Y(t_s) - Y(\infty)|T_s = t_s, T_u(t_s) \geq t_s) \tag{4.1}$$

The estimator of $ATET(t_s)$, proposed by Berg and Vikström, gives greater weights to never-treated individuals who have been in the initial state for a long (vs short) time. This is unbiased under the assumptions of sequential CIA, ”no anticipation” (Abbring and Berg 2003), common support and SUTVA. We use their short-run estimator since our outcomes could be measured before the individuals have left the initial state; that is, in period $t_s + \tau$ (i.e. τ periods after treatment starts). This estimator is given by:

$$\widehat{ATET}(t_s) = \frac{1}{\pi(t_s)N_{t_s}} \sum_{i \in T_{s,i}=t_s, T_{u,i} \geq t_s} Y_{t_s+\tau,i} - \tag{4.2}$$

$$\frac{\sum_{i \in N^0_{t_s+\tau \geq T_{u,i}}} w^{t_s}(T_{u,i}, X_i) Y_{t_s+\tau,i} + \sum_{i \in N^0_{T_{u,i} > t_s+\tau}} w^{t_s}_{\tau}(T_{u,i}, X_i) Y_{t_s+\tau,i}}{\sum_{i \in N^0_{t_s+\tau \geq T_{u,i}}} w^{t_s}(T_{u,i}, X_i) + \sum_{i \in N^0_{T_{u,i} > t_s+\tau}} w^{t_s}_{\tau}(T_{u,i}, X_i)}$$

where $\pi(t_s)$ is the share treated of N_{t_s}, the number of non-treated individuals still in the initial state at the beginning of t. $N^0_{t_s+\tau \geq T_{u,i}}$ are the never-treated individuals who have left the initial state when the outcome is measured and $N^0_{t_s+\tau \geq T_{u,i}}$ those who have not. The weights w^{t_s} and $w^{t_s}_{\tau}$ are given by

$$w^{t_s}(t_u, X) \quad = \quad \frac{p(t_s, X)}{\prod_{m=t_s}^{t_u}(1 - p(m, X))} \tag{4.3}$$

$$w^{t_s}_{\tau}(X) \quad = \quad \frac{p(t_s, X)}{\prod_{m=t_s}^{t_s+\tau}(1 - p(m, X))} \tag{4.4}$$

$$p(t, X) \quad = \quad Pr(T_s = t | T_s \geq t, T_u \geq t, X) \tag{4.5}$$

The first line of Equation (4.2) gives the mean for the treated and is observed. The second line gives the estimated outcome under no treatment, where the numerator and the first product in the denominator of Equation (4.3) and (4.4) correspond to the weights from the static IPW. $p(t, X)$ is the propensity to be treated in period t, given by Equation (4.5). The following products of the denominators in Equation (4.3) and (4.4) account for the duration in the initial state by including the treatment propensity for each following period, since the individual is still in the initial state. Never-treated individuals who have left the initial state when the outcome is measured are given the weights in Equation (4.3), while never-treated individuals still in the initial state at τ, when the outcome is measured, are given the weights in Equation (4.4). For these weights, only information available at τ is used.

In practice, the weights will be replaced by estimated weights based on estimated propensity scores for each period that the never-treated individuals are still at the job center.

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

4.4.3 Implementation

Because only a limited number of individuals participate in the Language Support 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 based on when participants enter the Language Support Program (see Figure 4.2), trading off having a sufficient number of participants in a given assignment period and losing variation in the data. The time of assignment is skewed towards very short durations at the job center. Most individuals even enter the Language Support Program in the first month they are enrolled at the job center, and many enter in the following months. When evaluating the program, we define $t_s = [1]$ as the first month, $t_s = [2]$ as month 2–3 after enrollment at the job center, $t_s = [3]$ as the subsequent 3-month period, and $t_s = [4]$ as the last 6-month period. Since very few individuals enter the program after being registered for more than a year at the job center, we limit our analysis to program participation within the first year. In Table 4.2, we display the number of participants for each defined assignment period.[13]

[13] We also consider a fifth period when estimating the weights. In this period we aggregate all participants who start the Language Support Program after more than 12 months.

Table 4.2. *Participants entering the Language Support Program per period*

Period	Month	Participants
1	1	354
2	2–3	88
3	4–6	107
4	7–12	88
5	13–	57
Total		694

Note: Participants starting the Language Support Program in the fifth period are only used to estimate the weights used in 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 (Luna et al. 2011; Persson et al. 2017), which we do using LASSO (Tibshirani 1996).[14] The original pool of covariates and the set chosen for the different groups of outcomes[15] are shown in Table A.2 in the Appendix. As expected, information about SFI courses is important and chosen for all outcomes, as is sex and time since immigration.

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 1% of the sum of weights for the controls (Huber et al. 2013). Table A.3 in the Appendix presents the summary statistics for our estimated propensity scores, which imply overlap between the two groups. It also contains information about weights and the number of observations excluded due to the trimming.

One way of evaluating if our empirical strategy is successful in comparing participants to similar non-participants is to look at the normalized differences. Normalized differences provide a way of assessing overlap that is independent of scale and sample size. According to Imbens (2015), normalized differences after weighting below 0.3 indicate balance in the observed characteristics. Table 4.3 outlines a summary of the normalized differences.[16] It is clear that

[14]LASSO minimizes the residual sum of squares subject to the sum of the absolute value of the coefficients being less than a constant, λ, and thereby excludes variables since some coefficients are set to zero. With the help of cross-validation, we choose the model with the largest λ; that is, within one standard deviation from the λ that minimizes the mean-squared prediction error. We use the *cvlasso* command in STATA developed by Ahrens et al. (2019, 2020).

[15]In principle, we could choose a set of covariates for each given outcome and follow-up month. Since many of our outcomes are highly correlated, we run LASSO using the cumulative number of months of employment, subsidized employment, and SA over months 1–24 since the program started. For SFI outcomes, we use the likelihood of passing an SFI course during the same period.

[16]They are presented in greater detail in tables A.5–A.7 in the Appendix.

our strategy is successful in achieving balance for the included variables. The weighting of non-participants reduces the normalized difference. The only outcome for which the normalized difference exceeds 0.3 is employment, for the indicator variables for year of registration at the job center.

Table 4.3. *Summary of the normalized difference in means, before and after dynamic inverse probability weighting (DIPW).*

	Mean	Min	Max	> 0.3	N
Employment outcomes					
Before weighting	0.340	0.012	1.059	9	21
After weighting	0.085	0	0.450	3	21
Subsidized employment outcomes					
Before weighting	0.463	0.013	1.071	6	9
After weighting	0.033	0	0.121	0	9
Social assistance outcomes					
Before weighting	0.338	0.026	0.973	13	24
After weighting	0.037	0.001	0.285	0	24
SFI outcomes					
Before weighting	0.466	0.122	1.047	8	12
After weighting	0.009	0.002	0.029	0	12

Notes: Means before and after weighting, as well as normalized differences for all variables, are depicted in tables A.5–A.6 in the Appendix. $>$ 0.3 shows the number of variables where the normalized difference is greater than 0.3 and N indicates the number of covariates included. The weights used are based on the information 24 months after the program started.

