

Labor market effects of a youth summer employment program in Sweden

Daniel Knutsson
Björn Tyrefors

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

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

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

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

ISSN 1651-1166

Labor market effects of a youth summer employment program in Sweden*

Daniel Knutsson[†] and Björn Tyrefors[‡]

February 14, 2024

Abstract

We evaluate a non-targeted summer youth employment program (SYEP) for high school students aged 16-19 in Stockholm, Sweden, where public sector job offers were as good as randomly assigned. In contrast to previous studies evaluating SYEP that targeted groups with lower socioeconomic status, we find substantial labor market effects but no effects on education, crime, or health outcomes. However, income is negatively affected except during the program year. The penalty increases in absolute terms but does not change much in relative terms over time. The penalty is consistently statistically significant and large just after high school graduation but there are indications that the penalty attenuates at ages 24. The adverse effects are the largest for applicants not enrolled in an academic track, who are males, and with less educated mothers. Interestingly, the extensive margin (having a job) is not the critical factor. Instead, a SYEP job offer affects the probability of obtaining more qualified and full-time employment after high school graduation. We argue that receiving a program job leads to less private-sector labor market experience, provides a negative signal, and disrupts (private) labor market connections, which is vital for those seeking a job just after high school.

Keywords: Labor market programs, Youth unemployment, Summer employment, Random list, SYEP

JEL codes: J13, J45, J38, J21

*We are grateful for comments provided at seminars and conferences at the IFN, Stockholm University, IFAU Uppsala, Gothenburg University, and AASLE 2022 Tokyo. A special thanks to the two referees at IFAU who provided invaluable feedback. We are further indebted to the staff at the labor market administration at Stockholm municipality for excellent data and information support.

[†]Corresponding author: Örebro University School of Business, 701 82 Örebro, Research institute of Industrial Economics, and Center for Health Economics Research, Uppsala University (e-mail, daniel.knutsson@oru.se; telephone: +46(0)19-30 30 00).

[‡]Research Institute of Industrial Economics (IFN), Box 55665, 102 15 Stockholm (e-mail, bjorn.tyrefors@ifn.se; telephone: +46(0)8-665 4500. and Dep. of Economics, Gothenburg University.

CONTENTS

1	Introduction	3
2	Background information on the Stockholm SYEP	9
3	Data and empirical approach	11
4	Results	25
5	Robustness	42
6	Conclusions	51
7	Appendix: Additional Tables and Figures	57

1 INTRODUCTION

Youth unemployment is a significant concern in many countries, particularly since early career unemployment can have long-term consequences.¹ However, there are stark differences between EU countries and the US. The youth unemployment rate in the US has been 2-5 times lower over time. For example, in Sweden, youth unemployment was as high as 20 percent in 2018, while in the US, youth unemployment was only around 8 percent (OECD, 2020). Thus, in the US, unemployed youth more often come from the most disadvantaged groups, while in the EU, representation is broader. Therefore, it is unsurprising that the groups targeted by youth labor market programs also generally differ across the two continents (Heckman et al., 1999).

As in many European countries, youth unemployment in Sweden is a general concern. While active labor market policies targeting youths on the labor market rarely provide considerable benefits, policies targeting youths while still in school could be highly effective (Card et al., 2010). A recent study on Swedish data shows that students involved in part-time work assigned through market mechanisms during high school have an 18 percent higher probability of attaining stable employment upon graduation. The study concludes that subsidized jobs during high school could serve as an effective policy tool in combatting youth unemployment (Hensvik et al., 2023). A broad literature from the US provides evidence of this sort through evaluations of summer youth employment programs (SYEPs) (e.g., Davis and Heller, 2020; Gelber et al., 2016; Heller, 2014). However, rigorous evidence from European contexts, where youth unemployment conditions are markedly different, is lacking.

In this paper, we evaluate a summer youth employment program (SYEP) where offers of subsidized summer jobs in the municipality of Stockholm (Sweden) were as good as randomly assigned. Youths aged 16-19 could apply for three weeks of municipal employment during summer. Our data include all applications to the program between 2012 and 2018 and consist of almost 100,000 applications made by close to 50,000 youths. The program targeted all youths in Stockholm within the age group; i.e., it did not target disadvantaged youths only.² We study if participation in the SYEP impacted the labor market trajectories of the youths.

Subsidized summer jobs have been studied extensively in the US but neglected in Europe.

¹The empirical literature on scarring effects goes back to Ellwood (1982), Corcoran (1982) and Heckman and Borjas (1980). A more recent study is Nordström Skans (2011), which finds that poor labor market performance early on results in persistent negative effects. See also Gregg (2001) and Biewen and Steffes (2010). See also Hensvik et al. (2023) for estimates on the importance of employment during high school for the transition into the regular job market.

²However, members of prioritized groups, i.e., high-school dropouts, youth with disabilities, or youth with social problems, were always offered jobs if they applied to the program. They make up around 7 percent of all applications.

Most recently, it was found that programs in Chicago, New York, and Boston did not impact labor market outcomes over the medium term and had minor, if any, effects on educational outcomes (Gelber et al., 2016; Heller, 2014; Leos-Urbel, 2014; Schwartz et al., 2021; Valentine et al., 2017). However, considerable reductions in criminal activity and even mortality have been documented (Gelber et al., 2016; Heller, 2014; Modestino and Paulsen, 2022). Recently, one proposed explanation for the puzzle of declines in crime with no indication of improved human capital or higher income has been suggested by Davis and Heller (2020). They argue that treatment effect heterogeneity is part of the answer and that the zero average employment effect could mask heterogeneity that explains the net decrease in crime. In particular, their findings suggest that the null average employment effect hides a group whose employment improves and that this subgroup is younger and more engaged in school than the group with no employment gains. This group is less disadvantaged/disconnected than those typically targeted by youth employment programs. Thus, there could be positive employment effects for more advantaged groups and evaluating a SYEP with more advantaged participants seems to be a plausible next step forward. The Stockholm SYEP is largely non-targeted and desirable for youths to participate in, including those with solid socioeconomic backgrounds. Youths receiving an offer of a SYEP job almost always accept the offer.

Our empirical design is based on oversubscription to the SYEP. After all applications are received, each applicant is assigned a random number. It was centrally set randomly by the computer system and never changed. Case workers then allocated jobs partly based on the rank ordering of the random number. Matching took place at the 14 local municipal offices within Stockholm municipality. The youths residing within these districts, i.e., within the area of each local office, were matched to local municipal jobs. Most important for our empirical design, the administrative staff could not alter this random number. Moreover, our random number was not public, meaning the youths could not respond to the draw before job offers were made.

Although the random number could not be manipulated, there were legitimate ways for caseworkers to deviate from the rank-ordered list. First, prioritized youths were always assigned jobs (e.g., high-school dropouts, youth with disabilities, or social problems). Second, youths who were offered a job last year (or had worked previously in the program) were only offered a job if there was no oversubscription. This “participation penalty” is the most significant deviation from the rule, as about 60 % re-apply even if they had a job offer from the program previously. Third, the case worker may care about an old applicant who has never participated before. Finally, program employees tried to match applicants to jobs based on their stated preferences and availability. For example, applicants may say they do

not want to work in care or maintenance, only want to work a specific time, or have allergies. The case worker should also take these considerations into account.³

In sum, the SYEP considers factors other than the random number when assigning job offers. The random number results only in a tie-breaking experiment if two similar applicants compete for one suitable job. This results in a relatively smooth relationship between the random number and the probability of receiving an offer, with few visible cutoffs for eligibility in the data.⁴ Importantly, for our empirical design, the individually assigned random number itself could not be influenced. Hence, it is exogenous to a job offer as long as no sample restrictions are made (e.g., excluding/controlling for prioritized applicants or re-appliers and age, etc.) or "bad controls" are introduced.

Consequently, we cannot rely on the same empirical strategy as the canonical study by Angrist (1990). Our analysis is different since we do not have a cutoff determining eligibility status due to the discretion of the case worker, as discussed. Instead, we have to rely on random numbers that could not be manipulated. Thus, the instrument in our primary analysis is a continuous variable instead of a binary eligibility indicator. In the main analysis, we assume linearity, but we allow for nonlinear and flexible estimations, and our main findings are not sensitive to the linearity assumption (See Table 12).

When evaluating the SYEP in the municipality of Stockholm, we find considerable income effects but no statistically significant effects on education, crime, or health outcomes. Surprisingly, only the impact on income during the program year is positive, while the medium-term effects of receiving an offer are negative. More precisely, in the program year, being offered a SYEP job increases income by 400-800 Euros (depending on age at application). This rise in income amounts to about 25-50 percent compared to the average income in the application year. During the first year after application, when the regular applicant is often in high school, a job offer is related to lower income. Still, the effects are small and statistically insignificant. However, just after high school graduation, at age 20-23, the effects are substantial and negative. Just after graduation, the punishment of an offer grows in absolute terms over time and is around 1,000 Euros when the applicants are 23, or about 8.5 % compared to the mean. At age 24, there are indications that the effects level off, and the estimate is not statistically different from zero.

Our primary explanation is that the youths not offered a job are more likely to gain private sector experience, which affects the probability of obtaining a more qualified job and increases the chances of attaining full-time employment right after high school. Our

³See <https://jobba.stockholm/feriejobb/soka-feriejobb/#step-2>

⁴See Figure A1 in appendix for the relation between the Job offer indicator and the rank order in the 14 district in 2012. In only a few out of 14 district there is a visible jump in the distribution. Other program years show a similar picture.

results show that a program job offer increases subsequent program participation reducing the prospects of regular employment outside the program. Private sector experience is arguably most important for students on vocational tracks and other students who are unlikely to continue university studies immediately after graduation. Consistent with this argument, our heterogeneity analysis shows that the negative effects are the largest for applicants not enrolled in an academic track, males, and with less educated mothers. Since youths in vocational tracks enter the labor market earlier, they should have high incomes in the medium term. Accordingly, we find that most of the adverse effects on later life income dynamics comes from youths at the higher end of the income distribution but that there are no or minor effects for youths at the lower end of the same distribution. Youths that enter the labor market after high school, and that are most in need of connections to the regular labor market, are hurt the most by participating in the program.

The Summer Youth Employment Program (SYEP) emerges as a promising and efficient intervention. Drawing on Swedish data, Hensvik et al. (2023) present compelling evidence indicating that after-school or summer employment opportunities, assigned through market mechanisms, play a pivotal role in facilitating the transition to the labor market for students pursuing vocational tracks. Their study reveals that students engaged in part-time work exhibit an 18 percent higher likelihood of securing stable employment upon graduation, often within the establishments where they gained prior work experience (Hensvik et al., 2023). This noteworthy finding underscores the profound impact that youths' employment choices during high school can exert on their subsequent employment trajectories and income prospects. However, our analysis casts doubt on the assertion made by Hensvik et al. (2023) regarding the substitutability of standard summer jobs allocated through market mechanisms with subsidized program jobs.

Our heterogeneity analyses show that the most negatively affected youths are in high-school vocational tracks (i.e., most likely to start to work after graduation). Although employment during high-school should ease the transition to the labor market, two features of the program could instead have impeded these transitions. First, the Stockholm SYEP provided relevant jobs for only a subset of applicants (e.g., child care and elderly care). Indeed, we find that the youths most harmed by a job offer were males in more technical vocational tracks with little harmful effects on females. Second, the possibility of continuous employment at the same establishment over time within the program was limited due to eligibility rules. A previous job offer reduced the likelihood of a renewed job offer in the following year. Focusing on SYEP jobs instead of establishing connections with potential future employers could have been costly for these youths. Our pattern of lower wages, and sorting into different sectors/job types is consistent with this interpretation.

One policy implication of our findings is that the SYEP should recognize its relation to firm-specific skills and networks. Emphasizing firm-specific investments could be done by providing only summer jobs that provide real job training, for example, by subsidizing private summer jobs, which in turn increases the probability of obtaining a non-subsidized summer or after-school job after the program (Le Barbanchon et al., 2022). Suppose a SYEP is not designed this way (i.e., there is no subsequent round, or the skills are not transferable to other sectors). In that case, our results suggest that the program is costly to taxpayers and harmful to many participants. It is worth pointing out that the program may well be beneficial for prioritized or disadvantaged groups. Under our study design we cannot evaluate the effects for this group as they are not part of the random assignment. However, based on the international evidence of the beneficial effects of SYEP for disadvantaged groups, it may be worth directing the resources to the most underprivileged only.

Our findings contribute to the almost nonexistent evaluation literature of SYEPs in Europe.⁵ Alam et al. (2015) found that female sophomores increased their earnings over time through participation in a similar program in the small town of Falun, Sweden. Using a much larger dataset from the largest urban region in Sweden, we cannot confirm these findings, as our results suggest that there are no statistically significant income gains for females. One potential explanation for the result may be that Stockholm is Sweden’s most extensive and vital youth labor market and that private sector summer jobs are more abundant.⁶

Most evidence of similar programs comes from the US. Gelber et al. (2016) analyzed a SYEP in New York and documented income dynamics similar to those we found. The lottery winners had higher incomes in the same year but lower incomes in the following years, with the effects fading after four years. Our findings mimic those of Gelber et al. (2016), except that we estimate much larger negative impacts of program participation. We argue that we find larger adverse effects than the NYC SYEP, where mostly disadvantaged youth participated, because our applicant pool is more representative of the national youth population. Compared to the national average, household income in our sample is 20-30 percent higher and the applicants parents have higher educational attainment.⁷ A population of more advantaged applicants has better prospects of finding a regular job in the labor market, thus earning higher incomes without a SYEP job. Another difference may be that the jobs evaluated by Gelber et al. (2016) were typically private-sector jobs, whereas the

⁵Evaluation using either RCT or quasi-random variation as empirical design.

⁶See <https://arbetsformedlingen.se/statistik/sok-statistik>.

⁷In 2015, we calculate that the average parental income was between 55 and 60 thousand euros before taxes. In the same year the average disposable household income was 34.6 thousand euros (SCB, 2017). Income taxes should average 30 percent and abstracting from transfers gives us a lower bound of the population average household income. Additionally, parental education among the applicants was higher than the national average as we show in Table A1.

Stockholm SYEP program provides mostly municipal jobs.

Another interesting finding in Gelber et al. (2016) was that applicants who won the lottery had a lower mortality risk. While our experiment is severely underpowered for an analysis of mortality — we have only 75 deaths in the total sample — we engage with this question by investigating the program effects on prescription drugs and hospital visits. In contrast to the New York setting, summer youth employment in Stockholm did not have any discernible impact on health outcomes. Again, this could be due to a more advantaged population in our setting. Concerning criminality, we do not find that a job offer affects committed crimes. Not finding any discernable effects on criminal activity differs from previous studies (Gelber et al., 2016; Heller, 2014; Modestino and Paulsen, 2022), which typically study more disadvantaged youths with higher baseline criminal activity. A reoccurring finding in the literature on summer employment programs is that they document sizable adverse effects on crime without any discernable employment effects. These findings are jointly somewhat puzzling. Davis and Heller (2020) address this puzzle by exploring heterogeneous effects. They find positive employment effects among less disadvantaged youths. Even though our complier group is advantaged compared to the sample in the study by Davis and Heller (2020), we can still investigate heterogeneous effects in our population. Our findings confirm the analysis in Davis and Heller (2020) in that the impact of SYEP jobs is heterogeneous over family socioeconomic status (SES), but we deviate as none of our subgroups do benefit of participating. Low maternal education and vocational track students face the most significant income reductions, while students from academic families on academic tracks are less adversely affected. Simultaneously, low SES students enter the labor market earlier than academic track students and have higher incomes in the short to medium term. Our findings suggest that the kind of training jobs available in the Stockholm SYEP should not be provided to youths with good employment prospects on the regular labor market, especially among youths who can benefit from temporary regular employment before graduation. A joint implication of our findings is that high returns to SYEPs require careful targeting and knowledge of contextual factors.

We conclude that SYEPs that do not provide real job opportunities have little prospect of being beneficial to a general group of youths and can even be detrimental in the medium term. Subsidizing jobs in the private sector could be a better option but may be dependent on the prospect of continued employment without program subsidies. Targeting resources to youths with the lowest probability of finding regular summer and part-time work is likely a more efficient strategy that reduces public spending and avoids harming the income trajectories of youths in the medium term. Thus, our paper also adds to the literature on harmful active labor market programs. Previous evaluations of policies in OECD countries indicate

that these programs usually have, at best, a modest impact on participants' labor market prospects. But at the same time, they also suggest considerable heterogeneity in the effects of these programs. For some groups, a compelling case can be made that these policies generate high rates of return. In contrast, these policies did not impact other groups and may have been harmful or neutral (see Heckman et al. (1999) for a thorough review or Card et al. (2010) for evidence of programs aimed at youths).

The rest of this paper is organized as follows. Section 2 presents background information on the program under study, followed by section 3, where we describe the data and empirical design. In section 4, the results are presented. In section 5, we provide robustness checks, and in section 6, we conclude the paper.

2 BACKGROUND INFORMATION ON THE STOCKHOLM SYEP

Municipalities and regions in Sweden employed 82,600 youths in 2018 (SKR, 2019). The Stockholm SYEP has accommodated approximately 7,000 summer workers between the ages of 16 and 19 annually since 2012, and the Stockholm municipality budgeted at least 10 million euros for the SYEP in 2018. The municipality states that the program's purpose is to aid youths in their transition to the labor market, provide youths with meaningful summer activities, and increase interest in municipal sector jobs (AMF, 2018).

The city of Stockholm has long provided summer jobs to high-school-aged youths, with somewhat different rules at the local city district offices in Stockholm. In 2012, the SYEP was restructured, and the application process became centralized. From that year, the job offer allocation process became formalized, and a new centralized digital platform for applications was created. All youths between 16 and 19 years of age residing in Stockholm were eligible to apply to the program using the new web-based application tool (before 2015, only 16 to 18-year-olds were eligible).

All applicants are assigned a random number in the central computer system, and the city district case handlers can never manipulate the number. Matching took place at the 14 local city district offices within Stockholm municipality. The youths residing within these districts, i.e., within the jurisdiction of each local office, were matched to local municipal jobs based on an ordered list of their random number.

However, according to the program guidelines, the random number is not the sole assignment mechanism. First, case handlers should always provide job offers to prioritized youths. The prioritized group includes youths who have dropped out of high school, youths with disabilities, and youths with social problems (AMF, 2018). Approximately 7 percent of the youths who applied to the program were prioritized to receive a job offer. Second,

youths who were offered a job last year (or had worked previously in the program) were only offered a job if there was no oversubscription. This "participation penalty" is the most significant deviation from the rule, as about 58 percent of program participants re-apply. Third, applicants are encouraged to state their preferences, previous experiences, hobbies, and availability. Language skills may also play a role in specific jobs (e.g., elderly care). In sum, the Stockholm SYEP considers factors other than the random number when assigning job offers.

The amount of information to consider for case handlers makes the assignment mechanism a complicated task with many degrees of freedom. We interpret the guidelines such that the random number results only in a tie-breaking experiment if two enough similar applicants compete for one suitable job. This assignment process results in a relatively smooth relationship between the random number and the probability of receiving an offer, with few visible cutoffs for eligibility in the data.⁸ An essential feature for our empirical design is that the individually assigned random number itself could not be influenced and is exogenous to a job offer.

Table 1 shows the different types of jobs offered. The largest categories are childcare, care of disabled people, and maintenance. Importantly, program jobs cannot act as substitutes for regular employment. Not being allowed to perform everyday tasks meant that summer workers could only provide "quality enhancing" services. "Quality enhancing" duties could include helping clean public parks or updating the Instagram of a municipal agency. Therefore, it is not surprising that it has been pointed out that these jobs require less effort than a regular private summer job (Nyström, 2021).

The Stockholm SYEP jobs should last for three weeks, for 90 hours of work (see Figure A2 in the appendix). The wage is approximately 10 euros per hour, with age-specific variations, leading to a total gross income of roughly 900 euros over the whole period.⁹ This hourly wage is about 10 percent higher than, for example, the lowest-paid jobs in fast-food restaurants (HRF, 2020). Thus, the SYEP jobs required relatively less effort and paid relatively high wages. Moreover, the application process imposed few time and social costs, as it was web-based with no interviews. Consequently, there was excess job demand, with more applications than jobs available.

⁸See Figure A1 in appendix for the relation between the Job offer indicator and the rank order in the 14 districts in 2012. In only a few out of 14 district there is a visible jump in the distribution. Other program years show a similar pattern.

⁹For each year of age, it increases by around 0.1 euros per hour.

TABLE 1
TYPES OF JOBS PROVIDED BY THE PROGRAM

Description	Observations
Child care	13471
Elderly or care of disabled	11050
Office/Administration/Projects	1678
Culture/Events	2147
Maintenance/cleaning/service outdoors	12779
Maintenance/cleaning indoors	3852
Other	5889
No information	41229
Total	92095

Notes. This table summarize information received from the municipal labor market administration in Stockholm (Arbetsmarknadsförvaltningen) on the program jobs. The information describes the types of jobs that the youths performed. However, there are many missing observations for this variable. Source: Authors calculations from data.

3 DATA AND EMPIRICAL APPROACH

3.1 Data

We have access to the program application data from the system created by the municipal central labor market administration (Arbetsmarknadsförvaltningen) in Stockholm in 2012. In total, we have information on close to 92,000 applications to the program from 50,000 applicants for the years 2012 to 2018. These data contain the entire pool of applicants to the program for each year with information on their local district of residence and prioritized status. Moreover, we have information on the applicant’s job status, which allows us to distinguish between a job offer and job acceptance. Importantly, the data include the random number, which enables us to make causal inferences regarding the effects of a job offer. The yearly number of provided jobs, job offers, and applications is presented in Figure 1. Every year, there is an oversubscription of about 30 - 50 %, although it varies by district. Interestingly, very few, less than 15 percent, of youths offered a job declined. Hence, we argue it is a reasonable approximation to think of the treatment as taking a job instead of being offered a job, although the take-up is not 100 percent.

