The Effects of Working while in School: Evidence from Uruguayan Lotteries*

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Abstract

Shall we encourage students to work while in school? We provide evidence by leveraging a one-year work-study program that randomizes job offers among students in Uruguay. Using social security data matched to over 120,000 applicants, we estimate an increase of 9% in earnings and of 2 percentage points in enrollment over the four post-program years for treated youth. Survey data indicate that enrolled participants reduce study time, but this does not translate into lower grades. Students mainly substitute leisure and household chores with work. A decomposition exercise suggests that work experience is the main mechanism behind the increase in earnings.

Keywords: student employment, randomized lottery. **JEL Codes:** J08, J22, J24, I21, I28.

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1 Introduction

Should students work while they are enrolled in school? Among OECD countries, the share of students aged between 15 and 19 who were working in 2016 averaged 14%, but it ranged from less than 10% in countries such as France, Italy, Japan, Mexico, and Chile to more than 40% in Denmark, the Netherlands, and Switzerland.[1] While some countries have promoted policies encouraging youth to study without working (e.g., the Bolsa Familia conditional cash transfer program in Brazil), others have designed programs that encourage youth to work while in school (e.g., the Federal Student Work Experience Program in Canada). This disagreement among policy-makers calls for more evidence on the effects of working while in school. The empirical literature has not reached a consensus on these effects and lacks experimental estimates. Furthermore, economic theory provides ambiguous predictions on the effects of working while in school.

On the one hand, theory suggests that working while in school might smooth the school-to-work transition. Youth may acquire skills at work that cannot be obtained at school. These could be hard skills (e.g., knowing how to write business reports) and soft skills (e.g., teamwork, personality factors), either general or sector-specific (Heckman et al., 2006; Alfonsi et al., 2020; Adhvaryu et al., 2018). Similarly, early work experience can provide a signal to employers, revealing workers' productivity or motivation, which could be particularly relevant when school grades or diploma lack information on skill levels (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014). Furthermore, employment may provide students with funding to continue with their studies (Keane and Wolpin, 2001). On the other hand, work could subtract time from study, and unless youth manage to better organize their time, it may harm academic outcomes, and reduce general human capital acquired at school (Eckstein and Wolpin, 1999).

Empirical papers aiming to resolve this ambiguity face the challenge of addressing students' selection into employment - an issue that typically confounds the effects of working while in school. We provide the first estimates that use randomized lotteries to address the selection issue. We leverage a large-scale youth employment program offered by lottery in Uruguay. The program targets students aged 16 to 20

[1] We computed these statistics from OECD (2018). In the U.S. this share was 20% in 2016, and the average for Latin America was 16% in 2014 (CEPAL and OIT, 2017).

throughout the country, offering them a first formal work experience in the main state-owned companies (e.g., the government-owned electricity company, telecommunications company, national bank, etc.). Lottery winners receive an offer for a part-time job (between 20 and 30 hours a week) that lasts between 9 and 12 months and typically consists in a clerical position, in administration or operations, focused mainly on support tasks. Program participants are required to be enrolled at a high school or university at the moment of application and throughout the duration of the program. The features of the Uruguayan program also appear in other work-study programs, such as the Federal Student Work Experience Program in Canada, which offers part-time jobs in the government for full-time students during the academic year.

The Uruguayan case represents a unique opportunity to learn about the effects of working while in school. It has the features of a social experiment without suffering from common implementation issues (Rothstein and von Wachter, 2017).[2] First, offers to participate in the program are randomly allocated. Second, the program has been implemented by governmental agencies for the last six years and receives applications from a large sample of students (i.e., from more than one-third of all the students aged 16 to 20 in the country). Third, the sample of applicants to the program is representative of the student population, including both poor and non-poor households, which implies that participation bias is a less relevant issue in our case (Czibor et al., 2019).

We use rich administrative data that allow us to recover the main outcomes for all applicants, reducing concerns about attrition. The data cover the universe of lottery participants, including 122,195 lottery applications. We observe all applicants' monthly earnings and social transfers in social security data from 2011 to 2017, and their enrollment in the registers of public schools and universities. We complement the administrative data with a survey measuring school grades, time use and soft skills at the end of the program year.

During the year of the program, earnings and the employment rate of treated youth more than double with respect to the control group.[3] More importantly, we find a

[2]The program was not conceived as a social experiment, but it implemented lotteries to deal with a much larger number of applications than available vacancies. We started studying the program five years after its initial implementation.

[3]The main results are discussed in terms of treatment on the treated (ToT) effects and compared

significant and positive effect on yearly earnings and employment after the end of the program. Over the four years following the program, the post-program effect on earnings amounts to US$285. This represents 9% of the earnings of comparable youth in the control group. Post-program earnings effects are driven by both effects on employment at the extensive margin (3 percentage points over a control complier mean of 70%), and by wage effects conditional on employment. Monthly wages of program participants employed during the post-program years are US$26 higher - a 5% increase over the control complier mean. The positive effect on wages survives a bounding analysis that accounts for selection into employment, and suggests that working while in school increased youth productivity.

While treated youth acquire more work experience, they also acquire more education. During the program year, the program conditionality on enrollment leads to greater school retention by 12 percentage points. Post-program enrollment rates, when there is no longer any enrollment requirement, still remain higher in the treatment group. Over the two years following the program, the enrollment rate of treated youth is 2-3 percentage points higher than in the control group, where 56% of youth are enrolled. Consistent with previous work (e.g., Eckstein and Wolpin, 1999; Buscha et al., 2012), the persistent effects on enrollment suggest that working while in school does not crowd out future school investment, but instead provide some evidence for crowding in. We do not find evidence that this enrollment effect is differential across poor and non-poor households, which reduces support to the hypothesis that credit-constrained youth save the income shock due to program wages to finance extra years of education.[4] Instead, our survey data indicate that treated youth expect higher returns to secondary education, which might foster investment. Moreover, we do not find evidence that the extra education acquired in the treatment group is of lower quality. We estimate that returns to education are similar in treatment and control groups. Furthermore, our survey data show that grades obtained by participants during the program year are not lower than those in the control group. While program participants enrolled in school exhibit some reduction in class hours and study time outside school, these effects are not

to the control complier mean (i.e., the mean for youth who would have participated in the program if they had won the program lottery). Take-up of treatment, defined as completing a program job, was 70%. Intention-To-Treat effects, presented in the appendix, draw a qualitatively similar picture.

[4]This result is consistent with Keane and Wolpin (2001) who find in a structural model of working while in school that relaxing credit constraints does not significantly affect attendance decisions.

large enough to significantly affect school grades. Treated youth are able to work and study by mostly reducing time devoted to leisure and household chores.

We also find persistent post-program increases on the probability of working while enrolled in school and reductions in the share of youth not working or studying.[5] During the two years following the program, the share of working students among treated youth is 4 percentage points higher (12% of the control complier mean). Four years after the program, when almost all control youth have quit school, we find an important reduction of 5 percentage points in the share of youth not working or studying, over a mean of 20% of NEETs in the comparison group.

In summary, the program increases both youth work experience and education. To find out which channel contributes more to the earnings effect, we conduct a decomposition exercise. We leverage the panel dimension of our data, and estimate the returns to both education and work experience using Mincerian earnings regressions with individual fixed effects. We find that the increase in education of treated youth accounts for 21% of the earnings effect, while the increase in work experience accounts for 50% of the post-program increase in earnings. The contribution of work experience is the result of quantity and price effects that move in opposite directions. On the one hand, the increase in work experience priced as in the control group, i.e., quantity effect, would imply an increase in earnings greater than the estimated treatment effect. On the other hand, the returns to experience are lower in the treatment group than in the control group, implying a negative price effect.

The lower returns to work experience are concentrated among the treated youth who do not accumulate additional experience after the program year. Consequently, we focus on the type of work experience acquired in program firms, and consider how youth leverage this experience to find high-wage jobs when their program jobs end. State-owned companies face stringent rules on hiring/firing for their regular jobs, and program firms hire less than 5% of treated youth over the four years after the program.[6] This implies that youth earnings during the post-program years depend on the type of human capital acquired on the program job;

[5]Reducing the number of youth in this group, which is close to the NEET (Not in Employment, Education or Training) category, is one of the objectives of many governments around the world, including the Uruguayan one.

[6]The program rules prevent program firms from keeping participants on the same job after the end of the program.

in particular, on whether it is sector-specific or rather general. We do not find evidence that earnings effects are concentrated in the sectors of the program firms. This suggests that the human capital acquired in program jobs is rather general and valued by the market. Alternatively, the lower returns to experience in the treatment group could be due to a slower rate of general human capital accumulation in program jobs. Our survey data indicate that even though youth are more likely to read, write and use computers than control youth, they have less frequent meetings with colleagues, which suggests fewer opportunities to enhance their soft skills. In fact, we find that personality traits, grit and work attitudes measures do not differ between treated and control youth at the end of the program year.[7]

Finally, we provide evidence on the program effects on youth welfare (Heckman, 2010). Program jobs crowd out both time dedicated to household chores and leisure. We use answers to reservation wage questions in our survey to estimate the value of leisure to our population. This allows us to subtract from the program effects on earnings the change in utility due to reduced leisure time. Consistent with the fact that youth choose to participate in the program, we estimate that the program increases youth earnings adjusted for leisure loss by $836.4 during the program year and by $242.1 during every of the four post-program years in our data. Interestingly, the reduction in the time program youth dedicate to household chores points to program effects on the within-household division of roles.[8]

Our paper contributes to the literature estimating the effects of working while in school by providing the first estimates using randomized lotteries to deal with selection into employment. The previous literature using non-experimental methods has not reached a consensus about the magnitude of the returns to working while in school on labor market outcomes. The three following papers provide evidence using U.S. data from the National Labor Survey on Youth (NLSY). First, Ruhm (1997) finds significant returns to working part-time while in school up to nine

[7] There is evidence that work experience can change soft skills. For example, Gottschalk (2005) finds that an exogenous increase in work experience generates more positive views of work (i.e., improved internal locus of control) among welfare recipients. Similarly, Adhvaryu et al. (2018) find that on-the-job soft-skills training can improve communication and extraversion.

[8] Both treated women and men cut the time allocated to household chores by almost 50% with respect to the control group, but women dedicate to household chores twice as much time as men do. This is reflected in smaller reductions in study time and leisure for treated women than for treated men, although the differences are not statistically significant. Overall, we do not find evidence of statistically significant treatment effect heterogeneity by gender either on labor outcomes or on enrollment (see Appendix Table A18).

years after high-school graduation for both men and women. Second, Hotz et al. (2002) take into account dynamic selection into employment for a sample restricted to men, and find returns that are not statistically significant. Third, Ashworth et al. (2017) use a new dynamic selection model that incorporates two unobserved random factors and estimate significant long-run in-school work returns among men. Over a shorter horizon after graduation, our results point to high earning returns to completing a part-time job in state-owned companies, for men and women.

In contrast, the literature seems to have reached a consensus pointing to some negative, but limited effects of working while in school on educational outcomes (Eckstein and Wolpin, 1999; Buscha et al., 2012; Stinebrickner and Stinebrickner, 2003). Our estimates also point to some negative effects on study time of enrolled students during the program year, which are not large enough to affect grades. After the program year, we further find persistent positive effects on enrollment, confirming a limited role for negative effects of working while in school on educational outcomes.

The program we study has two characteristics that can make our estimates differ from the previous non-experimental literature on working while in school, on top of the random allocation of job offers. First, program jobs are provided by large state-owned companies, they are part-time and well paid. We show below that program participants are assigned to more sophisticated tasks (e.g., using computers, writing reports) than youth employed outside the program, which implies a higher scope for learning and human capital accumulation (Lagakos et al., 2018). Second, the program conditionality on school enrollment limits potentially negative extensive margin effects, though there could still be intensive margin effects on study effort.

Our study also complements the recent experimental literature that finds limited effects of summer jobs on labor market outcomes (Gelber et al., 2016; Davis and Heller, 2017). Summer employment accounts for only a fraction of youth yearly employment. For example, it represents only 31% of yearly employment of teenagers enrolled in school in the U.S. and 28% in Uruguay. We find more positive effects for jobs that last almost a year, involve more sophisticated tasks, and are concurrent with schooling.[9]

[9] We obtained the share of summer employment for teenagers in the U.S. from 2017 CPS data, and that in Uruguay using the administrative data for the control group in our sample. See Appendix

Finally, our paper contributes to the literature evaluating active labor market policies (ALMP) using social experiments and randomized control trials. Our paper is the first to our knowledge providing evidence based on randomized lotteries on a work-study program. The literature has mainly focused on the evaluation of labor market policies that provide vocational training, wage subsidies or job search assistance, while work-study programs are not commonly discussed (for recent surveys or meta-analyses see Card et al., 2017; Escudero et al., 2017; McKenzie, 2017; Behaghel et al., 2018). There is also little causal evidence on apprenticeship programs, which are a close substitute (Crepon and Premand, 2018; Adda and Dustmann, 2019).[10] We show that a program combining both work and regular study experience yields earnings effects greater than the worldwide average effects of vocational training reported in McKenzie (2017). While youth employment programs typically target dropouts, and/or disadvantaged youth, our evidence suggests that the earnings effects of working while in school are not concentrated among disadvantaged youth and the program benefits also non-poor youth.[11]

The paper proceeds as follows. Section 2 describes the Uruguayan work-study program. Section 3 discusses theoretical insights of the main expected effects of the program. Section 4 presents the data and the econometric model. Section 5 delivers causal estimates of the program effects on core labor market and education outcomes. Section 6 presents a decomposition of the earnings effect to gauge the quantitative importance of the work experience and education channels. Section 7 presents estimates of the program effects on youth welfare. Finally, Section 8 concludes.

2 The Uruguayan work-study program

Since 2012, the work-study program "*Yo Estudio y Trabajo*" (referred to YET hereafter) provides youth aged 16 to 20 who live in Uruguay with a first formal work

B for details on the computation.

[10]As work-study programs, apprenticeship programs combine both school attendance and within-firm work. However, they differ to the extent that they are vocational and the school curriculum and occupation are linked together.

[11]For examples of field experiments evaluating youth employment programs, see among others Attanasio et al. (2011); Card et al. (2011); Groh et al. (2016); Alfonsi et al. (2020). For a review of social experiments in the U.S. labor market, see Rothstein and von Wachter (2017).

experience in state-owned companies for up to one year. The program is a cross-institutional initiative coordinated by the Ministry of Labor and Social Security of Uruguay, and offered each year in most main cities.[12]

All youth aged 16 to 20 who reside in Uruguay are eligible to apply for YET as long as they satisfy two key conditions: 1) they are enrolled in an educational institution, and 2) they have not worked formally for more than 90 consecutive days.[13] Using the microdata including all observations in the 2011 Population Census, we estimate an application rate of 34.6 percent for the 2012 edition of the program. The characteristics of the eligible population and of the program applicants are overall similar, in particular in terms of household socio-economic vulnerability (see Appendix C for details).

Assignment to the program is done by lottery at the locality level.[14] The number of program participants in each locality depends on the number of jobs offered by the public firms that partner with the program in that locality. Lottery candidates are randomly ranked within locality. Sequential rounds of program offers are made until all local program slots are filled. From the third edition of the program in 2014, quotas were introduced in the largest localities to guarantee participation of minority youth from African origin (8 percent), with disabilities (4 percent) and transgender youth (2 percent). From the fourth edition in 2015, a new quota was introduced for youth from vulnerable households (11 percent), i.e., poor households receiving a conditional cash transfer.

Program participants must visit a government center to present the required documentation.[15] They have to attend a two-day orientation workshop provided by the National Institute of Employment and Professional Training and are assigned

[12]According to the 2011 Census, Uruguay has a population of 3.3 million divided in 19 departments and 298 localities, with around 60 localities with more than 5,000 inhabitants classified as cities. The program offers positions in 77 localities, which include almost all the main cities in Uruguay.

[13]Applications are completed online or using a computer at an employment center and, if selected, applicants must show proof of enrollment from an educational institution certifying a minimum level of attendance (240 hours), an official identification card and the electoral card if older than eighteen. Upon selection, the no formal employment requirement is cross-validated with social security data and proof of enrollment is required every three months.

[14]Candidates select the locality in which they want to participate, which is supposed to be that in which they live and/or study. However, nothing in the application system restricts this choice or prevents candidates from applying to more than one locality.

[15]At that stage, those aged 16-17 receive information about how to obtain work permits.

a supervisor who follows their progress in the program. Participants staying at the job for the full contract period are awarded a work certificate.

Importantly, firms cannot choose the youth they want to hire, and candidates cannot select the firm in which they want to work. The program administration performs the matching of participants to available job positions. While doing so, the program administrators prioritize the compatibility between schooling and work hours over the relevance of the job tasks with respect to the studies specialization. This process implies that there is very little job-candidate matching in terms of skills.[16],[17]

The job offered within the program is part-time, with a total of 20 to 30 hours per week, and overtime is not allowed. Participants are supposed to work during the normal operating hours of the firm, with the condition that working hours do not prevent them from attending school. The contract is temporary (9 to 12 months), and cannot be extended. Remuneration is fixed and amounts to $446 per month for a 30-hour-per-week job in 2016 (around $3.7 per hour).[18]

Firms must pay youth wages out of their own budget. We visited several program firms to gather qualitative information regarding why they participate in the program. Informal conversations with employers suggest two main reasons why they offer jobs within the program. First, the program allows them to offer part-time one-year contracts that are more flexible than regular in-house labor contracts, which are strictly regulated in the public sector. Second, program participation enhances the firm's reputation with the central administration.

All program firms belong to the public sector. The majority of these are large state-owned companies and only a few positions are offered in the public administration.[19] For example, the four main program employers of the fifth edition

[16]Informal conversations with the program administrators indicated that distance from home to the firm, and hours at school were the two main variables considered in the matching process.