For participating individuals, the follow up period is defined from the start date of the program. However, there is no such date for non-participants. To know when to measure their outcomes, we impute start dates. For each assignment period, we draw a date with replacement from the pool of start dates for the participants. Finally, we exclude observations that emerge in the program in a given follow-up month, as well as those with a (simulated) start date after December 31, 2018 after having calculated the weights.

To obtain standard errors we conduct a bootstrap procedure with 99 replications. For each bootstrap replication, we do the covariate selection, propensity score estimation and calculation of weights, giving us a distribution of potential differences between the participants and similar non-participants.

4.4.4 What is the counterfactual?

As seen in the main results, we study the effects of participating in the Language Support Program, regardless of whether an immigrant was matched to a workplace to do an internship (LSI), or to do an internship with a subsequent PSEP (LSE).

Before we examine the results, we also want to understand what the counterfactual is. That is, in what programs would participants have enrolled had they not been assigned to the Language Support Program? Since we do not observe this, Table 4.4 portrays enrollment in activities for the unweighted and weighted non-participants. We focus on activity participation 6 months following the simulated program start, since most Language Support Programs have ended by then (see Figure 4.3).

Table 4.4. *Activity participation for non-participants, 6 months after program start*

	(1) Non-participants	(2) Weighted non-part.
Language support program	.00973	.0415
Other language activities	.0311	.0673
Work preparation and training	.0141	.0342
Job search, guidance, matching	.352	.315
Courses	.0572	.107
Internship	.0711	.0659
Public sector employment	.0101	.0385
Other programs	.0125	.0493
Observations	41,212	39,926

Notes: Refer to registrations at the job center. One observation represents an individual, spell, and period cell. The weights used in Column (2) are based on the covariates chosen for employment outcomes. Other programs are old programs where the municipality of Stockholm could not define a given type of activity.

As seen in Table 4.4, a very small share of non-participants later participate in the Language Support Program, but the share increases substantially after weighting, indicating that the empirical strategy puts greater weight on individuals with a greater probability of taking part in the program. As mentioned in the previous section, non-participants who later enter the Language Support Program are only included to calculate weights, and are excluded from the analysis when entering the program.

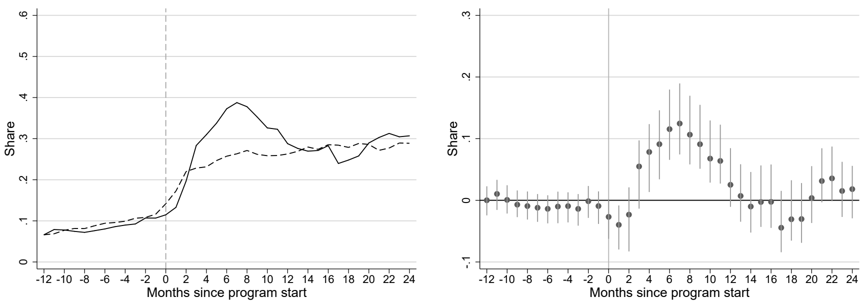
Focusing on the weighted non-participants (Column 2), we see that a large share (31.5%) of non-participants get activities related to job searching, guidance, and matching. As explained above, this type of support is also given to participants in the Language Support Program. The crucial difference is that the participants engage in these activities in their native language. The caseworkers at the job centers may receive support from interpreters, which is likely helpful when providing information. However, to coach and motivate clients may be harder if caseworkers do not share their language, given their limited

Swedish language skills. Of the weighted non-participants, 10.7% also attend courses during the 6-month period, mostly shorter classes such as computer courses. To some extent, non-participants also participate in regular internships (6.6%) and PSEPs (3.9%). 6.7% take part in other language activities, including Swedish language classes to specialize in different professions.

The activities in Table 4.4 are not mutually exclusive and the proportion participating in different activities may seem low. In addition to the programs in the table, all individuals who register at the job center are assigned an individual caseworker (coach). Another explanation is that many individuals leave the job center during this 6-month period. One month after the (simulated) program start, 13.3% have left the job center, and after 6 months, this figure reaches 44%.

From Table 4.4 it is clear that the counterfactual is a mix of different activities for those still registered at the job center. To some extent, these activities are similar to the Language Support Program, but without the support of a bilingual caseworker and less contact with workplaces.

Figure 4.4. Results, employment



(a) Observed and counterfactual *(b)* ATET

Note: Employment is defined as receiving positive earnings in a given month since the start of the program. The solid line in Figure 4.4a denotes the share of employed for participants, and the dashed line represents the weighted non-participants. The difference between these lines is displayed in Figure 4.4b with 95 % CIs based on 99 bootstrap replications.

4.5 Results

We are interested in how participation in the Language Support Program affects the labor market integration of immigrants. Figure 4.4 shows the effect of participating in the program on employment, comparing participants and observably similar non-participants.[17] The figure to the left (Figure 4.4a) indicates how the share employed among participants and the weighted non-participants

[17]Using a stricter definition of employment, earning above a threshold in a given month (see Figure A.2 in Appendix), does not change the outcomes.

evolves from month 12 before the (simulated) program start until month 24 after the start of the program. The figure to the right portrays the difference (ATETs) between the two lines, with 95% confidence intervals (CIs).

Judging by the pre-program employment trends and ATETs, it seems that our approach is successful in finding non-participants that are good matches to the participants. Both groups follow the same trend in employment (Figure 4.4a) and the ATETs are very close to zero (Figure 4.4b).

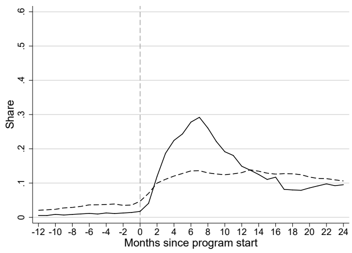
There is a small negative ATET on employment just after the program has started, which we interpret as a lock-in effect that approximately corresponds to the introductory course. After-ward, there is a sharp increase in employment among the participants; 6–8 months after entering the program, the participants are 10 percentage points more likely to be employed than the weighted non-participants. Compared to the mean employment rate before the reform (Figure 4.4a), this corresponds to an approximate increase in the likelihood of being employed at 100%, and an increase of 35% compared to the observably similar non-participants. After this peak, the positive employment effects gradually decrease in the size, and disappear one year after the start of the program.

