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

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Abstract

Does working while in school smooth students' transition into the labor market? We provide evidence on this question by leveraging a one-year work-study program that randomized job offers among over 90,000 student applicants in Uruguay. Program rules forbade employers from employing participants in the same job after program completion, and less than 5 percent of participants ever worked in the same firm again. Two years after the program, participants had 8 percent higher earnings. Our results suggest that the program's focus on work-related skills was a key mechanism for earnings impacts.

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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 work-study program in the U.S.). 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., work attitudes such as teamwork and adaptability), either general or sector-specific (Heckman, Stixrud and Urzua, 2006; Adhvaryu, Kala and Nyshadham, 2018; Alfonsi et al., 2020). 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 youth employment program offered by lottery in Uruguay. The program targets students aged 16 to 20 throughout the country, offering them a first formal work experience in the main state-owned companies (e.g., the government-owned electricity company, telecommunications

[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).

company, national bank, etc.). Every year, around 850 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. These characteristics of the Uruguayan program also appear in other work-study programs, such as the Federal Student Work Experience Program in Canada, and the Federal work-study program in the U.S., which both offer part-time jobs 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). First, offers to participate in the program are randomly allocated. Second, 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 less likely to be an issue in our case (Czibor, Jimenez-Gomez and List, 2019). Third, the work-study program explicitly states enhancing students' skills as an objective, and the program rules prevent program firms from keeping participants on the same job after the end of the program. Thus, the Uruguayan program cannot work as a direct placement program, and our study then analyzes the other channels at play when students work while in school.

We use rich administrative data that allow us to recover the main labor and education outcomes for all applicants, reducing concerns about attrition. The data cover the universe of around 90,000 lottery participants. 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.[2] More importantly, we find statistically significant positive effects on yearly earnings and employment after the end of the program. The effect on earnings is of US$242 two years after the program, which represents 8% of the earnings of comparable youth in the control

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

group. It is driven by both effects on employment at the extensive margin, and on wages conditional on employment. The effect on wages amounts to 6% of the complier control mean, and survives a bounding analysis that accounts for selection into employment. This 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, school retention increases by 12 percentage points, consistently with the program conditionality on enrollment. 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 4 percentage points higher than in the control group, when around 50% of youth are enrolled. In line 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. The enrollment effect is stronger for poor households than for non-poor households, which gives support to the hypothesis that credit-constrained youth save the income shock due to program wages to finance extra years of education. Our survey data also indicate that treated youth expect higher returns to secondary education, which might foster investment. Furthermore, we provide evidence that the extra education acquired in the treatment group is of the similar quality to that 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 large enough to significantly affect school grades. Grades obtained by participants during the program year are not lower than those in the control group. Time-use data indicate that 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. Reducing the number of youth in this group, which is close to the NEET (Not in Employment, Education or Training) category, is a key policy objective around the world, and it is also a priority for the Uruguayan government.

In summary, the work-study program smooths the students' transition to the labor market. It builds human capital through the education channel, as educational attainment increases by 0.17 years. Beyond the cognitive skills typically acquired in school, we find small program effects on soft skills. Among the Big 5 personality traits, the program significantly increases conscientiousness by 10% of a standard deviation. It also improves work attitudes that are related to time management and flexibility by around 15% of a standard deviation. As program youth get work experience, they accumulate work-related skills. Furthermore, the human capital acquired through the work-experience channel is not sector specific, but seems rather general. We do not find evidence that earnings effects are concentrated in the sectors of the program firms, or that they differ by program firm's sector. The human capital transferability seems to be a strength of the Uruguayan work-study program.

Overall, we do not find significant heterogeneity in program effects, except in enrollment effects which are stronger for students from poor households. This suggests that our results might be relevant to work-study programs targeting college students in financial needs, as the U.S. Federal Work-Study program does. Finally, we conduct an extrapolation exercise to predict program effects on earnings later in the life cycle. We make the conservative assumption that the extra work experience acquired by treated students during the program and the two following years does not matter in the long run, and then only the extra education fostered by the program contributes to mid-career wages. Using Mincerian returns to education, we predict that earnings of treated students would be around 1.7% higher than those of control students.

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 in the U.S (Ruhm, 1997; Hotz et al., 2002; Ashworth et al., 2020). Over a short horizon after graduation, our results point to significant earning returns to completing a part-time job in state-owned companies, for both men and women.[3] In contrast, the literature seems to have reached a consensus pointing to limited effects of working while in school on educational outcomes (Eckstein and Wolpin, 1999; Stinebrickner and Stinebrickner, 2003; Buscha et al., 2012). We confirm the lack of negative effects and further show

[3]Overall, we do not find evidence of statistically significant treatment effect heterogeneity by gender on labor outcomes or enrollment.

that some of the large positive effects on enrollment during the program year persist beyond that year, even when the enrollment conditionality of the work-study program no longer holds.