We combine the data on the program participants with data from Statistics Sweden on their labor market and educational outcomes and parental information using unique personal identifiers. Furthermore, we add data from The Swedish National Council for Crime

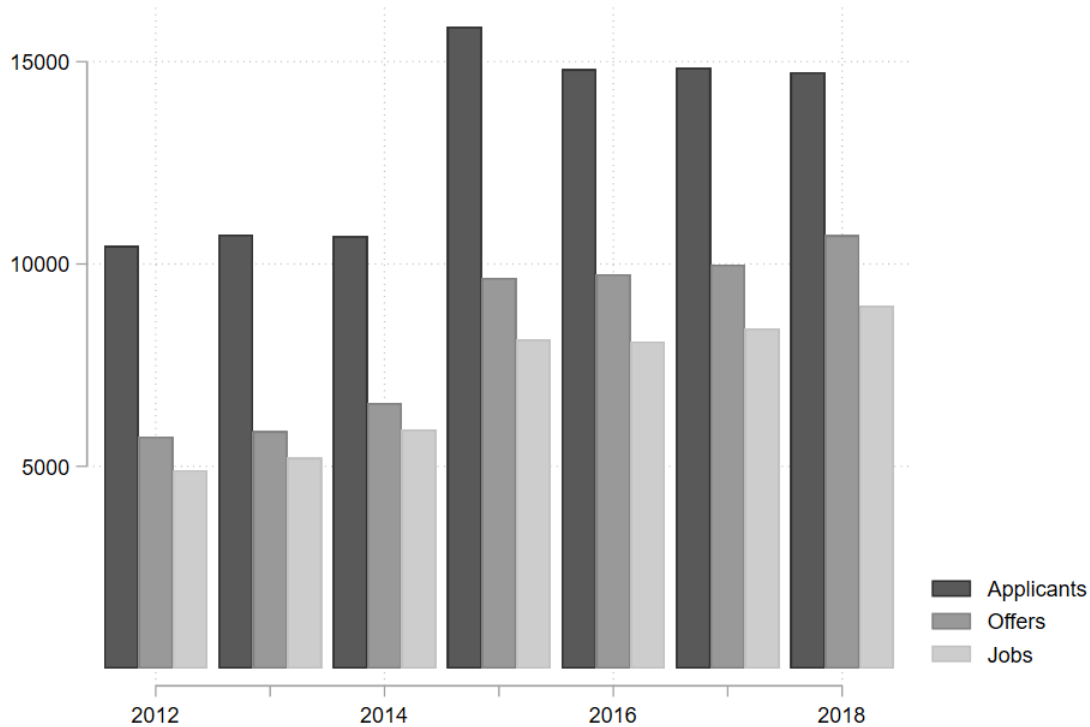


FIGURE 1

Applications, Job Offers, and Jobs

This figure describes the data that we have acquired from the municipal labor market administration in Stockholm (Arbetsmarknadsförvaltningen) on program participation. It describes the total number of applicants, job offers, and jobs by each year. In 2015, also 19 years olds were allowed to apply to the program and the program thereby expanded to facilitate more applicants.

Prevention on criminal activity and health care utilization from The Swedish National Board of Health and Welfare. Most of our outcome variables are available up until 2020.¹⁰ Thus, for the earliest cohort in 2012, we can observe outcomes up to 8 years after their application year.¹¹

The primary outcome that we study is wage income. This measure is relevant, as it captures the labor market success of each applicant and is well measured yearly through the income tax registers we use. One of the main ambitions of the program was to ease the transition between schooling and the labor market. If the program successfully facilitated

¹⁰Income and employment data is available up until 2020. Crime data and prescription drugs are available until 2019 while data on contacts with specialized healthcare are available until 2018. University registration data are available up until 2021.

¹¹For some outcomes (e.g., educational outcomes), we have data up to 2019.

this transition, we would expect to see income gains among applicants offered a job. These effects are also relevant when analyzing whether the program was cost-effective. We can observe income up to age 24 for all applicants who were 16 and older in 2012. In the same way, we can observe outcomes up to age 23 for applicants who were 16 and older in 2013 and so forth.

Since every year there is a new experiment in every district, we are not worried that our data is unbalanced by year of application, as it will pose no threat to internal validity.¹² We show that our different samples are balanced regarding pre-determined characteristics (Figure A5-A12 in the Appendix). Only seven pre-determined characteristics are statistically significant at the 5 % level for $8 \times 24 = 192$ regressions, less than expected from random sampling.¹³ Overall, our different samples seem to be balanced. Therefore, we can study the program's effects on income dynamics over time and investigate the effects of a program job offer on later labor market success.¹⁴

In addition to information from the tax registers, educational qualifications, and other information on the youths and their parents, we also have access to additional information in the Swedish administrative registers. We use several registers with educational data to derive pre-determined variables, such as the applicants' 9th-grade educational performance (GPA) and whether they were registered in an academic or vocational high school track at the time of the application.

Moreover, we construct an outcome related to higher education activity. This outcome indicates whether an applicant was registered at a university at any point up to age 24.¹⁵ With this variable, we can investigate whether the program had any effects on the educational performance or preferences of the youths. This information is vital as educational decisions can have a meaningful impact on the dynamics of wage income over time.

Labor market opportunities are tightly connected theoretically to criminal activity through the opportunity cost of crime (Becker, 1968). Accordingly, criminal behavior can be affected by SYEP participation (see, e.g., Gelber et al. (2016)). Therefore, we add data from The

¹²However, we are clearly losing observations for the later ages with a reduction in sample size as a consequence.

¹³For the age 23 sample, there are however 5 statistically significant characteristics. This could of course be by chance but seems a little worrisome. We therefore also run regressions where the predetermined characteristics are included as control as robustness in Table 13, which will be discussed more thoroughly in the robustness Section 5.

¹⁴However, we should point out that the effects for later ages are based on the early years of the program, which may limit the external validity.

¹⁵These data are available up until 2021. For applicants that were 16 or older in 2012 we can observe their university registration status up to age 25 but for younger applicants, that applied later to the program, we observe a shorter follow up (for example, 16-year-old applicants in 2018 are observed until 2021 when they are 19).

Swedish National Council for Crime Prevention (BRÅ) on yearly convictions and suspicion of criminal activity. We further add information on health outcomes. Although health effects are not expected ex-ante in this setting, previous research has found intriguing results on mortality (Gelber et al., 2016). Unfortunately, our data are unsuitable for evaluating effects on mortality since applicants are uncommon to die in our population (in total, we observe 75 deceased). Instead, we use yearly medical prescriptions and hospitalization data to analyze whether the program under study affected health.

Our data consists of 92,095 applications by 48,901 applicants between 2012 and 2018 and are described in detail in Table 2. 63 percent of all applications resulted in a job offer, and 85 percent of youths offered a job accepted. The program was popular, and 51 percent of all applicants between 2012 and 2017 applied again in the following year. Prioritized applicants could apply outside the age limit, and some applied for jobs up to 7 times. Few could apply this many times, and the average number of applications per applicant in the data is 1.9. These applications resulted in 1.2 job offers per applicant and one accepted job on average. The Rank Order, presented in Table 2, is our instrumental variable and is further detailed in Section 3.2.

Unlike US-based SYEPs, the population of applicants we study is not very disadvantaged on average (Gelber et al., 2016). The applicant’s household income is 20-30 percent higher than the national average.¹⁶ Furthermore, Table 2 shows that only 85 percent of all the applicants are enrolled in high school, 23 percent are born outside Sweden, and 60 percent are enrolled in an academic high school track. Moreover, 41 percent of the applicants’ mothers have more than a high school education, and 50 percent of the applicants had a positive income in the year before the application. The applicants also have higher grades than the national average. The national average GPA among 9th graders was around 215 between the years we studied, while the applicants in our data have an average GPA of 225.¹⁷ If anything, the program applicant pool is more advantaged than the national average of youths.

Differences in sample size for parental information are due to missing data in the administrative registries. Some youths cannot be matched to a living parent with a valid personal identity number, or the parent does not have information on educational attainment due to, e.g., migration.

Table 3 presents summary statistics for the income and health-related outcome variables. The income in the application year is roughly 1,500 euros, close to a 200 % increase compared to the year before the application (see Table 2). After the application year, there is a steady increase in income until age 23, when it seems to level off. Concerning the extensive margin,

¹⁶See footnote 7

¹⁷See recent and historical national statistics at: <https://www.skolverket.se>.

TABLE 2
SUMMARY STATISTICS: PREDETERMINED CHARACTERISTICS

	Mean	Std dev	Min	Max	Observations
Program job offer	0.632	0.482	0	1	92095
Accepted job	0.852	0.355	0	1	58222
Reapplied Next Year	0.511	0.500	0	1	77358
Reapplied next year if job offer	0.354	0.478	0	1	77358
No Applications	1.883	0.995	1	7	48901
Total number of job offers	1.191	0.937	0	5	48901
Total number of accepted jobs	1.014	0.892	0	5	48901
Prioritized	0.073	0.259	0	1	92095
Rank Order (0, 1)	0.500	0.289	0	1	92095
Income Year Before (€)	517.694	1233.387	0	58450	91285
Not in High School	0.150	0.357	0	1	92095
Prescriptions 2011	1.676	4.535	0	177	92095
Hospital visits 2011	0.681	2.027	0	70	92095
Mother Education > 12 yrs	0.411	0.492	0	1	72813
Positive Income Year Before	0.498	0.500	0	1	92095
Age > 16	0.760	0.427	0	1	92095
Immigrant	0.231	0.421	0	1	92095
Academic Program	0.662	0.473	0	1	92095
Female	0.505	0.500	0	1	92095
9th grade GPA	225.656	68.818	0	320	85795
Father education (yrs)	12.037	2.523	8	19	67741
Father total income (€)	38671.341	35368.836	0	1754670	77754
Father work income (€)	36035.722	36741.533	0	1754670	77754
Father retirement benefits (€)	583.459	2824.505	0	52350	77754
Father unemployment benefits (€)	314.121	1682.604	0	21010	77754
Father age	47.507	6.698	25	83	77754
Mother education (yrs)	12.105	2.539	8	19	72813
Mother total income (€)	29141.480	20914.159	0	484380	82687
Mother work income (€)	26259.658	22457.533	0	484380	82687
Mother retirement benefits (€)	295.125	1856.842	0	36940	82687
Mother unemployment benefits (€)	306.255	1561.488	0	18770	82687
Mother age	43.680	5.726	24	71	82687

Notes. The variable described in this table are measured before the first application to the program. Income variables are measured in euros. Differences in observations reflect missing information. Immigration or parental deaths can result in missing data on parental characteristics.

having a positive income during the year, most of the applicants have some income, and the rate is steady at about 85 % after graduation from High School. Visits to specialized care (in-patient) and drug prescriptions increase until age 24. These data were only made

available until 2018 (prescription drugs until 2019), so we have very few observations at higher ages. We use fewer observations to measure the outcome variables at higher ages. In the tax registry data, for example, we only observe applicants at age 24 who were 16 years or older in 2012, since we only have this information until 2020.

We further describe our other outcome variables in Table 4, presenting the crime data and information on university registration at different ages. Very few applicants were convicted of a crime during our study period. Slightly more were registered as suspected of crimes. We never observe an age-specific average greater than 0.001 number of convictions. However, many applicants start a university education. At age 20, 8.3 percent were registered at a university. At higher ages, this increases to between 11 and 13 percent of all applicants. Since we have educational data until 2021, we have more data at higher ages for this outcome.

To better understand the population under study and the generalizability of our results, we provide information on our population of applicants to the program and compare them to the general population of Swedish youth in Appendix Table A1. Using data on unique applicants, we find that our population is similar to the general population of 15–19-year-olds in Sweden. Twenty-two percent of youth in the general population have two parents with a university education, while the corresponding number is twenty-four percent among program applicants. The applicants to the Stockholm SYEP are more advantaged than the Swedish population of youths on average. Engaging such an advantaged pool of applicants makes the Stockholm SYEP unique compared to the body of studies from the US.

TABLE 3
SUMMARY STATISTICS: OUTCOME VARIABLES

	Mean	Std dev	Min	Max	Observations
Income Application Year (€)	1459.486	2586.280	0	61150	91943
Income age 17 (€)	533.779	1149.268	0	59910	92011
Income age 18 (€)	1473.727	2447.980	0	58450	91879
Income age 19 (€)	4827.311	5444.840	0	82410	88244
Income age 20 (€)	9842.982	9178.088	0	85430	80504
Income age 21 (€)	11360.145	10564.889	0	102510	68944
Income age 22 (€)	12563.445	11601.978	0	129460	54708
Income age 23 (€)	12449.887	12701.156	0	122920	39964
Income age 24 (€)	10280.758	12905.209	0	111070	25953
Positive income application year	0.834	0.372	0	1	91943
Positive income age 17	0.476	0.499	0	1	92011
Positive income age 18	0.726	0.446	0	1	91879
Positive income age 19	0.864	0.342	0	1	88244
Positive income age 20	0.886	0.317	0	1	80504
Positive income age 21	0.860	0.347	0	1	68944
Positive income age 22	0.859	0.348	0	1	54708
Positive income age 23	0.863	0.343	0	1	39964
Positive income age 24	0.859	0.348	0	1	25953
In-patient visits application year	2.092	2.974	0	106	92095
In-patient visits age 17	0.347	1.741	0	66	77685
In-patient visits age 18	0.982	2.810	0	94	57480
In-patient visits age 19	1.882	3.463	0	78	35267
In-patient visits age 20	2.886	3.925	0	67	21693
In-patient visits age 21	3.249	4.224	0	104	14803
In-patient visits age 22	3.475	4.494	0	57	9869
In-patient visits age 23	3.536	4.684	0	98	4908
In-patient visits age 24	3.438	3.987	0	60	1808
Prescribed medications application year	3.058	4.913	0	245	92095
Prescribed medications age 17	0.735	2.964	0	103	92020
Prescribed medications age 18	1.659	4.355	0	119	88554
Prescribed medications age 19	2.246	4.873	0	205	80893
Prescribed medications age 20	2.678	5.706	0	264	69405
Prescribed medications age 21	2.844	6.155	0	254	55193
Prescribed medications age 22	3.002	6.447	0	232	40424
Prescribed medications age 23	3.294	7.480	0	249	26329
Prescribed medications age 24	3.491	7.920	0	201	12829

Notes. This table describes the main outcome variables. Income variables are measured in euros. For the variables measured at age x , we only include observations for applicants under that age. In this way we do not include pre-treatment values or the information of the application year which we study separately.

TABLE 4
SUMMARY STATISTICS: CONTINUED

	Mean	Std dev	Min	Max	Observations
Crimes convicted application year	0.000	0.014	0	2	92095
Crimes convicted age 17	0.000	0.005	0	1	92020
Crimes convicted age 18	0.000	0.011	0	1	88554
Crimes convicted age 19	0.000	0.016	0	2	80893
Crimes convicted age 20	0.000	0.013	0	2	69405
Crimes convicted age 21	0.000	0.010	0	1	55193
Crimes convicted age 22	0.000	0.021	0	2	40424
Crimes convicted age 23	0.000	0.011	0	1	26329
Crimes convicted age 24	0.000	0.022	0	2	12829
Crimes suspected application year	0.001	0.077	0	14	92095
Crimes suspected age 17	0.000	0.032	0	9	92020
Crimes suspected age 18	0.001	0.052	0	6	88554
Crimes suspected age 19	0.001	0.050	0	5	80893
Crimes suspected age 20	0.001	0.098	0	19	69405
Crimes suspected age 21	0.000	0.026	0	2	55193
Crimes suspected age 22	0.001	0.030	0	3	40424
Crimes suspected age 23	0.001	0.063	0	9	26329
Crimes suspected age 24	0.002	0.119	0	12	12829
University registration age 19	0.029	0.167	0	1	92020
University registration age 20	0.083	0.275	0	1	88554
University registration age 21	0.120	0.325	0	1	80893
University registration age 22	0.126	0.331	0	1	69405
University registration age 23	0.120	0.325	0	1	55193
University registration age 24	0.112	0.315	0	1	40424

Notes. This table describes the main outcome variables. For the variables measured at age x , we only include observations for applicants under that age. In this way we do not include pre-treatment incomes or the income of the application year which we study separately. “Any university education” is a binary variable based on university registration information up to age 24. The follow up period will differ between applicants, depending on the year of application and the age of the applicant, since we observe these data up until 2021.

3.2 Empirical approach

In general, several challenges are associated with estimating the causal effects of SYEPs. First and foremost, jobs can be allocated based on expected gains. If youths expected to benefit more from a summer job are provided with a job more often, the selection to treatment is negative. The negative selection to treatment comes from youths with weaker labor market prospects more often being assigned to the treatment group. They are compared with youths with better labor market prospects who are not offered jobs. A comparison of means would

then underestimate the effect of the program. To alleviate this concern and any others related to selective sorting, we use the random assignment created by the job allocation rules as an instrument.

Allocation of the oversubscribed summer youth jobs in Stockholm was partly executed through an ordered list of random numbers. The random number assigned to applications was an essential criterion for giving jobs to applicants. Within each local municipal office, applications were ordered based on their random numbers, and jobs were assigned to the first applicant on the list and then to those further down the list, conditional on the deviations as discussed. This method of allocating job offers was convenient, as no re-randomization had to be performed to fill available jobs after the first round of offers. Essentially, this means we are pooling experiments over the different local municipal offices and over time. Previous empirical research has used similar assignment mechanisms (Angrist, 1990).¹⁸ The empirical implications of this type of treatment assignment are detailed below.

We use the random number as an instrument variable (IV) for receiving a job offer. Although the random number could not be manipulated by any staff or the youths themselves, case handlers could deviate from the rank ordering when matching applicants to jobs. One assignment rule stated that previous employment through the program would make it more difficult to secure another job in later years. Youths who had previously received a job offer through the program were not considered until applicants without a previous job offer had been serviced. Moreover, we do not observe the complete application with the stated preferences over job types and availability during the summer. The case handlers used this information, which could have led to deviations from the rank-ordered list.

Nevertheless, since we use an IV approach, we allow case workers to make any deviations from the rank order allocation (e.g., hire their kids). The exogenous variation can intuitively be thought of as randomly receiving a relatively low number for a local office, meaning that your probability of receiving a job offer was relatively low compared to applicants in the same year who randomly received a relatively high number.

To align the instrument to the job allocation level, we recode the random number at the year-by-office level to $[0, 1]$ based on the rank ordering within the year and local office

¹⁸In Angrist (1990) analysis of veteran status on labor market outcomes, the author uses a random sequence number (RSN) that was assigned to each birthday for each cohort (from 1 to 365). Depending on the recruitment needs at the time, a cut-off was established that determined the eligible draftees. Our analysis is somewhat different in that we do not have a cut-off determining eligibility status due to deviations by case handlers. See Figure A1 in appendix for the relation between the job offer indicator and the rank order in the 14 districts in 2012. In only one out of 14 district there is a visible jump in the distribution. Other program years show a similar picture. Instead we use the random number in our analysis as a continuous variable instead of a binary eligibility indicator. Moreover, our random number was not public, meaning that the youths had no opportunity to respond to treatment before job offers were made.

groups. The applicant with the lowest number within a local office and year gets a zero, the applicant with the highest number receives a value of one, and the applicant with the median number gets 0.5. This ordering leads to a natural interpretation of the reduced form effect, i.e., going from the lowest to the highest number and, thereby, from the lowest to the highest probability of a job offer within each local office and year.¹⁹

Although job offers were allocated over these local random lists similarly across offices, each office supplied a different number of jobs and had a different number of applicants for the available jobs. These features of the allocation mechanism imply that job offer probabilities could differ significantly between offices, even for individuals with the same within-office and year rank order. To pool the experiments, we include local office-by-calendar-year fixed effects in each specification.²⁰ The estimation strategy that we use throughout the paper is to estimate three types of equations.

$$(1) \quad \text{Job Offer}_{itd} = \alpha + \beta_1 \cdot \text{Random Number}_{itd} + \theta_{td} + \epsilon_{itd}.$$

Equation (1) describes a first stage model relating the exogenous variation (the instrument) to the uptake of the program (a job offer). The interpretation of β_1 in this equation is how much more likely an applicant is to receive a job offer when moving from the lowest random number in her local office to the highest.

$$(2) \quad Y_{itd} = \alpha + \beta_2 \cdot \text{Random Number}_{itd} + \theta_{td} + u_{itd}.$$

Equation (2) relates the instrument directly to the outcome variable Y . Without very restrictive assumptions, we can interpret β_2 as the effect on the outcome of moving from the lowest random number in her local office to the highest.

$$(3) \quad Y_{itd} = \gamma + \beta_3 \cdot \widehat{\text{Job Offer}}_{itd} + \theta_{td} + v_{itd}.$$

Finally, Equation (3) describes the 2SLS estimand β_3 , showing the effect of a job offer on the outcome variable among compliers ($\frac{\beta_2}{\beta_1}$). Throughout the paper, we present results from all three models for our main results.

¹⁹This transformation does not affect our 2SLS specifications.

²⁰In practice, this amounts to greater precision as the fixed effects explain much of the residual variation in the outcome. It also controls for macro factors such as different cycles in the economy potentially affecting program year cohorts differently when entering the job market.

In equations (1), (2), and (3), i indexes each applicant, t indexes calendar year, a indexes age, and d indexes each local municipal office. We allow the outcome to be measured at different ages, up to age 24, to evaluate dynamic effects beyond the direct impact in the same year. In our primary analyses, we do not include controls for priority, reapplications, or age at application fixed effects as it may be related to deviations from the random list assignment and could induce sample selection bias. Nevertheless, we present results when controlling for these factors as a part of our robustness analysis (see Tables 12 and 13).

In the first stage, given by equation (1), the standardized random number is used as an instrumental variable, and being offered a job in the program is the endogenous variable of interest. The IV estimand, β_3 , gives the intention-to-treat effect, as we allow applicants to decide whether to accept a job offer or to decline. However, as few of the youths offered a job declined, the IV estimands are very similar, with either a job offer or taking a job as the endogenous variable (see Table 2). In the primary analysis, we assume instrument linearity, but we allow for both nonlinear and more flexible estimations in the robustness section. The results are not sensitive to the linearity assumption (See Table 12).

Moreover, the IV estimand in equation (3) has a local average treatment effect (LATE) interpretation if the treatment effects are heterogeneous. We refrain from indexing β_3 by each individual for convenience, but at the same time, we consider the estimand to be a weighted average of heterogeneous treatment effects. In this heterogeneous effects framework, we can characterize the relevant subgroups based on the instrument and endogenous variable. The causal effects we estimate are for the subgroup of compliers in the data (Angrist and Pischke, 2008). The compliers are youths who receive a job offer if they receive a high random number but do not receive an offer if they receive a low draw. It is also essential to describe the always-takers, never-takers, and defiers in this setting.

The always-takers in our analysis are those youths who receive a job offer irrespective of their group rank order. Many of these always-takers are the youths who reside in local offices with no oversubscription of job applicants (in addition to the prioritized youths) and who are thereby always offered a job. As we have 14 different local offices over seven years, our data contain 98 local-office-year groups. Among these groups, 6 provide more than 90 percent of applicants with a job offer and can be classified as undersubscribed.²¹

In the groups with many jobs to the number of applicants, almost all applicants are always takers, and the rank order of the random number induces zero, or close to zero, experimental variation. Moreover, some youths could receive a job offer irrespective of their

²¹Offers may not go out to applicants that are unavailable at summer periods when jobs are available. Some applicants may not be eligible for a program job and thereby not receive an offer. Since no local office is able to provide 100 percent of applicants with a job at any year, we consider a high ratio of job offers to applicants as showing undersubscription.

random number based on their characteristics. In the case of a district with few first-time applicants and many applicants who had received a job offer previously, the first-time applicants would almost certainly receive a job offer according to the program guidelines.