This one-year temporary increase in employment could be driven by subsidized employment. If it is explained only by individuals who enter an LSE after the introductory course, the effect is part of the program. It may also be explained by individuals receiving public subsidized employment after participating in the LSI program; for instance, facilitated by the cooperation between the Unit for Language Support Interventions and the Youth Employment Program.

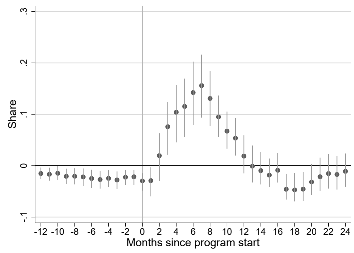
To better understand the employment results, we also study participation in subsidized employment. Comparing the post-program increase in (subsidized) employment among the participants in figure 4.5a to 4.4a, we see that the rise in employment among participants seems to, for the most part, be explained by increased subsidized employment. While employment at most reaches 40% in this group, subsidized employment accounts for 75% of this. However, there is a difference in the share with subsidized employment between participants and weighted non-participants before the program start, indicating that participants may be negatively selected.

We also examine if the program decreases the recepiency of SA. If the program leads to improved labor market integration, we expect the receipt of SA among the participants to decline relative to the weighted controls. The share receiving SA is balanced between participants and the weighted controls before the reform (Figure 4.5c). After starting the program, we observe a faster drop in the receipt of SA for the participants, and a negative ATET for most of the period in months 1–14 after the program started. This is significantly longer than the program is expected to last, and suggests that the reduction in SA persisted for some time after the program ended. From month 15 after the program started, the weighted non-participants catch up to the participants and the ATETs are close to zero.

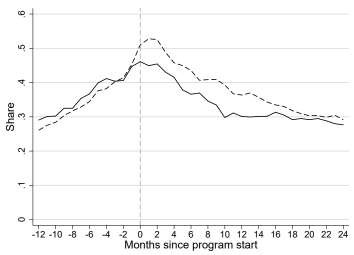
Figure 4.5. Weighted results, other outcomes



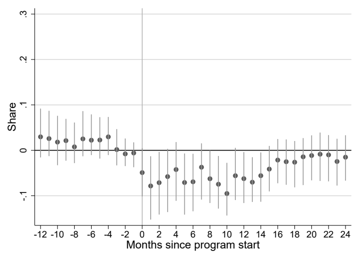
(a) Subsidized employment



(b) Subsidized employment, ATET



(c) Receipt of SA



(d) Receipt of SA, ATET

Note: The solid lines in figures 4.5a and 4.5c denote the share of participants with subsidized employment and with SA, respectively, and the dashed line represents the weighted non-participants. The difference between these lines is shown in figures 4.5b and 4.5d with 95 % CIs based on 99 bootstrap replications.

4.5.1 Earnings and social assistance payments

The results shown in the previous section indicate how participation in the Language Support Program affects employment, subsidized employment, and the receipt of SA in the extensive margin. We are also interested in how large the financial gains or losses from participating in the program are. In this section, we therefore study total labor income and SA payments during the two-year period in which we follow the participants after they have entered the program.

In the first panel in Table 4.5, we begin by exploring the effects on total earnings (Column 1) and SA (Column 2) twelve months prior to the program beginning, to make sure that we have found a good comparison group. The difference between the participants and weighted non-participants is low and not statistically significant. Studying the mean incomes in the pre-period, we clearly see that, earnings are, in general, low in the groups we study; the weighted non-participants earn 7,200 SEK in the year prior to the program. As already seen in Figure 4.4, less than 10% are employed for each month during this period. Compared to earnings, the amount SA is higher and weighted non-participants receive an average of 21,700 SEK.

During and after the program, the participants earn more than the observably similar non-participants, with an estimated difference of 19,500 SEK, corresponding to a relative increase of 21% compared to the mean among the weighted non-participants. However, we do not see any differences when it comes to social assistance.

Table 4.5. *Cumulative ATET on earnings and social assistance*

	(1) Earnings	(2) Social assistance	(3) Pass SFI	(4) Pass SFI C–D
Pre-program effects, month -12– -1				
ATET	-840	-466	.0781	-.00152
St err	1,064	1,281	.0271	.00999
Mean	7,204	21,668	.239	.0957
During and post-program effects, month 1–24				
ATET	19,531	-377	.123	.128
St err	7,658	8,207	.0272	.0288
Mean	93,295	68,103	.5	.3

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

4.5.2 Swedish language skills

One of the explicit goals of the programs is to improve the participants' language skills. Even if we do not have access to a direct measure of language skills, we have information about grades from Swedish language courses (SFI). We examine two outcomes: passing an SFI course, and passing any of the higher-level SFI courses (C or D) during the specified time interval.

One concern is that, because part of the selection is based on language skills, we may be comparing individuals with very different starting points in the SFI course system when they (fictitiously) enter the program. As seen in Table A.7, participants and non-participants are different in terms of SFI history before weighting, but after the dynamic IPW, they are very similar.[18]

To also determine if we have balance in the year before entering the program, we begin the analysis by estimating the differences in having received grades from SFI in the twelve months prior to the program. The results are displayed in the top panel of Column 3 in Table 4.5. In the year prior to the program, we find that participants are 8 percentage points more likely to have received any grade from SFI compared to the observably similar non-participants. This indicates that even if participants and observably similar non-participants earned similar grades from the SFI courses, the participants have entered the SFI system more recently. For higher grades, however, we find no difference in the pre-period (Column 4).

In studying the two years following the start of the program, we observe a clear, positive effect on passing an SFI course of 12 percentage points, as portrayed in the bottom panel of Table 4.5. As seen in Column 2, this is explained by grades from any of the higher levels of SFI courses, where the estimated difference is almost 13 percentage points.

While SFI grades are not a direct measure of language skills, and individuals may be taking SFI courses on a pass/no pass basis because they already speak Swedish, the positive effects of passing higher-level SFI courses are in line with a positive impact on language skills. There is thus no indication that the program is crowding out language acquisition, as suggested by Arendt and Bolvig (2020).