Our study also contributes to the literature evaluating students' employment programs. We find stronger positive effects on labor market outcomes than recent experimental evaluations of summer job programs in the U.S. (Gelber, Isen and Kessler, 2016; Davis and Heller, 2017) and in the Philippines (Beam and Quimbo, 2021), which offer shorter-duration jobs.[4] On the contrary, our findings are in line with the recent non-experimental evaluation of the Federal Work-Study program in the U.S., which also offers subsidized employment throughout the school year (Scott-Clayton and Minaya, 2016).

Finally, our paper contributes to the literature evaluating active labor market policies (ALMP) using social experiments and randomized control trials (for recent surveys or meta-analyses see Card, Kluve and Weber, 2017; McKenzie, 2017; Escudero et al., 2019). Our paper is the first to our knowledge to provide evidence based on randomized lotteries on a work-study program. 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).

The paper proceeds as follows. Section I describes the Uruguayan work-study program. Section II discusses theoretical insights of the main expected effects of the program. Section III presents the data and the econometric model. Section IV delivers causal estimates of the program effects on core labor market and education outcomes. Section V discusses suggestive evidence on mechanisms. Section VI summarizes results on treatment effect heterogeneity and discusses effects to be expected beyond the two years after the program. Finally, Section VII concludes. Codes and public data are available for replication (Le Barbanchon, Ubfal and Araya, 2021*a*).

[4]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 obtained the share of summer employment for teenagers in the U.S. from 2017 CPS data (U.S. Bureau of Labor Statistics, 2017), and that in Uruguay using the administrative data for the control group in our sample. See Appendix F for details on the computation.

I The Uruguayan work-study program

Since 2012, the work-study program *"Yo Estudio y Trabajo"* (YET) provides youth aged 16 to 20 who live in Uruguay with a first formal work 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 (see Appendix C for more institutional details).

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. Using the microdata including all observations in the 2011 Population Census (Instituto Nacional de Estadistica Uruguay, 2011, 2013), 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 D for details).

Assignment to the program is done by lottery at the locality level.[5] 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. They have to attend a two-day orientation workshop provided by the National Institute of Employment and Professional Training and are assigned a supervisor who follows their progress in the program. Participants staying in 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 per-

[5] Candidates select the locality in which they want to participate, which is supposed to be that in which they live and/or study.

forms 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.[6] For example, high school is organized in morning or afternoon shifts. Students attending the morning shifts at school are matched to firms where they can work in the afternoon (and vice versa). This process implies that there is very little job-candidate matching in terms of skills.

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 amounted to $446 per month for a 30-hour-per-week job in 2016 (around $3.7 per hour).[7] The program wage compares favorably to the national minimum wage fixed at $372 per month for a full-time job.

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. For example, the four main program employers of the fifth edition 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 E for more details on the program firms of the fifth edition).

[6] 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.

[7] Throughout the paper, we convert Uruguayan pesos to U.S. dollars using the January 2016 exchange rate of 0.033 dollars per peso.

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 E 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 soft skills twice: during the program and at the end of it.

There are between 30 and 46 thousand applicants to each of the first three program editions. However, there are less than a thousand program jobs offered every year (see Appendix Table A1). Consequently, the share of participants offered a job is between 2 to 3 percent, implying a low probability of obtaining a program job. 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. We explain how we handle repeated applications when we discuss the empirical specification.

II 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 (e.g., knowing how to write business reports). 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, Stixrud and Urzua, 2006; Groh et al., 2016; Acevedo et al., 2017; Adhvaryu, Kala and Nyshadham, 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 described above, we expect early work experience to have a signaling role. When 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.[8] We do not expect 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 direct placement 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 (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

[8] 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 informative 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.

participants. Program earnings could help credit-constrained youth 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 V, we explore the mechanisms and conduct heterogeneity analysis documenting the various channels.

III Data and econometric model

III.A 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 Appendix Table A1), 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 from the National Administration of Public Education and the State University cover enrollment in public education institutions (secondary, tertiary, universities and out-of-school programs) at a yearly frequency. 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 year of the program.

Table 1 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 applicant 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.

Among lottery applicants, around 71 percent are enrolled in public secondary education, 49 percent are in academic schools, in general regarded as more prestigious, and 22 percent in technical schools (see Appendix C for more details on the education system in Uruguay). Around 15 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. 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.

One youth in four lives in a vulnerable household that receives a conditional cash transfer, which is targeted at the 200,000 poorest households in the country. Households receiving also a food card, granted to the poorest 60,000 households in the country, are considered highly vulnerable. 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 around $170.[9] On average, applicants worked for 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

[9]Throughout the paper, we winsorize earnings at the top 1 percent.

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 81 percent, though this slightly higher attrition rate does not generate imbalances in baseline covariates between offer and control students (see Appendix Table E1).