The never-takers are those youths who never receive a job offer, irrespective of their rank order. Never-takers in our data can also emerge due to program rules that left some youths without a job offer, regardless of their rank order. Youths who worked in the program the previous year have a lower chance of being offered a job in the following years. If local offices had few jobs and many applicants, those youths who had previously worked in the program would likely be never-takers.

Finally, we do not consider defiers an issue in this setting, as the case handlers would have to behave counterintuitively to generate such applicants. A defier is offered a job if they receive a low number but not if they receive a high number. It is challenging to rationalize such behavior by caseworkers. The primary way that we believe this can happen is if case handlers use priority status strategically. Youths who would benefit from a job but with a low rank order may more often be classified as prioritized to ensure they are provided a job. In the Appendix, we test if this seems to be an issue and find that case handlers do not behave this way systematically (Appendix Table A2).

The LATE interpretation of the just-identified IV estimand, when controls are included, has recently been scrutinized. Under those conditions, the LATE interpretation requires that the specification is either "rich" or saturated regarding the control variables and the instrument, depending on which monotonicity assumption is invoked (Blandhol et al., 2022). Otherwise, the regression weights could be negative, and the LATE interpretation is obscured. We thereby refrain from including additional control variables in our main specification. As saturating the specification in the control variables and instruments can lead to a significant number of instruments, this can lead to weak instrument bias if they are weakly correlated with the endogenous variable (Bound et al., 1995).

Moreover, it is well known that IV is biased in finite samples and that the size of standard regression hypothesis-test results is distorted under arbitrarily weak instruments. We, therefore, follow Lee et al. (2021) and Keane and Neal (2023) in using the Anderson–Rubin (AR) test for inference for our main results. This test has the correct size/coverage and is robust to arbitrary weak instruments.

3.3 Identifying assumptions

Our instrument is as good as randomly assigned by construction. Each year, the IT department servicing the application platform randomly generated a number for each applicant. This random number was provided to caseworkers as input in the job assignment process.

Since the number could not be changed or manipulated, the exogeneity assumption following IV estimation is trivially fulfilled.

To further strengthen the case that the treatment assignment mechanism, i.e., the random number, is unrelated to the youths' characteristics, we test for balance by estimating equation (1) with the pre-determined variables as outcomes. Figure 2 shows the balance test for 24 pre-determined characteristics for the application year for the entire sample from 2012-2018. In none of the 24 tests can the hypothesis that the estimated coefficients are different from zero be rejected at a 5 percent significance level.

Since the random numbers were automatically generated without any possibility of the case handlers or other municipal employees changing them, any imbalances must be strictly by chance. Moreover, there were no incentives to manipulate the rank ordering since the case handlers could deviate from the random ordering of applicants when assigning jobs.

We also estimate our main regressions for outcomes with more extended follow-up periods (ages 20-24) in our primary analyses. Since we have data on applicants from 2012-2018, the estimation samples differ based on the application year. For outcomes measured at age 24, we can use only applicants 16 or older in 2012, 17 or older in 2013, and so on since we have outcome data up to 2020. Therefore, we also provide the same type of balance test for all the samples used in Appendix Figures A5-A12. Seven pre-determined characteristics are statistically significant at the 5 % level for $8 \times 24 = 192$ regressions, less than expected from random sampling. For the age 23 sample (Figure A11), however, there are five statistically significant characteristics. The imbalance for the age 23 sample may reflect the smaller sample sizes and the corresponding increase in variability. We are not concerned about these five estimates since they are small in magnitude at around 1-2 percentage points or 3-4 percent of a standard deviation. Indeed, results are generally robust when we run regressions with the pre-determined characteristics included as controls. However, the effect for the 23-year-old sample attenuates. These and other robustness tests are available in Table 13 and are discussed more thoroughly in Section 5. In sum, the balance test results assure us that we can estimate the causal effects of the program.

3.3.1 Exclusion restriction

We have so far argued and substantiated that the instrument is exogenous. However, identifying the reduced form effects of the random number is inherently different from estimating the IV effect of program participation. For a causal interpretation of receiving a job offer, we also need to assume the excludability of the instrument from the structural regression equation.

The contextual features of the program motivate the plausibility of the exclusion re-

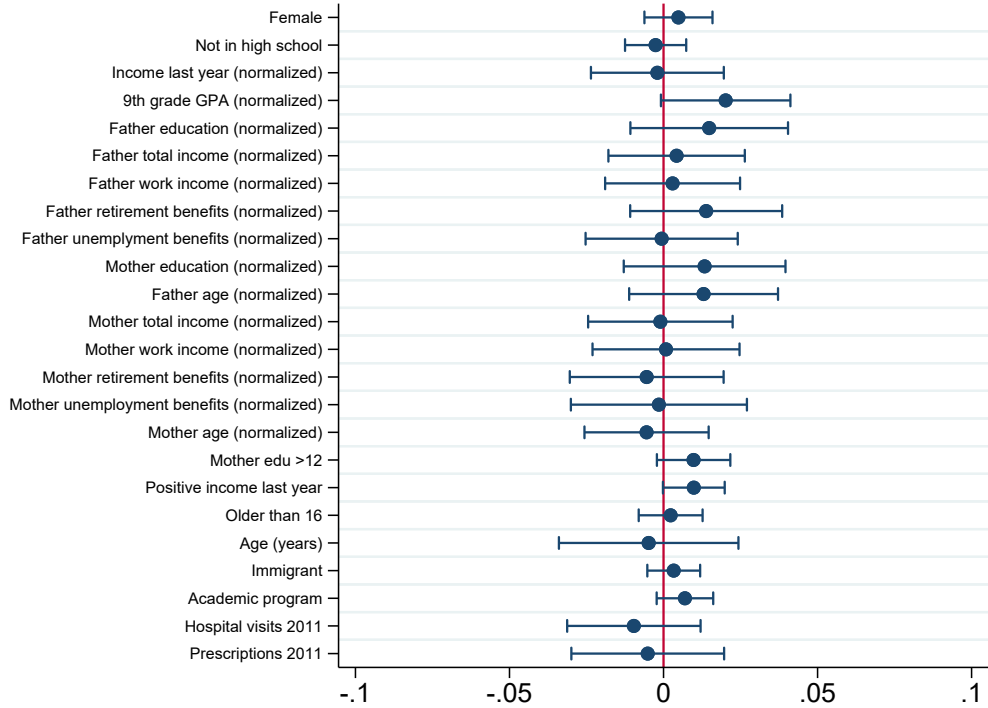


FIGURE 2

Balance of predetermined characteristics: Full Sample

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

striction assumption. Applicants were not informed of their random number and could not respond to it before the job offers were made. The random number, and hence the rank order, could not affect the applicants other than leading to a job offer in the same year.

However, a high random number, and hence a high probability of receiving a job offer, also results in a lower likelihood of getting a job offer next year (i.e., a penalty), irrespective of the random number assigned the following year. A high number would then imply that the probability of the youths receiving a job offer this year increased but that their likelihood of next year decreased. One concern of this program rule is that all youths participate and that we can only identify the timing of a program job offer between them. Appendix Table A9 shows that this is not the case. We find that a job offer increases the total number of job offers in our data by 0.85 and the total number of accepted jobs by 0.75. These findings

suggest that we are estimating the extensive margin effects of job offers and not just the timing of offers. We further address this potential violation of the exclusion restriction in Section 5.3.

We use all applications in our analyses to avoid inducing bias from selecting a sample among those randomized. That means we include both prioritized groups and reapplication. Since they are also given a random number, which cannot be altered or manipulated yearly, we do not introduce selection bias. Excluding them could potentially introduce both selection or collider bias.²²

A significant concern with the exclusion restriction assumption comes from ordering job offers. Youths that were lucky to receive a high number and thus have the highest probability of receiving a job offer would be more likely to receive an offer that also aligns with their stated preferences. On the other hand, youth with a lower random number could have a worse match if popular jobs are scarce. If match quality is also a part of the treatment, this violates the exclusion restriction. However, we argue that this potential second channel can be scrutinized and "tested" for with data. If match quality is essential, it would imply a nonlinear relationship between the rank order and income, i.e., the reduced form effect is nonlinear, even though the first stage relation is linear. We test for that in the robustness section (Section 5). Our evidence shows that both the first stage and reduced form are linear in the random number for the majority of our estimating samples, which points in the direction that the job offer matters, not the match quality between jobs and applicants' preferences. Thus, we argue that the exclusion restriction is likely to hold.²³

4 RESULTS

We use an IV approach and estimate the LATE of a job offer on the outcome variables. Since the complier group in our sample could differ from the average applicant, we provide some summary statistics on the complier group before presenting the results. We provide information on average complier characteristics and those from the entire population in Appendix Figure A4.²⁴ The figure shows the average of 15 pre-determined characteristics

²²However, we also show the results from using only first-time applicants and the results removing the applicants that are prioritized in Table 13. In fact, our results are robust to this exclusion so sample selection bias may not be a big problem in practice.

²³Since an exclusion restriction never can be really tested, it may be worth pointing out that all reduced form effects are valid without the exclusion restriction assumption, and we report them throughout the paper for completeness.

²⁴With a continuous instrument, it is difficult to characterize compliers. However, with a binary instrument, it is possible (Angrist and Pischke, 2008). We dichotomize our instrument to zero in the lowest—and to one in the highest—20 percent of observations within each local office jurisdiction and year. We then use only observations with a value for the discrete instrument and implement the `ivdesc` Stata program

together with the corresponding complier average. We find that the complier group is more advantaged than the complete pool of applications. Compliers have better-educated parents with higher incomes. They have higher grades, are less likely to attend a vocational high school program, are more likely to participate in an academic high school program, and are less likely to have immigrated to Sweden.²⁵

That the complier group, whose rank order determines whether they are being offered a job or not, are more advantaged is expected because always takers are, by construction, more disadvantaged. The always-takers partly consist of prioritized youths, where social disadvantage determines their status. Additionally, always takers come from local offices with jobs available to all applicants. Local offices that provided equally as many jobs as they had applicants were located in disadvantaged suburbs in Stockholm, and providing all applicants with jobs was their target. These features of the always-taker group ensure that compliers will, on average, be less disadvantaged. Moreover, the descriptive evidence suggests that the applicants for whom we can estimate the causal effects of a program job offer have parents with a higher educational level than the average same-aged youth in Sweden (see Table A1). As a result, the program in this paper differs from US-based SYEPs that often service the most disadvantaged youths (e.g. Davis and Heller, 2020; Gelber et al., 2016).

4.1 Results application year

Table 5 shows the first stage, reduced form, and treatment effects in panels A, B, and C for different ages (16-19) for the program application year. Starting with the first stage estimates in Panel A, they are all close to 0.35. Thus, receiving the highest number instead of the lowest increases the probability of a job offer by around 35 pp. Even though the relationship is very stable across age, our empirical model also builds on the assumption that the probability of receiving a job offer is linearly related to the random number. Figure 3 below, which shows the relationship corresponding to Table 2, Column 5, Panel A, supports that a linear approximation is entirely reasonable.

The first stage estimated coefficients are highly significant. However, recent studies have shown that 2SLS bias due to weak instruments cannot be reliably ignored by rule-of-thumb tests, such as an F-statistic in the first stage greater than 10 (Staiger and Stock, 1997). To avoid concerns about test size distortions and weak instrument bias, we report the Anderson

(Marbach, 2020). We find similar results using only 10 percent cut-offs but use the 20 percent cut-off to have more power.

²⁵We have created two variables to describe high school program. First, a dummy variable for academic program, where all other applicants are represented by a zero (i.e., even those not in school). Second, a dummy variable for attending a vocational high school program but only including applicants in high school. Jointly, these variables complement each other and to a large extent overlap.

Rubin (AR) Wald test p-value for the hypothesis, $\beta_3 = 0$. This test is robust to weak instruments and is reported in all our main regression tables.

In our second stage estimations, the AR test p-value is close to the p-value suggested by standard 2SLS inference and always smaller than 0.05, except for 19-year-old applicants ($p=0.057$), implying that the estimated effects do not suffer from weak instrument bias. The estimates show slight variation depending on age at application. There are two main takeaways from our first-stage results. First, the correlation between the instrument and the endogenous variable is strong. Second, the random number was an important determinant of job offers in the program.

Turning to the reduced form effect, we find statistically significant wage income increases for all ages (the least significant t-stat is for age 19 at 1,976). Thus, receiving the highest number instead of the lowest increases the income by about 120-280 euros in the application year. We present the IV estimates in Panel C. The program-year wage effect of receiving a summer job offer is stable at around 350-450 Euros until age 19 when the effect doubles. Some of the effect size increase can be explained by older program participants' higher wages. An additional year of age gave applicants an hourly wage increase of around 0.1 Euro. The program age-wage ladder alone suggests that a 40 percent income difference can be expected, comparing the 16 to 19-year-old. Moreover, as the 19-year-old applicant group is relatively small, the complier group at this age could be somewhat different than others. However, when relating the treatment effect with the mean, the effects decrease monotonically with age, from about 67 % at age 16 to 22 % at age 19. The decreasing effects are consistent with finding a job outside the program is easier the older the applicant is.

This difference should be about 900 euros if these jobs were the only jobs available and no one declined. The estimated effect between zero and 900 euros suggests that many applicants did not have alternative summer employment if they were unlucky and ended up without a job offer.²⁶ Since the wages and working hours in the program were fixed, and since we know that few turned down an offer, we conclude that many applicants had alternative employment during year zero if they did not receive a program job offer.

The instantaneous effects suggest that many youths had outside options for SYEP employment. Given the imperfect outside options, two possible counteracting effects on labor market attachment exist. First, some applicants participate in the SYEP that would otherwise not have any employment. Second, some participants substituted regular work with program jobs. The importance of each channel depends on the labor market value of a program job and the value of regular employment for the affected youths. These effects could

²⁶Since approximately 85 percent of applicants receiving a job offer also accepted, this feature of choice does not matter much for our interpretation of alternative employment (see Table 2).

TABLE 5
APPLICATION YEAR JOB OFFER EFFECTS ON INCOME (€) FOR
DIFFERENT APPLICATION AGES

Applied at:	Age 16	Age 17	Age 18	Age 19	All
	(1)	(2)	(3)	(4)	(5)
Panel A: First Stage					
Rank Order	0.3869*** (0.0426)	0.4095*** (0.0313)	0.3420*** (0.0333)	0.3402*** (0.0475)	0.3708*** (0.0294)
Panel B: Reduced Form					
Rank Order	157.6169*** (25.4345)	180.7159*** (29.3777)	116.9131** (47.9621)	284.0106* (143.7130)	175.4422*** (31.7026)
Panel C: 2SLS					
Job Offer	407.1715*** (44.5684)	440.8606*** (66.5025)	341.9391** (135.5498)	834.0966** (394.9247)	472.8332*** (80.7998)
Outcome mean	606.09	916.02	1470.37	3632.51	1459.49
Observations	21674	28824	26422	12248	91943
AR p-value	0.0000	0.0000	0.0190	0.0565	0.0000

Notes. Each column and row described a different regression. The application year income effect is the causal effect of a high rank order (reduced form) or a job offer (IV) on income in the same year as the application. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

have been reinforced if SYEP participation affected the likelihood of applying again the following year. Appendix Table A9, column (1), shows that a job offer increases reapplications in the next year by 17 percent (8.5 pp). Thus the program could have negatively affected participants if program jobs had low value on the labor market and regular employment was meaningful for future labor market outcomes. We next investigate how outcomes after program participation are affected.

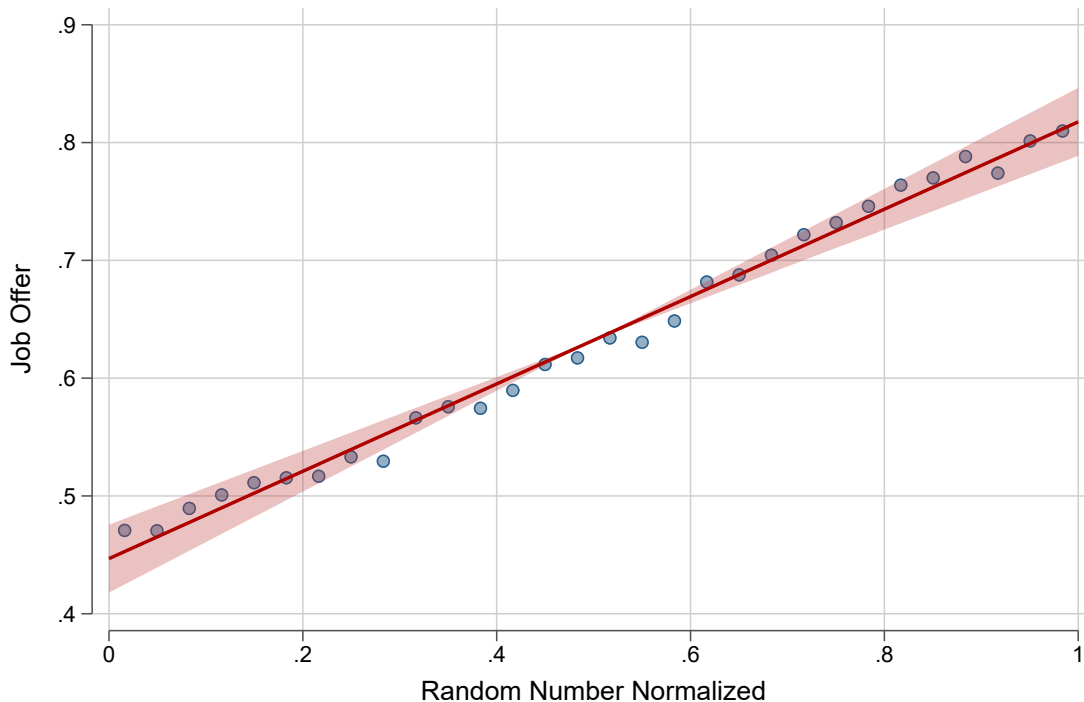


FIGURE 3

First Stage Effect of High Rank Order: Application Year

The binned scatterplot show the reduced form effects of a high random number in the program on a job offer at the year of application to the program. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level, where we also condition on fixed effects.

4.2 Effects on wages after the application year

Table 6 shows the first stage, reduced form, and treatment effects in panels A, B, and C for different ages (17-24) but after the application year. Since the estimating sample will vary, we provide a balance test in the Appendix (Figure A5-A12) for each subsample, and we find very few significant estimates, close to what is expected from random sampling, as discussed in section 3.3.

Not surprisingly, the first stage estimates are relatively stable, around 0.35-0.40, and the AR test closely mimics the 2SLS inference. Interestingly, the reduced income effects are negative for all ages after the program year (Panel B). During high school, at ages 17 to 19, the reduced form effects are small in absolute terms and only statistically significant at age 17. After graduation, at age 20, the effect sizes increase in absolute terms and become statistically significant up to age 23. At these ages, receiving the highest number instead of

the lowest decreases the wage income by about 300-400 euros up to age 23. At age 24, the effect attenuates and is no longer statistically different from zero.

Turning to the treatment effects in Panel C, the negative impact of a job offer (and essentially SYEP participation) is quite large and significant in the post-graduation period. The magnitude is between 700-1000 euros, except at the age of 24 when the impact is attenuated. Compared to the mean income at ages 20-23, the effect sizes correspond to around 7-8 % and relatively stable.

This pattern of income dynamics is partly consistent with Gelber et al. (2016), which found statistically significant negative effects up to three years after program participation (in addition to the positive income effect in the year of participation), which then attenuate to zero. Our results deviate from previous findings in that the negative impact on income is more significant in magnitude. Our primary analysis differs since we evaluate the effects at different ages instead of years after the program. To relate to Gelber et al. (2016), we follow their setup by evaluating up to four years after the program, presented in Table A7. When evaluating the reform similarly (i.e., years after participation), our negative effects increase in absolute value for four years. They are a substantially larger compared to the mean and statistically significant for all post-years (the negative post-program effects in Gelber et al. (2016) is about 2 %, while ours is about 7 %).²⁷ This finding is not very surprising since four years after the program captures for the vast majority in our sample the strong effects after high school graduation that we documented above.²⁸ To further corroborate the mechanism that the adverse effects occur when leaving high school, we also estimate the impact of a program offer holding age at application constant. However, this may be questionable as age at application may be part of the job assignment procedure and, therefore, a bad control, as discussed. The results are presented in the appendix in Table A8. It confirms that for all age groups except 19-year-olds, the wage punishment shows up in the data the years after high school (around 20 and 21 years old).

At ages 20 to 23, program participation caused around an 8 % lower annual wage income (approximately 1,000 euros) among compliers. Considering that these effects come from participation in a small employment program several years ago, the effects are substantial. We evaluate the extensive margin effects in Table 7 to understand the results further. Interestingly, as shown in Table 7, there are no extensive margin effects except for 17-year-olds. Thus, we conjecture that the impact on wage income, as presented in Table 6, comes from

²⁷In our data, the mean is 31,793 and the estimate is -2,194. These figures lead to a percentage effect of $100 * (-2,194 / 32,671) = -7$ percent. Gelber et al. (2016) finds an estimate of the same time horizon of -341 from a control group mean of 15,892.

²⁸In our experiment applicants must be 16 years old when applying while in Gelber et al. (2016) the requirement is 14. Thus our average age is 17.4 while in Gelber et al. (2016), it is lower at 16.5.

the intensive margin, better-paid job, or more hours worked.