[17]Note that the absence of matching probably lowers the returns to work experience in program jobs, so that our estimated program effects may be a lower bound for the returns of working while in school.

[18]More precisely, the remuneration is fixed at four times the minimum tax unit used in Uruguay, which means 13,360 pesos per month for a 30-hour-per-week job in January 2016. We use the nominal exchange rate of 0.0334 pesos per U.S. dollar in January 2016 throughout the paper. Pregnant women and mothers of kids below the age of 4, who represent around 4% of the lottery applicants, are entitled to wages that are 50% higher. The program wage compares favorably to the national minimum wage fixed at 372 USD per month for a full-time job.

[19]Thus the program would fit under the category of "public sector employment" programs (Heck-

are: the state-owned commercial bank of Uruguay (hiring 22% of program participants), the state-owned electricity company (19%), the state-owned telephone company (9%), and the state-owned oil and gas company (6%). Among smaller employers, we find public administration offices such as the ministry of education or social security administration (see Appendix D for more details on the program firms of the fifth edition). Table 1 reports, for each edition of the program, the main sector of the firm recorded in the administrative data. Most program firms are in the civil sector, which comprises all state-owned companies (except banks) and the public administration (between 64% and 81% of jobs). The second largest sector is banking, which includes the state-owned banks (between 16% and 31% of jobs). Finally, a few jobs are offered by public laboratories (3%-5% of jobs) classified as part of the industry and trade sector, which is the sector involving the majority of private firms in the country.

The program establishes that work activities must be in administration or operations, and should be focused mainly on support tasks. Indeed, 93% of participants in the fifth program edition report working as clerks during the program (see Appendix D for more details about tasks performed on program jobs). Furthermore, the program documentation explicitly states that the early work experience should help participants develop soft skills valued in the labor market such as commitment, teamwork, adaptability, flexibility, reliability, a strong work ethic, and communication skills. The direct supervisor assigned by the program to each participant should evaluate these non-cognitive skills twice: during the program and at the end of it.[20]

Table 1 reports the number of applications, applicants and positions for each edition of the program. There are around 46,000 applicants in the first program edition in 2012. This represents a large fraction -around one-third- of eligible youth in the population. There is a downward trend in applications over time, probably due to the program spending more resources in advertising in the first two editions, and due to longer lottery registration time windows in the first two editions. However, we do not see any notable trend in applicants' characteristics over time (see Appendix C). Compared to the tens of thousands of applicants, there are less than a thousand program jobs offered every year. Consequently, the share of participants

man et al., 1999).

[20] We did not get access to these evaluations.

offered a job is between 2 to 3 percent, implying a low probability of obtaining one. Moreover, the program is small relative to the relevant labor markets, which reduces the possibility of important spillovers from treated to control youth.

As participants may apply to more than one locality in a given edition, the number of applications is slightly larger than the number of applicants: 4 percent of the applicants apply to more than one locality in a given year. Multiple applications across years are more common: 27 percent of applicants apply to more than one edition; most applied to two editions. Of the 30 percent of lottery winners who do not complete a program job, 80 percent never start one. Those who start a program job are not allowed to participate in a later edition, while those who do not start one are allowed to apply again without receiving any priority. We explain how we handle repeated applications when we discuss the empirical specification.

3 Theoretical Channels

The work-study program YET offers part-time temporary jobs in public firms to adolescents who are enrolled in school. We expect that this early work experience will increase the human capital of participants as they acquire hard skills in the workplace. Participants might also acquire soft skills while in the firm, such as work attitudes, self-esteem, communication skills, conflict resolution, time management, teamwork, etc. (Heckman et al., 2006; Groh et al., 2016; Acevedo et al., 2017; Adhvaryu et al., 2018). The corresponding increase in human capital will probably cause higher employment rates and wages after the program ends - to the extent that the skills acquired in the program firms are transferable to other firms in the labor market.

In addition to the *human capital* channel just described above, we expect early work experience to have a signaling role. When future potential employers receive job applications from program participants, they may infer from their early work experience that participants are motivated or trustworthy and have skills above the hiring bar. This *signaling* channel will further contribute to positive employment and wages, unless program participation stigmatizes youth.[21] We do not expect

[21] Even if employers might be aware that participants obtained the early work experience by chance (through a lottery), and thus would not interpret being hired in a program job as informa-

a significant role for a *screening* channel whereby program firms acquire private information on youth to decide whether to hire them after the program, as it is against the YET guidelines.

A third channel - the *learning* channel - is related to the imperfect information youth might have about their on-the-job abilities (see Arcidiacono et al., 2016). Early work experience enables them to learn whether they are good at and/or like the type of clerical jobs program firms offer. A priori, the effect of ability learning on employment in the short-term is ambiguous and it depends on the expectations of participants before they enter the program. But later on, ability learning probably allows youth to better sort across occupations, and increase their earnings through improved matching with jobs.

While the channels mentioned above mainly affect employment and wages, YET may also trigger crowding-out effects on schooling investment. As students spend working hours in firms, they may invest less time and effort in studying. This could reduce the general cognitive skill level of participants. However, as participants lose their jobs if they drop out of school, crowding-out effects should be limited, at least at the extensive margin, during the program year. The enrollment condition of the program may even trigger some crowding-in effects during the program year. The program effect on future earnings may also transit through this education channel.

On top of these channels, the program entails a positive shock to the income of participants. Program earnings could then help credit-constrained youth to finance their education expenses, or spend more time searching for a good job. We expect these effects (i.e., increase in enrollment or decrease in employment rates right after the program) to be stronger for youth living in poor households.

In our main analysis, we estimate the resulting effects of these different channels on average earnings, employment, wages, and educational attainment. In Section 6, we present a decomposition exercise and heterogeneity analysis that suggest which channels are stronger.

tive about skills that are unobserved in the CV, being able to complete the year in the program jobs can still be a meaningful signal. Moreover, potential employers can ask for reference letters from program employers, which would further reduce information asymmetry (Abebe et al., 2020). Finally, successful participants can show their work certificate awarded at the end of the program.

4 Data and econometric model

4.1 Data

We use four sources of data: YET-program administrative data, social security and educational records for all applicants, and a survey with a representative sample of applicants to the 2016 edition. All data can be matched at the youth level. First, we have data from the online application form that youth must complete in order to participate in YET lotteries. These data include basic demographic information (age, gender, locality), and educational level. From YET administrative records, we also have information on the lottery draws, subsequent offers, and program participation. This allows us to compute the overall number of positions offered, number of positions accepted and completed (see Table 1 above), and dummies for each of the quotas considered in the program.

The social security data record monthly labor earnings of each applicant and whether the applicants' households receive social transfers. Educational records cover enrollment in public education institutions (secondary, tertiary, universities and out-of-school programs) at a yearly frequency.[22] The social security and educational records are available from 2011 to 2017. Consequently, we restrict our main sample of analysis to the first three program editions (2012, 2013 and 2014), so that we can observe outcomes for at least 2 years after the program.

Table 2 describes our sample of applicants and checks that treatment and control groups are balanced. Panel A presents data from the application form: gender, age, and whether participants applied to the program in Montevideo, the capital city. Panels B and C report data from the administrative records measured before application: education, subsidies from social programs, and labor outcomes. We present data at the application level and control for lottery design when comparing controls and youth receiving a program offer. Overall, the differences between the two groups are negligible, confirming that lotteries were appropriately conducted.

Since 2008, general secondary education is compulsory for youth aged 12-17 years old. It encompasses six years of instruction, divided into two three-year cycles. The second cycle is aimed at youth aged 15-17 years old and has a course load

[22] Sources are the National Administration of Public Education and the State University.

from 34 to 36 weekly hours.[23] There are two possible tracks: the academic track, which is in general regarded as more prestigious, and the technical track. Among lottery applicants, around 71 percent are enrolled in public secondary education: 49 percent in academic schools and 22 percent in technical schools. 16 percent of applicants attend the State University, which is free of tuition fees. This is a lower bound for enrollment at university, as the data only record whether the student has taken at least two exams or started a new track in a given year.[24] Finally, 3 percent of applicants are enrolled in tertiary non-university programs or in official out-of-school programs. The residual 10 percent of applicants are not enrolled in public institutions during the year before the program. They are most likely enrolled in private institutions, as in the application form all applicants report being enrolled at an educational institution.[25] One youth in four lives in a household that receives a conditional cash transfer, and is thus considered to live in a vulnerable household. Households receiving a food card as well are considered highly vulnerable.[26] One youth in ten belongs to this highly vulnerable household category.

Social security data indicate that 15 percent of applicants worked formally for at least one month in the 12 months before applying to the program, with average yearly earnings of $163.[27] On average, applicants worked less than one month the year before the program, as expected, since not having worked formally for more than 90 consecutive days is a requirement to enroll in the program.

To complement the administrative data, we surveyed a representative sample of 1,616 students who applied to the lottery in the Fall 2016 (fifth program edition). The survey was in the field in November and December 2017, just before the end of most program jobs. The survey has two main objectives: describing the program experience (program jobs and time use), and measuring soft skills and school

[23]Gross enrollment rates in 2015 were 96% for the first cycle and 82% for the second cycle, while completion rates were below 50%, with very high repetition rates (Source: "Anuarios Estadísticos de Educación del Ministerio de Educación y Cultura y Departamento de Estadística").

[24]For the first edition, we do not have administrative data from universities for the year before the program; only in this case do we use data reported in the application form.

[25]A 10% share of private institutions enrollment is in line with data from the 2011 Census.

[26]Eligibility to social benefits is means-tested. A poverty index is used to select the 200,000 poorest households that receive a cash transfer, and among them, the 60,000 poorest households that receive a food card. The social food card is a prepaid card that can be used to purchase goods at a network of social shops around the country. The amount received by each household varies with number of children and total household income.

[27]Throughout the paper, we winsorize earnings for the top 1 percent and convert Uruguayan pesos to U.S. dollars using the January 2016 exchange rate of 0.033 dollars per peso.

grades around the end of the program. From the YET administrative data, we selected all applicants who received a program offer and a random subsample of unlucky applicants. The overall response rate of the survey is 79 percent. The response rate in the offer group is slightly higher (81 percent), although this differential attrition does not generate unbalances in baseline covariates between offer and control students (see Appendix Table D1).[28]

4.2 Econometric model

In the main analysis, we focus on Treatment effects on the Treated (ToT). We define treatment as completing a program job and receiving a work certificate. We define the variable *Offered* as ever-receiving a program job offer. To obtain the causal treatment effect, we leverage the lottery design and instrument the treatment dummy with the *Offered* variable.

Under this definition of treatment, the local average treatment effect is equal to the ToT because no youth can finish a program job and receive the work certificate if not offered the program (i.e., there are no always takers). This effect is identified under the exclusion restriction, which assumes that the only reason why youth who are offered the program see their outcomes affected is that they complete the program. In the appendix, we present estimates for different specifications that make the exclusion restriction weaker, and we obtain consistent results.[29] We also explore an alternative definition of treatment that allows us to estimate a parameter that is perhaps closer to the effect of working while in school, but relies on

[28]The difference between the response rate of the treatment and control group is 3.6 p.p., statistically significant at the 10 percent level. The regression of a dummy for attrition on treatment, baseline covariates and their interaction gives a p-value of 0.61 for the joint test that the coefficients of the interactions between treatment and covariates are jointly zero. Results available upon request.

[29]First, we present intention-to-treat estimates (ITT), which do not rely on the exclusion restriction (see Tables A7 and A10), and we find effects that are in the order of 70 percent of the ToT estimates, with the same pattern in terms of sign and statistical significance. This is in line with a first stage of 70 percent in our main estimates. Second, we redefine treatment as working at least one month in a program firm during the program year. We estimate a local average treatment effect since there could be always takers: indeed 1.3 percent of youth not offered the program, work in a program firm during the program year. The exclusion restriction now requires the absence of effects of the program job offer that are unrelated to having worked in a program firm. Among those who did not complete the job, 80 percent of youth have not worked any month in program firms, and thus there is important overlap in the treatment definition with respect to our main specification. Results presented in Table A13 are very similar to our main estimates.

stronger assumptions. Again results are consistent with those we find in our main specification.[30]

The choice of the ever-offered variable as an instrument is a reasonable estimation strategy in the context of randomized waiting lists when the offer rate is small (de Chaisemartin and Behaghel, 2018).[31] Appendix Table A1 reports the first stage regression of the *Treated* dummy on the *Offered* variable by edition. Overall, more than 70% of youth receiving a program offer complete their program jobs. This strong first stage is homogeneous across editions.[32]

We analyze data at the application level. To maximize statistical power, all applications, including those by the same applicant in different localities and different editions, are included.[33] Given the small offer rate (around 2-3%), this choice hardly affects the estimates.[34]

We consider the following specification at the application level a of individual i in edition e:

$$Y_{i(a),t,e} = \alpha_1 + \gamma_t Treated_{i(a),e} + Locality \times EditionFE_a + QuotaFE_a + \#App_{i(a),e} + \rho_t X_{i(a),0,e} + \epsilon_{1,i(a),t,e} \quad (1)$$

[30]In particular, we define treatment as working (in any firm) and studying (being enrolled in school) during the program year. In this case, we estimate a local average treatment effect of working while in school (i.e., the effect for those who only work while in school because they are offered a program job). The first stage is 42% and treatment effects are larger than those obtained under our main specification, but exhibit the same pattern in term of signs and statistical significance (see Appendix Table A14). The exclusion restriction requires that there are no effects of being offered a program job that are due to participation in the program and are not mediated through working and studying, such as getting access to better student jobs.

[31]In Appendix Table A15, we verify that alternative estimators, namely the double re-weighted ever offer estimator of de Chaisemartin and Behaghel (2018), yield robust results.

[32]Appendix Table A2 shows that the effects of receiving a program job offer in Year 0 on the probability of YET participation in future years (i.e., Years 1-4) are negligible. They are negative as youth who complete a program job are not allowed to participate in future editions. Thus, we do not expect the effects on earnings to be mediated through the impact of YET on future YET participation.

[33]We deal with multiple applications in the following way. When a student receives an offer following application a in locality 1 in edition year e, we first set $Offered_{a,e} = 1$. Then, we also set $Offered_{b,e} = 1$ for every application b of the same individual in the same edition-year but in a different locality. In practice only 4 percent of youth applied to more than one locality. All other applications in different edition-years e' are by construction such that $Offered_{a',e'} = 0$. The variable *Treated* is adjusted following the same procedure.

[34]As we show in the Appendix, our results are robust to restricting the set of applications in the estimation sample to one application per individual, or to the first edition to which a given youth applied.

$$Treated_{i(a),e} = \alpha_2 + \delta Offered_{i(a),e} + Locality \times EditionFE_a + QuotaFE_a + \#App_{i(a),e} + \beta X_{i(a),0,e} + \epsilon_{2,i(a),e} \quad (2)$$

where $Y_{i(a),t,e}$ is the outcome of individual i, t periods after the application date in edition e. $Treated_{i(a),e}$ indicates whether individual i completed a program job offered in edition e. $Offered_{i(a),e}$ indicates whether individual i received a program job offer for any application submitted in a given edition. To control for lottery design, we include $Locality \times Edition$ fixed effects and quota fixed effects. This takes care of variation in the probability of receiving a job offer across lotteries depending on the local number of program jobs offered and on the potential quotas. To further control for individual variation in the offer probability (and thus in the treatment probability), we include the number of applications of individual i in a given edition: $\#App_{i,e}$. To increase precision, we include a vector of covariates $X_{i(a),0,e}$ measured at application. It comprises gender, age, whether the youth comes from a household that receives a cash transfer, earnings and level of education in the year before applying to the program. Standard errors are clustered at the individual i level. Our parameter of interest is γ_t, which we estimate using two-stage least squares as explained above; it captures the ToT effect t periods after application.

5 Main results

In this Section, we present the program effects on labor market outcomes and educational attainment. When we use the administrative data on labor market outcomes and on education enrollment, we pool the first three editions of the program. Survey results refer to the fifth edition.

Some of the tables include a significant number of hypothesis tests. We conduct adjustments for multiple testing within each table (considered as a family of outcomes) and we find in general robust results. We note in our discussion the few cases where the inference is not robust to the adjustment.[35]

[35]We obtain family-wise adjusted p-values using the implementation by Jones et al. (2018) of the free step-down procedure of Westfall and Young (1993), which allows to re-sample over entire clusters instead of individual observations. Detailed results are available upon request. Our results are also robust to adjusting for our main four families of outcomes: post-program averages of earnings, employment, wages and enrollment at any level.

5.1 Effects on labor market outcomes

Graphical overview Figure 1 reports the main program effects on quarterly labor earnings. The dashed line shows the time-evolution of average earnings of the treatment group. By construction, these individuals are compliers since there are no always takers in the sample (no youth can participate in the program if not offered a job). We compute the average earnings of the corresponding compliers in the control group.[36] The solid line in Figure 1 plots its time-evolution. Before the application date, earnings of both control and treatment groups are close to zero, as required by the eligibility condition of the program. After application, the control mean steadily increases, as aging youth gradually enter the labor market, and reaches around $1,400 per quarter (Y-axis on the right side of the graph), 4 years after the program ends. By contrast, the average earnings of treated individuals rise sharply just after application, and remain on a plateau of about $700 per quarter over the year of the program. Around one year after the start of the program,[37] treated earnings decrease sharply and converge back to the control earnings level. This corresponds to the end of the program, when the temporary jobs within the program must end according to program rules. After this convergence, treated earnings follow an upward trend, but at a steeper rate than control earnings. One year after the program ends, treatment effects are already statistically significant. The dots in Figure 1 report treatment effect estimates $\hat{\gamma}_t$ from Equation (1), with their confidence intervals (vertical lines). After the program ends, treatment effects steadily increase, and reach around $400 per quarter (Y-axis on the left side of the graph) by the end of the period covered by our data.[38]

Earnings Effects Table 3 summarizes the treatment effects on yearly earnings (in

[36]Control compliers are youth who did not receive any offer and were not allowed to work in a program job, but would have worked if they had received an offer. The control complier mean is obtained as the difference between the mean for those who complete the program and the ToT effect.