III.B Econometric model

In the main analysis, we focus on Treatment effects on the Treated (ToT). We define treatment as working at least one month in a program job. 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 work in a program job if not offered the program (i.e., there are no always takers). This effect is identified under the following exclusion restriction: the only reason why youth who are offered the program see their outcomes affected is that they work in a program job. In the appendix, we present intention-to-treat estimates (ITT) that do not rely on the exclusion restriction, and we obtain consistent results. We also explore an alternative definition of treatment that allows us to estimate a parameter that may be closer to the effect of working while in school, but relies on stronger assumptions. Under this alternative specification, we define treatment as working in any firm while being enrolled in school during the program year. Results are even stronger, and overall consistent with our main estimates (see Appendix Table A12). This alternative specification assumes that the *type* of in-school job has no effect on future labor and educational outcomes. In particular, it assumes that there are similar effects of program jobs and of the potential control jobs students would have accepted if they had not been offered a program job. Since program jobs are well-paid temporary jobs, we see this alternative specification as less appropriate.

The ever-offered variable (instead of the first round of offers) is a reasonable instrument in the context of randomized waiting lists with small offer rates (de Chaisemartin and Behaghel, 2020).[10] In practice, the first stage is strong - 77% of youth receiving a program offer work in a program job -, and it is homogeneous across program editions (see Appendix Table A2).

We analyze data at the applicant level and deal with applicants who apply several times in the following way. We randomly select one application for each youth in the control group (who are never offered a program job). To maximize statistical power, we select the application generating an offer for each applicant receiving at least one offer. Results are robust when selecting a random application in the ever-offered group, or when analyzing the data at the application level (see Appendix Tables A13 and A14).[11]

We consider the following specification at the applicant level i in edition e:

$$Y_{i,t} = \alpha_1 + \gamma_t Treated_i + Locality \times EditionFE + QuotaFE + \#App_i + \rho_t X_{i,0} + \epsilon_{i,t} \tag{1}$$

$$Treated_i = \alpha_2 + \delta Offered_i + Locality \times EditionFE + QuotaFE + \#App_i + \beta X_{i,0} + v_i \tag{2}$$

where $Y_{i,t}$ is the outcome of individual i, t periods after the application date in edition e. $Treated_i$ indicates whether individual i worked in a program job offered in edition e. $Offered_i$ indicates whether individual i received a program job offer. 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 : $\#App_i$ in edition e. To increase precision, we include a vector of covariates $X_{i,0}$ measured at application date. It comprises gender, age, whether the youth comes from a vulnerable or highly vulnerable household, earnings and level of education in the year before applying to the program. 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.

[10] In Appendix Table A15, we verify that alternative estimators, namely the double re-weighted ever offer estimator of de Chaisemartin and Behaghel (2020), yield robust results.

[11] When selecting at random among the offered group, the treatment effect estimate suffers from an attenuation bias because of measurement error in the treatment variable.

IV 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, and analyze effects until 2 years after the program year.[12] 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 such adjustment.[13]

IV.A 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.[14] 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 $800 per quarter (Y-axis on the right side of the graph), 2 years after the program. By contrast, the average earnings of treated individuals rise sharply just after application, and remain on a plateau of about $800 per quarter over the year of the program. Around one year after the start of the program,[15] treated

[12]In Online Appendix B, we restrict the sample to the first edition of the program and present results until 4 years after the program year.

[13]We obtain family-wise adjusted p-values using the implementation by Jones, Molitor and Reif (2019) of the free step-down procedure of Westfall and Young (1993).

[14]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 work in a program job and the ToT effect.

[15]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

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, 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 almost $100 per quarter (Y-axis on the left side of the graph) by the end of the period covered by our data.

Earnings Effects Table 2 summarizes the treatment effects on yearly earnings (in Column 1), on employment (in Columns 2 and 3) and on monthly wages (in Column 4). During the program year, treated youth earn $1,864 more than control youth, whose yearly earnings are around $1,000 (Column 1, Row 1). Row 2 reports the effects during the year after the end of the program (labelled Year 1), and Row 3 two years after (labelled Year 2). Treatment effects on yearly earnings are positive at all horizons, and statistically significant in Year 2.[16] They increase over time from $86 up to $242 in the second year after the program, corresponding to an increase in yearly earnings from 4% to 8%.[17]

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 $42 (=242-200).

Employment Effects Earnings effects are partly driven by employment effects at

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.

[16]The Online Appendix presents a series of robustness checks. Results are robust to not including controls $X_{i,0}$ in the regression (Table A3), clustering standard errors at the locality level (Table A4), not winsorizing earnings (Table A5) or computing ITT effects (Table A6).

[17]To adjust for multiple hypotheses, we consider that we are testing for 8 hypotheses (as many as post-program outcomes). Out of 8 coefficients, the main change is for that on positive earnings in year 2, which is no longer statistically significant at the 10 percent level.

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. 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 do not fully account for the yearly earnings effects.

Wage Effects Column (4) of Table 2 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 in a separate analysis below. *Monthly* wages in program jobs are lower than the wages of employed youth in the control group by $23 (7%). 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 E5). 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 $28 higher, corresponding to a 6% increase over the control mean.