TABLE 6
REDUCED FORM AND 2SLS ESTIMATES: WAGE INCOME (€) IN YEARS AFTER PROGRAM APPLICATION

Outcome Measured at Age:	17 yr (1)	18 yr (2)	19 yr (3)	20 yr (4)	21 yr (5)	22 yr (6)	23 yr (7)	24 yr (8)
Panel A: First Stage								
Rank Order	0.3834*** (0.0423)	0.3972*** (0.0328)	0.3882*** (0.0308)	0.3931*** (0.0317)	0.4026*** (0.0346)	0.3996*** (0.0374)	0.3775*** (0.0402)	0.3536*** (0.0446)
Panel B: Reduced Form								
Rank Order	-88.6129** (42.0108)	-58.6187 (39.6879)	-62.6829 (72.6333)	-274.3865*** (91.4009)	-396.6533*** (136.0086)	-399.3731*** (146.8137)	-399.0336** (198.6907)	-168.2397 (322.6535)
Panel C: 2SLS								
Job Offer	-231.1239** (105.0713)	-147.5728 (97.8481)	-161.4843 (186.7386)	-697.9421*** (235.1676)	-985.1514*** (359.6005)	-999.3744*** (378.1394)	-1057.1284** (529.7282)	-475.8171 (891.6568)
Outcome mean	1028.16	1900.42	5181.81	9842.98	11360.15	12563.44	12449.98	10281.75
Observations	22088	50807	73621	80504	68944	54708	39964	25953
AR p-value	0.0392	0.1405	0.3878	0.0040	0.0050	0.0101	0.0570	0.6035

Notes. Each column and row described a different regression. The table presents first stage, reduced form and IV results for the different samples defined by age at evaluation. The outcome is the taxable income in Euros at the age described in the table. Column (1) presents the estimated coefficient when estimating effects for 17 year old youths. In this specification we only include applicants aged 16 to measure the dynamic effects. In the next column we include all applicants aged 16 and 17 when analyzing outcomes measured at age 18. We present the raw outcome mean, the number of observations and p-values from Anderson-Rubin tests. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE 7
REDUCED FORM AND 2SLS ESTIMATES: POSITIVE INCOME IN YEARS AFTER PROGRAM APPLICATION

Outcome Measured at Age:	17 yr (1)	18 yr (2)	19 yr (3)	20 yr (4)	21 yr (5)	22 yr (6)	23 yr (7)	24 yr (8)
Panel A: First Stage								
Rank Order	0.3834*** (0.0423)	0.3972*** (0.0328)	0.3882*** (0.0308)	0.3931*** (0.0317)	0.4026*** (0.0346)	0.3996*** (0.0374)	0.3775*** (0.0402)	0.3536*** (0.0446)
Panel B: Reduced Form								
Rank Order	0.0177** (0.0087)	0.0064 (0.0057)	0.0061 (0.0047)	0.0003 (0.0038)	0.0017 (0.0043)	-0.0007 (0.0045)	0.0002 (0.0053)	0.0003 (0.0093)
Panel C: 2SLS								
Job Offer	0.0462** (0.0231)	0.0162 (0.0147)	0.0157 (0.0121)	0.0008 (0.0097)	0.0043 (0.0105)	-0.0016 (0.0113)	0.0004 (0.0140)	0.0010 (0.0260)
Outcome mean	0.88	0.87	0.88	0.89	0.86	0.86	0.86	0.86
Observations	22088	50807	73621	80504	68944	54708	39964	25953
AR p-value	0.0462	0.2703	0.2027	0.9368	0.6805	0.8848	0.9772	0.9703

Notes. Each column and row described a different regression. The table presents first stage, reduced form and IV results for the different samples defined by age at evaluation. The outcome is a binary variable indicating if the applicant had a non-zero taxable income in the year they turned 17 to 24. Column (1) presents the estimated coefficient when estimating effects for 17 year old youths. In this specification we only include applicants aged 16 to measure the dynamic effects. In the next column we include all applicants aged 16 and 17 when analyzing outcomes measured at age 18. We present the raw outcome mean, the number of observations and p-values from Anderson-Rubin tests. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Mechanisms, heterogeneity, and other outcomes

4.3.1 Mechanisms

We do not observe hours worked in the data and can never disentangle if those offered a job work less or end up with lower-paid jobs per hour. However, at least we can test whether a program job affects job types after graduation. To test this, we use professional job codes (SSYK) to assign applicants to high-, medium-, and low-qualification jobs based on their work descriptions as documented in November of the year the applicants are observed.²⁹ Moreover, we can classify jobs into private and public and from sector codes and single out municipal employment. We present these results in Table 8. Although the estimates sometimes are imprecise, the overall picture is consistent with the fact that having had a program job increases the probability of ending up in low-quality and public jobs compared to not being offered a program job. For example, just after graduation, a program participant is 3 pp. more likely to have a low-qualified job than a non-participant. Thus, this speaks in favor of a program job affecting that participants end up in lower-paid jobs. Lastly, we also present the results if an offer affects the probability of a full-time job in Table 8.³⁰ A program offer decreases the likelihood of a full-time job by around 3-4 pp. up to age 23. Again, it attenuates at the age of 24.

If we only found that the participation in public sector employment increased, it would be consistent with the fact that the lower wages were only a function of the public sector paying less than the private. A pure shift in sector employment is consistent with the SYEP aim to increase the interest in municipal jobs. Since public work is associated with other amenities than private sector jobs, it may still enhance welfare for the applicants even though there is a wage punishment. Since there is also sorting concerning the quality of jobs and full-time employment, we argue that wage punishment also contains a welfare loss for applicants.

²⁹High-qualification jobs are skilled professions, for which a university degree is required or which are related to managerial tasks. Jobs with medium qualification requirements compose the largest category and include factory work, transport, services, and education, and so on. The category of jobs with low qualification requirements comprise, as defined by Statistics Sweden, work at fast-food restaurants as the largest item (see, Appendix Table A3).

³⁰We proxy a full-time job as someone having more than the full-time minimum wage in the municipal sector in 2015 by the collective agreement. In 2015, the monthly salary was 18 080 SEK or about 1800 euros. Thus, a full-time job is a job that pays more than $1800 \cdot 12 = 21,600$ euros per year. Source: The Swedish Trade Union Confederation, www.lo.se.

TABLE 8
JOB OFFER EFFECT ON TYPE OF JOB

Outcome Measured at Age:	20 yr	21 yr	22 yr	23 yr	24 yr
	(1)	(2)	(3)	(4)	(5)
High Quality Job					
Program job offer	0.0014 (0.0112)	-0.0004 (0.0118)	-0.0068 (0.0127)	0.0080 (0.0222)	0.0164 (0.0271)
Medium Quality Job					
Program job offer	-0.0317* (0.0177)	-0.0269 (0.0167)	-0.0148 (0.0178)	-0.0074 (0.0254)	-0.0148 (0.0319)
Low Quality Job					
Program job offer	0.0303** (0.0148)	0.0273** (0.0134)	0.0215 (0.0132)	-0.0006 (0.0142)	-0.0016 (0.0157)
Private Sector Job					
Program job offer	0.0021 (0.0146)	-0.0139 (0.0130)	-0.0259 (0.0159)	-0.0123 (0.0196)	0.0010 (0.0290)
Public Sector Job					
Program job offer	0.0005 (0.0136)	0.0173* (0.0096)	0.0233* (0.0135)	0.0108 (0.0165)	-0.0034 (0.0209)
Municipality Sector Job					
Program job offer	-0.0014 (0.0123)	0.0161* (0.0085)	0.0272** (0.0107)	0.0174 (0.0123)	0.0005 (0.0172)
Wage Income > 21.6k €					
Program job offer	-0.0327*** (0.0099)	-0.0272** (0.0119)	-0.0283* (0.0166)	-0.0472** (0.0185)	0.0002 (0.0279)

Notes. Each column and row described a different regression. The table presents 2SLS results for the different samples defined by age at evaluation. The outcomes are indicator variables described on each row. Job quality (high/medium/low) is defined from Swedish professional codes (SSYK) according to Statistics Swedens categorization. High quality job are managerial jobs and jobs requiring university education. Low quality jobs require no education and consist of a subset of service jobs, such as fast-food jobs. Sectoral codes comes from Statistics Sweden (SNI). The outcome *Wage Income > €21.6k* use a binary variable indicating if the yearly taxable wage income was above €21.6k as the dependent variable. Column (1) presents the estimated coefficient when estimating effects for 20 year old youths that applied at one, or more, occasions between ages 16-19. Note that professional codes are under-reported in the registry data, especially at lower ages. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.3.2 Heterogeneity

One significant difference between the program in Stockholm and other similar programs in the US is that all age-eligible youths could apply to the program.³¹ While universally

³¹In Gelber et al. (2016), all 14-21 can apply, so eligibility rules do not differ. However, in practice the applicant pool differ and are more disadvantaged in Gelber et al. (2016). From their summary statistics (Table I), we can infer that only 13 percent of applicants are characterized as white and that the mean family income was 39,524. Considering that the median family income in 2012 was around 70,000 USD, we can conclude that the NY SYEP service a disadvantaged population of youths. See, NYC Median Family

available programs can reduce problems with low take-up due to the stigma of participation and can increase support from taxpayers, who can be reluctant to finance services they are not eligible to use, they can also lead to inefficient targeting. Even though being offered a program job was, on average, harmful to the applicant's income trajectories, there could still be subsets of applicants for whom the program was beneficial. Understanding where the program was effective is important because it means that the program could improve welfare by focusing on, e.g., disadvantaged groups instead of allowing anyone within the eligible age range to apply.

To better understand how the effects we estimate are generated and to explore which youths are the most affected, we present a heterogeneity analysis in Table 9. We split the sample into several relevant dimensions to explore heterogeneity in the treatment effect and estimate separate regressions for the different samples. We start by exploring how our results differ based on sex. This margin is relevant, as many available job types in the Stockholm program were in female-dominated sectors (e.g., childcare and elderly care). If female applicants moved into these occupations to a greater extent after graduating from high school, the signal value and experience of a program job could have been positive but negative for males. In columns (1) and (2), we show that there is a compelling difference between males and females. Females are seeing no statistically significant wage punishment, but males do. We interpret this finding as consistent with females having more labor market-relevant program employment opportunities and that program jobs compete less with relevant regular summer employment. Thereby, the program may be less detrimental for females.

Next, we investigate whether high school program type is an essential determinant of the income effect of a program job offer (columns (3) and (4)). We compare youths enrolled in academic tracks with youths in vocational tracks (we add youths in special programs and those not in high school into the vocational program group). Youths in academic programs could be less affected by a job offer, as they are less likely to enter the labor market immediately after high school and more likely to study at a university. Job experience during high school could be a more important signal to the labor market for youths in vocational programs than academic tracks. In line with this reasoning, we find that the negative effect of a job offer is smaller among youths in academic programs and not statistically significant. The point estimate is larger by a factor of 3-4 between the groups. A related measure correlated with the probability of entering the labor market directly after high school graduation is the parents' education level. In columns (5) and (6), we report our main results for youths whose mothers have high (> 12 years) and low (≤ 12 years) levels of education, and the conclusion is similar to comparing vocational and academic track students.

Income Up for First Time since Great Recession: Fiscal Policy Institute.

Another way to describe the alternative employment opportunities available to the youths is to document whether they were employed the year before their application to the program. Youths employed before the program already had some attachment to the labor market. Those youths with stronger attachment could have been more negatively affected by program job offers, as they had better outside options. A program job offer could have disrupted this labor market connection if the youths preferred a program job to employment in their previous workplace. We confirm these predictions and show in columns (7) and (8) that youths with an income in the year before their program application were more negatively affected by job offers.

The findings presented in this section are mainly consistent with the fact that males, unlikely to continue university studies but connected to the labor market before applying to the program, see the most considerable wage punishment by participating in the program. Our results suggest that a program job provides a negative signal and that a program job offer could have disrupted this private labor market connection. While the low precision of our IV estimates does not permit us to statistically distinguish between the coefficient estimates for different samples in Table 9, the patterns are compelling. Although we have not found any evidence of subgroups benefiting from being offered a program job, i.e., being better off compared with the case of not receiving a job offer, we can address this question more firmly by exploring the distributional effects of program job offers.

To further understand how vital individual heterogeneity is in this context, we use quantile regressions to study the income dynamics. Figures A13 to A17 in the appendix show the reduced form results for wage income using quantile regressions over a series of quantiles for ages 20 to 24.³² We find that, for all ages, youths at the higher end of the conditional income distribution experience larger income decreases if they were randomized to a high rank order. The reduced form estimates range from close to zero, in the lower part of the income distribution at all ages, to around 1,000 Euro in the highest quantile at age 23 (i.e., the 90th quantile). From these regressions, we can conclude that no one seems to have benefited from the program because their incomes increased. Even youths with low expected income trajectories were no better off. We conclude that while a targeted program would be a more efficient way to allocate resources, it would not produce any gains in terms of the outcome variables and the complier group we consider.

³²The IV quantile regression procedures that we tested in STATA did not converge for older ages with fewer observations. Instead we follow the fixed effect quantile regression procedure of Machado and Santos Silva (2018), using the local office and year groups as fixed effects, and present reduced form effects.

TABLE 9

HETEROGENEITY IN THE EFFECT OF A JOB OFFER ON WAGE INCOME (€) AT DIFFERENT AGES

Sample	Female Applicant = 0 (1)	Female Applicant = 1 (2)	Academic HS Track = 0 (3)	Academic HS Track = 1 (4)	Maternal Edu > 12 Years = 0 (5)	Maternal Edu > 12 Years = 1 (6)	Positive Income Before Application = 0 (7)	Positive Income Before Application = 1 (8)
Panel A: At age 20								
Income								
Program job offer	-1219.77*** (390.14)	-227.20 (354.05)	-1367.77** (545.52)	-456.70 (280.78)	-1124.47*** (361.90)	21.25 (397.54)	-685.06* (395.34)	-815.24** (392.24)
Outcome mean	9583	10100	8705	10450	10338	10217	9342	10289
Observations	39976	40528	27996	52508	37860	25966	37943	42561
Panel B: At age 21								
Income								
Program job offer	-1358.51*** (508.53)	-624.01 (500.68)	-2063.01*** (727.12)	-565.56 (383.95)	-1309.66*** (496.88)	-173.28 (564.23)	-535.86 (431.18)	-1472.20*** (570.16)
Outcome mean	11592	11130	11699	11171	12332	10811	10841	11812
Observations	34296	34648	24705	44239	32765	22157	32077	36867
Panel C: At age 22								
Income								
Program job offer	-1317.83** (544.61)	-587.26 (481.97)	-2043.09** (898.51)	-528.01 (431.99)	-1573.55** (691.97)	-39.62 (546.30)	-782.63 (653.27)	-1254.97** (531.91)
Outcome mean	13273	11868	13835	11818	13807	11235	12024	13032
Observations	27085	27623	20227	34481	26473	17660	25428	29280
Panel D: At age 23								
Income								
Program job offer	-1741.24** (721.82)	-182.09 (795.47)	-2228.13* (1193.87)	-392.14 (620.92)	-2033.54** (840.09)	891.39 (847.61)	-816.52 (667.96)	-1334.16 (905.06)
Outcome mean	13345	11574	14018	11466	13476	11003	10288	14352
Observations	19764	20200	15410	24554	19566	12988	18706	21258
Panel E: At age 24								
Income								
Program job offer	-967.81 (1257.89)	222.99 (1034.67)	-187.09 (1795.06)	-320.86 (768.78)	-2042.51 (1506.33)	2239.99** (1131.00)	-915.87 (817.35)	-285.27 (1594.61)
Outcome mean	11136	9446	12656	8555	10974	9019	7779	12560
Observations	12817	13136	10920	15033	12737	8523	12371	13582

Notes. Each column and panel display results from a different 2SLS regression. Panel A use the total taxable wage income at age 21 as the dependent variable while Panel B and C use the same outcome measured at age 22 and 23 respectively. We split the samples based on the characteristic described over each pair of columns. Incomes are measured in Euros, defined as: 1€=10SEK. All results are estimated by 2SLS according to equation (1). Standard errors are clustered at the level of the included fixed effects, i.e., the local municipal office and year level. Significance is described by * p < 0.1, ** p < 0.05, *** p < 0.01.

4.3.3 *Effects on crime, health, and education*

The predominantly US-based literature on SYEPs has documented summer jobs' effects on health and crime outcomes. The case for these effects in the Swedish context is weaker given the more advantaged population, the more inclusive welfare system, and the lower rate of criminal activity in general. However, to establish a firm relation to the literature, we document effects on crime and health in Tables 10 and 11.

Table 10 presents the treatment effects for the program year in panel A. In panels B-D we present the treatment effects for the high school years (ages 16-19) after the application year. Since criminal activity is often high among teenagers, and incapacitation effects may be important in this context, we consider these ages most relevant to study. Nevertheless, we find no strong evidence suggesting that a Stockholm SYEP job offer affects criminal behavior or health in the program year or after the program but still in high school. Concerning criminal activity (columns (1) and (2) in Table 10), there is a stable pattern with job offers generally negatively associated with crime outcomes. However, none of the estimates are statistically significant in contrast to the predominantly US-based literature on SYEPs. Taken at face value, the effects are large compared to the mean. However, the mean is extremely low due to the advantaged sample population under study. For example, at age 19, only 0.02 percent of the population was convicted of a crime. Therefore, we are cautious in interpreting these effects at face value in relation to the mean.

There is no stable pattern for health outcomes, even if a job offer increases hospital visits statistically significantly at 17. However, we interpret this as a non-robust result, given the number of regressions estimated and the sign switches for different ages.

In Table 11, we continue to present results for the post high school graduation ages. Since the youths have now completed high school, we also estimate the job offer effect on registration to higher education. Two post-high-school graduation estimates are statistically significant (university registration at 22 and hospital visits at 24). There are no stable patterns for the health and university outcomes, and the effects are typically small compared to the mean. Therefore, we abstain from interpreting these estimates but conclude that these findings show that academic educational choices, health, or criminal behavior are neither affected to a significant extent by program participation nor can they explain our stark findings on income.

TABLE 10
JOB OFFER EFFECTS ON CRIME AND HEALTH

	Crimes Convicted	Crimes Suspected	Hospital Visits	Prescribed Medications
	(1)	(2)	(3)	(4)
Panel A: Application Year				
Program job offer	-0.0000 (0.0004)	0.0022 (0.0025)	-0.1241 (0.0865)	0.1407 (0.1490)
AR p-value	0.93	0.37	0.16	0.34
Outcome mean	0.0002	0.0010	2.0918	3.0575
Observations	92095	92095	92095	92095
Panel B: Age 17				
Job Offer	-0.0009 (0.0007)	-0.0036 (0.0034)	0.8681** (0.4369)	-0.1972 (0.3936)
AR p-value	0.17	0.28	0.06	0.62
Outcome mean	0.0001	0.0008	3.4818	3.0633
Observations	22067	22067	7732	22067
Panel C: Age 18				
Job Offer	-0.0007 (0.0006)	-0.0022 (0.0032)	-0.0730 (0.2782)	-0.2215 (0.1950)
AR p-value	0.28	0.50	0.79	0.25
Outcome mean	0.0002	0.0011	3.4448	3.0965
Observations	47455	47455	16381	47455
Panel D: Age 19				
Job Offer	-0.0007 (0.0007)	0.0002 (0.0019)	-0.0941 (0.2371)	-0.0296 (0.1608)
AR p-value	0.36	0.91	0.69	0.85
Outcome mean	0.0002	0.0010	3.2189	2.7433
Observations	66243	66243	20617	66243

Notes. Each column and panel display results from a different regression. Column (1) use information on convictions for crime as the outcome while column (2) use information on if the applicant was suspected for a crime. Columns (3) and (4) takes the yearly sums of hospital visits and the number of prescribed medications as the dependent variables. Panels A to D presents the estimated effects of a job offer on the outcomes for ages 17 to 19 respectively, and in column (1) for any age in the same year as the application. All models are estimated by 2SLS according to equation (1). Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 11
JOB OFFER EFFECTS ON CRIME, HEALTH, AND EDUCATION

	Crimes Convicted	Crimes Suspected	Hospital Visits	Prescribed Medications	University Registration
	(1)	(2)	(3)	(4)	(5)
Panel A: Age 20					
Job Offer	-0.0002 (0.0003)	-0.0021 (0.0020)	0.0340 (0.2416)	0.1505 (0.1815)	0.0035 (0.0089)
AR p-value	0.53	0.27	0.89	0.40	0.69
Outcome mean	0.0001	0.0010	2.8856	2.6778	0.0827
Observations	69405	69405	21693	69405	88554
Panel B: Age 21					
Job Offer	-0.0006* (0.0004)	-0.0015 (0.0011)	-0.0803 (0.3286)	0.1564 (0.2217)	0.0077 (0.0105)
AR p-value	0.09	0.17	0.81	0.47	0.46
Outcome mean	0.0001	0.0004	3.2487	2.8443	0.1205
Observations	55193	55193	14803	55193	80893
Panel C: Age 22					
Job Offer	-0.0004 (0.0012)	-0.0018 (0.0013)	-0.0754 (0.4187)	0.1252 (0.2861)	0.0206** (0.0094)
AR p-value	0.74	0.15	0.86	0.66	0.03
Outcome mean	0.0003	0.0006	3.4745	3.0018	0.1256
Observations	40424	40424	9869	40424	69405
Panel D: Age 23					
Job Offer	-0.0006 (0.0004)	-0.0018 (0.0022)	0.3736 (0.8203)	0.0576 (0.4040)	0.0068 (0.0118)
AR p-value	0.13	0.39	0.65	0.89	0.56
Outcome mean	0.0001	0.0008	3.5361	3.2937	0.1203
Observations	26329	26329	4908	26329	55193
Panel E: Age 24					
Job Offer	-0.0024 (0.0017)	-0.0133 (0.0098)	2.1876** (1.0674)	-0.2857 (0.8009)	-0.0120 (0.0136)
AR p-value	0.15	0.17	0.15	0.72	0.37
Outcome mean	0.0003	0.0018	3.4381	3.4906	0.1120
Observations	12829	12829	1808	12829	40424

Notes. Each column and panel display results from a different regression. Column (1) use information on convictions for crime as the outcome while column (2) use information on if the applicant was suspected for a crime. Columns (3) and (4) takes the yearly sums of hospital visits and the number of prescribed medications as the dependent variables. Panels A to F presents the estimated effects of a job offer on the outcomes for ages 19 to 24 respectively. All models are estimated by 2SLS according to equation (1). Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.1 *Non-linearity*

We have relied on linearity between the job offer and the random number in the above analyses. In this section, we analyze and relax this functional form assumption. We start by showcasing the functional form graphically in Figures 3 and 4. Visually, the linear assumption seems not to be invalidated but for the samples of 23- and 24-year-olds there are cubic form pattern. In the Appendix Table A5, we present reduced form regressions allowing both a quadratic and cubic fit. Reassuringly, for the 20-22 age samples none of these quadratic and cubic functional forms are statistically significant. Moreover, the F-test of the joint null hypothesis, that the coefficients on the quadratic and cubic rank order are jointly zero cannot be rejected.³³ However, for the samples of 23- and 24-year-olds linearity can be rejected. We conclude that the instrument both graphically seem linear and that the statistical tests cannot reject linearity for the 20-22 samples, but we need to relax the assumption for the 23-24 sample further in the coming analysis.

Given that linearity seems to be a good approximation of the first-stage relationship at least for the 20- to 22-year-olds age sample, non-linearities in the reduced form can be informative. As discussed above, a major concern with the exclusion restriction assumption comes from the ordered allocation of job offers over the rank order. Youths that were lucky to receive a high number and thus have the highest probability of receiving a job offer would be more likely to receive an offer that also aligns with their stated preferences. On the other hand, youth with a lower random number could have a worse match if popular jobs are scarce. If match quality is also a part of the treatment, and not only receiving an offer, then this would imply a nonlinear relationship between the rank order and income, i.e., the reduced form effect is nonlinear, even though the first stage relation is linear.