[37]There is a delay of a few months between the application deadline and the start of program jobs, when lotteries are drawn, offers are rejected and/or accepted, and organizational workshops are set. In addition, the start of program jobs is staggered. Consequently, we define as program start the date when some first treated individuals start their program jobs, and we define as program end, 12 months after the program start. This duration gives enough time for the program jobs that start last to lapse.

[38]Appendix Figure A1 replicates the analysis of Figure 1 restricting the sample to the first application cohort (2012 edition), for which we have four years of post-program data. This illustrates that the timing of the program effects in Figure 1 is not due to changes over time in the sample composition.

Column 1), on employment (in Columns 2 and 3) and on monthly wages (in Column 4). During the program year, treated youth earn \$2,001 more than control youth, whose yearly earnings are \$972 (Column 1, Row 1). Row 2 reports the effects during the year after the end of the program (labelled Year 1), Row 3 two years after (labelled Year 2), etc. Treatment effects on yearly earnings are positive at all horizons, and statistically significant from Year 3 (they are not statistically significant in the very short run, during the year after the program, and significant at the 10 percent level in Year 2).[39] They increase over time from \$52 up to \$1,113 in the fourth year after the program, corresponding to an increase in yearly earnings from 2.5% to 22%. The effect on average yearly earnings over the four post-program years amounts to \$285 - a 9% increase over the control complier mean.[40]

Informal Earnings By definition, this is an effect on earnings in the formal sector. Data from the 2013 Continuous Household Survey in Uruguay (ECH) show that 16-20 year-old youth earn around \$200 per year in the informal sector. We use this estimate to compute a conservative lower bound on the program effect on total earnings. Assuming that formal earnings induced by the program completely crowd out informal earnings, we still find a positive effect on total earnings of around \$85.

Employment Effects Earnings effects are partly driven by employment effects at the extensive margin, shown in Columns (2) and (3). Column (2) reports treatment effects on the yearly number of months with positive earnings. During the program year, treated youth work 7 months more than control youth, who have on average less than 3 months with positive earnings. Treatment effects in Year 1 and 2 on months of work per year are small and not statistically significant; they become positive from Year 3 and statistically significant in Year 4. During the fourth year following the program, treated youth work half a month (8%) more than control

[39]The Appendix presents a series of robustness checks. Results are robust to not including controls $X_{i(a),0,e}$ in the regression (Table A3), clustering standard errors at the locality level (Table A4), restricting the sample to one application per individual (Table A5), not winsorizing earnings (Table A6) or computing ITT effects (Table A7). The main relevant change is that the coefficient in Year 2 becomes significant at the 5 percent level in several specifications, and if anything, estimates are a bit larger.

[40]To adjust for multiple hypotheses, we consider that we are testing for 20 hypotheses. We do not include the post-program averages since they are linear combinations of other outcomes. Out of 20 coefficients, the main changes are for those on earnings (year 2) and months with positive earnings (year 4), which are no longer statistically significant at the 10 percent level. If we focus on the pooled regressions for the four post-program averages, results are robust to the adjustment.

youth. Column (3) reports the treatment effect on having at least one month of the year with positive earnings. We find slightly more positive and statistically significant effects on this measure of employment. Although positive, employment effects cannot fully account for the yearly earnings effects.

Wage Effects Column (4) of Table 3 reports treatment effects on monthly wages. The estimation sample is restricted to youth with at least one month of positive earnings during the year. We address the issue of selection into employment further below. Monthly wages in program jobs are lower than the wages of employed youth in the control group by $25 (8%). The survey data, where we observe hours worked by the end of the program year, show that the effect on hourly wages is positive and statistically significant (see Appendix Table D5). This is in line with treated youth being more likely to work in part-time jobs than employed youth in the control group during the program year. The monthly wage effects become positive from Year 1 after the program, and statistically significant from Year 2. In Year 2, the monthly wages of employed youth in the treatment group are $26 higher, corresponding to a 5% increase over the control mean. Treatment effects increase further over time, up to $72 in Year 4 - 11% of the control complier mean. The positive effect on wages suggests that the program increased youth productivity.

Bound analysis To tackle the issue of differential selection into employment by treatment status, we present Lee bounds for the ITT effect on wages. Table 4 first reports the ITT effects on wages of employed youth. We obtain statistically significant positive effects from Year 2 on, as in the ToT analysis in Table 3. The ITT effect on wages of employed youth is the result of a causal wage effect and of a composition effect that selects some youth into employment when offered the program. We cannot observe the wages that youth induced to work because of the program would have if they did not participate in the program, and we need extra assumptions to identify the causal wage effect. We follow Lee (2009) and obtain bounds for the average effect on wages for the *always-employed* (i.e., individuals who would be employed regardless of the offer status). We compute lower (upper) bounds by trimming, from the sample of employed youth offered a job, those youth with the p% higher (lower) wages, where p is 100 times the ratio of the ITT effect on employment over the employment rate of the offered group.

Table 4 reports that the lower bound of the causal wage effect is significantly positive in Years 2 and 3. In Year 4, the lower bound is not different from 0, while the upper bound is as high as $80. Confidence intervals for these bounds are constructed following the procedure described in Imbens and Manski (2004). Lee bounds are obtained under an individual-level weak monotonicity assumption, which in our case requires that the probability of being employed after the program would be higher in the case of being offered the program job than in the case of not being offered the program job. The fact that our ITT estimates on employment are positive at all horizons provides supporting evidence for the plausibility of this assumption.[41] Several recent papers consider an additional assumption of weak monotonicity of potential outcomes, which tightens the bounds (Attanasio et al., 2011; Blanco et al., 2013; Alfonsi et al., 2020). If we assume that the average potential wages in case of being offered the job are larger for the *always-employed* than for the *never-employed*, then we obtain a new lower bound for the causal wage effect equal to the ITT on wages, while the upper bound is still the same as before (Blanco et al., 2013). Under this additional assumption, even for Year 4 the confidence interval for the bounds excludes zero.

Overall, the bound analysis shows that the employment effect at the extensive margin is unlikely to induce selection effects large enough to undo the positive effects found on wages of employed youth. We can thus conclude that the program leads to positive effects on wages, our best proxy for productivity. The magnitude amounts to around 4% (when we divide the lower bound estimate in Year 3 by the control complier mean in Table 3). There are several mechanisms that could trigger such a productivity effect. We explore them in Section 6.

Comparison with previous findings We estimate average effects on earnings (9%) and wages (5%) equal to half of what Ruhm (1997) obtains for 20-hour in-school work in the U.S. The magnitude of our wage effect estimates is comparable to Ashworth et al. (2017), and greater than Hotz et al. (2002). Our estimates are thus in the ballpark of previous U.S. estimates of in-school work effects on wages of youth in their 20s (even late 20s). Compared with this previous literature, we study effects on wages observed when youth are younger (around 20 years old).

[41]As Lee (2009) points out, one can test whether the distribution of baseline covariates is still balanced in the selected sample for periods when there is no effect of treatment on employment. We replicate our balance table for the employed sample at Year 3, when we do not find any effect of the program on employment, and we find a p-value of the joint test of significance equal to 0.69. If we do the same test for Year 4, we do see significant differences between the selected treatment and control samples. This provides additional evidence for the monotonicity assumption.

One key question is whether our effects estimated on younger youth would persist later in their life-cycle. Previous findings indicate a rather positive answer. Ruhm (1997) finds that returns to in-school work are similar whatever the age (between 25 and 29 years old) when wages are observed. In Section 6, we discuss further the expected longer-horizon effect in our context. Differently from (Hotz et al., 2002; Ashworth et al., 2017), who restrict their sample to males, our sample comprises both men and women. This allows us to test for heterogeneous returns of in-school work by gender. In Appendix Table A18, we show that our estimated returns are not statistically different by gender.

Last, as clarified above, we study the effect of jobs that are well paid, in state-owned companies, involving tasks that have a larger scope for learning and human capital accumulation than those studied in the U.S. literature. To explore the importance of job characteristics, we leverage the data on program firms industries. In Appendix Table A12, further commented in Section 6, we provide some evidence that whether the program job is in banking or the civil sector does not make a large difference on post-program earnings. However, our data do not allow us to study heterogeneity of treatment effects by finer types of job offered, which is an important topic for future research.

5.2 Effects on educational outcomes

Enrollment Effects Table 5 reports the treatment effects on enrollment in educational institutions at various horizons. In Column (1), we pool together all educational institutions, while we consider each educational level separately in Columns (2) to (5). At the end of the program year, overall enrollment of treated youth increases by 12 percentage points from a control average of 76%. This is consistent with the enrollment requirement of the program. The direct effect of the program is to reduce the share of high school dropouts. During the three years after the end of the program, the positive effect on enrollment persists, but it is small and only statistically significant in Year 2. In Year 4, the effect is negative, and not statistically significant. This results in an average effect over all the post-program years of 2 percentage points, which is statistically significant at the 5% level.[42] Overall, the

[42]We present robustness checks in the Appendix. Table A8 presents results without including controls, Table A9 restricting the sample to one application per participant, and Table A10 shows the ITT effects. Overall results are quite robust. In the case when we keep only one application per

effect is driven by enrollment in secondary education (see Column 2).[43]

Schooling quality Our survey data allow us to measure more precisely investment in schooling and school grades during the program year. We do not find evidence that the quality of education is lower for program participants. Table 6 first confirms with survey data for participants to the 5th program edition that the program increases retention in school. Column (1) reports that the enrollment of treated youth in high school is 10 p.p. higher.[44] Moreover, there is no effect on truancy, since we do not observe effects on missing school in the last school week (Column 2). However, we do observe some negative effects at the intensive margin. Column (3) shows a reduction in weekly class hours by almost 2 hours, which represents a 7 percent decrease with respect to the control complier mean. This is probably associated with a change in regular class schedule for the treatment group. Additionally, Column (4) shows a 2-hour reduction in weekly study time outside school, which is statistically significant and represents 39 percent of the control complier mean.[45] The crowding-in and the crowding-out effects actually offset one another, so that, on average, time dedicated to school investment for the whole sample is left unaffected by the program (see results on time use in Table 12 below). Furthermore, this reduction in study time does not translate to lower grades. Column (5) shows that the program has no effect on the grade point average of high school students; the coefficient is small and not statistically significant. The confidence interval excludes effects that are larger than 6 percent of the control mean.[46] We find suggestive evidence that the reported GPA measure is informative

applicant, we see that coefficients for any enrollment in Years 1-3 increase and become statistically significant.

[43]To adjust for multiple hypotheses, we consider that we are testing for 20 hypotheses. We do not include the post-program averages or the enrollment at any level since they are linear combinations of other outcomes. The coefficients on secondary education for years 1-3 and for out-of-school programs for year 1 that were significant at the 10 percent level before the adjustment, lose statistical significance after the correction.

[44]The treatment effect on secondary education enrollment estimated in survey data is similar to the treatment effect found for Year 0 in administrative data in Table 5 (10 vs 12 p.p). This mitigates the concern that measurement error in the survey, potentially related to the enrollment conditionality of the program, biases our analysis of educational outcomes.

[45]The p-value for the coefficient on class hours is 0.09 once we correct for the 4 multiple hypotheses in this table (excluding enrollment), while that on study time becomes 0.06.

[46]Grades range from 1 to 12 points. We see small positive treatment effects on both having a low current GPA (between 1 and 5), and a high one (between 9 and 12), and a small negative effect on having a GPA between 6 and 8, but none of these are statistically significant. We also asked university students to report their average performance (below average, above average or average),

and the reduction in study time and class hours in the treatment group is consistent with small effects on grades.[47] Overall, our evidence suggest that the increase in enrollment does not come at the expense of schooling quality or achievement.[48]

Persistent Enrollment Effect? While the effects on enrollment during the program year are probably driven by the program requirement and its enforcement, the enrollment effects over the post-program years are unconstrained behavioral responses. This suggests that conditionality in a given period generates *compliance* even after the conditionality is removed. One potential explanation for the persistent enrollment effect relates to the income shock embedded in the program. Under this explanation, the income shock due to program wages could be saved by credit-constrained youth to finance additional education after the program. We test for this explanation by comparing the treatment effect for poor (more likely to be credit-constrained) vs. non-poor households. More precisely, among the poor, we distinguish between youth in vulnerable households who receive social transfers and youth in highly vulnerable household who are also given a food card. Table 7 reports no statistically significant heterogeneity in treatment effects across vulnerability groups on enrollment or earnings (although interaction coefficients are imprecisely estimated). This does not support a strong income effect of the program.[49] An alternative explanation for the persistent effect on enrollment re-

and treatment effects (available upon request) are again not statistically significant.

[47]Using control group observations, we run a regression of GPA on the three inputs included in Columns 2-4 of Table 6. We find that the three variables are significantly correlated with GPA; class hours and study time present a positive relationship and missing school the week before the survey a negative one. Using these estimates, the predicted reduction in GPA based on the estimated treatment effects is of 0.10 points.

[48]School grades are a popular proxy for cognitive skills. Table 6 could then be interpreted as evidence of the absence of negative treatment effects on cognitive skills. However, this abstracts from selection into schooling, which can blur the picture. It is possible that the crowding-in at the extensive margin triggers a negative selection of low-grade students who would have dropped out of school in the absence of the program. Furthermore, the crowding-out at the intensive margin can depress grades if study effort decreases. Then the absence of effects on school grades may be related to a more subtle mechanism. Indeed, we provide evidence that the tasks performed in program jobs are probably enhancing the cognitive skills of students, as typically measured in school grades. Table D6 reports the treatment effects on job tasks. Treated workers are significantly more likely to read, write and use a computer every day than control workers. Treated workers are less likely to measure weights and distance and they perform less physically demanding tasks. Work effort of treated youth is thus targeted to tasks that may help them perform better in high school exams.

[49]In Appendix Table A17, we explore whether the absence of the income effect on poor households is due to the program crowding out social transfers. Households of program participants that were receiving cash transfers before the program (vulnerable) are less likely to receive cash transfers

lates to changes in student expectations of returns to education. Work experience in program jobs may lead students to update their expectations upwards. In our survey, treated youth report a higher expected probability of finding a job if one graduates from high school than the probability reported by control youth. The magnitude of the effect is of 3 percentage points from a mean of 70% in the control group (see Appendix Table A19). We do not find any significant treatment effect on the expected returns for other graduation levels (incomplete high school, tertiary or university), which is consistent with the persistent effects being concentrated in high school enrollment. As the effect on expected high school returns is only significant at the 10% level, we consider this evidence as rather suggestive.

Enrollment in Private Schools Finally, one concern is that we do not observe enrollment in private institutions in the administrative data. If the program increased attachment to the public education sector and more youth switched to private schools in the control group, then we would overestimate the effects on enrollment. However, using our survey data, we show in Appendix Table D3 that there are no treatment effects on the type of schools students are enrolled.

5.3 Effects on working and studying

Beyond marginals of employment and education enrollment, we explore the program effects on their joint distribution. Table 8 divides the population into four groups: working and studying in Column (1), working without studying in Column (2), exclusively studying in Column (3) and not working or studying in Column (4). The last group is close to the NEET category (Not in Employment, Education or Training). As expected, the share of working students strongly increases during the program year, from an already high share of 27% for the control compliers. The treatment effect on the share of working students persists in Years 1 and 2, and amounts to 4 percentage points (11-13% of the control mean). This corresponds to reductions in the share of the other three groups, including NEETs. Interestingly, the enrollment effect of 1.6 p.p. for Year 1 (Table 5) is the result of an increase in working students by 4 p.p. (Table 8, Column 1) and a decrease in non-working students by 3 p.p. (Column 2). A similar pattern emerges from the

during the program year than comparable households of control youth. However, the crowding-out is likely to have small effects on total household income, as social transfers represent between 10% (food card) and 20% (cash transfers) of the monthly program wages.

treatment effects in Year 2. This pattern could be explained by treated youth learning how to simultaneously work and study, so that working youth are less likely to drop out of school after the program. It suggests another explanation for the persistent enrollment effect, which could be mediated by treated youth developing stronger habits that combine both work and study. Of course this pattern of treatment effects is also consistent with more complex (and not monotonic) responses. It is at best suggestive of the link between persistent enrollment and persistent work-study.[50]

As youth age, there are no longer any significant effects in Year 3. In Year 4, when almost all control youth have quit school (18% are working students and 5% are students only), the program effects entirely correspond to transferring youth from the NEET group to the working group. The program then decreases the share of NEET youth by 5 p.p. (25% of control mean).

Overall, we find empirical evidence for substantial positive treatment effects on earnings, wages, and employment, and limited effects on education after the program. We now discuss the mechanisms leading to the positive earnings effects.

6 Mechanisms

In this section, we conduct exploratory analysis to study the mechanisms driving the program effects. The program increases educational attainment and labor market experience, both proxies for human capital. Through a decomposition exercise, we quantify which channel, education or work experience, contributes the most to the earnings effects. The decomposition exercise also shows that work experience has lower returns on earnings for youth receiving a program offer. We provide suggestive evidence of one channel that may explain these lower returns, namely the lack of soft skills improvement during the program job.

[50]For example, we could split youth into two types: *always-in-school* and *marginally-in-school* students. The *always-in-school* students do not change their enrollment status when treated, but may react by finding jobs. The *marginally-in-school* youth do not change their work status when treated, but may refrain from dropping out because of the treatment. Accordingly, *always-in-school* students drive the increase in the work-study share, while *marginally-in-school* youth drive the increase in the overall enrollment rate, irrespective of their work status.