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 3 first reports the ITT effects on wages of employed youth. We obtain statistically significant positive effects in Year 2, as in the ToT analysis in Table 2. 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 had not participated 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 their 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 3 reports that the lower bound of the causal wage effect is positive and statistically significant in Year 2. We construct confidence intervals for the identified interval following the procedure described in Imbens and Manski (2004). In Year 2, the confidence interval excludes zero.[18]

Overall, this suggests 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. There are several mechanisms that could trigger such a productivity effect. We explore them in Section V.

Comparison with previous literature We can compare our results with three papers that provide evidence on the effects of working while in school 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 years after high-school graduation for both men and women. Second, Hotz et al. (2002) take into account dynamic selection into employment for a male sample, and find returns that are not statistically significant. Third, Ashworth et al. (2020) use a new dynamic selection model that incorporates two unobserved random factors and estimate significant long-run returns to in-school work among men. We estimate effects on earnings (8%) and wages (6%) equal to half of what Ruhm (1997) obtains for in-school work in the U.S. Our wage effects are comparable in magnitude to those found by Ashworth et al. (2020), and larger than those found by 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). Last, we compare our findings to a fourth paper evaluating the U.S. Federal Work-Study (FWS) program (Scott-Clayton and Minaya, 2016). Every year, FWS provides wage subsidies to around 600,000 university students working in part-time jobs, mostly on-campus. Scott-Clayton and Minaya (2016) find, in line with our results, that working in a FWS job increases youth employment rate 6 years after college entry by 2 percentage points. Compared with this previous literature, we study effects on wages observed when youth are younger (around 20 years old). One key question is whether our effects

[18] Lee bounds are conservative compared to similar bounds obtained in recent papers (Attanasio, Kugler and Meghir, 2011; Blanco, Flores and Flores-Lagunes, 2013; Alfonsi et al., 2020), which consider as lower bound the ITT effect itself. We would have stronger causal effects on wages under their additional assumptions.

estimated on younger youth would persist later in their life-cycle. Previous findings indicate a rather positive answer. Ruhm (1997) observes wages for youth aged 25 to 29 and find homogeneous returns over that age range. In Section VI, we discuss in more detail the expected longer-horizon effect in our context.

Differently from Hotz et al. (2002) and Ashworth et al. (2020), 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 A19, we show that our estimated returns are not statistically different by gender.

Our study has one other important difference with the previous literature on working while in school. We study the effect of jobs that are well paid, in state-owned companies, involving sophisticated tasks (e.g., using computers, writing reports) 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 A18, further commented in Section V, 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.

IV.B Effects on educational outcomes

Enrollment Effects Table 4 reports 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 12.6 percentage points from a control average of 73%. 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 two years after the end of the program, the effects on enrollment are smaller, but they persist and remain statistically significant.[19] The effect is mainly driven by enrollment in secondary education (see Column 2).[20]

[19]We present robustness checks in the Appendix. Table A7 presents results without including controls, and Table A8 shows the ITT effects. Overall results are robust.

[20]To adjust for multiple hypotheses, we consider that we are testing for 8 hypotheses. This corresponds to all post-program outcomes, except enrollment in any level (column 1), which we

Enrollment in Private Schools One concern is that in the administrative data we do not observe enrollment in private institutions. 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 survey data, we show in Appendix Table E3 that at the end of the program year there are no treatment effects on the type of schools students are enrolled.

Schooling investment 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 5 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 11 percentage points higher (similar effect as in the administrative data).[21] Moreover, there is no effect on truancy, since we do not observe significant 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 6 percent decrease with respect to the control 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 33 percent of the control mean.[22] The crowding-in (on enrollment) and the crowding-out (at the intensive margin) 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 Appendix Table E8). Furthermore, this reduction in study time of enrolled students does not translate into significantly lower grades. Column (5) shows that the program has only small effects on the grade point average of high school students; the coefficient is not statistically significant and the 95% confidence interval excludes negative effects larger than 4% of the control complier mean.[23] We find suggestive evidence

do not include as it is the sum of columns (2) to (5). The coefficients on secondary education for year 2, on tertiary non-university for year 2 and for out-of-school programs for year 1 lose statistical significance after the correction.

[21] 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.

[22] The p-values for the coefficients on class hours, study time and GPA are 0.12, 0.12 and 0.2, respectively, once we correct for the 4 multiple hypotheses tested in this table (excluding enrollment).

[23] Columns (2) to (5) in Table 5 are conditional on enrollment. Their causal interpretation depends

that the reported GPA measure is informative and the reduction in study time and class hours in the treatment group is consistent with small effects on grades.[24] Overall, our evidence suggests that the increase in enrollment does not come at the expense of schooling quality or achievement. Similarly, Scott-Clayton and Minaya (2016) finds small effects on the first-year GPAs of participants to the U.S. Federal Work-Study program.