Figure 5 shows the reduced form effect at post-graduation ages. Visually, a linear approximation seems appropriate for all ages. Furthermore, in Appendix Table A6, we augment the linear rank order model with quadratic and cubic terms. None of these higher-order polynomials are statistically significant. Moreover, the F-test of the joint null hypothesis that the coefficients on the quadratic and cubic terms are zero cannot be rejected. We conclude that the reduced form graphically seems linear and that the statistical tests cannot reject linearity. Thus, this speaks against the fact that the job match quality is a channel affecting wages and that the treatment effect goes solely through the program job channel.

In order to investigate the sensitivity of linearity we we relax some of the imposed restrictions of the used regression model, instrument homogeneity, and instrument linearity. In

³³We follow Stock and Watson (2015, ch. 8) for a textbook treatment in determining functional form.

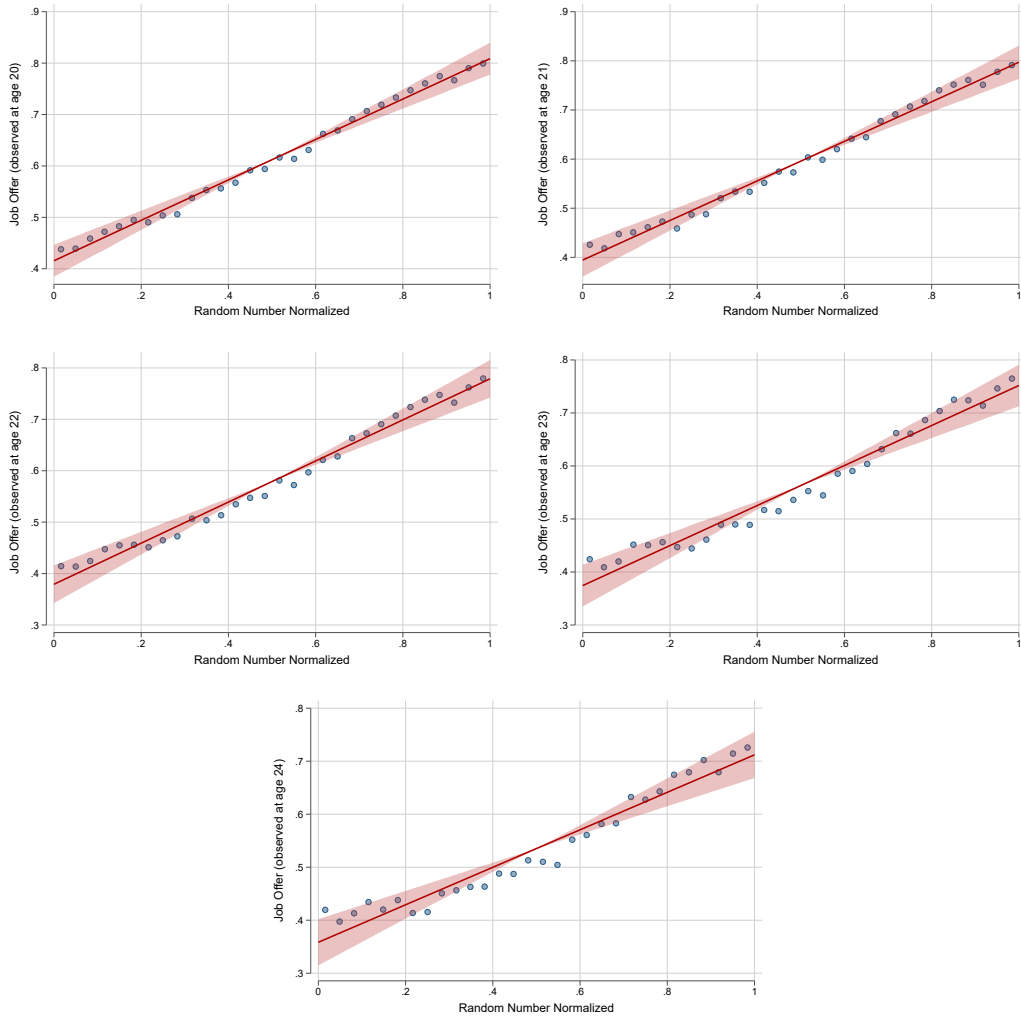


FIGURE 4

First Stage Effects of High Rank Order at ages 20-24

These binned scatterplots show the reduced form effects of a high random number in the program on a job offer at different time intervals from the application to the program. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level, where we also condition on fixed effects.

particular our results for the 23- and 24-year-old sample may be more sensitive as discussed above.

We start by considering the functional form we impose on the instrument and present the findings in Table 12. Although we have concluded that a linear functional form approximates the first-stage relationship, imposing that each local-office-year group has a common first stage could be a strong assumption. Different local offices had a different number of available jobs to assign. They had a different number of applicants and could have deviated from the

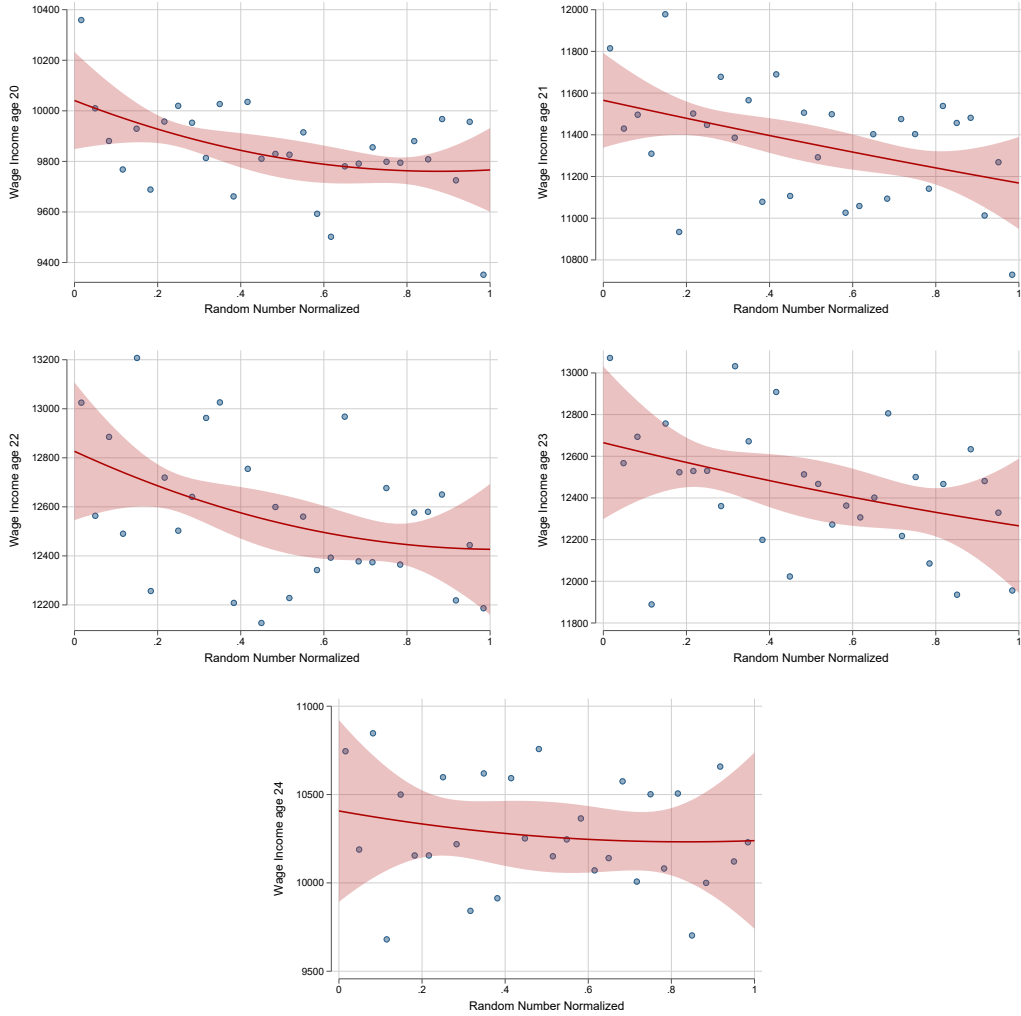


FIGURE 5

Reduced Form Effects of High Rank Order on Income (€) at ages 20-24

These binned scatterplots show the reduced form effects of a high random number in the program on income at different time intervals from the application to the program. Income is measured in Euros, defined as: 1€=10SEK. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level, where we also condition on fixed effects.

rank order in different ways. This heterogeneity could also have changed over time, for example, as 19-year-old applicants were allowed in 2015. Hence, the first-stage relationships could differ between and be nonlinear within each experimental group.

To understand how the restrictions we have imposed on the instrument affect our estimates, we first use a predicted instrument in Table 12, column (1) (Wooldridge, 2002, pp. 623-625). This instrument is created by estimating a probit first stage from our basic speci-

fication and taking the corresponding predicted probabilities to use as the instrument. This predicted instrument is more flexible, as it is not restricted to being constant for all groups and allows for non-linearity in rank order. Using this more flexible instrument leads to more precisely estimated effects of a job offer on wage income for all ages. The IV effects estimated using this instrument are about the same magnitude as in our main Table 6, except to the age of 23 where the effect is no longer statistically significant. This more flexible instrument is more efficient but does not change the overall interpretation from our basic specification with a linear instrument.

To further relax some of the restrictions we have imposed on our instrument, we use 98 instruments, one for each local-office-year group, in columns (2) and (3). First, we include instruments for each group based on the linear instrument in column (2) and then use the predicted instrument for each group in column (3). Using these more flexible instruments, we find that the effects on wage income up to age 23 are more precise and slightly smaller in magnitude relative to our baseline. The decrease in magnitude ranges from 17 to 45 percent from the estimates in the baseline model. In contrast, the effect is substantially larger and statistically significant for the applicants we can measure at the age 24. Given the nonlinearities found for this sample in the first stage relation, this put some doubt on whether our conclusion that wage punishment is attenuated at age 24 is correct. Using a more flexible form there are no sign of attenuation.

The slightly smaller estimates of the IV effect due to more flexible instruments could be due to several causes. First, heterogeneity within each local office and year may lead to higher weights assigned to groups where the samples are accidentally imbalanced. Second, including 98 instruments could lead to weak instrument bias. Since we find that the precision increases when using more flexible instruments, it is possible that we do not introduce a weak instrument problem but rather that we better capture the exogenous variation in the rank order. In the overidentified case, separating between these explanations is challenging. It is difficult to argue that the more flexible specifications are clearly preferred. Although the effect sizes, overall, are somewhat smaller, we still find substantial effects of a similar magnitude to our basic specification. Our conclusions is that the wage punishment just after leaving high school, remain unchanged.

Another way to determine whether and how the flexible instruments improve the estimated results is to remove those experimental groups with weak first stages. The flexible instruments should better handle groups in which the first stage is weak or close to zero, i.e., where job applicants are not oversubscribed. To remove these groups, we run separate first-stage regressions for each group and save the estimated coefficients and their corresponding t-values. We then restrict the estimation sample based on whether groups have a first-stage

coefficient above 0.3 or the t-value for the first-stage coefficient is above 3.³⁴ The results are presented in columns (4) and (5) of Table 12. The results are comparable or smaller in magnitude relative to our baseline result and closer to the results we obtain when using group-specific instruments. One interpretation of these findings is that the group-specific instruments reduce the bias stemming from experimental groups with small or imprecise first-stage estimates. While our baseline specification might somewhat overestimate the causal effect of job offers on future income, the overall interpretations remain intact.

Finally, in column (6) of Table 12, we use Poisson regression to estimate the reduced form effect of a high-rank-order on wage income. This estimator is used because a log transformation of the dependent variable (i.e., wage income) is unattractive due to the many zero values. The bunching of zero income, amounting to between 10-15 percent of all observations, could also make the OLS estimator less attractive. This model returns coefficients that can be interpreted in percentages and retains many of the robustness properties of OLS, even when the outcome is not in counts (Motta, 2019). Using this regression model, we find the results consistent with the OLS estimates evaluated at the control group means. For example, the effect of a high-rank-order on income at age 20 is -2.8 percent, according to the Poisson model. From Table 6, panel B, we can relate the estimated OLS coefficient for the effect on the same outcome (-274) to the outcome mean (9,843), implying a result of $100 * (-274 / 9,843) = -2.8$ percent. This shows that our findings are not spurious due to the use of wage income in levels and that the preferred OLS estimator seems to handle the zero-valued observations.

We conclude that the wage punishment arising just after high school graduation is a very robust result, although the estimates are sometimes slightly smaller, also considering non-linear estimators. Whether the effects are decreasing at 24 can be debated.

³⁴These values are chosen arbitrarily. With these cut-off values, we drop close to 30 percent of the sample in each specification, which we believe is a reasonable amount. In addition, a first-stage coefficient estimate less than 0.2 is rather low and indicates that there are many jobs available or that case handlers are greatly deviating from the rank order. These are situations in which we have little experimental variation.

TABLE 12
ROBUSTNESS I

	Probit FS	Group Instruments	Probit/group Instruments	FS $\hat{\beta} > 0.3$	FS $t > 3$	Poisson Regression (RF)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Income (€) Age 20						
main						
Job Offer	-807.6114*** (222.3451)	-607.9557*** (198.2720)	-618.6901*** (194.8964)	-727.9855*** (223.0371)	-643.0968*** (219.8894)	-0.0322*** (0.0095)
Observations	78134	78134	78134	60493	49423	78134
Panel B: Income (€) Age 21						
main						
Job Offer	-719.1918** (312.1818)	-546.0381* (293.4478)	-524.9823* (281.4973)	-646.0387** (321.4113)	-761.5506** (324.8456)	-0.0351*** (0.0120)
Observations	68864	68864	68864	52491	44551	68864
Panel C: Income (€) Age 22						
main						
Job Offer	-836.4560** (354.2368)	-645.3240* (330.5083)	-627.2909* (327.0014)	-679.6818** (334.7498)	-819.3079** (333.3795)	-0.0317*** (0.0116)
Observations	54688	54688	54688	40838	35774	54688
Panel D: Income (€) Age 23						
main						
Job Offer	-738.4240 (466.4578)	-876.4980** (385.9344)	-771.5082** (377.5454)	-774.2733* (430.9714)	-998.9303** (435.1902)	-0.0320** (0.0160)
Observations	39952	39952	39952	29288	25990	39958
Panel E: Income (€) Age 24						
main						
Job Offer	-383.5937 (746.1220)	-1323.7842** (567.6522)	-1122.8548** (560.0204)	-652.7113 (656.1084)	-809.9256 (674.5762)	-0.0164 (0.0316)
Observations	25946	25946	25946	18863	16688	25952

Notes. Each column and row display results from a different regression. All models and specifications use income as the dependent variable and we present results for different ages. Column (1) use a poisson estimator that produce estimates readily interpretable in percentages. Unlike the other columns, we only present the reduced form estimates in column (1). In column (2) we use a predicted instrument from a Probit model in the 2SLS estimator. In column (3) and (4) we interact the original and the predicted instruments with group dummies to derive up to 98 different instruments for each local office and year group. In columns (5) and (6) we restrict the sample based on group specific first stage results. We run first stage regressions for each group and save the coefficients and t-values. We then restrict the sample based on the size of the first stage coefficient (5) and the significance of the first stage coefficient (6) as described in the table. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Robustness concerning sample and controls

In the previous section, we showed that our inferences are robust to several choices we make concerning the functional form of the instrument and estimation model. Nevertheless, profound decisions in the research process also relate to the sample and specification. We aspire to keep as much data as possible and allow repeated applications from the same applicant. An alternative approach is to use only first-time applicants.³⁵ A benefit of using this sample is that we only observe each applicant once and can follow events after the application. The downside is that we heavily select within each randomization group and end up with around half the original sample.

In Table 13, column (1), we present the results on the effect of a job offer on wage income using first-time applications. The effects are larger than using all applicants. For example, at age 22, the estimated impact for first-time applicants is 1,570, while it is 1,000 when using all data. We can expect the estimates from first-time applicants to be more volatile since they comprise only half the sample we have. Participating numerous times in a program with random success means that shocks (successes) should be averaged out to some extent. Our conclusions remain, and we prefer using a larger sample that more closely mimics the randomized population each year.

In column (2), we exclude the prioritized youths; in column (3), we use first-time applications and exclude the prioritized youths. Excluding prioritized youths may be unnecessary since they were also allocated a random number. However, since we can expect prioritized youths to be outliers in a social disadvantage sense, their exclusion can be motivated as a way to check for outliers. Nevertheless, excluding prioritized youths has a minor impact on the estimates, while jointly excluding the prioritized and reapplications mimic the results in column (1).

While prioritized youths may have poor income prospects, we may also observe youths with the opposite potential. Applicants with, for example, a high income before applying to the program may have much better income trajectories afterward. They may, therefore, influence the estimated effects disproportionately if they, by chance, are provided more often with a high or low rank order. To account for these high-income earners, we exclude applicants with an income of more than 10,000 euros in the year before applying to the program. These youths could have worked extensively before their application and accounted for 810 applicants. The resulting estimates are presented in column (4) of Table 13, showing that these potentially influential youths do not affect the estimated effect sizes to any extent in

³⁵Although the application system was centralized and changed in 2012, we cannot guarantee that earlier application than 2012 were not considered in the allocation of jobs to applicants. In this way, being a first time applicants in our data means the first time an applicant is observed.

the medium term.

Although our balance test provided evidence suggesting that the instrument was unrelated to pre-determined characteristics³⁶, we include all control variables in our basic model in column (5). We do this because the different outcomes we test rely on different samples. Estimated effects at higher ages rely on smaller samples where minor imbalances could be more influential. Even though our instrument is random by construction, imbalances due to chance are still possible. Since we have missing observations for some variables, we construct a set of dummy variables corresponding to each included variable that indicates missing values. We also recoded the missing values to zero to have the total sample in each regression specification. After including the pre-determined variables presented in Table 2, we find that the results do not change significantly relative to those from our baseline specification. Therefore, our results are unlikely to be explained by chance imbalances in the observed characteristics we have access to. However, given the decrease in the effect at age 23, we hold it open so that the attenuation visible at 24 may start at 23.

One pre-determined applicant characteristic that we do not control for above is age. Age at application is essential when we study wage income trajectories at specific ages after the application. Applicants aged 19 may have a vastly different income expectation at age 20 than a 16-year-old applicant. Age-specific differences come from summer employment opportunities outside the program becoming better with increasing age. To ensure that we do not build age-related imbalances into our estimated effects, we rerun regressions for most of our primary outcomes and include age at application dummies. The results are presented in Appendix Table A4, and we find that this inclusion makes little difference for our estimated effects.

³⁶Except for the sample aged 23.

TABLE 13
ROBUSTNESS II

	First time Applicants	No Prioritized Applicants	First time & No Prioritized	Inc $t - 1$ < 10,000€	Control Variables
	(1)	(2)	(3)	(4)	(5)
Panel A: Income (€) Age 20					
Job Offer	-984.3424*** (376.8555)	-658.7027*** (237.5402)	-781.1695** (364.0532)	-771.9327*** (241.0041)	-848.1214*** (238.8756)
Observations	39548	72423	36336	77429	78134
Panel B: Income (€) Age 21					
Job Offer	-1238.8997*** (465.1433)	-922.5023*** (338.0270)	-1026.7431** (419.0204)	-969.4999*** (355.0424)	-916.1456*** (353.5824)
Observations	33971	63788	31158	68220	68864
Panel C: Income (€) Age 22					
Job Offer	-1569.9837*** (546.5254)	-896.1430** (361.7505)	-1374.3250*** (502.1258)	-995.9087*** (374.9650)	-826.3390** (379.7142)
Observations	27510	50729	25274	54229	54688
Panel D: Income (€) Age 23					
Job Offer	-2012.5241*** (668.0277)	-1069.4068* (547.5435)	-2138.6556*** (649.4361)	-1014.8983** (515.2359)	-762.9341 (507.0706)
Observations	21168	37230	19572	39665	39958
Panel E: Income (€) Age 24					
Job Offer	-1545.8805** (724.4194)	-678.9276 (836.5673)	-1799.6102*** (695.6240)	-447.4145 (895.9105)	-176.6690 (865.5007)
Observations	15183	24145	14040	25791	25952

Notes. Each column and row display results from a different regression. All models and specifications use income as the dependent variable and we present results for different ages. In column (1) we include only first time applications such that each individual can be observed only once. In column (2) we remove the prioritized group from the sample. In column (3) we use first time applications and remove the prioritized group. Column (4) restrict the sample based on applicants' incomes in the year before the first application to the program. Finally, in column (5) we include all control variables described in Table 2. To keep as many observations as possible, we recode the variables with missing values to zero and include a variable specific dummy variable indicating missing values in the specification. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Further tests of the Exclusion Restriction

We note, in Section 3, that a feature of the program could violate the exclusion restriction. This feature is the dynamic effect of the rank order on future program participation. This feature suggests that a job offer today decreases the likelihood of applicants receiving a job offer in the future. This program rule implies that we may estimate both effects from

program participation and age at participation.

While this program feature could obscure the interpretation of our IV estimates, we substantiate that there is little cause for concern. First, we estimate the magnitude of the program feature in Table A10. Compared to our direct first-stage estimate of 0.37, the dynamic effects are minor. The likelihood of receiving a job offer in the following years decreased by 6.7 percentage points. Later dynamics are all small and insignificant. Since a high-rank order increases the likelihood of getting a job offer in the same year by 37 pp but only reduces the chance of getting a job offer in the future by 6.7 pp, the impact of this program feature must be small.

We can cautiously test if the dynamic influence of rank order can change our estimated effects and, thereby, the interpretation of our findings. To do this, we select a sample from our data that this feature cannot influence. We select one-time applicants and applicants who are old enough not to be eligible for the program in the next year. Table A11 presents the wage income dynamics from age 20 to 24 for this sample. Although the sample comprises less than half the original sample size, the results are essentially the same. We conclude that while the program rules pose concerns for our IV interpretation, their impacts are likely negligible.

6 CONCLUSIONS

Youth unemployment is a major concern in many countries today, and policies to help youths enter the labor market are high on their policy agendas. Different types of active labor market programs that target high school-aged youths are used extensively to ease the transition for schooling into the labor market. The public sector can, for example, offer subsidized employment in public services during the summer. We study such a program with random assignment, implemented in Stockholm between 2012 and 2018, and investigate how the program affected youths' labor market success, criminality, health and university attendance.