6.1 The education channel vs. work experience channel

We first pool the data over all the years after the program and report the Intention-to-Treat effects. Column (1) of Table 9 shows that a program offer increases yearly earnings by \$196.2 - a 6% increase over the control mean.[51] Columns (2) and (3) report the ITT effects on average educational attainment and experience, both computed at the end of the previous year. Consistent with the results in the previous section, a program offer increases education by 0.14 years and average labor market experience by 0.43 years. These effects combine both direct effects during the program year - additional enrollment due to the program requirement and work experience in program firms - and post-program effects.[52] To what extent do these quantity effects on human capital account for the observed earnings effect? Answering this question requires an estimate of the price of human capital in the youth labor market. Figure 2 plots the raw relationship between earnings and either education level (upper Panel) or labor market experience (lower Panel). Comparing the two panels suggests that returns to labor market experience are steeper than returns to education. We thus expect the education channel to contribute less to the earnings effects than the experience channel. Figure 2 also suggests that returns to labor market experience in the offer group are lower than in the control group, especially for low levels of labor experience. The program-induced difference in returns - referred to as a price effect - then lowers the contribution of the experience channel. To quantify the contribution of both the quantity and the price effects, we now perform a decomposition exercise.

Framework of the decomposition exercise Let us denote δ the ITT effect on earnings. It is defined as $\delta = \mathbb{E}\left[Y(1) - Y(0)\right]$, where $Y(1)$ are the potential earnings if offered to participate in the program and $Y(0)$ the potential earnings if not offered to participate. Thanks to the lottery randomization, it is identified by the difference

[51] For the decomposition exercise, we focus on the sample where we keep one application per individual. We drop from the controls the initial level of education and baseline earnings and we use current age instead of age at application to better capture trends in the life-cycle earnings profile. Therefore, results in Table 9 are slightly different from the main ITT results presented in Table A7.

[52] More precisely, we compute both education and enrollment at the end of the previous year. For Year 1 - the first year after the program year - human capital is measured at the end of the program year. Effects are then direct effects of the program (i.e., labor market experience in program jobs and extra enrollment due to the program requirement). Starting in Year 2, the work experience of treated youth has been acquired in both program firms (Year 0) and in regular firms (Year 1). Effects then also capture persistent program effects.

in average observed outcomes between the offer group (receiving an offer, $O = 1$) and the control group (conditional on the lottery design effects):

$$\delta = \mathbb{E}\left[Y|O = 1, Lottery\right] - \mathbb{E}\left[Y|O = 0, Lottery\right].$$

In the following expressions, we suppress the lottery design controls for the sake of readability.[53]

We assume that the earnings of control youth follow a structural relation:

$$Y_i = \alpha^C + f^C(E_i) + \epsilon_i$$

where E_i is a vector of Education and work Experience. f^C is a non-linear pricing function of human capital in the labor market, and ϵ_i represents individual heterogeneity. Similarly, we assume that the earnings of treated youth have a structural form such as:

$$Y_i = \alpha^T + f^T(E_i) + \epsilon_i$$

The structural relations allow for non-linear returns that may depend on the treatment group (i.e., $f^C \neq f^T$). We use the structural relations to decompose the earnings effect:

$$\begin{aligned}
\delta &= \mathbb{E}\left[Y|O = 1\right] - \mathbb{E}\left[Y|O = 0\right] \\
&= \mathbb{E}\left[\alpha^T + f^T(E_i) + \epsilon_i|O = 1\right] - \mathbb{E}\left[\alpha^C + f^C(E_i) + \epsilon_i|O = 0\right] \\
&= \alpha^T - \alpha^C + \mathbb{E}\left[f^T(E_i)|O = 1\right] - \mathbb{E}\left[f^C(E_i)|O = 0\right] + \mathbb{E}\left[\epsilon_i|O = 1\right] - \mathbb{E}\left[\epsilon_i|O = 0\right] \\
&= \underbrace{\alpha^T - \alpha^C}_{u} + \underbrace{\mathbb{E}\left[f^T(E_i) - f^C(E_i)|O = 1\right]}_{p} + \underbrace{\mathbb{E}\left[f^C(E_i)|O = 1\right] - \mathbb{E}\left[f^C(E_i)|O = 0\right]}_{q} \\
&\quad + \underbrace{\mathbb{E}\left[\epsilon_i|O = 1\right] - \mathbb{E}\left[\epsilon_i|O = 0\right]}_{e}
\end{aligned}$$

We further assume that individual heterogeneity is not affected by treatment, so that randomization yields $e = 0$.[54] Then the contribution of human capital (ed-

[53] We also condition on some exogenous individual characteristics X such as age and gender, as in our main specification, which we omit for readability.

[54] Note that lottery design controls - omitted from the expression for the sake of readability - also include location fixed effects. This accounts for potential endogenous sorting of applications across location.

ucation and work experience) to the earnings effect is the sum of a price effect p and a quantity effect q. The term u captures the unexplained effect related to other mediators than education or work experience. By convention, the quantity effect is evaluated at the price in the control group.[55]

Empirical results of the decomposition To quantify the decomposition, we first estimate the structural parameters: the marginal returns to one extra year of education and to one extra year of experience. We leverage the panel structure of our data, and estimate the following regression:

$$Y_{i,t} = IndivFE_i + \gamma^C_{edu,1} Education_{i,t-1} + \gamma^C_{exp,1} Experience_{i,t-1} + \gamma^C_{exp,2} Experience^2_{i,t-1}$$
$$+ Offered \times \left(\delta_{edu,1} Education_{i,t-1} + \delta_{exp,1} Experience_{i,t-1} + \delta_{exp,2} Experience^2_{i,t-1}\right)$$
$$+ \beta X_{i,t} + \nu_{i,t}$$

where $Y_{i,t}$ are total labor earnings of worker i in year t after the application date, $Education_{i,t-1}$ is education level (in years), and $Experience_{i,t-1}$ is formal work experience (in years), both measured at the end of the previous year. The estimation sample is restricted to the post-program period. Table 9 reports the estimation results in Column (4). The estimates confirm the conclusions drawn from Figure 2.[56] Returns to education are not statistically different across offered and control. This result further confirms that the additional education acquired because of the program is not of lower quality. On the contrary, returns to experience are statistically different. The average marginal effect of an extra year of experience is \$871.2 with standard error of 18.8 in the control group, while it is \$652.1 with a standard error of 111.2 in the offer group. The difference is statistically significant at the 5% level.[57] These estimates, together with the mean education and experience in the treatment group, allow us to perform the decomposition exercise, reported in Table 10. We find that out of the \$196 effect on yearly earnings, quantity effects from experience contribute the most, up to 174%. Price effects from experience actually

[55]Results are robust to reverting the base group. If the price effect is calculated for the control group: $\mathbb{E}\left[f^T(E_i) - f^C(E_i) | O = 0\right]$ and the quantity effect evaluated at the prices of the treatment group: $\mathbb{E}\left[f^T(E_i) | O = 1\right] - \mathbb{E}\left[f^T(E_i) | O = 0\right]$, we get similar patterns for both education and experience channels.

[56]Estimation results are robust to excluding youth benefiting from quota rules. This is not surprising as quota youth represent only 838 applicants, which is less than 1% of our main estimation sample.

[57]The difference in marginal effects is \$219 with standard error 113.

contribute negatively: -121%. The contribution from the educational human capital is one order of magnitude lower, at most 16% for its quantity effect. Similarly, the contribution of unobserved mediators is small: 26%.[58] Overall, the experience component explains more than 50% of the earnings effect.

While our setting allows us to analyze effects over a longer horizon than usual experimental standards, an open question is whether they would persist beyond the fourth year after the program. As earnings effects trend upwards, we may expect the effects on youth welfare to grow. However this depends on the rate of diminishing returns to work experience that would eventually trigger a convergence between program participants and the control youth later in their working life. If we make the stark assumption that the work experience channel eventually fades out, there still remains the education channel. As we measure the program effect late in the education investment cycle, the effect on educational attainment is likely to persist beyond the fourth year after the program. Consequently, the earnings effect due to the education channel - equal to 1.9% $(= 0.21 * 0.09)$ - may be interpreted as a lower bound of the life-cycle effect of the program.

6.2 Returns to work experience

The previous finding raises the fundamental question of why work experience for youth offered the program has lower returns on earnings. As shown in Figure 2, the lower returns are concentrated among youth with low experience (less than one year). By construction, treated youth with less than one year of experience acquired it in program firms only. Consequently, we consider several explanations focused on the type of experience acquired in program firms, and how youth can leverage this experience to find new jobs when their program jobs end. In fact, the transition from program jobs to regular jobs is a key and unavoidable step for program participants. The program rules prevent program firms from keeping participants on the same job after the end of the program year. In practice, state-owned companies face stringent rules on hiring/firing on their regular jobs and hire less than 5% of treated youth over the four years after the program.

[58] If we revert the base group as explained above, we get quantity effects from experience and education contributing 119% and 21%, respectively. While price effects from experience and education contribute -66% and 1%.

The first explanation for lower returns to experience for program youth relates to the sector specificity of human capital acquired in program firms. Treated youth work in state-owned companies, mostly in the civil and public banking sectors, while non-program labor market opportunities are mostly provided in the private trade/industry sector. If human capital is sector-specific, program participation should increase earnings in the civil and banking sectors, but not in the industry/trade sectors. Program participants may even have lower earnings in the industry/trade sector as they lag behind controls in terms of sector-specific experience. Consequently, the first work experience of program youth might provide them with lower average returns across sectors. To assess this explanation, we estimate the effects on earnings in each sector separately.[59] We report the detailed estimation results in Appendix Table A11. Overall, we find that earnings effects are not concentrated in the sectors of the program firms. In Years 1 to 3, the earnings effects are even stronger in the industry/trade sector, and non-significant in the civil sector.[60] Consequently we do not find stark evidence of sector specificity, which is in line with previous research finding that individuals move to occupations with similar tasks requirements and thus human capital is portable across sectors (Gathmann and Schönberg, 2010).

The second explanation for lower returns to experience for program youth relates to the overall level of general/transferable human capital acquired in program jobs. More precisely, we consider soft skills, which are explicit targets of the YET program. If soft skills are accumulated in regular jobs (Deming, 2017; Adhvaryu et al., 2018), but program jobs fail to enhance the soft skills of participants, the overall level of human capital per work experience unit will be smaller for program partic-

[59] Alternatively, we document how the post-program earnings vary with the sector of program firms. If human capital is sector specific, we expect earnings of participants assigned to program firms in the banking sector to be higher than earnings of those assigned to program firms in the civil sector. Arguably, there are more opportunities in the private banking sector outside of the program than in the civil sector. In Appendix Table A12, we show that the post-program earnings of treated youth in program firms belonging to the public banking sector are larger than those participating in other state-owned companies (civil sector), but the difference is not statistically significant. If anything, the post-program earnings difference across sectors is probably related to youth working longer hours during the program when in the banking sector (see the significant earnings difference during the program in Column (1) across jobs with the same hourly wage rate fixed by the program).

[60] As we explained above, the administrative data only provide information on whether the firm pertains to one of four aggregate sectors: industry/trade, banking, civil sector or other low-qualified sectors (construction, agriculture and domestic workers).

ipants, leading to lower returns to experience. We first test whether the experience acquired during the program enhances youth soft skills. We measure them in our in-house survey of program applicants to the 2016 edition. The survey was conducted around one year after application, when most of the program participants were still working in their program firms. Panel A of Table 11 reports treatment effects on each dimension of the Big 5 personality test and a measure for grit, following the estimation of Equation (1) as before.[61] We do not find any statistically significant effect, even on grit, which has been shown to be a malleable skill (Alan et al., 2019; Ubfal et al., 2019). Moreover, the questionnaire included some specific questions on work attitudes and soft skills that can be useful in the workplace (e.g., the importance of working in teams, of completing tasks on time, of being punctual and flexible). Panel B of Table 11 shows no statistically significant differences across treatment and control groups in these dimensions. Finally, we obtained a behavioral measure of punctuality by recording whether youth arrived to the survey interview at the scheduled time. In line with the previous results, we find no statistically significant difference in punctuality between treated and control youth (Column 6). Across the board, the evidence goes against the program stated objective of enhancing the soft skills of students by exposing them to a real work environment. This evidence is in line with the type of jobs that the program offers where social interactions are less frequent than in the control group (see Column (7) of Appendix Table D6). At the end of the program year, treated youth have higher work experience, but similar levels of soft skills. The lack of soft skills accumulated in program jobs is then a credible explanation for the lower earning returns on work experience for program youth.[62]

A third alternative explanation relates to the signaling role of work experience. The signaling channel does not rely on human capital acquisition on the job, but rather

[61] The big 5 personality traits are measured with Likert-scale questions (15 questions in total, 3 questions for each dimension of the OCEAN Big 5 personality test). The questionnaire used is based on Pierre et al. (2014), including questions to capture the concept of grit (Duckworth et al., 2007).

[62] Our underlying assumption is that non-program jobs enhance soft skills. Gottschalk (2005) provides experimental evidence from the Self-Sufficiency Project that work experience enhances workers' locus-of-control. Similarly, Adhvaryu et al. (2018) show that on-the-job soft-skills training can improve personality traits. Using our survey data, we report in Appendix Table A16 the correlation between soft skill measures and the employment status of control youth. We find significant correlations of the expected sign. The correlations are statistically significant at the 5% level for 3 out of 10 independent measures. Of course, these correlations also reflect selection into employment and not only the effect of employment on skills. Unfortunately, we do not have panel data on soft skills, and cannot report the within-individual relation between soft skills and work experience.

on how workers may signal their permanent productivity to the market. Showing some work experience on their CV, students can signal their productivity and motivation to potential employers. If potential employers know that selection in program jobs is random, then program participation mostly signals students' motivation. Consequently, work experience in program jobs may provide less precise signals on youth productivity to the labor market than non-program jobs. This would also lead to lower returns to work experience for program youth. Recent evidence from correspondence studies in European countries indicates that resumes with work experience in subsidized jobs do not generate lower call back rates than resumes with non-subsidized work experience (Cahuc et al., 2017). This suggests a limited role for this alternative explanation, that should be further investigated in future research.[63]

7 Youth Welfare analysis

In this section, we provide evidence on the program effects on youth welfare, beyond effects on earnings. We leverage our survey module on the time use of program participants and their opportunity cost of work during the program year.

During the program year Table 12 yields unique information on how the increase in working time due to the program crowds out other activities. The program increases youth weekly working time by almost 11 hours. Hours worked in the treatment group are more than double those in the control group.[64] We do not find evidence of work crowding out or crowding in study time. The positive effect of the program on enrollment and the negative effect on study hours conditional on being enrolled cancel each other out. The main result in Table 12 is that wage employment crowds out both home production (Column 4) and leisure time (Column

[63] Related to the ability *learning* channel (from the worker side) mentioned in Section 3, it may also be the case that the program does not allow youth to learn their ability in a wide enough variety of occupations. Indeed, program jobs are certainly more homogeneous across firms than the variety of jobs youth can find outside of the program. This should also be further investigated in future research.

[64] Hours worked measured in the time-use survey reach almost 20 hours in the treatment group. This is slightly lower than the range stated on the program rules (20-30), and it is because some youth already left their program jobs by the time of the survey and report zero hours worked.

5).[65] Leisure time decreases by 14 percent and time dedicated to household chores decreases by 50 percent.[66]

What does this mean for the effects on youth welfare during the program year? To answer this question, we further assume that the disutility from working, studying, commuting and home production is the same. They are time-consuming activities that reduce leisure time. We also neglect the additional consumption that home production and study may yield. For household chores, this is reasonable as 90% of youth live with their families, and we expect other family members to take over youth tasks at home, without reduction in youth consumption.[67] Then we need to estimate the utility derived from leisure. We leverage the reservation wage question of our survey: "What is the minimum monthly wage for which you would accept a full-time job?" Monthly reservation wages average \$590 for a full-time job of 160 hours. This implies that one hour of leisure yields utility equivalent to \$3.7 of consumption ($= 590/160$).[68] Table 12 shows that the program decreases monthly leisure time by 21 hours ($= 4.9 \times 4.3$). The monthly loss of utility due to the program effect on leisure is then equivalent to \$77.7 ($=3.7 \times 21$). This is to be compared with the treatment effect on monthly earnings of \$147.4 at the end of the program year (see Table D2). The net effect on youth welfare is then \$69.7 per month, which adds up to \$836.4 over the whole program year.

Beyond the program year We cannot rely on time-use data to compute the program

[65] The effects on work, household chores and leisure are robust to adjusting the p-values for the 7 hypotheses tested in this table. Those on commuting and eating lose statistical significance with the adjustment.

[66] We do not find effects on sleeping time and there is a marginally statistically significant reduction on the time dedicated to eating (1.4 hours per week). Furthermore, we do not find evidence of program effects on youth health. Although few respondents report them, we do not find any significant treatment effect on the time spent visiting physicians or hospitals. This is confirmed by another direct question about health complications in the survey, where no effects are detected, and by the absence of effects on mortality rates registered in the administrative data.

[67] The reduction in the time that program youth spent on household chores probably imposes negative externalities on other household members. This reduction is of around 50% with respect to the control group for both treated women and men, but women dedicate to household chores twice as much time as men do. This time saving is reflected in smaller reductions in study time and leisure for treated women than for treated men, although the differences are not statistically significant (results available upon request). We leave the household welfare analysis for future research, as it requires household-level data.