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, as found in the context of compulsory school reforms by Meghir and Palme (2005). In our context, one potential explanation for the persistent enrollment effect relates to the income shock embedded in the program. 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. In Figure 2, we plot the treatment effect on yearly earnings and on enrollment for students in vulnerable households and for students in non-vulnerable households (we report regression estimates in Appendix Table A10). In the right-hand panel, enrollment effects are significantly higher for youth in vulnerable households than for youth in non-vulnerable households both during and after the program, which is consistent with the income-effect explanation. We do not find statistically significantly different treatment effects on post-program earnings across both groups in the left-hand panel.

A complementary explanation for the persistent effect on enrollment relates 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

on the eventual differential selection into enrollment induced by the program. We may be concerned that marginal students induced to remain enrolled because of the program are negatively selected. To partially address this issue, we add grades in the previous year as additional controls in Appendix Table A9. This hardly affects the estimated effects, building up confidence in Table 5 take-away.

[24]Using control group observations, we run a regression of GPA on the three inputs included in Columns 2-4 of Table 5. Using these estimates, the predicted reduction in GPA based on the estimated treatment effects is of 0.05 points.

from high school than the probability reported by control youth. The magnitude of the effect is of 3 percentage points over a mean of 70% in the control group and statistically significant at the 5% level (see Appendix Table A11). 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 small in magnitude, we consider this as rather suggestive evidence.

IV.C Effects on working and studying

Beyond the separate effects on employment and education enrollment, we explore how the program affects the joint distribution of these two variables. Table 6 studies the four possible outcomes: 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 28% for the control compliers. The treatment effect on the share of working students persists in Years 1 and 2, when it amounts to 4 p.p. (14-18% of the control mean). This corresponds to reductions in the share of the other three groups, including NEETs. Interestingly, the enrollment effect of 4 p.p. for Year 1 (Table 4) is the result of an increase in working students by 6 p.p. (Table 6, Column 1) and a decrease in non-working students by 2 p.p. (Column 2).[25] 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. The possibility that treated youth developed stronger work-study habits can be another explanation for the persistent effects on enrollment.

Overall, we find empirical evidence for substantial positive treatment effects on

[25] If we restrict the sample to applications to the first program edition, for which we have 4 years of post-program outcomes, we can explore longer run effects (See Appendix B). Four years after the program, when almost all control youth have quit school (17% are working students and 5% are students only), the program effects entirely correspond to transferring youth from the NEET group to the out-of-school working group. The program then decreases the share of NEET youth by 5 p.p. (25% of control mean).

earnings, wages, and employment, and limited effects on education after the program. We now discuss possible mechanisms leading to the positive earnings effects.

V Mechanisms

In this section, we conduct exploratory analysis of the mechanisms driving the program effects on earnings. The program rules prevent 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.[26] Therefore, the Uruguayan program gives us a setting where we can shut down the within-firm stepping-stone effects of work-study programs by which youth get hired in the firm where they work as students. Instead, the program emphasizes the importance of skills, more precisely transferable skills. We provide suggestive evidence about its effects on both the hard and soft skills of students. We further study program effects on earnings in various sectors to discuss skills sector-specificity.

The first channel by which the Uruguayan work-study program could enhance participants' *hard* skills is through on-the-job learning. The jobs offered by the program involve tasks that may enhance students' hard skills. Participants employed in program jobs are significantly more likely to read, write and use a computer every day than participants in the control group who are working (see Appendix Table E6). They are less likely to measure weights and distance, and they perform less physically demanding tasks. The second channel by which the program could enhance hard skills is through its indirect effect on formal education. As mentioned above, we find evidence that it increases the overall educational attainment of treated participants.

The work-study program states as an objective to enhance the *soft* skills of students by exposing them to a real work environment. We measure soft skills in our 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 7 reports treatment effects on each dimension

[26]Using data from the first program edition, we verify that around 4% of the treated youth have ever worked in a program firm during the four years after the program.

of the Big 5 personality traits and a measure of grit, following the estimation of Equation (1).[27] We do not find any significant effects on four of the five personality traits, nor on grit, which has been shown to be a malleable skill (Alan, Boneva and Ertac, 2019; Ubfal et al., 2019). The only trait with a marginally statistically significant effect at the 10% level is conscientiousness, with an effect of around 10% of the standard deviation in the control group.

The questionnaire also included some specific questions on work attitudes, and on 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 7 shows statistically significant differences across treatment and control groups in two of the four dimensions. Treated students rate completing tasks on time (Column 1) and adapting fast (Column 2) significantly higher than control students. However, we do not find significant effects on the importance of punctuality (Column 4), which is confirmed by the lack of effects on a behavioral measure recording whether youth arrived to the survey interview at the scheduled time (Column 6). We also find no significant effects on the importance of teamwork (Column 3), which may be explained by the type of jobs that the program offers where social interactions are less frequent than in the control group.[28]

To assess the economic importance of the statistically significant program effects on soft skills, we compute a back-of-the-envelope prediction of their effects on labor earnings. Using the control group survey data, we regress monthly labor earnings on the ten measures of soft skills (see Appendix Table A16). These estimates also reflect selection into employment and not only the effect of skills on earnings. We then predict the change in earnings following the soft-skill enhancement. We obtain that the predicted change in earnings is around 1% of the average earnings in the control group.[29] This is smaller than the Year-2 program effects on earnings, which amount to 8%.