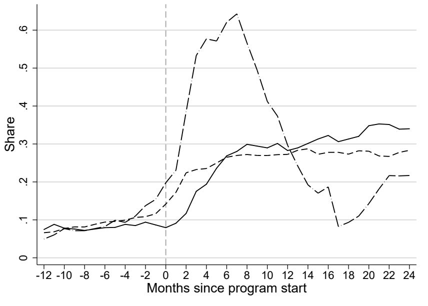
4.5.3 Comparing the Language Support Internship and Employment

Ideally, we would have liked to study the effects separately for those participating in the LSI and the LSE to see if there is a differential effect of the Language Support Program based on whether one participates in a 3-month internship or a 6-month PSEP. However, this is not possible due to the low number of participants, especially in the LSE. In Figure 4.6, we nevertheless

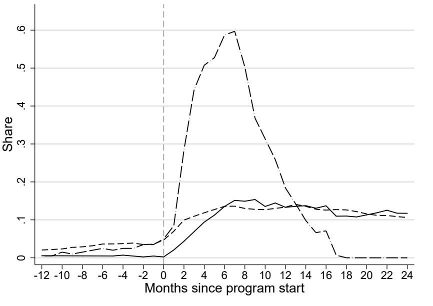
[18] Also, SFI variables that are not chosen by the LASSO are balanced after weighting; see the bottom panel in Table A.7.

divide the participants into those who participate in the LSI (solid line) and the LSE (the long dashed/dotted line) to describe how employment and subsidized employment evolve in the two groups. The dashed line represents the weighted non-participants used in the main analysis (see figures 4.4–4.5).

Figure 4.6. Results by type of ALMP



(a) Employment



(b) Subsidized employment

Note: In each figure, the solid line represents participants in the LSI, while the long dashed/dotted line refers to participants in the LSE, and the dashed line to the weighted non-participants.

First, we notice in Figure 4.6 that, especially for employment, the weighted non-participants, the LSI and the LSE participants are very similar up until just before the start of the program.

For participants in the LSI (solid line), there is a lock-in effect on employment during the expected duration of the program, that is, in the first 4 months after entering the program (Figure 4.6a). After this, we can see that the LSI participants experience a positive trend in employment, and that they are more likely to be employed after 14 months than the weighted controls. The lack of a positive trend in participation in subsidized employment after the expected end of the program (Figure 4.6b) indicates that the positive trend in employment is driven by regular employment.

The trends in employment and subsidized employment for LSE participants (long dashed line) stand out compared to the other two groups. As expected, there is a very large increase in the share employed just after the start of the program, which is clearly explained by an increase in subsidized employment. As the PSEP ends, there is a sharp decline in employment, taking LSE participants back to the same level of employment as before the start of the program. This is well below the employment rates among LSI participants and the weighted non-participants, suggesting that participation in the LSE creates substantial lock-in effects. After this, the employment level for the LSE participants starts to rise again.

These findings suggest that participation in the LSI is beneficial for employment in the medium run, and that a potential explanation for why the results are more promising than for participants in the LSE (in the medium term) is the relatively modest lock-in effects.

4.5.4 Heterogeneous effects

We also examine whether the effects of the Language Support Program mask differences across sex and level of education. Since we only have a limited number of participants,[19] and the procedure to select the minimal set of confounders using LASSO requires a large number of observations, we include the covariates selected in Step 1 (i.e., the covariates that determine assignment to the program). As in the previous analysis by program type, we focus on the results where we compare the outcomes for participants and the weighted non-participants, without the bootstrapped standard errors, since these also require a large number of observations. Table A.8 shows summaries of the propensity scores, weights, and observations removed due to trimming and Table A.9 portrays the normalized differences before and after weighting for the different subgroups we study.

By sex

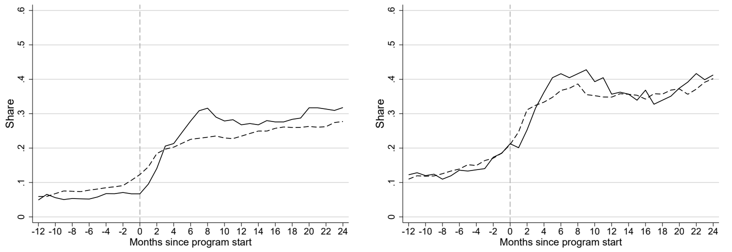
Even if labor market establishment takes a long time among all immigrants, this is especially true for women. Slik et al. (2015) also find that there are differences between women and men regarding learning a new language, finding that immigrant women learning Dutch outperform men in writing and speaking, even after controlling for other characteristics such as education level, age of arrival, length of residence and the number of hours spent studying Dutch. The effect of the Language Support Program may therefore be different for women and men.

First, as expected, we notice in Figure 4.7 that employment rates are lower among women than men before the start of the program. Just before entering the program, women have employment rates approaching 10%, while they are twice as high for men. For women, there is an initial lock-in effect for the first months of the program, after which participating women are more likely than their weighted non-participants to be employed for the rest of the studied period, even if the employment rates among participating women decreases slightly after month 8. We also note that the pre-trends suggest that women may be negatively selected compared to their weighted non-participants. Participating men do somewhat better than their weighted controls, with employment rates reaching above 40% (vs 35–40% for the comparison group), month 6–12 after the start of the program. After a year, the employment rates are similar in the two groups of men.

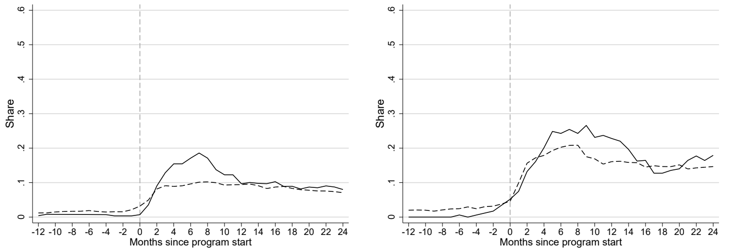
We are also interested in whether the results for subsidized employment are different for women than men. Immigrant women are less likely than men to

[19] Table A.1 displays the number of participants in each group and period.

Figure 4.7. Employment results by sex



(a) Employment, Women *(b)* Employment, Men



(c) Subsidized employment, Women *(d)* Subsidized employment, Men

Note: The solid line shows the share for participants, and the dashed line for the weighted non-participants.

participate in ALMPs that are more labor market oriented, such as subsidized employments (Andersson Joona 2020; Cheung et al. 2019).

For participants of both sexes, the proportion in subsidized employment rises sharply two months after the start of the program, reaching almost 20% for women in month 7 and 25% for men in months 5–11. Thereafter the share declines for both women and men and after months 12 and 15, respectively, the share with subsidized employment is similar to that of non-participants.

For observably similar non-participants, the proportion in subsidized employment also rises after the (simulated) start of the program, but there is a substantial difference between the sexes. While only around 10% of non-participating women have subsidized employment after the (simulated) start of the program, the share of non-participating men is 20% six months after the start of the program, after which it declines to 15% for the remainder of the follow-up period. The program thus seems to increase access to subsidized employment for both women and men, which may bring them closer to the labor market. Compared to the observably similar non-participants, the relative effect on women is especially important.