[68] We also assume that only the quantity and not the quality of leisure is affected by the program. A priori, it is possible that due to their higher income youth derive higher utility for the same level of time dedicated to leisure. However, we do not find any differential program effects on home vs. outside-home leisure activities: TV, music, video games vs. movies, sport events, etc.

effects on youth welfare beyond the program year. We thus follow a more structural approach within the neoclassical labor supply model. Worker i gets utility from leisure l and from consumption c: $U(c, l)$. She is endowed with T hours. She can work $\bar{h}$ hours in a full-time job and receive a total wage w. We assume that she has a level v of non-labor income, so that she consumes $c = w + v$. The utility when non-employed is $U(v, T)$. The reservation wage R verifies: $U(R + v, T - \bar{h}) = U(v, T)$.

Suppose that the program increases w from $w(0)$ to $w(1)$. Every worker with $w(0) \leq w(1) < R_i$ remains non-employed both when treated or control, and experiences no program effect on welfare. Workers with $w(0) < R_i \leq w(1)$ switch from non-employment to employment because of the program. The increase in their utility is:

$$U(w(1) + v, T - \bar{h}) - U(v, T) = U(w(1) + v, T - \bar{h}) - U(R_i + v, T - \bar{h}) = w(1) - R_i$$

where we assume that utility is separable with respect to consumption and leisure and linear in consumption. Finally, workers with $R_i < w(0) \leq w(1)$ are employed both when treated or control. Their increase in utility is:

$$U(w(1) + v, T - \bar{h}) - U(w(0) + v, T - \bar{h}) = w(1) - w(0).$$

Consequently, we can derive the average program effect on utility:

$$\begin{aligned}
\mathbb{E}\left[U(1) - U(0)\right] &= \mathbb{P}\left(R_i < w(0)\right) \times \mathbb{E}\left[w(1) - w(0) | R_i < w(0)\right] \\
&\quad + \mathbb{P}\left(w(0) < R_i \leq w(1)\right) \times \mathbb{E}\left[w(1) - R_i | w(0) < R_i \leq w(1)\right] \\
&= \mathbb{P}\left(R_i \leq w(1)\right) \mathbb{E}\left[w(1) | R_i \leq w(1)\right] - \mathbb{P}\left(R_i < w(0)\right) \times \mathbb{E}\left[w(0) | R_i < w(0)\right] \\
&\quad - \mathbb{P}\left(w(0) < R_i \leq w(1)\right) \times \mathbb{E}\left[R_i | w(0) < R_i \leq w(1)\right]
\end{aligned}$$

The quantity $\mathbb{P}\left(R_i \leq w(1)\right) \mathbb{E}\left[w(1) | R_i \leq w(1)\right] - \mathbb{P}\left(R_i < w(0)\right) \times \mathbb{E}\left[w(0) | R_i < w(0)\right]$ amounts to the treatment effect on earnings. It is identified in the data. The probability $\mathbb{P}\left(w(0) < R_i \leq w(1)\right)$ is the treatment effect on employment, which is also directly identified and equal to 3 p.p. over the four years after the program (see Table 3). The last term $\mathbb{E}\left[R_i | w(0) < R_i \leq w(1)\right]$ is the average reservation wage of youth induced to work because of the program. Assuming monotonicity, the reservation wage of youth induced to work because of the program can be recovered from that of the control youth at the margin between employment and non-employment.

Over the four post-program years 67% of control youth work; we thus use the 67th percentile of the reservation wage distribution to identify $\mathbb{E}[R_i|w(0) < R_i \leq w(1)]$. Using the survey data on monthly reservation wages, this yields \$616.7.[69] To convert the monthly reservation wages into yearly reservation wages, we compute the number of months worked by control youth induced to work because of the program. Assuming that always-employed youth do not change their intensity of labor supply within the year because of the program, this yields 2.33 months ($= 0.07/0.03$, i.e., the treatment effect on months worked over the treatment effect on employment). Finally, we subtract \$43.1 ($= 0.03 \times 2.33 \times 616.7$) from the treatment effect on earnings to obtain the average effect on welfare: \$242.

To sum up, we find, using reservation wage and time-use data, that the program increases youth earnings adjusted for the disutility of work by \$836 during the program year and by \$242 during every post-program year up to four years after the program.

8 Conclusion

In this paper, we provide the first evidence of the effect of working while in school that uses controlled random variation in job offers. We leverage an Uruguayan program that offers jobs to students by lottery. We find that working while enrolled in school during the program year improves labor market outcomes in the following four years. We see positive and statistically significant effects on formal earnings, employment and wages.

We also find persistent positive effects on education enrollment, which suggests limited crowding out of working on studying. We find a large increase in high school enrollment during the program year, which could be explained by the enrollment conditionality of the program. However, we also find effects after the program year, when there is no binding conditionality. Moreover, we find no evidence of significant negative effects on schooling effort and outcomes. Our time-use survey indicates that students manage to work while in school by reducing time dedicated to leisure and household chores. We show that youth welfare is still

[69] Given that we observe reservation wages in the end-of-program-year survey, this further assumes that the reservation wage distribution remains stable over the next four years beyond the program year.

positive after accounting for the decrease in utility due to this reduction in leisure. A topic for future research is to study how the welfare of other household members is affected by the extra time they have to dedicate to household chores.

We provide evidence that the accumulation of labor market experience contributes more to the effects of working while in school than the extra-education channel. The human capital that students acquire in state-owned companies is valued by private employers. However, we find that the work experience acquired thanks to the Uruguayan program has lower returns on future earnings than alternative jobs, probably because students did not enhance their soft skills while working in the program jobs. Our empirical analysis emphasizes human capital accumulation as a key channel. However, we cannot discard a signaling role of student work, which is certainly an interesting avenue for further research.

Our results support the further development of work-study programs in Uruguay, in countries sharing similar educational institutions and labor markets, and potentially beyond. We believe that the characteristics of the program we study - it offers well-paid jobs in clerical occupations and is complementary to schooling - are key ingredients of its success. Further analysis in other contexts could leverage job heterogeneity to shed light on these program design choices.

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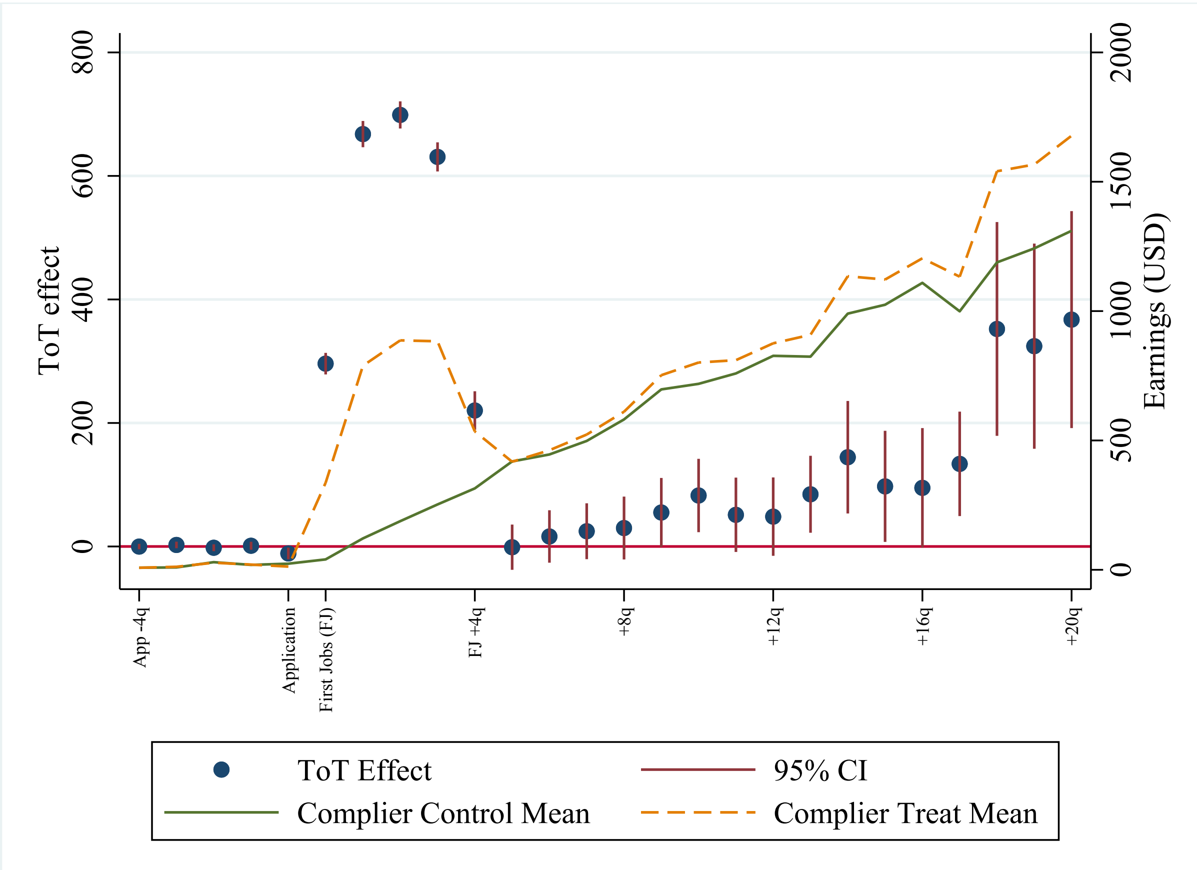
FIGURES

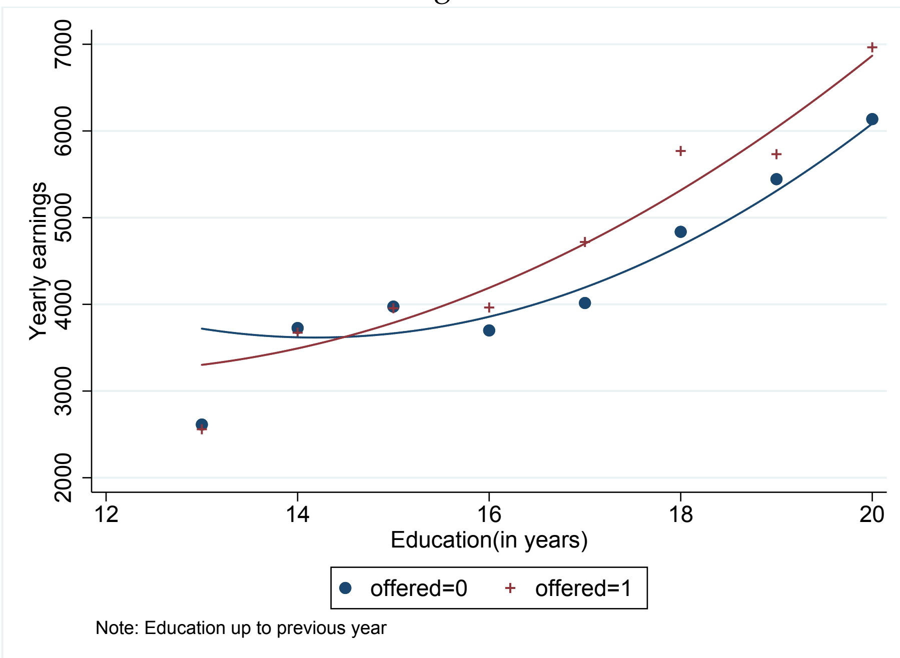
Figure 1: Quarterly earnings



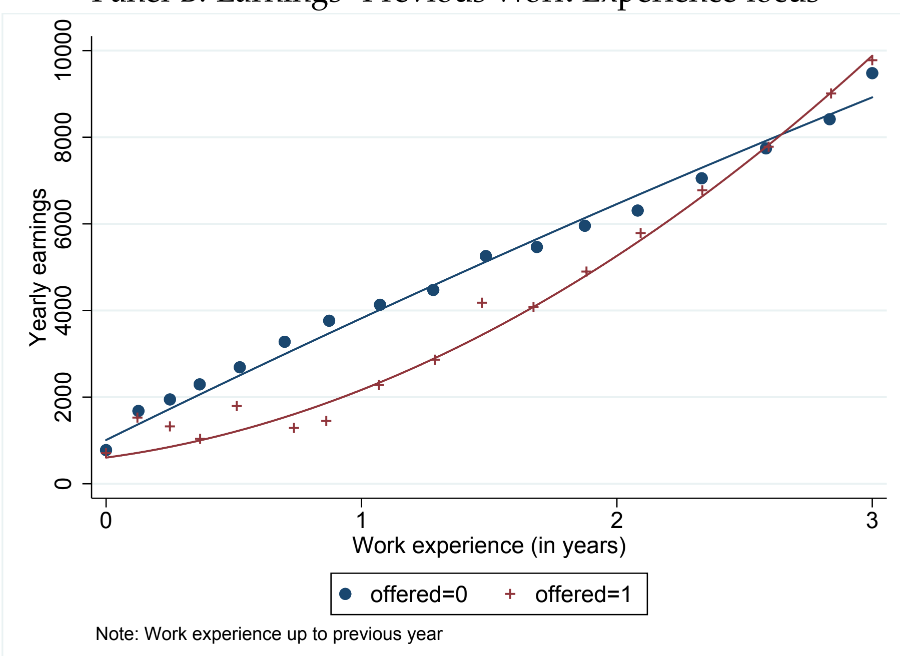
Note: This figure plots the evolution of quarterly treatment effects (left Y-axis), and of average quarterly earnings by treatment group (right axis). We use blue dots to report treatment effects, and red vertical lines for their 95% confidence intervals. During the program year, quarterly treatment effects amount to around 700 USD. The dashed yellow (resp. solid green) line reports quarterly earnings for the treated individuals (resp. compliers in the control group). During the program year, treated individuals earn around 750 USD per quarter.

Figure 2: Post-program yearly earnings profiles wrt. previous education and labor market experience, by treament

Panel A: Earnings-Education locus



Panel B: Earnings- Previous Work Experience locus



Note: These figures plot yearly earnings in the fourth year after the program against education levels in Panel A and previous work experience in Panel B. Yearly earnings are related to proxies of human capital measures at the end of the previous year. Namely, 2017 earnings are plotted against the education level attained at the end of 2016 and the stock of labor market experience as of the end of 2016. We plot the profiles separately for applicants receiving an offer (blue dots), or not (red crosses). Education counts the total number of years of school, including kindergarten years.

TABLES

Table 1: YET edition by edition

Edition	1	2	3	4	5
Application Date	May 2012	May 2013	May 2014	Sep 2015	Sep 2016
Applications	46,544	43,661	31,990	21,159	27,143
Applicants	46,008	42,643	30,969	20,537	26,137
Job Offers Made	754	981	955	722	843
Jobs Completed	549	686	660	541	632
Sector: Civil	0.81	0.73	0.71	0.64	0.64
Sector: Industry/Trade	0.03	0.05	0.04	0.05	0.05
Sector: Banking	0.16	0.23	0.25	0.31	0.31
Localities	51	64	67	65	63

Source: YET Administrative Data.

Table 2: Balance checks between treatment and control groups - all editions pooled

	(1)	(2)	(3)	(4)	(5)
	Control		Offered		
	Mean	S.D.	Mean	S.D.	p-value[1]
Panel A. Demographics					
Female	0.60	0.49	0.61	0.49	0.33
Aged 16-18	0.72	0.45	0.71	0.45	0.64
Aged 19-20	0.28	0.45	0.29	0.45	0.64
Montevideo (Capital City)[2]	0.49	0.50	0.53	0.50	
Panel B. Education and Social Programs Year -1					
Enrolled in Academic Secondary Education	0.49	0.50	0.48	0.50	0.32
Enrolled in Technical Secondary Education	0.22	0.41	0.22	0.42	0.49
Enrolled in University[3]	0.16	0.37	0.16	0.37	0.89
Enrolled in Tertiary Non-University	0.01	0.11	0.01	0.10	0.43
Enrolled in Out-of-School Programs	0.02	0.13	0.02	0.14	0.80
Highly Vulnerable HH (Food Card Recipient)	0.09	0.29	0.09	0.29	0.93
Vulnerable Household (CCT recipient)	0.26	0.44	0.27	0.44	0.22
Panel C. Labor Outcomes Year -1					
Earnings (winsorized top 1%, USD)	163.17	578.73	151.63	571.44	0.34
Positive Earnings	0.15	0.36	0.15	0.35	0.73
Months with Positive Earnings	0.68	2.07	0.62	1.96	0.25
Panel D. Aggregate orthogonality test for panels A-C					
p-value (joint F-test)[4]					0.80
Observations	119,366		2,829		122,195

Source: Administrative Data and YET Application Form. Notes: [1]p-value reported in Column 5 is obtained from a regression of each variable on a YET job offer dummy with clustered standard errors at the applicant level, controlling for lottery design (lottery and quota dummies) and number of applications. [2] We do not test for differences in means for Montevideo since the lottery was randomized within each locality and we control for lottery design in all our specifications. [3]We code "Enrolled in university" by using two indicators available in the administrative data: "entering a new program that year" or "taking at least two exams that year", for the first edition we do not have data on Year -1 and we use the value self-reported by participants in the application form. [4] p-value corresponds to the orthogonality test in a regression of the YET job offer dummy on covariates, the regression also controls for lottery design and number of applications (coefficients not included in the F-test).

Table 3: Effect of YET on labor outcomes

	(1) Total earnings	(2) Months with positive earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	2001.48***	7.41***	0.60***	-24.81***
	(41.64)	(0.08)	(0.01)	(3.09)
	[972.36]	[2.57]	[0.40]	[321.32]
Post-Program Years				
Year 1	51.75	-0.06	0.04***	4.59
	(79.92)	(0.13)	(0.01)	(7.92)
	[2026.38]	[4.54]	[0.60]	[398.50]
Year 2	206.56*	-0.02	0.02	26.39***
	(110.24)	(0.14)	(0.01)	(9.97)
	[3083.94]	[5.60]	[0.67]	[498.05]
Year 3	432.84***	0.18	0.01	43.08***
	(165.44)	(0.18)	(0.02)	(13.35)
	[4107.04]	[6.40]	[0.72]	[583.19]
Year 4	1113.19***	0.57**	0.05**	71.86***
	(285.81)	(0.25)	(0.02)	(23.08)
	[5046.11]	[7.07]	[0.75]	[661.82]
Ys 1-4(Avg.)	285.35***	0.07	0.03***	26.22***
	(103.38)	(0.12)	(0.01)	(8.60)
	[3142.03]	[5.56]	[0.67]	[506.65]
Individuals	90,423	90,423	90,423	48,375
Applications	122,195	122,195	122,195	58,078

Source: Administrative data.