[27]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).

[28]Column (7) of Appendix Table E6 shows a negative program effect on meeting frequently at work with colleagues.

[29]Alternatively, we can use the returns to soft skills estimated in the U.S (Deming, 2017). A one standard deviation increase in soft skills increases hourly wages by 4%. Thus, the program effects on conscientiousness (10% of a standard deviation) would yield wage increases of 0.4%, one order of magnitude less than the Year-2 program effects on wages (6%).

Across the board, we find mixed evidence of program effects on soft skills, but pointing to small effects. This is in line with previous research showing evidence that soft skills can be accumulated in regular jobs (Gottschalk, 2005; Adhvaryu, Kala and Nyshadham, 2018), but might not be enhanced in temporary work experiences (Beam and Quimbo, 2021).

The contribution of the skill-enhancing channel to the earnings effects depends on the portability of skills acquired in program jobs. Treated youth work in state-owned companies, mainly in the civil and public banking sectors, while the majority of non-program labor market opportunities are provided in the private industry and trade sectors.[30] If human capital is sector-specific and skills acquired during the program are not transferable across sectors, program participation could increase earnings in the civil and banking sectors, but not in the main 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. The sector-specificity of human capital would weaken the work experience channel. To assess this mechanism, we first estimate program effects on earnings by aggregate sector (Appendix Table A17). Although estimates are noisy, we find that earnings effects are not concentrated in the sectors of the program firms. Second, we document how post-program earnings vary by program firms' sector (Appendix Table A18). Arguably, outside of the program, there are more opportunities in private banking than in the civil sector. After the program, however, we do not find statistically significant difference by program firms' sector. Consequently, we do not find evidence of sector specificity in acquired skills. This is in line with previous evidence that individuals move to occupations with similar tasks requirements and thus human capital is portable across sectors (see for example Gathmann and Schönberg, 2010).

Beyond the skill-enhancing human capital channel, program effects may be related to the *signaling* role of work experience or to the ability *learning* channel (from the worker side) mentioned in Section II.[31] Unfortunately, our data do not allow us to provide evidence for these channels. This should be further investigated in future research.

[30] The administrative data 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).

[31] See Cahuc, Carcillo and Minea (2021) for empirical evidence on the signalling role of subsidized jobs.

VI Discussion

In this section, we summarize the empirical evidence on heterogeneous treatment effects and discuss potential longer-term effects of the program.

VI.A Heterogeneous effects

Gender We find strong effects of the program on post-program earnings and enrollment for both young men and women, with no evidence of treatment effect heterogeneity (see Appendix Table A19). This is a relevant finding given that most of the above-mentioned literature studying the effect of working while in school in the U.S. focuses on male samples.

Poor vs non-poor households In an effort to reduce inequalities, many government programs target exclusively poor households. As the Uruguayan work-study program offers jobs to any student regardless of household income, it allows to compare treatment effects on poor vs. non-poor households. Figure 2 and Appendix Table A10 show that the difference in earnings effects after the program is not statistically significant between vulnerable and non-vulnerable households. This is imprecisely estimated though and we cannot rule out large differences. The magnitude of the effects implies treatment effects that are 60% higher for vulnerable households. The effect on enrollment is more precisely estimated, and indicates that students from vulnerable households experience significantly higher program effects. To the extent that educational attainment increases earnings later in life, this suggests that vulnerable households might benefit more from the work-study program in the long run.

Age and baseline education In Appendix Figure A1, we plot treatment effects two years after the program by baseline education level (academic high school, technical high school, and university) and by age at program application. Unfortunately, estimates are noisy for each subgroup and we are not well-powered to detect statistically significant differences. We should then take the following comment as suggestive evidence. High-school students aged 19 at application seem to benefit the most from the program. We also find large treatment effects on the enrollment of 18 year old students initially enrolled in academic high school, and on the earnings of 18 year old university students. These subgroups are actually at the margin between secondary education and tertiary education. This suggests that work-study programs may work better when students face pivotal schooling choices.

The last panel in Appendix Figure A1 shows treatment effects for university students, which is the target group of the U.S. Federal Work-Study program. We find similar effects of the Uruguayan work-study program on this older subgroup. The estimates also suggest that targeting the U.S. program to students enrolled in their first year of college may yield larger earnings effects.

VI.B Program effects in the longer run

In our main analysis, we pool the first three program editions and estimate effects over two years after the program for all editions. Focusing on the first program edition, we find increasing positive and significant effects up to four years after the program (see Appendix B). While this horizon is longer than usual experimental standards, an open question is whether effects would persist beyond the four post-program years. As earnings effects trend upwards in the post-program years, we may expect these effects to grow.