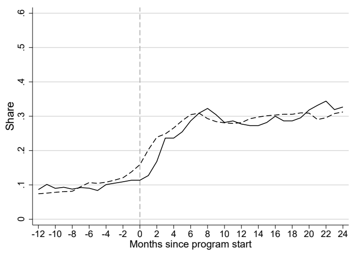
In these analyses, many participants are excluded due to the trimming of the samples – 139 women and 59 men – see Table A.8. For sensitivity analysis, the Appendix presents results where we use less restrictive trimming and only exclude observations with estimated weights larger than 2% of the total weights of the controls. In doing so, the number of excluded women and men are reduced to 90 and 38, respectively. As seen in Figure A.3, the employment rates among women are somewhat higher, but the conclusions remain the same.

By education level

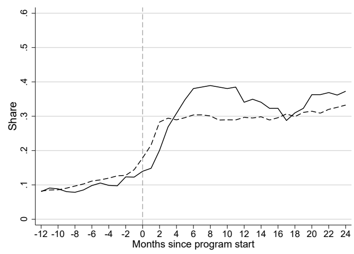
We know from the study by Lochmann et al. (2019) that highly educated immigrants benefited more from language training than immigrants with lower educational attainment in France. For example, it is possible that individuals with more schooling have higher learning efficiency (Lochmann et al. 2019).

We therefore divide our sample by education level, defining individuals with a low level of formal education as those with at most compulsory schooling. We label individuals with at least high school education as highly educated.

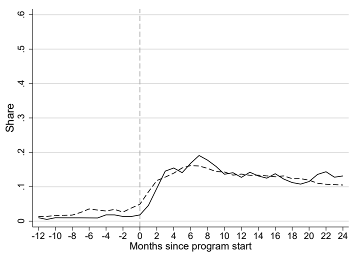
Figure 4.8. Employment results by education level



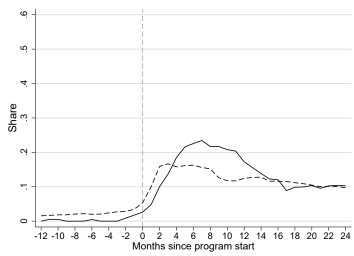
(a) Employment, Low level of education



(b) Employment, High level of education



(c) Subsidized employment, Low level of education



(d) Subsidized employment, High level of education

Note: The solid line represents the share for participants, and the dashed line for the weighted non-participants.

The results in Figure 4.8 imply that the positive effect of the Language Support Program on employment is explained by highly-educated individuals, which is in line with the findings in Lochmann et al. (2019). However, this effect is driven by subsidized employment; there are very small differences between the participants and weighted controls for individuals with low levels of schooling, and an approximately 10 percentage point higher share among subsidized employment for participants (vs. non-participants) with higher levels of schooling.

Since we exclude many participants here as well (94 with a low level of education and 97 with a high level of education, see Table A.8) we perform the same sensitivity analysis as we did for women and men. Using a less strict trimming rule reduces the excluded number of participants to 56 for low and 60 for high levels of education. In doing this, the employment rates rise to some extent, but the patterns remain the same.

4.6 Conclusions

Labor market integration for immigrants, especially for refugees, takes time in many Western countries. New measures to speed up integration are therefore of great policy interest, and filling the knowledge gap regarding which programs directed at immigrants work is of great importance for many refugee receiving countries. In this paper, we shed light on this issue.

We study a labor market program directed at immigrants with poor language skills in Swedish called the Language Support Program. The program consists of two parts. First, participants receive an introductory course to Swedish working life by a bilingual caseworker in their native language. Second, they are matched with a workplace where they either do a 3-month internship or a 6-month PSEP. The aim of the program is to improve the participants' language skills and understanding of the Swedish labor market by interacting with coworkers and being in a regular workplace. Throughout the program, participants have access to support from a bilingual caseworker.

We compare participants and observably similar non-participants for two years after entering the program, and find that participation increases employment by as much as 10 percentage points compared to non-participants, shortly after having entered the program. The positive effect on employment is only temporary, lasting for 8 months. The main temporary increase in employment is driven by subsidized employment. However, this is propelled by the participants who, throughout the program, take part in a PSEP. When we study employment patterns by kind of ALMP – PSEP or internship – we find that, employment rates for participants with an PSEP drop substantially when the program ends. However, for participants with an internship employment rates follow a positive trend after leaving the program. Furthermore, participants improve their formal language skills, measured by passing Swedish language courses at the highest levels. This contrasts with the results of Arendt and Bolvig (2020), who find that the work-first approach crowds out language acquisition. The support and access to a bilingual caseworker may explain why our results are less discouraging. However, our setting does not allow us to disentangle the mechanisms. This is an important question for future research.

As we divide the sample by sex and level of education respectively, we find that the positive effect on (subsidized) employment is most pronounced for women and individuals with higher educational attainment. The latter result is in line with the findings in Lochmann et al. (2019), who reveal that individuals with higher education benefit more from language training. In particular for women, where at most 10% of the non-participants obtain subsidized employment in the two years following the start of the program, the Language Support Program gives women access to more labor market oriented programs. This is especially interesting in light of earlier literature indicating that immigrant women are much less likely than immigrant men to gain access to this type of programs (Andersson Joona 2020; Cheung et al. 2019). Our findings also indicate that, although we do not see any overall effects on employment in the medium term, women and individuals with a high level of education are more likely than observably similar non-participants to be employed. As the program also seem to improve language skills and to give women in particular access to subsidized employment, it would be of interest to, as data for more recent years become available, follow the participants for a longer time period to see if the program leads to positive effects on employment in the long run.

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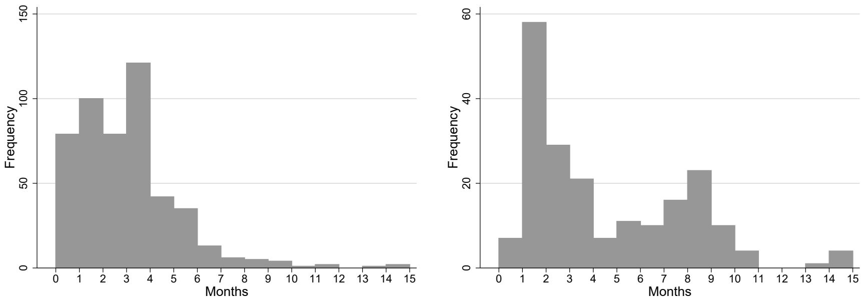
Appendix A: Additional tables and figures

Figure A.1. Duration of the Language Support Program by type



(a) Internship program *(b)* Employment program

Table A.1. *Participants entering the Language Support Program per period, by sex, and education level*

Period	Women	Men	Low	High
1	221	133	177	177
2	49	39	43	45
3	83	24	55	52
4	54	34	39	49
5	44	13	38	19
Total	451	243	352	342

*Notes:*Participants starting the Language Support Program in the fifth period are only used to estimate the weights utilized in the dynamic IPW, but not included when estimating the treatment effects. Low refers to having studied at most compulsory schooling, and high denotes having studied more than compulsory schooling.