Our findings are in part consistent with recent evidence from similar programs in the US showing that non-program employment decreases with corresponding income losses following a SYEP job (Gelber et al., 2016). However, in our context the wage punishment much larger and longer lasting. Moreover, we show that the probability of having a job is not affected but after high school graduation, the affected group end up with jobs of lower qualification and are less likely to have a full-time job. In addition, our analysis makes it explicit that the negative effects are appearing just after high school graduation.

One explanation that has been proposed in the literature is that more advantaged youths

benefit from program jobs (Davis and Heller, 2020). We find the opposite. First, our complier group is relatively advantaged compared to the US-studies and no subgroup show significant gains in income except at the program year. Second estimating quantile regressions we find that, for all ages, youths in the higher end of the conditional income distribution experience larger income decreases. The more advantaged youths are hurt more by program job offers in the Stockholm SYEP. However, we must again point out that the Stockholm SYEP may very well be beneficial for disadvantaged youths, a group which we cannot study with our empirical design.

Lastly, we cannot confirm the consensual results from the US-studies on crime, nor do we find any substantial results on health outcomes or university registration. Thus, the Stockholm SYEP seems only to come with costs both for the taxpayers and for the affected complier participants and little gains in other behavioral aspects, at least for the group under study, the relatively advantaged youths of the Stockholm SYEP.

Our results highlight the importance of contextual factors in the evaluation literature of SYEP. Launching a large non-targeted summer job program seem to be a hindrance instead of a help for more advantaged youths. If policymakers still would like to offer this group summer job opportunities in this context, subsidizing private summer jobs holds the promise to be more efficient. Subsidizing private summer jobs could also help in keeping the supply of summer jobs constant even if the public program jobs is removed from the youth labor market. Another policy change would be to directing the resources only to the most disadvantaged, similar to the US-based SYEP.

REFERENCES

- Alam, M., K. Carling, and O. Nääs (2015). The program and treatment effect of summer jobs on girls' post-schooling incomes. *Evaluation Review* 39(3), 339–359.
- AMF (2018). Anvisningar för feriejobb till unga.
- Angrist, J. D. (1990). Lifetime earnings and the vietnam era draft lottery: Evidence from social security administrative records. *The American Economic Review* 80(3), 313–336.
- Angrist, J. D. and J.-S. Pischke (2008). *Mostly harmless econometrics*. Princeton University Press.
- Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of political economy* 76(2), 169–217.
- Biewen, M. and S. Steffes (2010). Unemployment persistence: Is there evidence for stigma effects? *Economic Letters* 106(3), 188–190.
- Blandhol, C., J. Bonney, M. Mogstad, and A. Torgovitsky (2022). When is tsls actually late? Working Paper 2022-16, University of Chicago, Becker Friedman Institute for Economics.
- Bound, J., D. A. Jaeger, and R. M. Baker (1995). Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak. *Journal of the American Statistical Association* 90(430), 443–450.
- Card, D., J. Kluge, and A. Weber (2010). Active labour market policy evaluations: A meta-analysis. *The economic journal* 120(548), F452–F477.
- Corcoran, M. (1982). *The Employment and Wage Consequences of Teenage Women's Nonemployment*. University of Chicago Press.
- Davis, J. and S. Heller (2020). Rethinking the benefits of youth employment programs: The heterogeneous effects of summer jobs. *The Review of Economics and Statistics* 102(4), 664–677.
- Ellwood (1982). *Teenage Unemployment: Permanent Scars or Temporary Blemishes*. University of Chicago Press.
- Gelber, A., A. Isen, and J. B. Kessler (2016). The effects of youth employment: Evidence from new york city lotteries. *The Quarterly Journal of Economics* 131(1), 423–460.

- Gregg, P. (2001). The impact of youth unemployment on adult unemployment in the ncds. *Economic Journal* 111(475), 626–653.
- Heckman, J. J. and G. J. Borjas (1980). Does unemployment cause future unemployment? definitions, questions and answers from a continuous time model of heterogeneity and state dependence. *Economica* 47(187), 247–283.
- Heckman, J. J., R. J. Lalonde, and J. A. Smith (1999). The economics and econometrics of active labor market programs. *Handbook of Labor Economics* 3(1), 1865–2097.
- Heller, S. B. (2014). Summer jobs reduce violence among disadvantaged youth. *Science* 346(6214), 1219–1223.
- Hensvik, L., D. Müller, and O. Nordström-Skans (2023). Connecting the young: High school graduates’ matching to first jobs in booms and great recessions. *The Economic Journal* 133(652), 1466–1509.
- HRF (2020). Ungdomslön för dig under 20 år. <https://www.hrf.net/lon-och-villkor/din-lon/ungdomslon-for-dig-under-20-ar/>. The Swedish Hotel and Restaurant Workers Union. Accessed: 2022-04-25.
- Keane, M. and T. Neal (2023). Instrument strength in iv estimation and inference: A guide to theory and practice. *Journal of Econometrics* 235(2), 1625–1653.
- Le Barbanchon, T., D. Ubfal, and F. Araya (2022). The effects of working while in school: Evidence from employment lotteries. *American Economic Journal: Applied Economics*. Forthcoming.
- Lee, D. S., J. McCrary, M. J. Moreira, and J. R. Porter (2021). Valid t-ratio inference for iv. *National Bureau of Economic Research Working Paper Series* (29124).
- Leos-Urbel, J. (2014). What is a summer job worth? the impact of summer youth employment on academic outcomes. *Journal of Policy Analysis and Management* 33, 891–911.
- Machado, J. and J. Santos Silva (2018). Xtqreg: Stata module to compute quantile regression with fixed effects. Boston College Department of Economics, Statistical Software Components S458523, revised 13 Oct 2021.
- Marbach, M. (2020). Ivdsc: Stata module to profile compliers and non-compliers for instrumental variable analysis. Technical Report Statistical Software Components S458808, Boston College Department of Economics. revised 19 Mar 2021.

- Modestino, A. S. and R. Paulsen (2022). School's out: How summer youth employment programs impact academic outcomes. *Education Finance and Policy*. Forthcoming.
- Motta, V. (2019). Estimating poisson pseudo-maximum-likelihood rather than log-linear model of a log-transformed dependent variable. *RAUSP Management Journal* 54(4), 508–518.
- Nordström Skans, O. (2011, March). Scarring effects of the first labor market experience. *IZA Discussion Paper* (5565).
- Nyström, S. (2021, 04). Grönsaksodlingen är bästa skolan in i arbetslivet.
- OECD (2020). Youth unemployment rate. <https://data.oecd.org/unemp/youth-unemployment-rate.htm#indicator-chart>.
- SCB (2017). Inkomstrapport 2015: Individer och hushåll. Ekonomisk välfärdsstatistik 2017:1, Statistics Sweden. https://www.scb.se/contentassets/9318ee9ece614a54aff26f69cb5504c2/he0110_2015a01_br_he50br1701.pdf.
- Schwartz, A., J. Leos-Urbel, J. McMurry, and M. Wiswall (2021). Making summer matter: The impact of youth employment on academic performance. *Quantitative Economics* 12, 477–504.
- SKR (2019). Feriejobb för ungdom – sommaren 2019. Sveriges kommuner och regioner, rapport.
- Staiger, D. and J. H. Stock (1997). Instrumental variables regression with weak instruments. *Econometrica* 65(3), 557–586.
- Stock, J. H. and M. W. Watson (2015). *Introduction to econometrics (3rd updated edition)*.
- Valentine, E. J., C. Anderson, F. Hossain, and R. Unterman (2017). An introduction to the world of work: A study of the implementation and impacts of new york city's summer youth employment program.
- Wooldridge, J. M. (2002). *Econometric Analysis of Cross Section and Panel Data*. MIT Press.

7 APPENDIX: ADDITIONAL TABLES AND FIGURES

7.1 Tables

TABLE A1
 APPLICANTS AND THE GENERAL POPULATION

Parental education	Program applicants:	General population:
	All Ages 16-19	Born in Sweden ¹ Ages 15-19
Both parents university	24%	22%
Only father university	14%	11%
Only mother university	18%	20%
No parental university education	44%	47%
Observations	31,160	Population

Notes. (1). Temarapport 2016:1 Utbildning. Samband mellan barn och föräldrars utbildning. Statistics Sweden, SCB. Authors calculations from data. We require both parents to have a registered education for these averages. Information on the applicants' parental education require that both have a valid educational code in the data. Authors calculations from own data.

TABLE A2
ARE CASE HANDLERS PRIORITIZING APPLICANTS BASED ON THEIR RANK ORDER?

Sample :	All	2012	2013	2014	2015	2016	2017	2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome: 1(Prioritized)								
Rank Order	-0.0048 (0.0038)	-0.0290 (0.0213)	0.0037 (0.0068)	-0.0088 (0.0066)	0.0094 (0.0078)	-0.0105* (0.0049)	0.0021 (0.0106)	-0.0075 (0.0062)
Observations	92095	10440	10723	10684	15860	14804	14847	14737

Notes. Each column and row display results from a different reduced form regression. The outcome is an indicator variable for if the applicant was prioritized, meaning that a case handler decided that the applicant should be offered a job irrespectively of rank order or other circumstances. Column (1) shows the reduced form results for the full sample while columns (2) to (8) analyze each year respectively. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3
TYPE OF OCCUPATIONS AT DIFFERENT AGES

	SSYK	Averages by age			
		20	21	22	23
Panel A: High Qualification Jobs:					
Managers	1	0.001	0.002	0.002	0.002
University degree occupations	2	0.015	0.018	0.029	0.046
University professional occupation	3	0.022	0.035	0.044	0.06
Panel B: Medium Qualification Jobs:					
Administration and customer support	4	0.067	0.085	0.102	0.105
- Customer support / receptionists	422	0.027	0.036	0.043	0.040
Service, care, and sales	5	0.27	0.33	0.36	0.36
- Retail staff	522	0.10	0.12	0.13	0.12
- Care of children	531	0.07	0.08	0.09	0.08
Farming, gardening, and fishing	6	0.002	0.003	0.003	0.003
Construction and manufacturing	7	0.015	0.020	0.023	0.025
Transport and factory manufacturing	8	0.007	0.010	0.012	0.018
Panel C: Low Qualification Jobs:					
Low qualification requirements	9	0.090	0.092	0.088	0.080
- Fast food staff	941	0.072	0.073	0.064	0.054
No SSYK code	-	0.592	0.405	0.337	0.301

Notes. This table presents how common different occupation codes are at different ages. We include each individual only once for each average and condition the data such that an SSYK occupational code is observed for each individual at each age (this data is available until 2017). 20,550 individuals without a missing SSYK code is observed at age 20. At age 23, only 3,946 individuals are observed. In the last row we present the fraction of observations without an occupational code that are either missing or not classified (having a value of ***). These individuals can not be classified for some reason or do not have a job. Subcategories for first digit SSYK codes are included in the complete category above them.

TABLE A4
INCLUDING AGE CONTROLS: WAGE INCOME (€) IN YEARS AFTER PROGRAM APPLICATION

Outcome Measured at Age:	17 yr	18 yr	19 yr	20 yr	21 yr	22 yr	23 yr	24 yr
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Income								
Job Offer	-230.4596** (105.5049)	-142.1565 (97.3721)	-167.8170 (184.5478)	-750.2061*** (233.5254)	-1010.8944*** (363.2285)	1012.6726*** (376.6388)	-660.9624 (511.7750)	-52.3171 (772.2764)
Panel B: Positive income								
Job Offer	0.0465** (0.0233)	0.0159 (0.0146)	0.0158 (0.0122)	-0.0002 (0.0097)	0.0040 (0.0105)	-0.0017 (0.0113)	0.0010 (0.0141)	0.0021 (0.0260)
Panel C: Crimes convicted								
Job Offer	-0.0009 (0.0007)	-0.0007 (0.0006)	-0.0007 (0.0007)	-0.0002 (0.0003)	-0.0006* (0.0004)	-0.0004 (0.0011)	-0.0006 (0.0004)	-0.0023 (0.0016)
Panel D: Crimes suspected								
Job Offer	-0.0037 (0.0034)	-0.0022 (0.0032)	0.0002 (0.0019)	-0.0022 (0.0020)	-0.0015 (0.0011)	-0.0018 (0.0013)	-0.0018 (0.0021)	-0.0126 (0.0096)
Panel E: In-hospital visits								
Job Offer	0.8435** (0.4299)	-0.0684 (0.2779)	-0.0939 (0.2367)	0.0225 (0.2342)	-0.0720 (0.3285)	-0.0664 (0.4149)	0.3633 (0.8112)	2.1029** (1.0601)
Panel F: Prescriptions								
Job Offer	-0.1964 (0.3951)	-0.2188 (0.1949)	-0.0305 (0.1604)	0.1318 (0.1812)	0.1517 (0.2227)	0.1169 (0.2867)	0.0471 (0.4040)	-0.2806 (0.8026)

Notes. Each column and row display results from a different reduced form regression. Each column shows the 2SLS results that mimic previous results with the exception that we include age dummies in this specification. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering. Significance is described by * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE A5
IS THERE A JOB MATCH EFFECT? TESTING FOR NON-LINEAR
FIRST STAGE EFFECTS

Outcome: Job Offer at Age	20	21	22	23	24
	(1)	(2)	(3)	(4)	(5)
Linear Instrument					
Rank Order	0.3931*** (0.0317)	0.4026*** (0.0346)	0.3996*** (0.0374)	0.3775*** (0.0402)	0.3536*** (0.0446)
Outcome Mean	0.61	0.60	0.58	0.56	0.53
Observations	80504	68944	54708	39957	25946
Quadratic Instrument					
Rank Order	0.3301*** (0.0913)	0.3212*** (0.0972)	0.2696*** (0.0954)	0.1648* (0.0911)	0.0998 (0.0858)
Rank Order Squared	0.0630 (0.0856)	0.0814 (0.0894)	0.1299 (0.0892)	0.2126** (0.0881)	0.2540*** (0.0901)
Outcome Mean	0.61	0.60	0.58	0.56	0.53
Observations	80504	68944	54708	39957	25946
Cubic Instrument					
Rank Order	0.1596 (0.1697)	0.1120 (0.1687)	0.0401 (0.1621)	-0.0501 (0.1428)	-0.1456 (0.1335)
Rank Order Squared	0.4893 (0.3597)	0.6046* (0.3543)	0.7033** (0.3396)	0.7507** (0.3155)	0.8688** (0.3368)
Rank Order Qubic	-0.2840 (0.2173)	-0.3488 (0.2164)	-0.3820* (0.2080)	-0.3589* (0.2027)	-0.4103* (0.2290)
Outcome Mean	0.61	0.60	0.58	0.56	0.53
F-test p-value	0.399	0.235	0.101	0.014	0.002
Observations	80504	68944	54708	39957	25946

Notes. Each column and row display results from a different reduced form regression. The columns describe the different outcomes and the different panels present different specifications. The reported p-value in the cubic specification comes from an F-test on the joint hypothesis that both the quadratic and cubic terms are zero. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A6
IS THERE A JOB MATCH EFFECT? TESTING FOR NON-LINEAR REDUCED FORM EFFECTS

Outcome: Wage Income (€) at Age	20	21	22	23	24
	(1)	(2)	(3)	(4)	(5)
Linear Instrument					
Rank Order	-274.3865*** (91.4009)	-396.6533*** (136.0086)	-399.3731*** (146.8137)	-399.0336** (198.6907)	-168.2397 (322.6535)
Outcome Mean	9842.98	11360.15	12563.44	12449.98	10281.75
Observations	80504	68944	54708	39957	25946
Quadratic Instrument					
Rank Order	-638.1592 (500.3396)	-439.2751 (571.8133)	-778.7474 (736.6288)	-492.2878 (923.6366)	-417.7031 (1267.3199)
Rank Order Squared	363.6088 (476.5135)	42.6180 (551.5961)	379.0507 (713.6489)	93.2270 (875.2446)	249.6201 (1214.5205)
Outcome Mean	9842.98	11360.15	12563.44	12449.98	10281.75
Observations	80504	68944	54708	39957	25946
Cubic Instrument					
Rank Order	-2019.4384* (1071.4626)	-2044.2832 (1391.8509)	-1865.2364 (1980.4967)	-1388.0007 (1980.0435)	-1475.0074 (2877.8550)
Rank Order Squared	3816.7439 (2426.6965)	4056.9897 (3177.0520)	3093.6858 (4494.5000)	2336.0071 (4732.1621)	2899.0721 (6699.6656)
Rank Order Qubic	-2300.9101 (1588.9678)	-2675.7750 (2047.2977)	-1808.3937 (2877.0070)	-1495.9096 (3204.9868)	-1767.8619 (4459.7517)
Outcome Mean	9842.98	11360.15	12563.44	12449.98	10281.75
F-test p-value	0.264	0.428	0.748	0.881	0.899
Observations	80504	68944	54708	39957	25946

Notes. Each column and row display results from a different reduced form regression. The columns describe the different outcomes and the different panels present different specifications. The reported p-value in the cubic specification comes from an F-test on the joint hypothesis that both the quadratic and cubic terms are zero. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A7
 REPLICATION OF GELBER ET AL. (2016). TABLE III

Outcome:	Wage Income (€)	Any Job	Convicted Crimes	Suspected Crimes	Prescriptions Drugs	Hospital Visits	University Registrations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year 0							
Program job offer	472.8332*** (80.7998)	0.3413*** (0.0241)	-0.0000 (0.0004)	0.0022 (0.0025)	0.1407 (0.1490)	-0.1241 (0.0865)	-0.0001 (0.0020)
Year 1							
Program job offer	-307.2549** (153.8474)	0.0767*** (0.0146)	-0.0002 (0.0005)	0.0007 (0.0020)	0.0903 (0.1544)	-0.1373 (0.0978)	-0.0057 (0.0035)
Year 2							
Program job offer	-288.5558 (221.1601)	-0.0347*** (0.0119)	-0.0012** (0.0005)	-0.0031* (0.0017)	0.0328 (0.1745)	-0.0615 (0.0983)	0.0097 (0.0059)
Year 3							
Program job offer	-406.9445 (259.8572)	0.0042 (0.0093)	-0.0006 (0.0005)	-0.0016 (0.0014)	0.2985 (0.1877)	-0.2391* (0.1388)	0.0077 (0.0085)
Year 4							
Program job offer	-1012.8229*** (377.9949)	0.0078 (0.0116)	-0.0014** (0.0007)	-0.0049** (0.0022)	0.3746 (0.2777)	-0.2121 (0.1674)	0.0096 (0.0101)
Year 0-4							
Program job offer	-1744.4632* (896.0029)	0.4078*** (0.0403)	-0.0049** (0.0020)	-0.0111 (0.0105)	1.5891 (1.0663)	-0.8774 (0.6257)	0.0119 (0.0136)
Year 1-4							
Program job offer	-2193.8566*** (851.3055)	0.0205 (0.0346)	-0.0047*** (0.0017)	-0.0133* (0.0075)	1.3322 (0.8924)	-0.6355 (0.5373)	0.0132 (0.0137)

Notes. Each column and row display results from a different reduced form regression. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A8
IV RESULTS IN YEARS AFTER APPLICATION BY AGE AT APPLICATION:
WAGE INCOME (€)

Years from Application:	0	1	2	3	4
	(1)	(2)	(3)	(4)	(5)
All ages					
Program job offer	472.8332*** (80.7998)	-307.2549** (153.8474)	-288.5558 (221.1601)	-406.9445 (259.8572)	-1012.8229*** (377.9949)
Outcome mean	1459.49	3432.44	6501.42	9827.70	12190.83
Observations	91943	91866	91707	76883	62011
AR p-value	0.0000	0.0533	0.1869	0.1111	0.0086
Age 16					
Program job offer	407.1715*** (44.5684)	-259.4026*** (100.6794)	19.3521 (189.6022)	-187.9934 (372.9299)	-1255.5378** (567.2121)
Outcome mean	606.09	1022.10	2010.58	5829.33	11279.37
Observations	21674	21653	21632	18194	14910
AR p-value	0.0000	0.0174	0.9187	0.6148	0.0319
Age 17					
Program job offer	440.8606*** (66.5025)	-278.9188** (126.3772)	-201.3168 (279.0812)	-654.0912 (528.9034)	-1578.6923** (722.5831)
Outcome mean	916.02	1801.30	5260.15	10588.76	12004.83
Observations	28824	28798	28733	24406	20118
AR p-value	0.0000	0.0297	0.4682	0.2149	0.0254
Age 18					
Program job offer	341.9391** (135.5498)	-113.7370 (334.3873)	-568.8492 (551.5629)	-1018.5778* (602.3279)	-933.0519 (706.1887)
Outcome mean	1470.37	4619.71	9573.44	11268.79	12543.27
Observations	26422	26381	26336	22291	18231
AR p-value	0.0190	0.7341	0.3004	0.0914	0.1898
Age 19					
Program job offer	834.0966** (394.9247)	-1046.3173 (659.1791)	-1153.9472 (989.0644)	184.1182 (912.1799)	1160.0287 (1865.6430)
Outcome mean	3632.51	8159.82	10135.48	11754.88	13485.12
Observations	12248	12237	12209	9546	6752
AR p-value	0.0565	0.1296	0.2518	0.8395	0.5077

Notes. Each column and row display results from a different reduced form regression. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A9
IV RESULTS ON APPLICATIONS, OFFERS, AND JOBS

	Applied Next Year	Applications	Offers	Jobs
	(1)	(2)	(3)	(4)
Program job offer	0.0848*** (0.0180)	0.1524*** (0.0305)	0.8484*** (0.0310)	0.7515*** (0.0323)
Outcome mean	0.51	2.41	1.54	1.33
Observations	77358	92095	92095	92095
AR p-value	0.0000	0.0000	0.0000	0.0000

Notes. Each column and row display results from a different reduced form regression. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A10
FIRST STAGE DYNAMIC EFFECTS

Outcome: Job Offer at t+	0	1	2	3
	(1)	(2)	(3)	(4)
Random number normalized	0.3708*** (0.0294)	-0.0671*** (0.0111)	-0.0045 (0.0034)	-0.0008 (0.0014)
Outcome mean	0.63	0.28	0.10	0.03
Observations	92095	92095	92095	92095

Notes. Each column and row described a different regression. We investigate how the rank order in year zero affects the job offer probability in future years. We define year a job offer in t+1 by the next application from t+0 within four years. T+2 is similarly defines as the third application within four years. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A11
SAMPLE NOT AFFECTED BY PROGRAM DYNAMIC EFFECTS: IV EFFECTS
ON WAGE INCOME (€)