However, we showed that earnings effects are related to the increase in human capital acquired in school (education channel) and on the job (work experience channel). The evolution of earnings effects then depends on the rate of diminishing returns to work experience that would eventually trigger a convergence between program participants and control youth later in their working life. If we make the conservative assumption that the work experience channel eventually fades out, earnings effects will be driven by the education channel only. As our data measure the program effect late in the education investment cycle, the effect on educational attainment (+0.17 years of education) is likely to persist beyond the fourth year after the program. Consequently, earnings effects due to the education channel may be interpreted as a lower bound of the life-cycle effect of the program. To predict these education-induced earnings effects, we need an estimate of the returns to education for middle-age workers. We estimate a Mincerian wage regression using Uruguayan Continuous Household survey data for workers aged 25 to 50 (Instituto Nacional de Estadistica Uruguay, 2013). We obtain that one extra year of education increases earnings by around 10%.[32] Taken at face value, this implies that the

[32]See Card (1999) and Psacharopoulos and Patrinos (2018) for estimates in other countries.

program effect on education would trigger an increase of 1.7% in earnings.

VII Conclusion

In this paper, we provide the first comprehensive 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 in a program job while enrolled in school improves labor market outcomes in the following two 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. A topic for future research is to study how the reduction of time that youth dedicate to household chores affect other household members.

We find that the human capital that students acquire in state-owned companies is transferable to private employers from other sectors. Our empirical analysis emphasizes human capital accumulation as a key channel. Nevertheless, we cannot discard a signaling or learning role of student work, which are relevant avenues 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

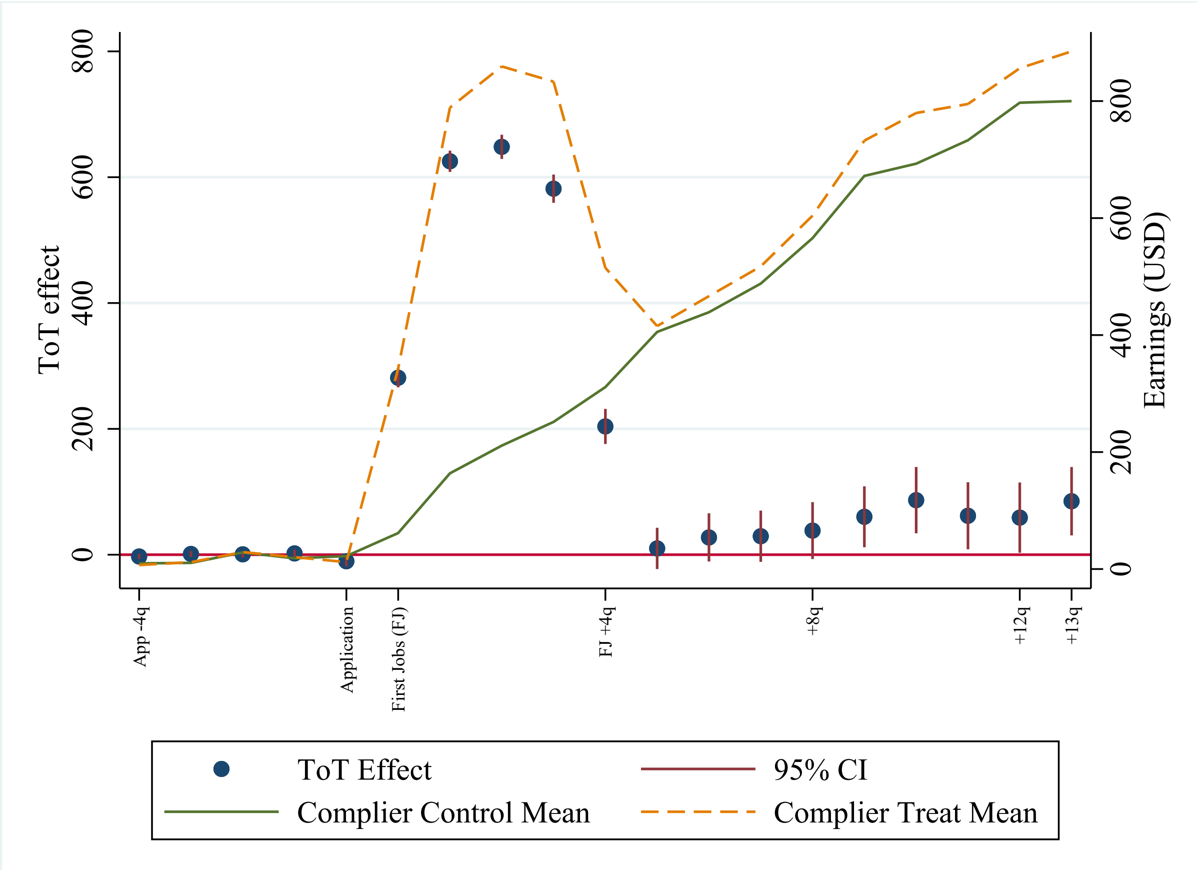

Figure 1: Quarterly earnings

Source: Administrative data.
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 600 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 800 USD per quarter.