Table A.2: *Original pool of covariates and variables, chosen as a limited set for different outcomes*

	(1) All	(2) Empl.	(3) Sub. Empl.	(4) SA	(5) SFI
Age	x				
Age^2	x				
Age 18-24	x				
Age 25-29	x				
Age 30-39	x				
Age 40-49	x				
Age 50-	x				
Female	x	x	x	x	x
Married	x				
Child in household	x				
Youngest child 0-3	x				
Youngest child 4-6	x				
Youngest child 7-10	x				
Youngest child 11-15	x				
Youngest child 16-17	x				
University, more than 2 yrs	x				
University, less than 2 yrs	x				
High school	x				
Less than high school	x				x
Education unknown	x				
0-2 yrs since immigration	x	x	x	x	x
3-5 yrs since immigration	x				
6-10 yrs since immigration	x				
$>$10 yrs since immigration	x				x
Born in E. Europe or C. Asia	x				
Born in North America	x				
Born in South America	x				
Born in Asia, excl. W. Asia	x	x		x	
Born in Africa , excl. N. Africa	x	x			
Born in W. Asia or N. Africa	x				
Born in other or unknown	x				
Own initiative to be registered	x	x		x	
JC Vällingby	x			x	
JC unga Globen	x				
JC Skärholmen	x			x	
JC Kista	x		x	x	
JC Farsta	x	x		x	
JC City	x				
JC enrollment in 2018	x	x			
JC enrollment in 2017	x	x		x	
JC enrollment in 2016	x				
JC enrollment in 2015	x				
JC enrollment in 2014	x				
JC enrollment in 2013	x				
JC enrollment in 2012	x				
JC enrollment in 2011	x	x		x	
JC enrollment in 2010	x	x		x	
JC enrollment in Jan.–March	x				
JC enrollment in April–June	x				
JC enrollment in July–Sept.	x				
JC enrollment in Oct.–Dec.	x	x			
Nr of spells at PES, at time of JC reg.	x		x	x	x
The ith quarter at PES when enter JC	x				

Continued on next page

Table A.2 – continued from previous page

	(1) All	(2) Empl.	(3) Sub. Empl.	(4) SA	(5) SFI
Ever PES, at time of JC enrolment	x	x	x	x	
Cumulated quarters at PES	x		x	x	x
Employed in t0-6	x				
Employed in t0-24	x				
Months employed t0-6	x				
Months employed t0-24	x				
Employers p. working month t0-6	x				
Employers p. working month t0-24	x				
Employed in t-12	x				
Employed in t-24	x				
Months employed t-12	x				
Months employed t-24	x				
Employers p working month, t-12	x				
Employers p working month, t-24	x				
Subsidized empl in t0-6	x				
Subsidized empl in t0-24	x				
Subsidized empl in t-12	x				
Subsidized empl in t-24	x				
Log earnings t0-6, 1000 SEK	x				
Log earnings t0-24, 1000 SEK	x	x		x	x
Log earnings t-12, 1000 SEK	x				
Log earnings t-24, 1000 SEK	x	x		x	x
Reason for SA, unemployment	x				
Reason for SA, other	x				
SA in t-12	x				
SA in t-24	x				
Log SA amount in t-12	x				
Log SA amount in t-24	x				
SA, nr of months t-12	x				
SA, nr of months t-24	x				
Log SA amount t-12	x				
Log SA amount t-24	x				
SA in t-1	x	x		x	
SA in t-2	x				
SA in t-3	x				
Pain rel. drug prescr., months t-6	x				
Pain rel. drug prescr., months t-12	x				
Pain rel. drug prescr. t-6	x				
Pain rel. drug prescr. t-12	x				
Psychotr. drug prescr., months t-6	x				
Psychotr. drug prescr., months t-12	x				
Psychotropic drug prescr. t-6	x				
Psychotropic drug prescr. t-12	x	x		x	x
Other drug prescr., months t-6	x				
Other drug prescr., months t-12	x				
Other drug prescr. t-6	x				
Other drug prescr. t-12	x				
Hospital visit t-12	x				
Hospital visit, months t-12	x				
Hospital visit t-6	x				
Hospital visit, months t-6	x				
0 quarter at PES, at JC reg	x				
1-2 quarter at PES, at JC reg	x				
3-8 quarter at PES, at JC reg	x		x		
> 8 quarter at PES, at JC reg	x				

Continued on next page

Table A.2 – continued from previous page

	(1) All	(2) Empl.	(3) Sub. Empl.	(4) SA	(5) SFI
0 months with SA, t-24	x				
1–6 months with SA, t-24	x			x	
7–12 months with SA, t-24	x				
1–12 months with SA, t-24	x				
13–24 months with SA, t-24	x				
Employed in t0-24	x				
1–12 months employed, t-24	x				
1–6 months employed, t-6	x				
1–12 months employed, t-12	x				
Employed in t0-24	x				
Months employed, stricter definition, t-6	x				
Months employed, stricter definition, t-12	x				
Months employed, stricter definition, t-24	x				
Not work ready	x				
Started SFI before t	x	x	x	x	x
Passed SFI before t	x				
SFI course D	x	x		x	x
SFI course C	x	x	x	x	
SFI course A	x	x		x	
SFI course B	x	x		x	
Refugee	x				x
Relative	x				
Relative, refugee	x				
Relative, other immigrants	x				

Note: Abbreviations used: SA - Social Assistance, JC - Job center, PES - Public Employment Service. $t0-x$ refers to the x month prior to enrollment at the job center, $t-y$ refers to the y month prior to the assignment period.

Table A.3. *Summary statistics of the estimated propensity scores and associated weights*

	Propensity Scores				Dyn. Inv. Prob. weights				
	mean	min	max	obs	Mean	min	max	obs	trim
Employment outcomes									
Participants	.241	.0000941	.827	639	1	1	1	637	3
Non-participants	.0117	1.43e-06	.805	41308	.0158	6.96e-07	3.12	36700	1
Subsidized employment outcomes									
Participants	.111	.000252	.341	639	1	1	1	637	0
Non-participants	.0138	1.83e-10	.341	41308	.0162	8.55e-10	.56	38050	0
Social assistance outcomes									
Participants	.248	.0000927	.917	639	1	1	1	637	64
Non-participants	.0116	4.23e-11	.889	41308	.0154	3.43e-10	7.43	36700	11
SFI outcomes									
Participants	.118	.0000839	.418	639	1	1	1	637	0
Non-participants	.0136	2.81e-09	.442	41551	.0157	1.99e-09	1.62	38905	0

Notes: The weights used are based on information from month 24 after the start of the program.