Notes: Two stage least squares regressions where we instrument the YET participation dummy with a job offer dummy. Controls for lottery design (lottery and quota dummies) and number of applications are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. **Total earnings**: total labor income over 12 months, winsorized at the top 1 percent of positive values and converted into U.S. dollars. **Month Pos. earnings**: number of months over 12 months with positive income. **Positive earnings**: indicator for positive earnings in any month over 12 months. **Wages**: Total earnings divided by Month Pos. earnings, it is missing for those who have not worked any month over the 12 months. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. The number of observations (applicants) for Columns (1)-(3) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (40,008) for Year 4. **Ys 1-4 (Avg)** reports results for a regression pooling all post-program years. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Bounds for the ITT effects on monthly wages (post-program years)

	(1) ITT effect on wages	(2) Lee bounds on wage effect	(3)	(4) Imbens and Manski 95% confidence interval
		Lower	Upper	
Year 1	3.29	-23.27***	20.84***	{-31.56, 30.00}
	(5.68)	(5.04)	(5.57)	
	[409.15]			
Year 2	18.99***	16.21**	28.72***	{4.60, 40.27}
	(7.19)	(7.06)	(7.02)	
	[501.88]			
Year 3	31.35***	30.49***	38.20***	{14.52, 54.12}
	(9.74)	(9.71)	(9.68)	
	[589.37]			
Year 4	53.91***	-3.64	82.80***	{-26.93, 110.90}
	(17.34)	(14.16)	(17.08)	
	[682.72]			

Notes: This table presents bounds on causal effect on wages for the "always employed" (individuals who would be employed regardless of whether they are offered the program job or not) based on the procedure described in Lee (2009). To obtain the upper bound, we trim the sample of observed wages in the offered group with the p% lower wages, where p is the ratio of the ITT effect on employment over the employment rate on the offered group. The lower bound is the symmetric case where we trim the p% of higher wages. Standard errors clustered at the applicant level shown in parenthesis and control means in brackets. We follow Imbens and Manski (2004) to construct confidence intervals for the bounds. The number of observations (applicants) is: 74,447 (58,625) for Year 1, 81,297 (62,657) for Year 2, 63,718 (52,529) for Year 3 and 34,495 (34,090) for Year 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Effect of YET on enrollment in education.

	(1) Any Level	(2) Secondary Education	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Programs
Program Year					
Year 0	0.119***	0.101***	0.012	0.005	0.004
	(0.010)	(0.012)	(0.008)	(0.004)	(0.005)
	[0.756]	[0.521]	[0.207]	[0.017]	[0.025]
Post-Program Years					
Year 1	0.016	0.024*	-0.000	0.003	-0.006*
	(0.014)	(0.013)	(0.011)	(0.005)	(0.003)
	[0.646]	[0.344]	[0.279]	[0.025]	[0.016]
Year 2	0.031**	0.021*	0.005	0.004	0.003
	(0.014)	(0.012)	(0.011)	(0.005)	(0.004)
	[0.472]	[0.236]	[0.213]	[0.028]	[0.007]
Year 3	0.019	0.023*	-0.011	0.003	0.005
	(0.017)	(0.013)	(0.011)	(0.005)	(0.004)
	[0.366]	[0.181]	[0.161]	[0.028]	[0.005]
Year 4	-0.007	0.001	-0.006	-0.008	0.008
	(0.020)	(0.017)	(0.009)	(0.007)	(0.005)
	[0.231]	[0.156]	[0.044]	[0.030]	[0.004]
Ys 1-4 (Avg.)	0.022**	0.020**	0.001	0.002	0.001
	(0.010)	(0.009)	(0.008)	(0.003)	(0.003)
	[0.483]	[0.253]	[0.206]	[0.027]	[0.009]
Individuals	90,423	90,423	90,423	90,423	90,423
Applications	122,195	122,195	122,195	122,195	122,195

Notes: Two stage least squares regressions where we instrument the YET participation dummy with a job offer dummy. Controls for lottery design (lottery and quota dummies) and number of applications are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. We code "registered at university" by using two indicators available in the administrative data: "entering a new program that year" or "taking at least two exams that year". For 2017 we do not have the data on taking two exams, and therefore the mean of university registration is underestimated (this applies to year 4, edition 1, year 3 edition 2, and year 2, edition 3). In Column (4), for edition 1 we use as baseline value of the outcome a dummy for self-reported registration at university. The number of observations (individuals) is 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. **Ys 1-4 (Avg)** reports results for a regression pooling all post-program years. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effect of YET on study effort during the program year (Year 0)

	(1) High school enrolled	(2) Absent last week	(3) Class hs per week	(4) Study time outside school (hs per week)	(5) GPA current
treated	0.10***	0.01	-1.85**	-2.51***	-0.20
	(0.04)	(0.05)	(0.86)	(1.04)	(0.16)
CCM	0.45	0.25	26.90	6.46	7.88
Applications	1,366	649	649	649	649
Applicants	1,272	604	604	604	604

Source: Survey.

Note: IV regression of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). **Class hs per week**: average hours attending high school (calculated as product of reported hours per day and days per week). **Study time outside school**: reported hours studying at home or outside school (time-use module). **GPA**: reported current GPA in high school (grades range from 1 to 12). GPA standard deviation amounts to 1.6. Standard errors clustered at the individual level shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: Effect of YET by baseline household vulnerability

	(1) Enrolled Any Level	(2) Total Earnings
	Avg Ys 1-4	
Treated (T)	0.019*	258.25**
	(0.012)	(124.53)
T * Vulnerable	0.028	-2.52
	(0.027)	(248.28)
T * Highly Vulnerable	-0.069	320.33
	(0.044)	(376.60)
Vulnerable	-0.067***	-140.66***
	(0.003)	(28.2)
Highly Vulnerable	-0.057***	-349.30***
	(0.005)	(38.4)
CCM	0.506	3308
Observations	381,139	381,139
Individuals	90,423	90,423

Source: Administrative data.

Notes: two stage least squares regressions where we instrument the YET participation dummy and the interaction with Vulnerable and Highly Vulnerable dummies with a job offer dummy and the corresponding interactions. Controls for lottery design (lottery and quota dummies) and number of applications are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis. We report pooled regressions over years 1-4 after the program. **Enrolled Any Level**: Enrolled in any level of public education. **Total earnings**: total labor income over 12 months, winsorized at the top 1 percent of positive values and converted into U.S. dollars. **Vulnerable**: dummy for being in a household receiving a cash transfer (26% of the sample) the month before the program. **Highly Vulnerable**: dummy for being in a household receiving a cash transfer and a food card (9% of the sample) the month before the program. It is a subset of the Vulnerable category. **CCM**: control complier mean of the dependent variable among those who are not vulnerable. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Effect of YET on working and studying

	(1) Work and Study	(2) Work No Study	(3) No Work and Study	(4) No Work No Study
Program Year				
Year 0	0.60***	-0.01	-0.48***	-0.11***
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.27]	[0.13]	[0.48]	[0.11]
Post-Program Years				
Year 1	0.04***	-0.00	-0.03**	-0.02*
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.37]	[0.24]	[0.28]	[0.12]
Year 2	0.04***	-0.02	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.30]	[0.37]	[0.17]	[0.16]
Year 3	0.01	-0.00	0.01	-0.02
	(0.02)	(0.02)	(0.01)	(0.01)
	[0.26]	[0.46]	[0.10]	[0.18]
Year 4	-0.01	0.06**	-0.00	-0.05**
	(0.02)	(0.02)	(0.01)	(0.02)
	[0.18]	[0.57]	[0.05]	[0.20]
Ys 1-4 (Avg.)	0.03***	-0.00	-0.01	-0.02**
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.30]	[0.36]	[0.18]	[0.15]
Individuals	90,423	90,423	90,423	90,423
Applications	122,195	122,195	122,195	122,195

Notes: Two stage least squares regressions where we instrument the YET participation dummy with the offer to take the YET job. Controls for lottery design (lottery and quota dummies) and number of applications are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Study: registered at public secondary education, out-of-school programs, tertiary or university. Work: positive income for any month during the year. We code "registered at university" by using two indicators available in the administrative data: "entering a new program that year" or "taking at least two exams that year", for 2017 we do not have the data on taking two exams, and therefore, the mean of university registration is underestimated (this applies to year 4, edition 1, year 3 edition 2, and year 2, edition 3). The number of observations (individuals) is 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. **Ys 1-4 (Avg)** reports results for a regression pooling all post-program years. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Earnings return to education and work experience

	(1) Earnings	(2) Education	(3) Experience	(4) Earnings
Offered	196.2***	0.142***	0.430***	
	(72.97)	(0.023)	(0.013)	
Education				313.81***
				(13.77)
Educ. $\times$ offered				89.92
				(85.69)
Work experience				1,065.6***
				(28.81)
Exp.^{2}				-123.84***
				(7.91)
Exp. $\times$ offered				-523.36**
				(227.55)
Exp.^{2} $\times$ offered				183.25***
				(58.63)
Control mean	3290.7	15.52	0.785	
Application FE				Y
Observations	283,630	283,624	283,630	283,624
Number applicants	90,422	90,420	90,422	90,420

Source: Administrative data.

Notes: OLS regressions of the outcome on an indicator for having being offered a YET job. Education counts the total number of years of school (including kindergarten years). We include as controls: age, gender, poverty indicator, and lottery (location times year) and quota dummies. Time invariant controls do not contribute to the estimation with fixed effects. The sample is restricted to one application per youth. Application fixed effects then amount to applicant fixed effects. Standard errors clustered at the applicant level shown in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 10: Contribution of education and work experience to earnings effect

	(1) in dollars	(2) % of ITT
Earnings effect (ITT): δ	196.2	
Quantity Effect : q		
Education	31.4	16.0
Experience	340.5	173.5
Price Effect: p		
Education	10.4	5.3
Experience	-236.5	-120.5
Unexplained	50.4	25.7

Note: The ITT effect on monthly earnings is decomposed into the sum of quantity and price effects of education and experience, and an unexplained residual contribution. Quantity effects describe the increase in earnings due to program-induced increase in educational attainment and experience, priced as in the control group. Price effects account for changes in the returns to either education or experience between the treated and control youth.

Table 11: Effects during the program: soft skills

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Big 5 and grit					
	Open	Conscientious	Extrav Scale 1-5	Agreeable	Neurotic	Grit
Treated	-0.041	0.046	0.007	-0.026	0.046	-0.049
	(0.036)	(0.040)	(0.057)	(0.041)	(0.068)	(0.043)
CCM	4.041	3.792	3.611	3.695	3.419	3.736
Control sd	0.493	0.565	0.734	0.533	0.835	0.579
	Panel B. Soft Skills Related to Labor Market					
	Finish on time	Adapts fast	Teamwork important Scale 1-5	Punctual	Index (1-4)	Unpunctual Interview
Treated	0.071	0.067	0.050	-0.002	0.047	-0.010
	(0.050)	(0.051)	(0.050)	(0.061)	(0.038)	(0.010)
CCM	4.047	4.006	4.246	4.169	4.117	0.0241
Control sd	0.679	0.650	0.677	0.811	0.494	0.149
Applications	1,366	1,366	1,366	1,366	1,366	1,366
Individuals	1,272	1,272	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV regression of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses.

Table 12: Effects during the program: time use

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Time (hours per week)						
	Working	Studying in or out of school	Commuting	Household chores	Leisure	Sleeping	Eating
Treated	10.90***	-1.990	2.143**	-3.170***	-4.936***	-0.784	-1.443*
	(1.509)	(1.811)	(0.984)	(0.780)	(1.885)	(1.402)	(0.769)
CCM	8.759	20.08	5.974	6.404	34.80	58.81	10.72
Applications	1,366	1,366	1,366	1,366	1,366	1,366	1,366
Individuals	1,272	1,272	1,272	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. The time-use survey questions are daily, we convert answers into weekly measures. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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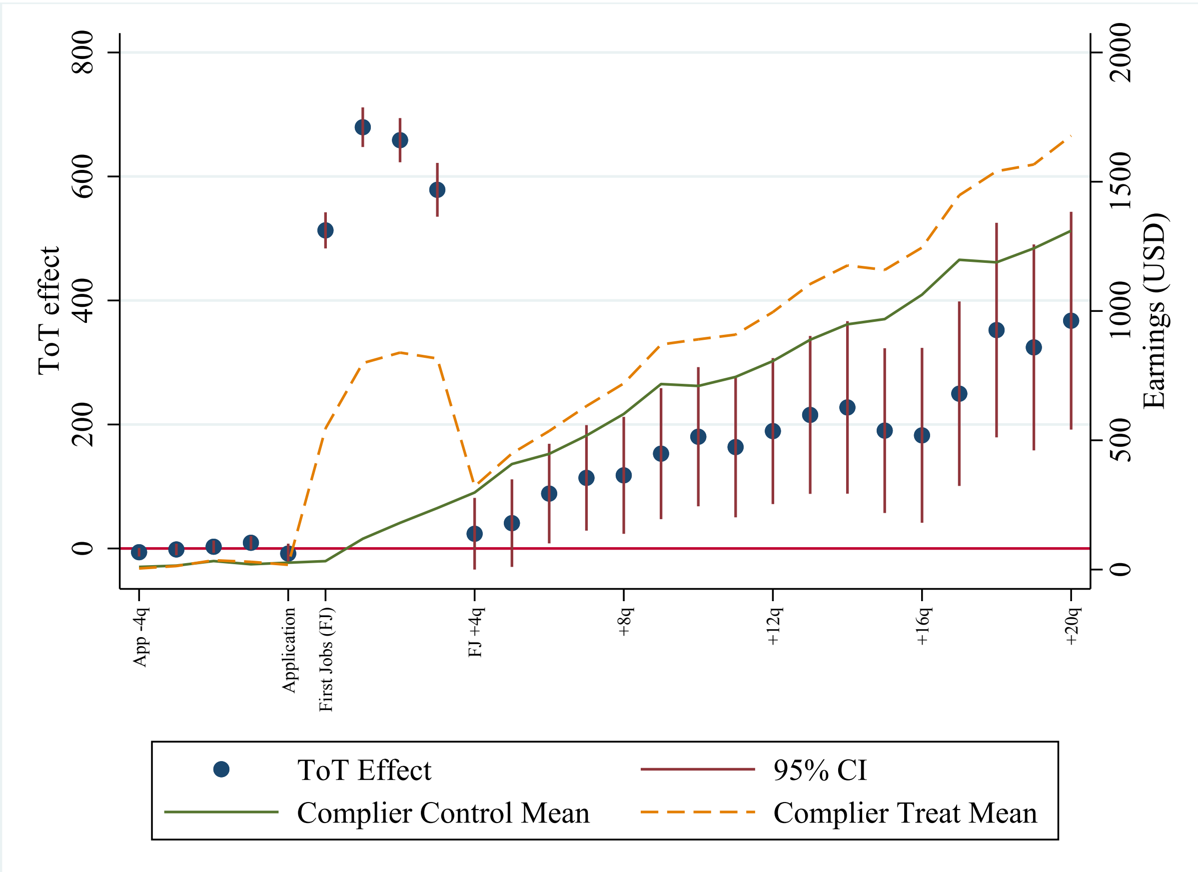
A Appendix Figures and Tables

Figure A1: Quarterly Earnings. Edition 1



Note: This figure replicates Figure 1, but restricts the sample to the first cohort of applicants to the program.