Wage Income at Age:	20 yr	21 yr	22 yr	23 yr	24 yr
	(1)	(2)	(3)	(4)	(5)
Program job offer	-870.5211* (449.2483)	-1243.8791** (633.6453)	-686.0166 (587.8236)	-1293.3750 (893.7786)	-85.4750 (1340.7962)
Outcome mean	9054.61	10819.70	12355.45	13573.80	13717.51
Observations	28518	26258	22012	17740	13567
AR p-value	0.0547	0.0497	0.2465	0.1645	0.9493

Notes. Each column and row described a different regression. The sample include applicants with only one application, applicants aged 18 in 2012 and 2013, and applicants aged 19 after 2014. In this sample, the applicants job offer status next year cannot be influenced by their random number this year since they do not participate next year. All models include office by year fixed effects and the standard errors are clustered at the level of the fixed effects. Significance is described by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7.2 Figures

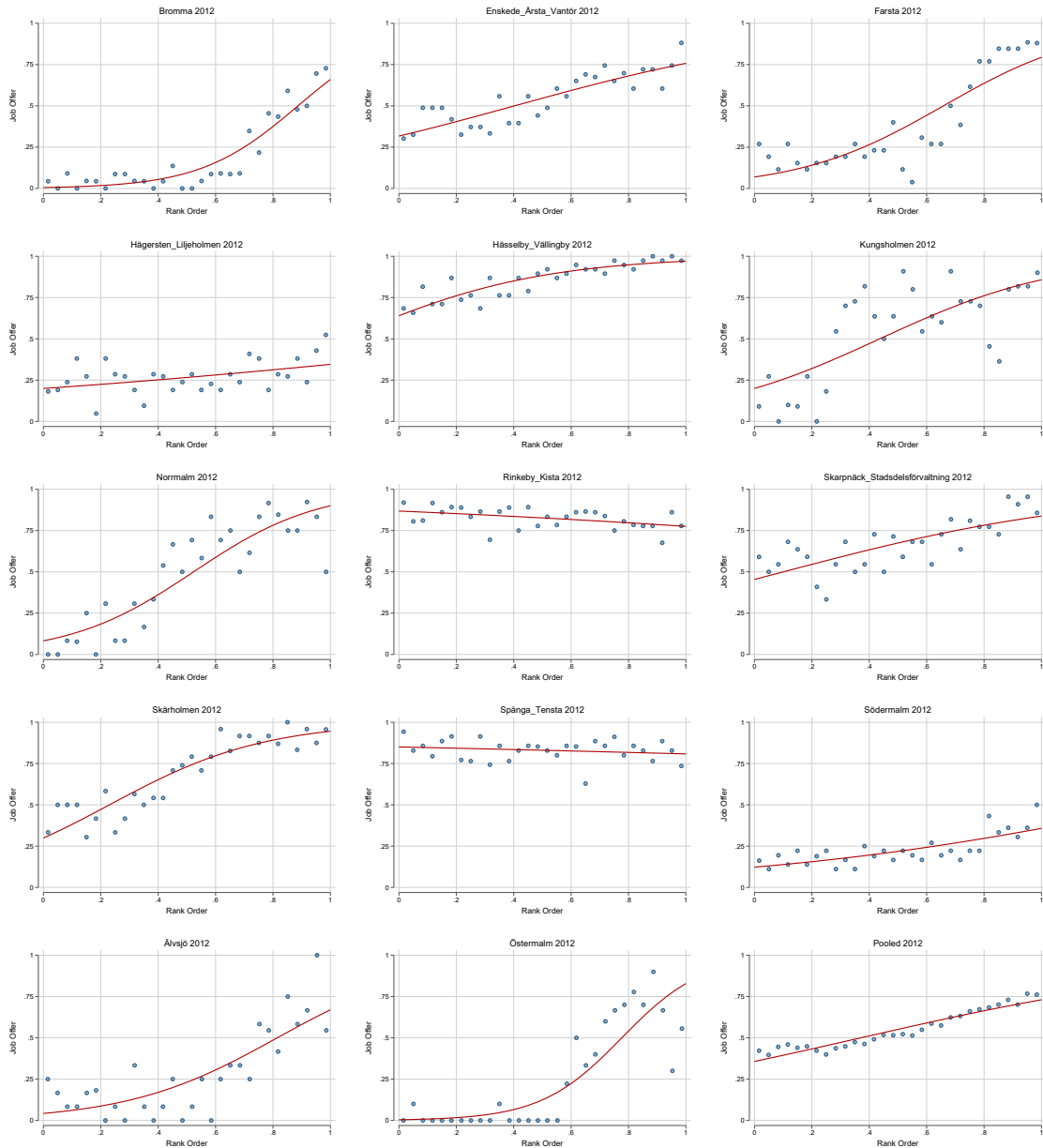


FIGURE A1

All First Stage Relationships in 2012

This figure shows binned scatterplots of the fraction job offers (y-axis) over the normalized random number (x-axis). Each subfigure use data from 2012 for each of the local offices that allocated jobs to applicants.

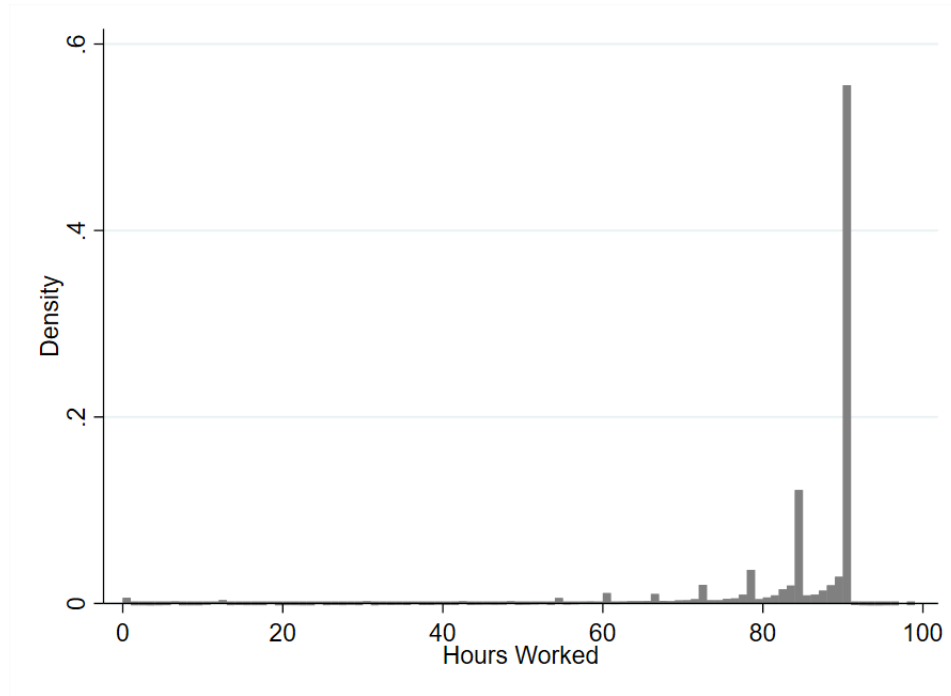


FIGURE A2

Hours Worked in the Program Jobs

This figure describes the data that we have acquired from the municipal labor market administration in Stockholm (Arbetsmarknadsförvaltningen) on program participation. It shows the hours worked for applicants that were offered, and accepted, a program job. The standard was 90 hours in total, 30 hours a week for three weeks, which is also the mode in the data. Information do not exist for all participating applicants on hours worked but this figure shows that most of them likely worked around 90 hours. There is around 50 percent missing information on hours worked in the data.

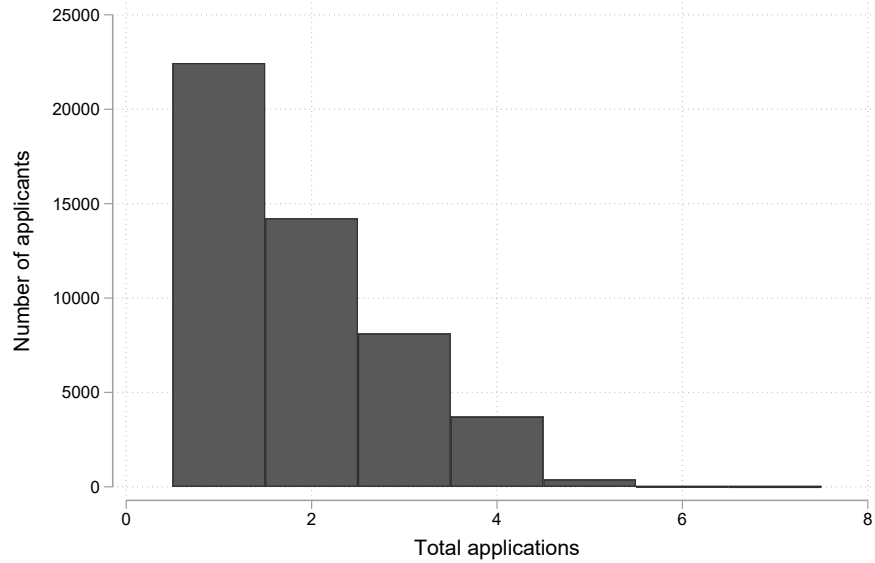


FIGURE A3

Applications by Each Applicant

This figure describes the data that we have acquired from the municipal labor market administration in Stockholm (Arbetsmarknadsförvaltningen) on program participation. It describes how many applicants that have a specific number of applications in the data. For example, some 23,000 applicants in the data have only one application registered during this period.

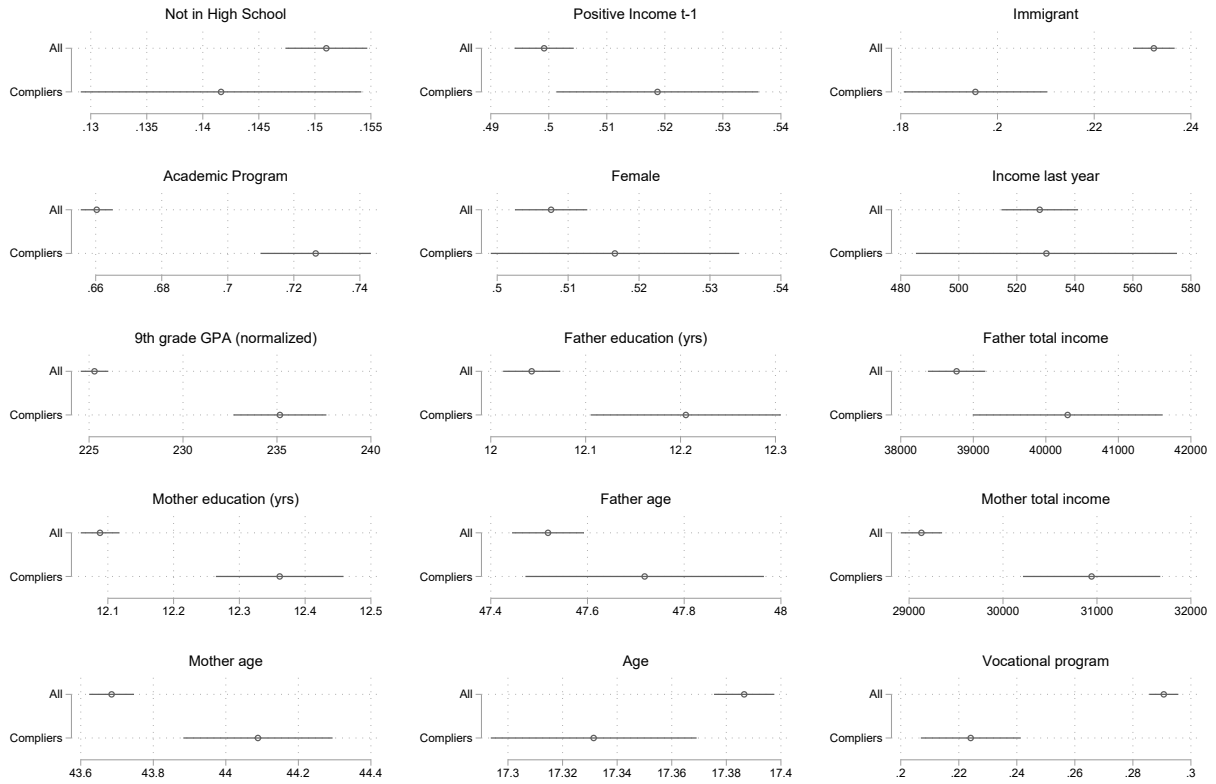


FIGURE A4

Complier characteristics compared to full sample

This figure shows complier characteristics in comparison to the full sample. To be able to characterize compliers in a simple way, we dichotomize the ordered random number. To do this we take the 20 percent of applicants with the lowest and highest rank order within each local office and year cell, and recode the instrument as one for those with the high rank order and zero for those with a low rank order. We remove applicants that are not in this span of the rank order. We use the Stata program *ivdesc* to calculate the means.

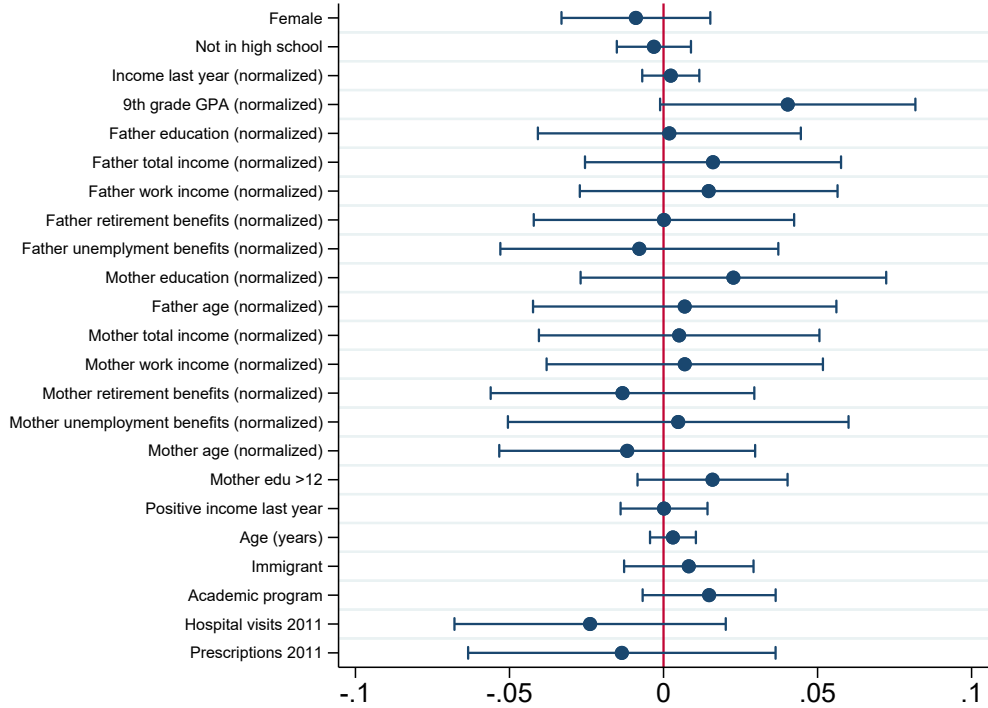


FIGURE A5

Balance of predetermined characteristics: Age 17

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 17. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

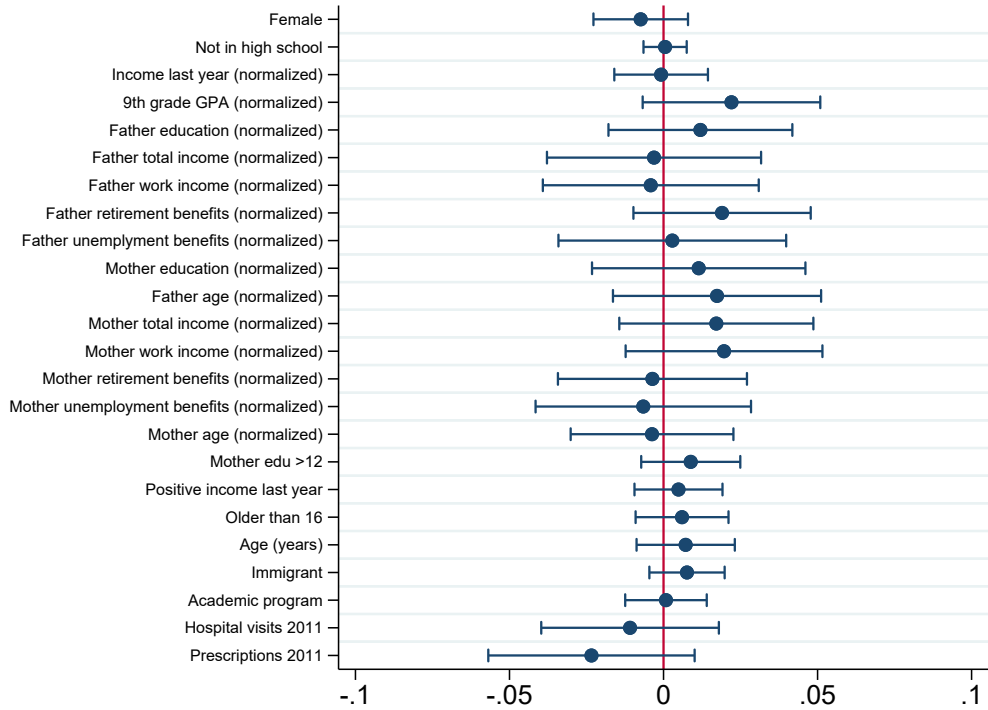


FIGURE A6

Balance of predetermined characteristics: Age 18

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 18. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

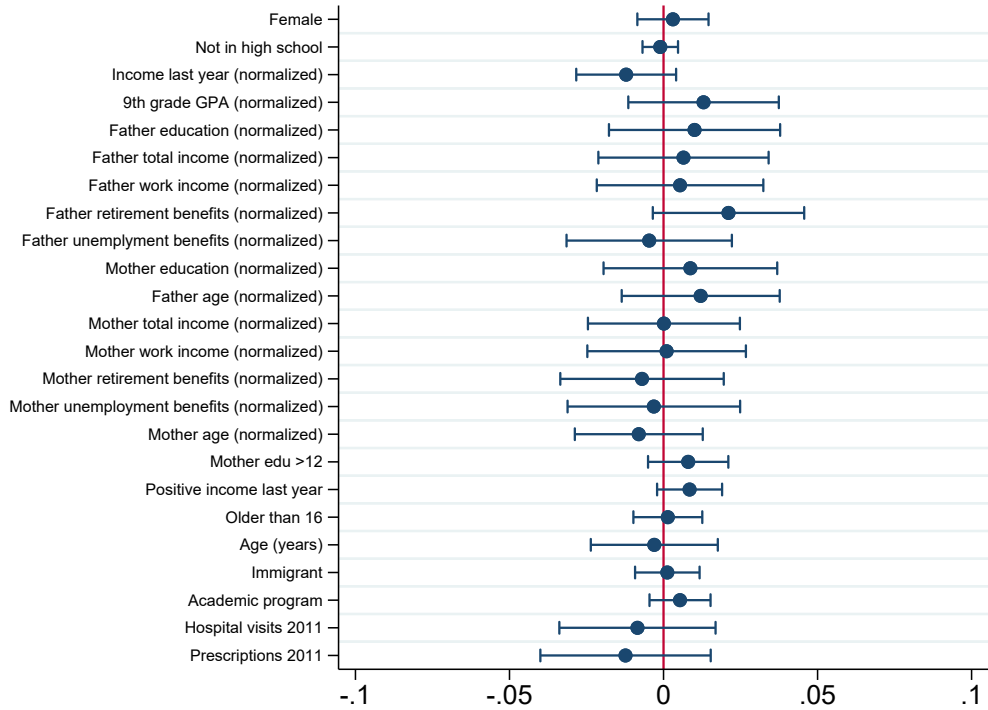


FIGURE A7

Balance of predetermined characteristics: Age 19

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 19. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

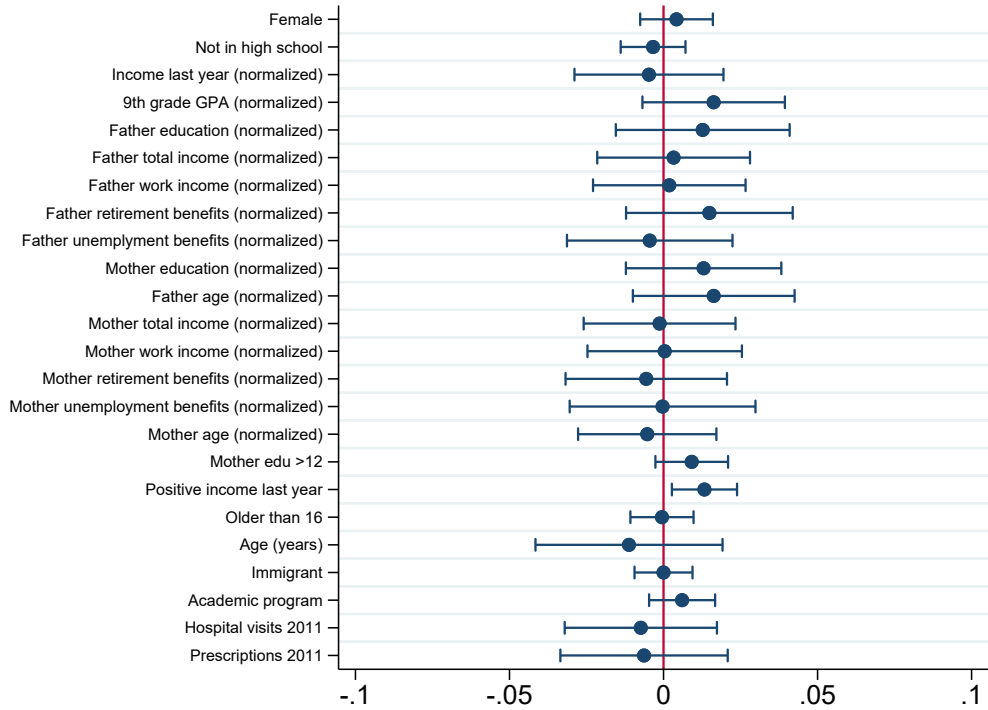


FIGURE A8

Balance of predetermined characteristics: Age 20

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 20. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