Table A.4. *Normalized difference (ND), before and after dynamic inverse probability weighting (DIPW), variables included for subsidized employment outcomes*

	Before DIPW			After DIPW		
	Part.	Non-part.	ND	Part.	Non-part.	ND
Female	.639	.546	.19	.639	.635	.00857
0-2 yrs since immigration	.703	.232	1.07	.703	.703	.00005
JC Kista	.149	.341	.458	.149	.147	.00524
Nr of spells at PES, at $t0$	1.23	2.18	.76	1.23	1.24	.0231
Ever PES, at $t0$	.983	.984	.0128	.983	.995	.115
Cumulated quarters at PES	4.33	8.73	.593	4.33	4.33	.0013
3-8 quarter at PES, at $t0$	.414	.217	.434	.414	.356	.121
Started SFI before t	.947	.796	.462	.947	.949	.00926
SFI course C	.411	.506	.19	.411	.416	.00964

Note: Abbreviations used: JC - Job center, PES - Public Employment Service, SFI - Swedish for Immigrants. $t0$ refers to enrollment at the job center and t to the start of the assignment period. The weights used are based on information from month 24 after the start of the program. Part. refers to participant.

Table A.5. *Normalized difference (ND), before and after dynamic inverse probability weighting (DIPW), variables included for employment outcomes*

	Before DIPW			After DIPW		
	Part.	Non-part.	ND	Part.	Non-part.	ND
Female	.637	.544	.191	.637	.627	.0213
0-2 yrs since immigration	.702	.235	1.06	.702	.683	.0403
Born in Asia, excl. WA	.0883	.149	.188	.0883	.0971	.0304
Born in Africa , excl. NA	.509	.397	.228	.509	.5	.0188
Own initiative to be registered	.0284	.0458	.0923	.0284	.0278	.00331
JC Farsta	.104	.178	.213	.104	.11	.0205
JC enrollment in 2018	.279	.0701	.572	.279	.149	.32
JC enrollment in 2017	.263	.0813	.496	.263	.095	.45
JC enrollment in 2011	.0631	.155	.298	.0631	.165	.324
JC enrollment in 2010	.136	.204	.182	.136	.249	.291
JC enrollment in Q4	.29	.228	.143	.29	.283	.0162
Ever PES, at $t0$	.983	.984	.0119	.983	.995	.114
Log earnings t0-24, 1000 SEK	1.81	4.47	.546	1.81	1.84	.00638
Log earnings t-24, 1000 SEK	1.91	4.67	.562	1.91	1.92	.00347
SA in t-1	.399	.62	.454	.399	.419	.0398
Psychotropic drug prescr. t-12	.0631	.14	.255	.0631	.0665	.0139
Started SFI before t	.946	.794	.465	.946	.946	.00048
SFI course D	.128	.28	.384	.128	.133	.0141
SFI course C	.409	.501	.187	.409	.419	.0215
SFI course A	.166	.112	.155	.166	.168	.00596
SFI course B	.792	.587	.454	.792	.783	.022

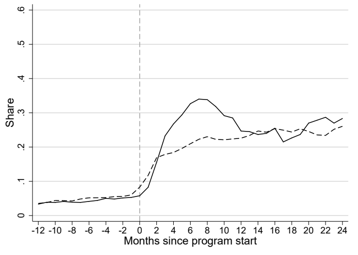
Note: Abbreviations used: JC - Job center, PES - Public Employment Service, SFI - Swedish for Immigrants. $t0$ refers to enrollment at the job center and t to the start of the assignment period. The weights used are based on information from month 24 after the start of the program. Part. refers to participant.

Table A.6. *Normalized difference (ND), before and after dynamic inverse probability weighting (DIPW), variables included for social assistance outcomes*

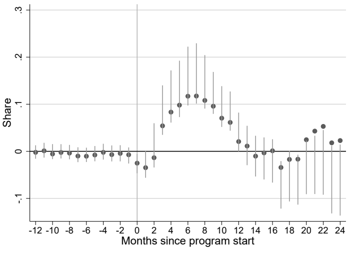
	Before DIPW			After DIPW		
	Part.	Non-part.	ND	Part.	Non-part.	ND
Female	.611	.543	.137	.611	.615	.00897
0-2 yrs since immigration	.67	.234	.973	.67	.648	.0467
Born in Asia, excl. WA	.0977	.149	.156	.0977	.105	.025
Own initiative to be registered	.0314	.0458	.0749	.0314	.0308	.00349
JC Vällingby	.0541	.162	.352	.0541	.0578	.016
JC Skärholmen	.0506	.168	.383	.0506	.0555	.0216
JC Kista	.166	.347	.424	.166	.176	.0263
JC Farsta	.115	.178	.178	.115	.122	.0224
JC enrollment in 2017	.251	.0813	.469	.251	.14	.285
JC enrollment in 2011	.0698	.155	.272	.0698	.077	.0274
JC enrollment in 2010	.15	.204	.141	.15	.161	.0293
Nr of spells at PES, at $t0$	1.25	2.16	.732	1.25	1.29	.0465
Ever PES, at $t0$	.981	.984	.0256	.981	.994	.119
Cumulated quarters at PES	4.4	8.66	.57	4.4	4.41	.00089
Log earnings t0-24, 1000 SEK	2.01	4.47	.499	2.01	1.93	.0192
Log earnings t-24, 1000 SEK	2.11	4.67	.513	2.11	2.02	.022
SA in t-1	.442	.621	.364	.442	.454	.0252
Psychotropic drug prescr. t-12	.0698	.14	.229	.0698	.0717	.00747
1-6 months with SA, t-24	.387	.316	.15	.387	.344	.0901
Started SFI before t	.941	.794	.442	.941	.938	.0117
SFI course D	.141	.28	.345	.141	.143	.00341
SFI course C	.419	.501	.166	.419	.421	.00396
SFI course A	.15	.112	.113	.15	.148	.0065
SFI course B	.775	.587	.412	.775	.767	.0196

Note: Abbreviations used: JC - Job center, PES - Public Employment Service, SFI - Swedish for Immigrants. $t0$ refers to enrollment at the job center and t to the start of the assignment period. The weights used are based on information from month 24 after the start of the program. Part. refers to participant.

Figure A.2. Weighted results, alternative employment measure



(a) Earnings>base amount



(b) Earnings>base amount, ATET

Note: The solid line in Figure A.2a shows the share with Earnings>base amount for participants, and the dashed line for the weighted non-participants. Figure A.2b outlines the difference between the lines with 95 % CIs based on 99 bootstrap replications.