Table A1: Effect of YET offer on YET participation (first stage)

	(1)	(2)	(3)	(4)
		YET Participation		
	All Editions	Edition 1	Edition 2	Edition 3
Won Lottery	0.71***	0.73***	0.70***	0.70***
	(0.01)	(0.02)	(0.02)	(0.02)
Fstat	6,110	2,001	2,077	2,088
Applications	122,195	46,544	43,661	31,990
Individuals	90,423	46,008	42,643	30,969

Notes: OLS regressions of YET participation in year 0 on the offer to take the YET job (winning the lottery). Controls for lottery design (lottery and quota dummies) and number of applications are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A2: Effect of YET offer in year 0 on YET participation every year

	(1)	(2)	(3)	(4)	(5)
	Year 0	Year 1	Year 2	Year 3	Year 4
Won Lottery Year 0	0.7115***	-0.0043***	-0.0023***	-0.0008***	-0.0001***
	(0.0103)	(0.0008)	(0.0001)	(0.0001)	(0.0000)
Individuals	121,178	121,178	121,178	121,178	121,178

Notes: OLS regressions of YET participation in year 0 on the offer to take the YET job in the following years. We keep only one application per edition per participant. Standard errors robust to heteroskedasticity shown in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: Effect of YET on labor outcomes - no controls

	(1) Total earnings	(2) Months Pos. earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	1987.01***	7.39***	0.60***	-36.09***
	(44.96)	(0.09)	(0.01)	(3.12)
	[986.83]	[2.60]	[0.40]	[332.59]
Post-Program Years				
Year 1	34.41	-0.08	0.04***	1.02
	(83.60)	(0.14)	(0.01)	(8.23)
	[2043.72]	[4.56]	[0.60]	[402.07]
Year 2	185.94	-0.04	0.02	23.75**
	(114.32)	(0.14)	(0.01)	(10.37)
	[3104.56]	[5.62]	[0.67]	[500.69]
Year 3	391.34**	0.15	0.01	40.60***
	(171.22)	(0.18)	(0.02)	(14.06)
	[4148.54]	[6.43]	[0.72]	[585.68]
Year 4	971.80***	0.49*	0.05**	63.86***
	(302.19)	(0.26)	(0.02)	(24.57)
	[5187.49]	[7.15]	[0.76]	[669.83]
Ys 1-4 (Avg.)	255.11**	0.04	0.03**	22.93**
	(108.56)	(0.13)	(0.01)	(9.12)
	[3172.28]	[5.58]	[0.67]	[509.95]
Individuals	90,423	90,423	90,423	48,375
Applications	122,195	122,195	122,195	58,078

Source: Administrative data.
Notes: Replicates Table 3 without including control variables. The number of observations (applicants) for Columns (1)-(3) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Effect of YET on labor outcomes - clustering at locality level

	(1) Total earnings	(2) Months Pos. earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	2001.48***	7.41***	0.60***	-24.81***
	(169.67)	(0.35)	(0.04)	(7.56)
	[972.36]	[2.57]	[0.40]	[321.32]
Post-Program Years				
Year 1	51.75	-0.06	0.04***	4.59
	(72.26)	(0.13)	(0.01)	(5.06)
	[2026.38]	[4.54]	[0.60]	[398.50]
Year 2	206.56***	-0.02	0.02*	26.39***
	(68.29)	(0.09)	(0.01)	(6.61)
	[3083.94]	[5.60]	[0.67]	[498.05]
Year 3	432.84***	0.18	0.01	43.08***
	(154.92)	(0.20)	(0.02)	(9.79)
	[4107.04]	[6.40]	[0.72]	[583.19]
Year 4	1113.19***	0.57**	0.05**	71.86***
	(278.32)	(0.25)	(0.02)	(17.64)
	[5046.11]	[7.07]	[0.75]	[661.82]
Ys 1-4 (Avg.)	285.35***	0.07	0.03**	26.22***
	(96.64)	(0.13)	(0.01)	(6.26)
	[3142.03]	[5.56]	[0.67]	[506.65]
Individuals	90,423	90,423	90,423	48,375
Applications	122,195	122,195	122,195	58,078

Source: Administrative data.
Notes: Replicates Table 3, but clustering the standard errors at the locality level. The number of observations (applicants) for Columns (1)-(3) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Effect of YET on labor outcomes - one application per participant

	(1) Total earnings	(2) Months Pos. earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	2024.22***	7.45***	0.61***	-25.86***
	(39.60)	(0.08)	(0.01)	(3.05)
	[941.77]	[2.53]	[0.39]	[322.02]
Post-Program Years				
Year 1	93.06	0.00	0.05***	7.20
	(77.95)	(0.13)	(0.01)	(7.65)
	[1986.74]	[4.44]	[0.59]	[399.17]
Year 2	259.82**	0.07	0.02*	30.10***
	(104.68)	(0.14)	(0.01)	(9.25)
	[2999.47]	[5.48]	[0.66]	[492.47]
Year 3	448.00***	0.22	0.01	41.62***
	(156.20)	(0.17)	(0.02)	(12.85)
	[4026.83]	[6.29]	[0.71]	[581.58]
Year 4	1070.15***	0.55**	0.06***	66.39***
	(285.23)	(0.25)	(0.02)	(23.06)
	[5079.21]	[7.05]	[0.75]	[669.22]
Ys 1-4 (Avg.)	315.73***	0.13	0.03***	27.46***
	(100.95)	(0.12)	(0.01)	(8.47)
	[3096.54]	[5.46]	[0.65]	[506.13]
Individuals	90,423	90,423	90,423	43,400
Applications	90,423	90,423	90,423	43,400

Source: Administrative data.

Notes: Replicates Table 3, but keeping one application per individual. For participants who were ever offered a job and applied to more than one edition and/or locality, we keep the application for the edition and locality in which they were offered the job. For participants never offered a job, we randomly select one application among all their applications. Results are robust to picking randomly one application for every applicant. The number of observations/applicants for Columns (1)-(3) is: 90,423 for Year 0-Year 2, 66,595 for Year 3 and 36,183 for Year 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6: Effect of YET on labor outcomes - no winsorizing

	(1) Total earnings	(2) Months Pos. earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	1997.85***	7.41***	0.60***	-25.72***
	(43.22)	(0.08)	(0.01)	(3.24)
	[982.45]	[2.58]	[0.40]	[322.76]
Post-Program Years				
Year 1	65.61	-0.07	0.04***	6.45
	(83.27)	(0.13)	(0.01)	(8.38)
	[2041.06]	[4.55]	[0.60]	[400.36]
Year 2	235.52**	-0.02	0.02	29.81***
	(115.18)	(0.14)	(0.01)	(10.61)
	[3104.20]	[5.60]	[0.67]	[500.57]
Year 3	485.00***	0.18	0.01	48.96***
	(174.00)	(0.18)	(0.02)	(14.49)
	[4109.33]	[6.40]	[0.72]	[583.51]
Year 4	1290.51***	0.56**	0.05**	91.13***
	(319.74)	(0.25)	(0.02)	(27.55)
	[4942.75]	[7.08]	[0.75]	[650.23]
Ys 1-4 (Avg.)	330.07***	0.07	0.03***	64.59***
	(92.16)	(0.12)	(0.01)	(24.63)
	[3513.88]	[5.56]	[0.67]	[946.54]
Individuals	90,423	90,423	90,423	48,375
Applications	122,195	122,195	122,195	58,078

Source: Administrative data.

Notes: Replicates Table 3, without winsorizing the dependent variables used in Column (1) and Column (4). The number of observations (applicants) for Columns (1)-(3) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Effect of YET on labor outcomes - ITT effects

	(1) Total earnings	(2) Months Pos. earnings	(3) Positive earnings	(4) Wages
Program Year				
Year 0	1420.83***	5.26***	0.42***	-19.67***
	(33.79)	(0.08)	(0.01)	(2.46)
	[1121.21]	[3.02]	[0.47]	[327.04]
Post-Program Years				
Year 1	36.74	-0.04	0.03***	3.29
	(56.79)	(0.09)	(0.01)	(5.68)
	[2121.09]	[4.61]	[0.61]	[409.15]
Year 2	146.63*	-0.01	0.01	18.99***
	(78.46)	(0.10)	(0.01)	(7.19)
	[3087.30]	[5.51]	[0.66]	[501.88]
Year 3	308.83***	0.13	0.01	31.35***
	(118.57)	(0.13)	(0.01)	(9.74)
	[4071.97]	[6.23]	[0.71]	[589.37]
Year 4	812.72***	0.41**	0.04**	53.91***
	(210.12)	(0.19)	(0.02)	(17.34)
	[5148.90]	[6.86]	[0.74]	[682.72]
Ys 1-4 (Avg.)	203.34***	0.05	0.02***	18.98***
	(73.90)	(0.09)	(0.01)	(6.23)
	[3264.50]	[5.56]	[0.67]	[521.43]
Individuals	90,423	90,423	90,423	48,375
Applications	122,195	122,195	122,195	58,078

Source: Administrative data.

Notes: Replicates Table 3, but presents ITT effects rather than ToT effects. Control means are presented in brackets. The number of observations (applicants) for Columns (1)-(3) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Effect of YET on enrollment in education - no controls

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Program Year					
Year 0	0.115***	0.099***	0.010	0.004	0.005
	(0.011)	(0.014)	(0.012)	(0.004)	(0.005)
	[0.760]	[0.523]	[0.209]	[0.018]	[0.024]
Post-Program Years					
Year 1	0.012	0.023*	-0.003	0.002	-0.006*
	(0.014)	(0.013)	(0.013)	(0.005)	(0.003)
	[0.651]	[0.345]	[0.282]	[0.026]	[0.016]
Year 2	0.027*	0.020	0.002	0.004	0.003
	(0.014)	(0.012)	(0.012)	(0.005)	(0.004)
	[0.476]	[0.237]	[0.216]	[0.029]	[0.007]
Year 3	0.016	0.022	-0.012	0.003	0.005
	(0.017)	(0.014)	(0.012)	(0.005)	(0.004)
	[0.368]	[0.182]	[0.162]	[0.028]	[0.005]
Year 4	-0.001	0.008	-0.007	-0.009	0.008
	(0.020)	(0.018)	(0.009)	(0.007)	(0.005)
	[0.225]	[0.149]	[0.045]	[0.030]	[0.004]
Ys 1-4 (Avg.)	0.017*	0.020**	-0.003	0.002	0.001
	(0.010)	(0.010)	(0.009)	(0.004)	(0.003)
	[0.488]	[0.253]	[0.210]	[0.028]	[0.009]

Notes: Replicates Table 5 without including control variables. The number of observations (applicants) is: 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Effect of YET on enrollment in education - one application per participant

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Program Year					
Year 0	0.136***	0.110***	0.020**	0.008**	0.003
	(0.010)	(0.011)	(0.008)	(0.004)	(0.004)
	[0.739]	[0.513]	[0.197]	[0.015]	[0.024]
Post-Program Years					
Year 1	0.044***	0.035***	0.013	0.003	-0.006**
	(0.013)	(0.012)	(0.010)	(0.004)	(0.003)
	[0.617]	[0.332]	[0.263]	[0.022]	[0.015]
Year 2	0.043***	0.026**	0.008	0.009*	0.000
	(0.013)	(0.011)	(0.010)	(0.005)	(0.002)
	[0.457]	[0.228]	[0.208]	[0.025]	[0.007]
Year 3	0.041***	0.032**	0.002	0.007	0.002
	(0.016)	(0.013)	(0.011)	(0.006)	(0.003)
	[0.346]	[0.172]	[0.152]	[0.025]	[0.004]
Year 4	0.013	0.013	-0.002	-0.006	0.010*
	(0.020)	(0.017)	(0.009)	(0.007)	(0.005)
	[0.213]	[0.146]	[0.040]	[0.027]	[0.003]
Ys 1-4 (Avg.)	0.041***	0.028***	0.010	0.006	-0.000
	(0.010)	(0.009)	(0.007)	(0.003)	(0.002)
	[0.461]	[0.244]	[0.195]	[0.024]	[0.009]

Notes: Replicates Table 5, but keeping one application per individual. For participants who were ever offered a job and applied to more than one edition and/or locality, we keep the application for the edition and locality in which they were offered the job. For participants never offered a job, we randomly select one application among all their applications. Results are robust to picking randomly one application for every applicant. The number of observations/applicants is: 90,423 for Year 0-Year 2, 66,595 for Year 3 and 36,183 for Year 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Effect of YET on enrollment. ITT effects

	(1) Any Level	(2) Secondary Education	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Programs
Program Year					
Year 0	0.08***	0.07***	0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
	[0.72]	[0.48]	[0.22]	[0.02]	[0.02]
Post-Program Years					
Year 1	0.01	0.02*	-0.00	0.00	-0.00*
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
	[0.60]	[0.30]	[0.28]	[0.03]	[0.01]
Year 2	0.02**	0.01*	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
	[0.47]	[0.21]	[0.23]	[0.03]	[0.01]
Year 3	0.01	0.02*	-0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
	[0.37]	[0.17]	[0.18]	[0.03]	[0.01]
Year 4	-0.01	0.00	-0.00	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
	[0.20]	[0.14]	[0.04]	[0.03]	[0.01]
Ys 1-4 (Avg.)	0.02**	0.01**	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
	[0.45]	[0.22]	[0.21]	[0.03]	[0.01]

Notes: Replicates Table 5, but presents ITT effects rather than ToT effects. Control means are presented in brackets. The number of observations (individuals) is 122,195 (90,423) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A11: Effect of YET on earnings by aggregate sector

	(1) Total earnings Industry	(2) Total earnings Civil	(3) Total earnings Banking	(4) Total earnings Low Qual.
Program Year				
Year 0	-589.23***	1985.05***	646.73***	-41.01***
	(36.83)	(37.19)	(30.53)	(5.97)
	[871.81]	[37.13]	[9.30]	[52.68]
Post-Program Years				
Year 1	34.79	-6.50	60.08**	-38.18***
	(72.59)	(35.67)	(26.47)	(12.80)
	[1675.14]	[202.07]	[39.03]	[95.01]
Year 2	273.20**	45.85	95.68*	16.08
	(122.45)	(70.04)	(51.93)	(26.71)
	[2486.52]	[299.96]	[62.48]	[92.03]
Year 3	300.29**	36.94	116.24*	-1.46
	(152.12)	(86.63)	(65.13)	(29.63)
	[3331.32]	[440.62]	[80.35]	[130.49]
Year 4	409.21	578.59***	43.96	26.58
	(256.05)	(211.47)	(86.92)	(61.25)
	[4105.23]	[594.97]	[87.36]	[129.02]

Notes: Two stage least squares regressions where we instrument the YET participation dummy with the offer to take the YET job. In Column (1), the dependent variable is earnings in firms belonging to the Industry/Trade sector. Columns (2) to (4) are resp. for the Public Sector (excluding public employees in public industries or banks), the Banking sector, and for Low-qualification jobs (construction, domestic workers and rural workers). Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Earnings are winsorized at the top 1 percent of positive values and converted into U.S. dollars. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. The number of observations (individuals) is 122,194 (90,422) for Year 0-Year 2, 90,205 (72,886) for Year 3 and 46,544 (46,008) for Year 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A12: Effect of sector of program job on earnings

	(1) Total earnings Year 0	(2) Total earnings Avg. Ys 1-4	(3) Enrolled Any level Year 0	(4) Enrolled Any level Avg. Ys 1-4
Program job in Banking	350.71***	333.04	-0.01	-0.00
	(51.98)	(210.37)	(0.02)	(0.02)
Program job in Industry	198.87	-11.44	0.01	0.02
	(169.80)	(501.54)	(0.03)	(0.04)
Control Mean (Civil Sec.)	2908.48	3354.19	0.88	0.50
Observations	1,994	5,838	1,994	5,838
Individuals	1,895	1,895	1,895	1,895

Notes: OLS regressions of earnings and enrollment in education on the sector of the program job. The sample is restricted to treated participants and the omitted reference category is the civil sector, which include all state-owned companies that are not in banking or industry. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis. Columns (2) and (4) present the results for pooled OLS regressions. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A13: Effect of working in program firm during program year

	(1) Total Earns.	(2) Pos. Earns.	(3) Wages	(4) Enrolled Any Level
	Avg Ys 1-4			
Treated (at least one month)	267.817***	0.027***	25.043***	0.020**
	(97.143)	(0.010)	(8.215)	(0.009)
CCM	3040.765	0.657	502.318	0.475
Observations	381,139	381,139	253,957	381,139
Individuals	90,423	90,423	73,681	90,423

Notes: Pooled two stage least squares regressions where we instrument a dummy variable taking the value of one if youth work at least one month in a program firm job during the year of the program with the offer to take the YET job. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. The control complier mean is obtained as the difference between the average outcome for compliers offered a YET job and the estimated local average treatment effect. To recover the former from the data we assume that the average outcome for and the share of always takers is the same among those offered and not offered a YET job. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A14: Effect of working and studying during program year

	(1) Total Earns.	(2) Pos. Earns.	(3) Wages	(4) Enrolled Any Level
	Avg Ys 1-4			
Work and Study	477.791***	0.048***	51.617***	0.036**
	(172.494)	(0.017)	(16.968)	(0.017)
CCM	2338.297	0.562	473.650	0.507
Observations	381,139	381,139	253,957	381,139
Individuals	90,423	90,423	73,681	90,423

Notes: Pooled two stage least squares regressions where we instrument a dummy variable taking the value of one if youth work (positive yearly earnings) and study (enrolled at any level) during the program year with the offer to take the YET job. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis and control complier means in brackets. The control complier mean is obtained as the difference between the average outcome for compliers offered a YET job and the estimated local average treatment effect. To recover the former from the data we assume that the average outcome for and the share of always takers is the same among those offered and not offered a YET job. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A15: Effects of YET - double-reweigthed ever-offer estimator

	(1) Year 0	(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 4
Earnings	1,992***	99.48	261.9**	400.8**	1,116***
	(41.70)	(92.50)	(125.0)	(185.3)	(348.8)
Enrollment	0.117***	0.016	0.030*	0.015	-0.011
	(0.012)	(0.015)	(0.016)	(0.019)	(0.023)
Applications	113,391	113,391	113,391	83,230	41,720
Applicants	85,290	85,290	85,290	68,196	41,420

Notes: This table presents the DREO estimator of Behaghel et al. (2018). The DREO accounts for potential bias due to larger shares of compliers in the offer group of randomized waiting-list designs. The Earnings results compare well to Column (1) of Table 3, the Enrollment results to Column (1) of Table 5. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A16: Soft skills and employment in the control group

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Big 5 and grit					
	Open	Conscientious	Extrav Scale 1-5	Agreeable	Neurotic	Grit
Employed	0.131***	0.103**	0.051	0.007	-0.065	0.063
	(0.048)	(0.052)	(0.074)	(0.055)	(0.087)	(0.057)
mean of depvar	4.031	3.809	3.646	3.681	3.429	3.721
sd of depvar	0.493	0.565	0.734	0.533	0.835	0.579
	Panel B. Soft Skills Related to Labor Market					
	Finish on time	Adapts fast	Teamwork important Scale 1-5	Punctual	Index (1-4)	Unpunctual Interview
Employed	0.084	0.208***	0.106	-0.026	0.093*	0.023
	(0.062)	(0.066)	(0.066)	(0.076)	(0.049)	(0.016)
mean of depvar	4.068	3.994	4.248	4.215	4.131	0.023
sd of depvar	0.679	0.650	0.677	0.811	0.494	0.149
Individuals	664	664	664	664	664	664

Source: Survey.
Note: OLS regression of soft skills measures on employment status in the control group. Standard errors clustered at the individual level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A17: Effect of YET on social transfers, by baseline household vulnerability

	(1) Vulnerable	(2) Highly Vulnerable	(3) Vulnerable	(4) Highly Vulnerable
	Year 0		Avg Ys 1-4	
Treated (T)	-0.005	-0.000	0.002	-0.007***
	(0.005)	(0.003)	(0.007)	(0.002)
T * Vulnerable	-0.189***	-0.004	-0.046	-0.001
	(0.032)	(0.015)	(0.029)	(0.013)
T * H. Vulnerable	0.057	-0.100**	-0.029	0.010
	(0.056)	(0.048)	(0.056)	(0.049)
Vulnerable	0.711***	0.062***	0.412***	0.054***
	(0.003)	(0.002)	(0.004)	(0.002)
Highly Vulnerable	0.120***	0.796***	0.140**	0.351***
	(0.004)	(0.004)	(0.006)	(0.005)
CCM No Vulnerable	0.023	0.002	0.040	0.008
Observations	122,195	122,195	381,139	381,139
Individuals	90,423	90,423	90,423	90,423

Source: Administrative data.