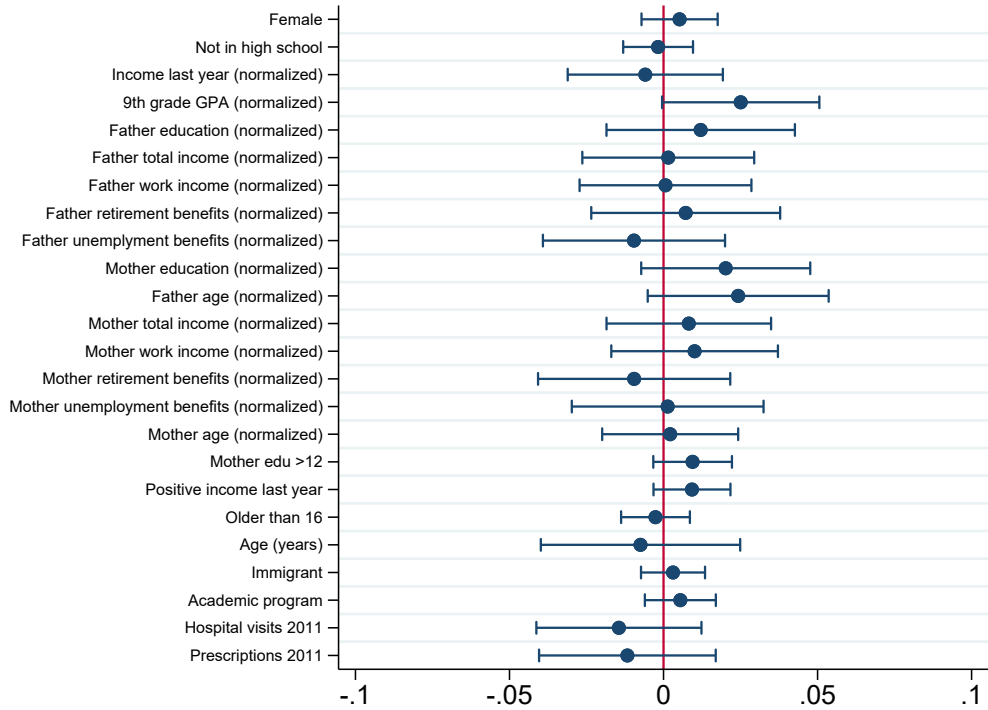


FIGURE A9

Balance of predetermined characteristics: Age 21

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 21. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

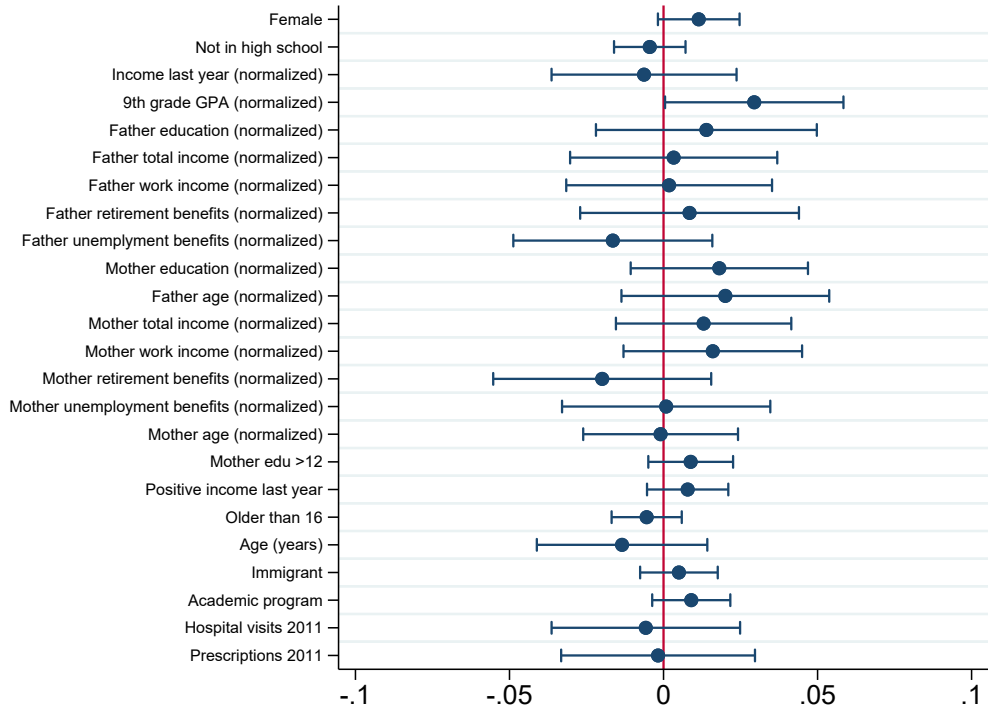


FIGURE A10

Balance of predetermined characteristics: Age 22

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 22. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

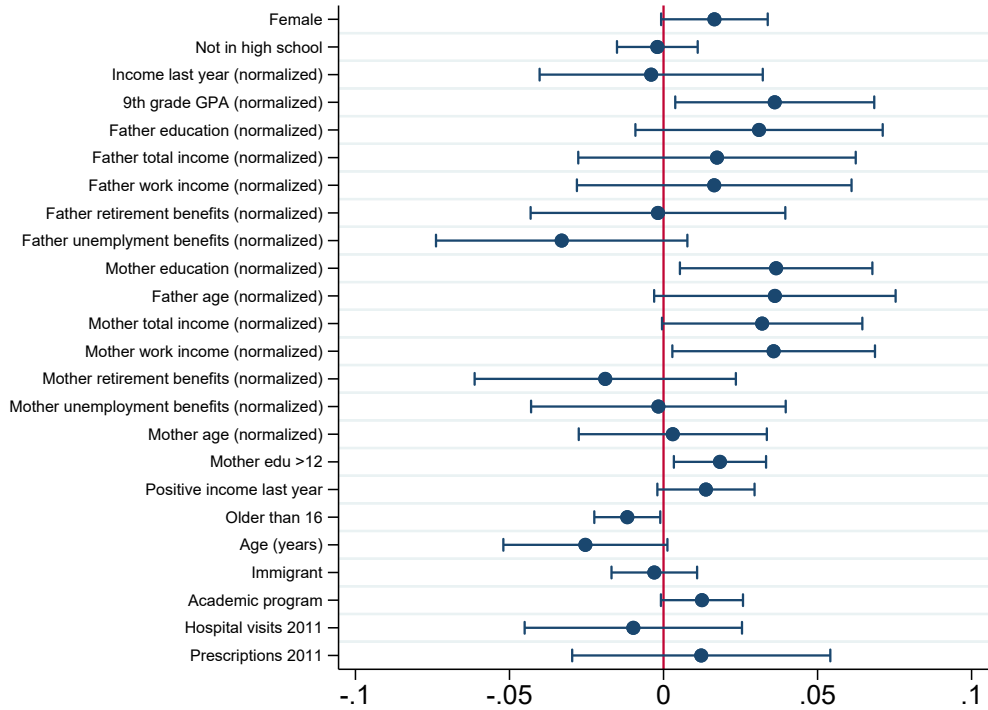


FIGURE A11

Balance of predetermined characteristics: Age 23

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 23. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

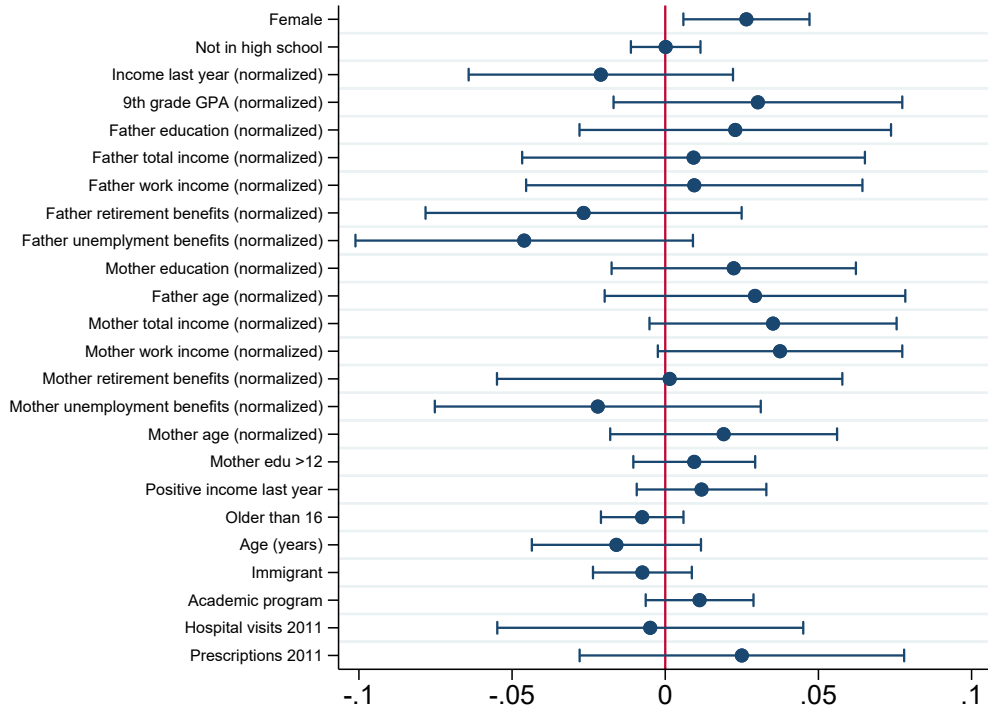


FIGURE A12

Balance of predetermined characteristics: Age 24

Each row in the figure represents a separate regression and estimated 2SLS coefficients and 95 percent confidence intervals of the job offer effect on the outcome according to equation (1). The outcomes are pre-determined characteristics which are described for each row/regression to the left of the figure. The sample includes all applicants between 2012-2018 that we can observe at age 24. Standard errors are clustered at the level of job offer assignment, i.e., the local municipal office and year level. Fixed effects are included at the level of the clustering.

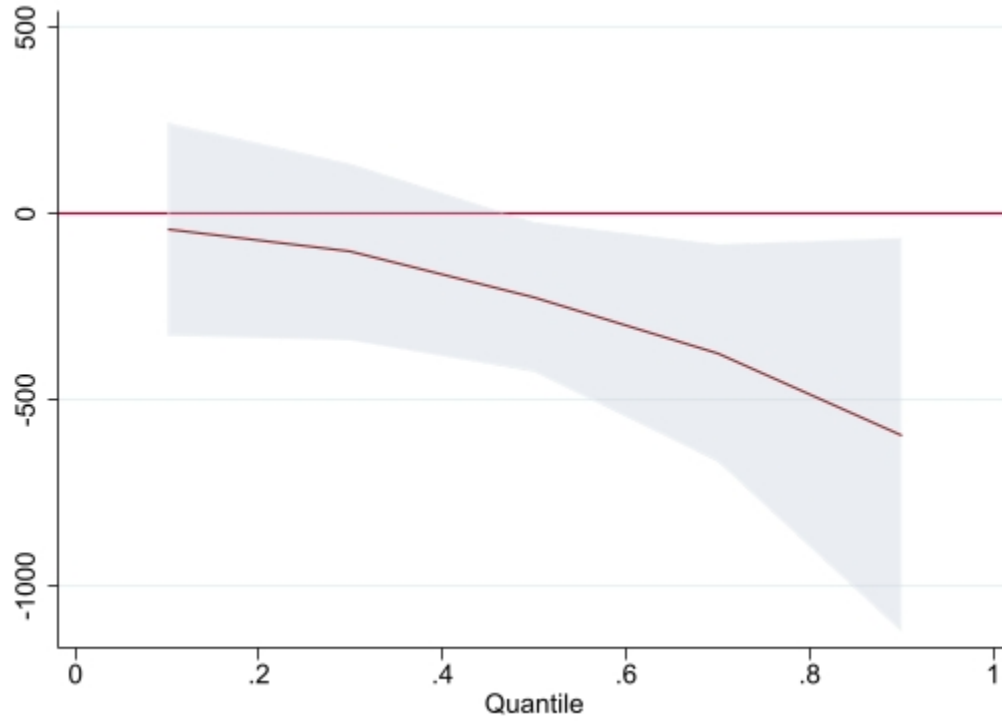


FIGURE A13

Reduced Form Quantile Estimates of Wage Income (€) at Age 20

This figure presents reduced form quantile regression estimates for the quantiles 0.1 to 0.9 using the wage income at age 20 as the dependent variable. Shaded area indicates the confidence intervals for each quantile estimate. We control for local office by year in the regression. We use the command *xtqreg* in Stata to estimate reduced form effects over the different quantiles (Machado & Santos Silva (2018)).

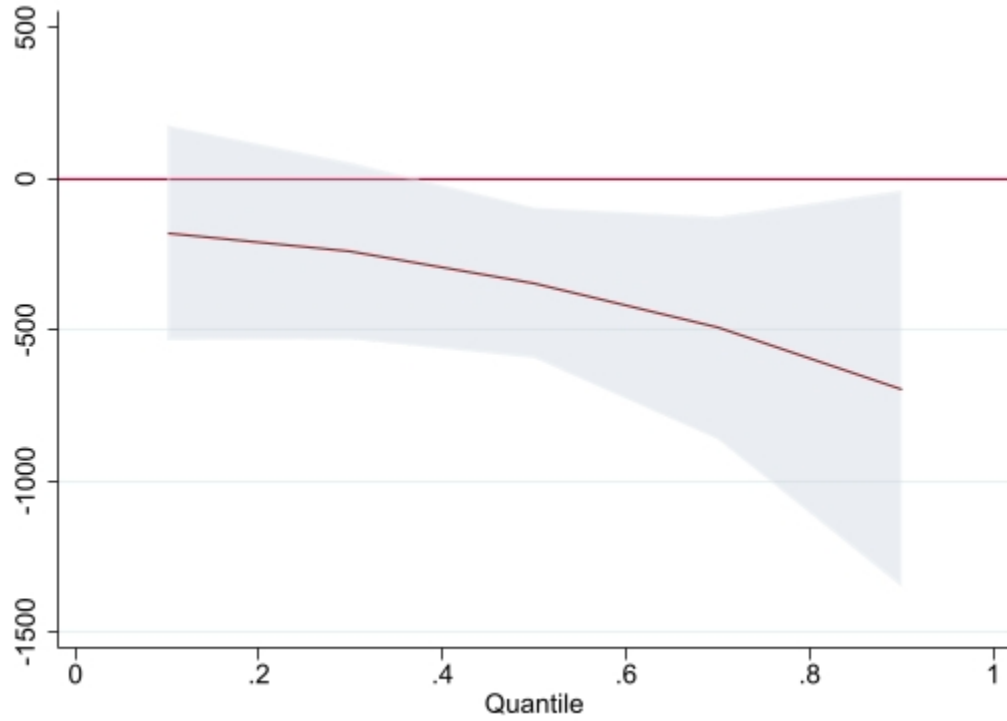


FIGURE A14

Reduced Form Quantile Estimates of Wage Income (€) at Age 21

This figure presents reduced form quantile regression estimates for the quantiles 0.1 to 0.9 using the wage income at age 21 as the dependent variable. Shaded area indicates the confidence intervals for each quantile estimate. We control for local office by year in the regression. We use the command *xtqreg* in Stata to estimate reduced form effects over the different quantiles (Machado & Santos Silva (2018)).

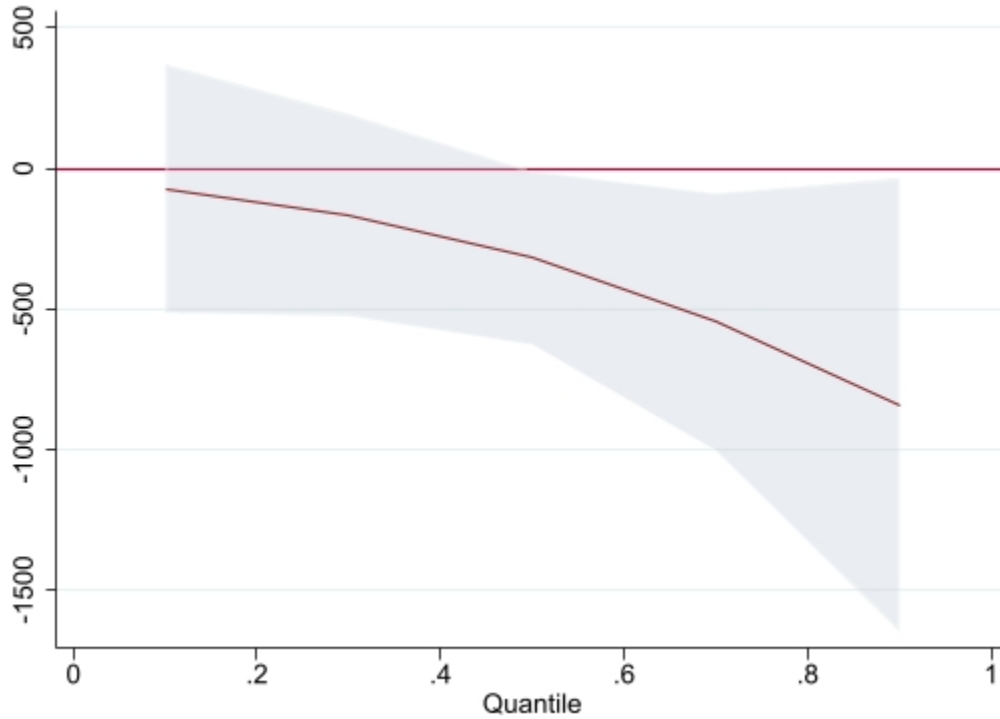


FIGURE A15

Reduced Form Quantile Estimates of Wage Income (€) at Age 22

This figure presents reduced form quantile regression estimates for the quantiles 0.1 to 0.9 using the wage income at age 22 as the dependent variable. Shaded area indicates the confidence intervals for each quantile estimate. We control for local office by year in the regression. We use the command *xtqreg* in Stata to estimate reduced form effects over the different quantiles (Machado & Santos Silva (2018)).

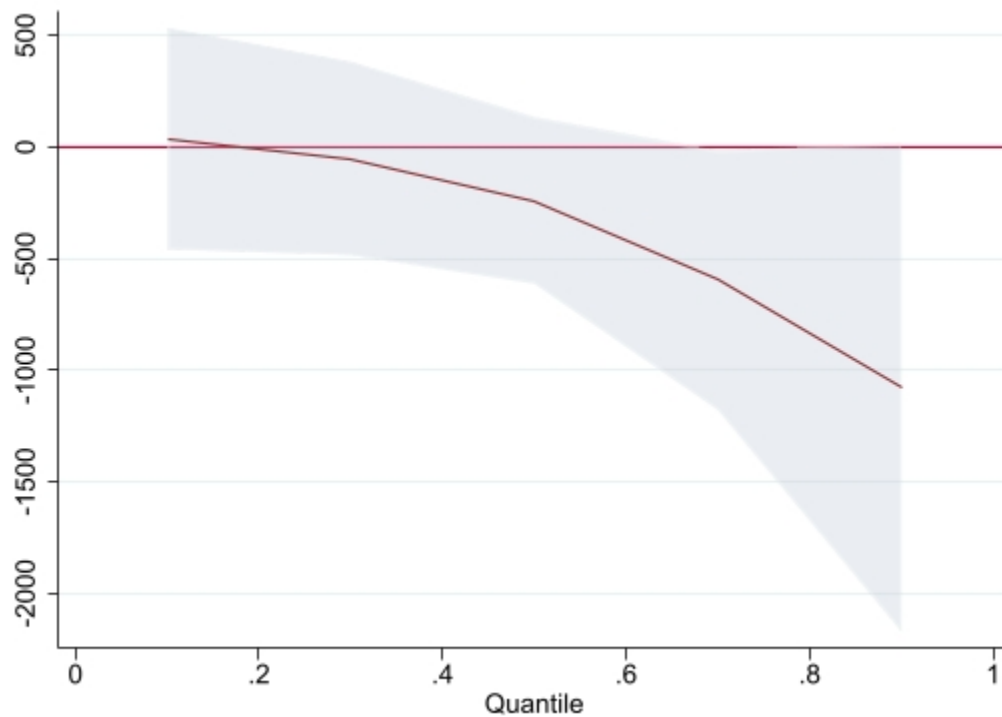


FIGURE A16

Reduced Form Quantile Estimates of Wage Income (€) at Age 23

This figure presents reduced form quantile regression estimates for the quantiles 0.1 to 0.9 using the wage income at age 23 as the dependent variable. Shaded area indicates the confidence intervals for each quantile estimate. We control for local office by year in the regression. We use the command *xtqreg* in Stata to estimate reduced form effects over the different quantiles (Machado & Santos Silva (2018)).

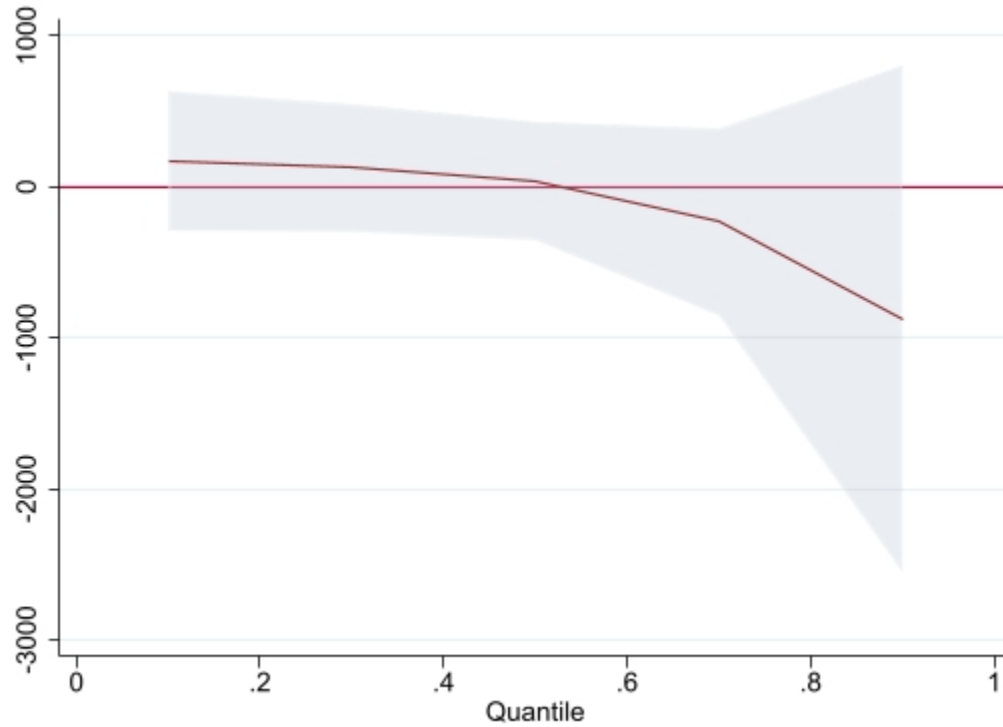


FIGURE A17

Reduced Form Quantile Estimates of Wage Income (€) at Age 24

This figure presents reduced form quantile regression estimates for the quantiles 0.1 to 0.9 using the wage income at age 24 as the dependent variable. Shaded area indicates the confidence intervals for each quantile estimate. We control for local office by year in the regression. We use the command *xtqreg* in Stata to estimate reduced form effects over the different quantiles (Machado & Santos Silva (2018)).