Table A.7. *Normalized difference (ND), before and after dynamic inverse probability weighting (DIPW), variables included for Swedish for Immigrants (SFI) outcomes*

	Before DIPW			After DIPW		
	Part.	Non-part.	ND	Part.	Non-part.	ND
Female	.639	.548	.186	.639	.636	.00526
Less than high school	.493	.432	.122	.493	.483	.0204
0-2 yrs since immigration	.703	.24	1.05	.703	.695	.0175
>10 yrs since immigration	.0235	.168	.506	.0235	.0232	.00227
Nr of spells at PES, at $t0$	1.23	2.13	.717	1.23	1.23	.00612
Cumulated quarters at PES	4.33	8.54	.568	4.33	4.3	.00668
Log earnings t0-24, 1000 SEK	1.8	4.48	.55	1.8	1.82	.00381
Log earnings t-24, 1000 SEK	1.9	4.7	.57	1.9	1.91	.00318
Psychotropic drug prescr. t-12	.0628	.14	.258	.0628	.0632	.00165
Started SFI before t	.947	.793	.468	.947	.947	.00299
SFI course D	.127	.277	.381	.127	.129	.00621
Refugee	.612	.504	.22	.612	.598	.0294
Other SFI variables, not included in the Propensity score						
Passed SFI before t	.408	.472	.128	.408	.391	.0357
SFI course A	.165	.11	.158	.165	.134	.086
SFI course B	.793	.585	.46	.793	.722	.165
SFI course C	.411	.5	.179	.411	.449	.0756

Note: Abbreviations used: JC - Job center, PES - Public Employment Service, SFI - Swedish for Immigrants. $t0$ refers to enrollment at the job center and t to the start of the assignment period. The weights used are based on information from month 24 after the start of the program. Part. refers to participant.

Table A.8. *Summary statistics of the estimated propensity scores and associated weights for different subgroups*

	Propensity Scores				Dyn. Inv. Prob. weights				
	Mean	min	max	obs	Mean	min	max	obs	Trim
Women									
Particpants	.33	.000124	.927	408	1	1	1	407	139
Non-participants	.0123	3.03e-09	.899	22275	.0164	4.18e-09	8.18	20215	19
Men									
Particpants	.277	.000108	.941	231	1	1	1	230	59
Non-participants	.0097	4.72e-07	.941	17198	.0143	4.73e-07	17.3	14230	14
Low education									
Particpants	.319	.000176	.888	315	1	1	1	314	94
Non-participants	.0142	6.46e-07	.898	15064	.0213	6.48e-07	9.28	13235	18
High education									
Particpants	.306	.000675	.948	324	1	1	1	323	97
Non-participants	.0103	6.31e-11	.887	21891	.0142	9.87e-10	7.39	19435	22

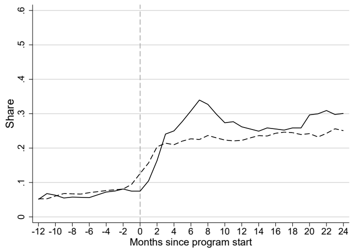
Note: The weights used are based on information from month 24 after the start of the program.

Table A.9. *Summary of the normalized difference in means, before and after dynamic inverse probability weighting (DIPW), for different subgroups.*

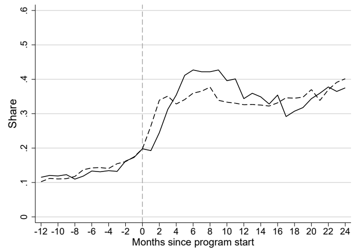
	Mean	Min	Max	> 0.3	N
Women					
Before weighting	0.222	0.021	0.623	9	28
After weighting	0.022	0	0.083	0	28
Men					
Before weighting	0.316	0.001	0.875	9	18
After weighting	0.042	0.001	0.238	0	18
Low education level					
Before weighting	0.254	0.032	0.675	7	20
After weighting	0.030	0.001	0.173	0	20
High education level					
Before weighting	0.261	0.002	0.789	11	33
After weighting	0.022	0.001	0.177	0	33

Note: > 0.3 shows the number of variables where the normalized difference is greater than 0.3. N is the number of covariates included in the propensity score. The weights used are based on information from month 24 after the start of the program.

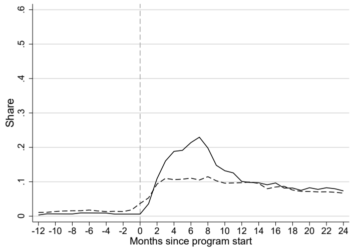
Figure A.3. Employment results by sex, less restrictive trimming.



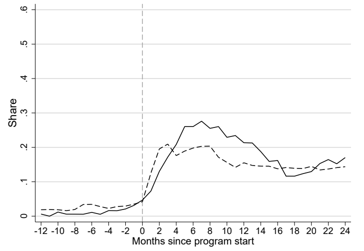
(a) Employment, Women



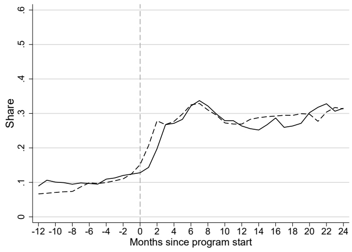
(b) Employment, Men



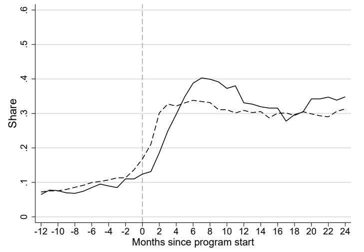
(c) Subsidized employment, Women



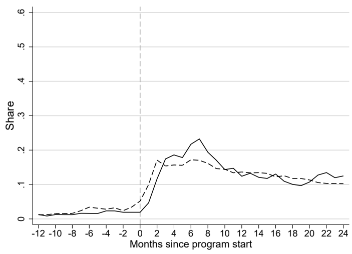
(d) Subsidized employment, Men



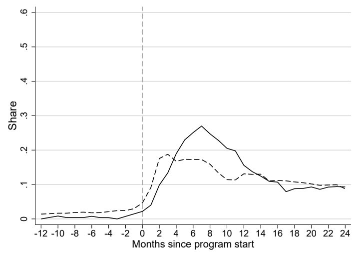
(e) Employment, Low level of education



(f) Employment, High level of education



(g) Subsidized employment, Low level of education



(h) Subsidized employment, High level of education

Note: The solid lines indicate the share with employment for participants, and the dashed line for the weighted non-participants. In these figures, the sample is trimmed by excluding individuals with weights exceeding 2% of the sum of weights for the controls, instead of 1% as in the main analysis. In the analysis by sex, this corresponds to 90 women and 38 men among the participants. In the analysis by education level, this corresponds to 56 low and 60 high educated among the participants.

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