Notes: two stage least squares regressions where we instrument the YET participation dummy, and the interaction with Vulnerable and Highly Vulnerable dummies with a job offer dummy and the corresponding interactions. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis. Columns (3)-(4) report the results from pooled regressions over years 1-4 after the program, while for columns (1)-(2) we conduct a cross-sectional regression for the year of the program. **Vulnerable**: dummy for being in a household receiving a cash transfer (26% of the sample) either the month before the program (used as independent variable), or for the month of April in the corresponding year after the program (dependent variable) **Highly Vulnerable**: dummy for being in a household receiving a cash transfer and a food card (9% of the sample) either the month before the program (used as independent variable), or at any month for the corresponding year after the program (dependent variable). It is a subset of the Vulnerable category. **CCM**: control complier mean of the dependent variable among those who are not vulnerable. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A18: Treatment Effect Heterogeneity by Gender

	(1) Enrolled Any Level	(2) Total Earns.
	Avg Ys 1-4	
Treated (T)	0.022	413.830**
	(0.017)	(186.293)
T * Female	-0.000	-206.304
	(0.021)	(221.441)
Female	0.019***	-780.521***
	(0.002)	(24.818)
p-value T+T*Female=0	0.090	0.086
Observations	381,139	381,139
Individuals	90,423	90,423

Source: Administrative data.

Notes: pooled two stage least squares regressions where we instrument the YET participation dummy, and its interaction with a female dummy with a job offer dummy and the corresponding interaction. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age below 18 at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Standard errors clustered at the applicant level shown in parenthesis. **p-value**: p-value of the test that the treatment effect for females is zero (sum of the treated and interaction coefficients). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A19: Effects during the program: expected returns to education

	(1)	(2)	(3)	(4)
	Expected probability (in%) of finding a job when one finishes...			
	3 years of high school	6 years of high school	tertiary education	university
Treated	-2.156	2.864*	0.753	-0.497
	(1.478)	(1.515)	(1.250)	(0.934)
CCM	42.22	70.60	85.33	94.30
Applications	1,366	1,366	1,366	1,366
Applicants	1,272	1,272	1,272	1,272

Source: Survey.

Note: IV estimates of Eq. (1). The dependent variable in Column (1) is the answer to the following survey question: "What is the probability of finding a job when one finishes the first 3 years of high school?" Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. * p<0.1.

B Computation of the share of summer jobs over total employment while in school

In this Section, we explain how we compute the contribution of summer jobs to overall employment of teenagers enrolled in school for the US and Uruguay.

Summer jobs have been the focus of recent papers in the US. We estimate the incidence of summer jobs on overall employment of 16-19 year-old teenagers enrolled in school. Summer jobs are not easy to isolate from aggregate employment and education statistics. If we define summer jobs as jobs starting and ending within the summer, we need detailed data on labor market transitions and on enrollment transitions to identify them. Instead, we focus on summer employment (June-July-August in the US), which is a larger category that includes summer jobs. Some summer employment starts before the summer or ends after it.

We use aggregate statistics from the 2017 Current Population Survey. From Table A-16 published in the website of the Bureau of Labor and Statistics,[70] we compute the employment rate of teenagers (16-19) enrolled in school, excluding summer months (June-July-August), and we obtain a share of 23%. The employment rate of enrolled teenagers remains stable over the summer months, probably because of a composition effect: the enrollment rate during the summer drops from 83% to 52%. As teenagers enrolled during the year who take summer jobs probably declare themselves as non-enrolled over the summer, we need to correct our estimates of summer employment for teenagers who regularly attend school. We then assume that the entire summer increase in jobs held by teenagers who report themselves as non-enrolled over the summer is due to teenagers enrolled in non-summer months. A priori, this yields an upper bound estimate of the employment rate of the enrolled population, which then amounts to 31%. Summer employment then contributes to 31% of yearly employment ($= 0.31/(0.31 + 3 * 0.23)$). This number is that reported in the introduction.

We also propose an alternative and less conservative estimate of summer jobs contribution. With aggregate monthly data, we assume that summer jobs correspond to the net increase in jobs over the summer months. As the employment rate increases from 23% to 31%, the net increase is 8 percentage points. Then we obtain a

[70] Tables are available at: https://www.bls.gov/opub/ee/2017/cps/monthly.htm

yearly contribution of summer jobs of 8% (= $(0.31 - 0.23)/(0.31 + 3 * 0.23)$).

We compute the contribution of summer employment in Uruguay using our administrative data on applicants. We take the ratio between the total number of youth working in summer months (Dec-Feb) over the total number of youth who work from the first July to the next June after they apply to the program. This calculation gives us a share of summer jobs equal to 28%, which is constant for all cohorts of the program (2012-2015).

C Program Youth vs Youth Population

Table C1 describes selection into program application. The Population Census conducted in Uruguay in 2011 registered 255,338 youth aged 16 to 20 (Column 1). Only 132,968 (54 percent) of them were attending school (Column 2). If we consider this number as the population eligible to participate in the program, then we have an application rate of 34.6 percent in the 2012 edition of the program. Two caveats are in order with this estimate. First, candidates could register into school in 2012 in order to apply to the program, which means that we overestimate the application rate. Second, some students in Column (2) worked formally for more than 90 days, which would lead us to underestimate the application rate. The second bias is probably moderate though, as only 7 percent of youth attending school earned positive income in a formal job (contributing to social security). In Column (3), we report the characteristics of the population of applicants - as declared on their application forms - to the 2012 edition.

Columns (2) and (3) allow to compare the characteristics of the eligible population and of the applicants, which are overall quite similar. Women and youth aged 19-20 are just slightly over-represented in the applicants' sample. We also see a share of applications in Montevideo larger than the fraction of people living there, which can be linked to the fact that participants are willing to move to the capital in order to work there. Finally, the share of youth coming from highly vulnerable households (those receiving a social food card) is similar between the applicant pool and the general population.

Column (4) presents the characteristics of the average applicants across the first three editions of the program, our main sample, we see a slight increase in the

Table C1: Characteristics of youth in Uruguay

VARIABLES	(1) Census All 2011	(2) Census Studying 2011	(3) YET First Ed. 2012	(4) YET Ed. 1-3 2012-2014
Female	0.49	0.55	0.58	0.60
Age 16-18	0.62	0.72	0.70	0.72
Age 19-20	0.38	0.28	0.30	0.28
Montevideo	0.38	0.42	0.52	0.49
Enrolled	0.54	1.00	1.00	1.00
Highly Vulnerable Household *	0.12	0.08	0.09	0.09
Worked formally last month *	0.14	0.07	0.06	0.07
Individuals	255,338	132,968	46,008	90,423
Applications			46,544	122,195

Source: Census 2011, YET Application Forms and Continuous Household Survey 2013 (ECH).
Notes: **Census Studying**: sample restricted to those who reported being currently attending an educational institution. **Montevideo**: based on locality of residence in Columns (1) and (2), and on locality for which they submitted the application in Columns (3) and (4). **Enrolled**: currently attending an educational institution. We impute a value of one to YET participants since everyone reported being enrolled at the application stage. **Highly Vulnerable Household**: respondent lives in a household receiving TUS food card. **Worked Formally Last Month**: for Columns (1) and (2) we use an indicator for reporting positive income in the month before the survey in a job that contributes to social security (formal). For Columns (3) and (4) we use an indicator for having positive income in the social security data the month before the application to the program. * Values reported in Columns (1) and (2) are from the 2013 household survey (ECH) since information is not available in the census.

share of women, and younger teenagers in comparison to the first edition, but overall the composition of applicants does not vary much over time and it is not very different from that of the general population of this age.

D Further evidence from the in-house survey

In this section, we describe in greater detail what happens during the program year, more precisely just before the program jobs end (9-12 months after the lottery). For some dimensions, such as education and labor market outcomes, we then document the exact content of the program, and compliance to the program rules.

Table D1 shows that, among survey respondents, the control group and the group of youth receiving a program job offer are overall balanced on baseline characteristics.

Table D2 reports the effect of being offered a program job on employment, educational enrollment and total income. This Table draws the big picture of the treatment group situation around the end of the program. Overall the estimates are in line with the evidence from administrative data at the same horizon. By the end of the program, the treatment group still experiences a significant increase in employment rates by 49 p.p out of a mean of 23 percent in the control group. The enrollment rate in education is also significantly higher in the treatment group by 9 p.p. (while 3 out of 4 youth are enrolled in education in the control group). Beyond marginal distributions, we obtain a significant increase in the share of students working and studying, the main first-stage objective of the program. Conversely, the program decreases the share of young youth who are neither in employment, education, or training (NEETs) by 12 p.p., which represents 60 percent of the mean for compliers in the control group. Column (5) of Table D2 reports the treatment effect on total monthly income (converted in dollars at the exchange rate at the time of the survey). Treated students earn $147 more on average. The program more than doubles the monthly income of youth.

Table D3 presents treatment effects on whether students are studying in public or private institutions. Conditionally on being enrolled, there are no effects on the type of schools students are enrolled at the end of the program year.

Tables D4 to D6 describe the employment experiences of program applicants: their employers, their jobs and their tasks, respectively. The estimation samples are restricted to employed youth, so results can be affected by selection and should be interpreted as descriptive evidence. Consistent with the program description above and with its objectives, employment is almost exclusively formal in the treatment

Table D1: Balance checks between treatment and control groups - respondents to the survey of the 5th edition

	(1)	(2)	(3)	(4)	(5)
	Control		Offered		
	Mean	sd	Mean	sd	p^+
Observations	666		703		
p-value F test*					0.115
Panel A. Demographics					
Female	0.65	0.48	0.64	0.48	0.39
Age	17.71	1.40	17.84	1.42	0.16
Number of kids	0.03	0.17	0.02	0.16	0.60
Father completed high school	0.29	0.45	0.32	0.47	0.52
Mother completed high school	0.42	0.49	0.43	0.49	0.70
More than 10 books at home	0.48	0.50	0.50	0.50	0.69
Panel B. Education and Social Programs					
School: hours per day	5.48	1.66	5.47	1.45	0.84
School: morning shift	0.41	0.49	0.49	0.50	0.02
School: afternoon shift	0.42	0.49	0.36	0.48	0.02
School: evening shift	0.17	0.37	0.15	0.36	0.97
School: Secondary Academic	0.60	0.49	0.54	0.50	0.06
School: Secondary Technical	0.25	0.43	0.26	0.44	0.64
School: Non-Formal Education	0.02	0.12	0.02	0.15	0.77
School: Teacher's College	0.01	0.09	0.02	0.12	0.20
School: Tertiary	0.01	0.10	0.03	0.17	0.00
School: University	0.11	0.32	0.13	0.34	0.57
Enrolled the year before the program (Sec or Tert.)	0.93	0.25	0.95	0.22	0.22
Repeated grade once in primary school	0.12	0.33	0.14	0.35	0.59
Household Receives Cash Transfer	0.19	0.39	0.16	0.37	0.56
Household Recipient of Food Card	0.12	0.33	0.10	0.30	0.27

Source: Survey and administrative data on applications.
Note: + p-value reported in column (5) is obtained from a regression of each variable on being selected in the lottery with clustered standard errors at the applicant level and controlling for locality dummies and number of applications. *p-value corresponding to the joint-hypothesis test in a regression of the treatment indicator on all variables presented in the table, the regression also controls for edition dummies, locality dummies and number of applications.

Table D2: Effects during the program: employment and education status.

	(1) Employed	(2) Study	(3) Work & Study	(4) NEET	(5) Tot. income month, $
Treated	0.488***	0.087***	0.452***	-0.123***	147.4***
	(0.035)	(0.029)	(0.034)	(0.024)	(15.02)
CCM	0.231	0.759	0.179	0.190	112.8
Applications	1,366	1,366	1,366	1,366	1,366
Individuals	1,272	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. CCM: Control Complier Mean. *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Effects during the program: public vs private education.

	(1) Study	(2) Public School
	Any Level	
Treated	0.087***	0.005
	(0.029)	(0.011)
CCM	0.759	0.972
Applications	1,366	1,080
Individuals	1,272	997

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. CCM: Control Complier Mean. *** p<0.01, ** p<0.05, * p<0.1.

group, while almost one third of the control group is employed in informal jobs (defined as those that do not contribute to social security). Column (2) of Table D4 shows that 94% of treated teenagers report being employed in the public sector, while in the control group less than 1 out of 5 applicants are working in that sector. This is consistent with the list of employers offering jobs on the program website. Actually, survey respondents in the treatment group declare that their main employers are: the National Bank (22 percent), the state-owned electricity company (19 percent), the state-owned telephone company (9 percent) and the state-owned oil and gas company (6 percent). These four largest employers hire 56 percent of the treatment group. Similarly, treated employees are significantly more likely to work in larger firms (larger than 50 employees), in the manufacturing industry, in the financial services and public services (industry classification in the survey is more detailed than in the administrative data). In a nutshell, the program crowds out small, informal employers from the retail trade industry, the main employer type in the control group.

Table D4: Effects during the program: employers type

	(1) Formal	(2) Public Employer	(3) Small firm < 50	(4) Manuf.	(5) Retail Trade	(6) Fin. services	(7) Public services
Treated	0.279***	0.769***	-0.413***	0.208***	-0.425***	0.353***	0.090**
	(0.042)	(0.049)	(0.055)	(0.035)	(0.053)	(0.033)	(0.039)
CCM	0.691	0.168	0.620	0.076	0.452	-0.014	0.117
Observations	641	641	631	641	641	641	641
Individuals	587	587	577	587	587	587	587

Source: Survey.
Note: OLS estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses.
Industry classification differs in the survey and in the administrative data. For example, state-owned companies producing electricity are classified in the manufacturing industry in the survey, and in the civil sector in the administrative data. *** p<0.01, ** p<0.05, * p<0.1.

Table D5 shows that treated youth are more satisfied with their job: there is a statistically significant increase by two thirds of a standard deviation in our job satisfaction index. Column (2) of Table D5 also shows that the share of part-time work (less than 29 hours per week) is significantly higher in the treatment group.

This translates into a lower total monthly wage. More importantly, (log) hourly wages paid to treated students are significantly higher than those paid to control group workers, this amounts to an increase in levels of 16 percent over the control mean.

Table D5: Effects during the program: jobs type

	(1) Job satisf. (scale 1-5)	(2) Part-time work < 29 hours	(3) Total wages month, dollars	(4) Hourly wage log, dollars
Treated	0.686***	0.324***	-44.38**	0.160***
	(0.115)	(0.0594)	(19.23)	(0.0583)
CCM	3.646	0.327	364.6	2.325
Control sd	1.062	0.474	209	0.653
Applications	641	641	641	627
Individuals	587	587	587	573

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In Table D6, we describe the occupations and tasks performed by employed youth. Consistent with the industries of the program employers, treated youth are much more likely to work as clerks: 93 percent of treated youth are clerks compared to 43 percent in the control group. Consequently, treated youth are much more likely to read, write and use computers on a daily basis in the workplace (Columns 2 to 4). Treated youth are less likely to measure weights or distances during their workday (Column 5). They declare that their work is less physically demanding (Column 6): we see a decrease in half a standard deviation in an index capturing how physically demanding the job is.[71] Surprisingly, treated employees declare that they have less frequent interactions with their colleagues, this could be due to the fact that they work in larger firms. Although their job is closer to office work, they might be less likely to work in teams (Column 7).

[71]Table D7 provides further details on the job tasks: treated youth read more pages and are less likely to carry heavy loads.

Table D6: Effects during the program: occupation & tasks

	(1) Clerical occupation	(2) Reading	(3) Writing	(4) Computers every day	(5) Measuring weights,dist.	(6) Physically demand. (scale 1-10)	(7) Freq. meetings colleagues
Treated	0.493***	0.275***	0.184***	0.470***	-0.137***	-1.509***	-0.195***
	(0.054)	(0.056)	(0.056)	(0.054)	(0.048)	(0.294)	(0.056)
CCM	0.435	0.562	0.542	0.381	0.252	4.367	0.392
Control sd	0.493	0.500	0.498	0.490	0.448	2.785	0.492
Applications	641	641	641	641	641	641	641
Applicants	587	587	587	587	587	587	587

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D7: Effects during the program: more details on tasks of employed youths

	(1) Pages read	(2) Pages written	(3) Carry > 25 kg
Treated	3.257**	0.609	-0.150***
	(1.316)	(0.583)	(0.043)
CCM	4.987	1.436	0.235
Control sd	11.88	4.457	0.439
Observations	641	641	641
Applications	587	587	587

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Standard errors clustered at the individual level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.