(a) Effect on Yearly Earnings (b) Effect on Enrollment

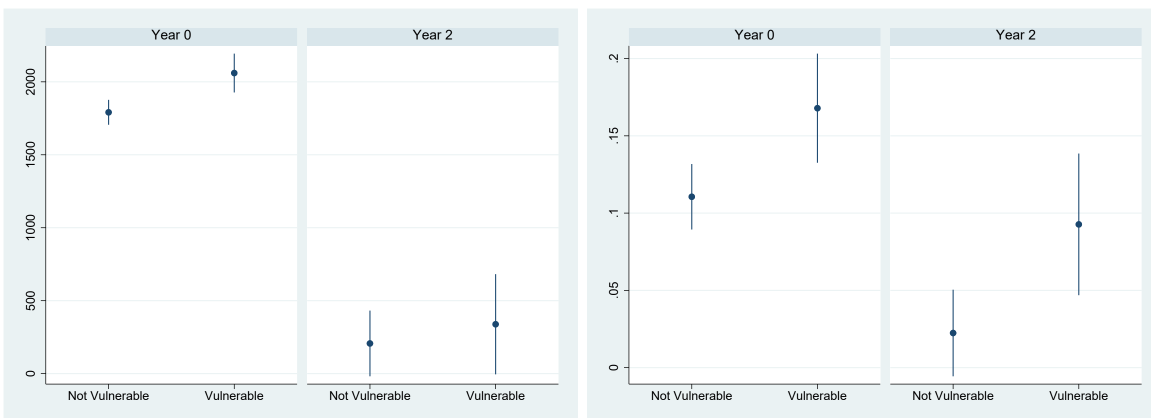

Figure 2: Treatment Effect Heterogeneity by Baseline Household Vulnerability

Source: Administrative data.
Note: This figure shows treatment effects of the work-study program by household vulnerability at application date. Vulnerable households include households receiving a cash transfer and/or a food card (labelled as *Highly Vulnerable* in Table 1). Panel 2a shows treatment effects on yearly earnings during the program year (Year 0) and two years after the program ends (Year 2). Panel 2b shows effects on enrollment. They are obtained by two stage least squares regressions of Equation (1), where we further interact the treatment dummy with the vulnerability dummy. Vertical lines represent 95% confidence intervals.

TABLES

Table 1: 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
Panel A. Demographic					
Female	0.58	0.49	0.60	0.49	0.15
Aged 16-18	0.71	0.45	0.72	0.45	0.88
Aged 19-20	0.29	0.45	0.28	0.45	0.88
Montevideo (Capital City)	0.49	0.50	0.55	0.50	.
Panel B. Education and Social Programs Year -1					
Enrolled in Academic Secondary Education	0.49	0.50	0.48	0.50	0.51
Enrolled in Technical Secondary Education	0.22	0.41	0.22	0.42	0.56
Enrolled in University	0.15	0.36	0.16	0.36	0.32
Enrolled in Tertiary Non-University	0.01	0.11	0.01	0.10	0.68
Enrolled in Out-of-School Programs	0.02	0.14	0.02	0.14	0.54
Highly Vulnerable HH (Food Card Recipient)	0.10	0.30	0.09	0.29	0.25
Vulnerable Household (CCT recipient)	0.27	0.45	0.27	0.44	0.72
Panel C. Labor Outcomes Year -1					
Earnings (winsorized top 1%, USD)	172.29	601.28	154.19	581.75	0.22
Positive Earnings	0.15	0.36	0.15	0.35	0.83
Months with Positive Earnings	0.71	2.14	0.62	1.97	0.11
Panel D. Aggregate orthogonality test for panels A-C					
p-value (joint F-test)					0.45
Observations	87,737		2,686		90,423

Source: Administrative data and YET Application Form. Notes: the p-value reported in Column 5 is obtained from a regression of each variable on a YET job offer dummy with robust standard errors, controlling for lottery design (lottery and quota dummies) and number of applications. 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. 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. **p-value (joint F-test)**: 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 2: Effect of YET on labor outcomes

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Program year				
Year 0	1863.91	6.85	0.56	-23.47
	(36.85)	(0.08)	(0.01)	(2.79)
	[1004.26]	[2.76]	[0.44]	[318.87]
Post-Program years				
Year 1	86.08	-0.01	0.05	7.13
	(71.73)	(0.12)	(0.01)	(7.12)
	[1976.26]	[4.38]	[0.59]	[400.61]
Year 2	242.47	0.06	0.02	28.65
	(96.41)	(0.13)	(0.01)	(8.63)
	[2965.23]	[5.38]	[0.65]	[494.64]
Observations	90423	90423	90423	59743

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 18 or less 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. **Months with 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 Months with earnings, it is missing for those who have not worked any month over the 12 months. Standard errors robust to heteroskedasticity shown in parenthesis and control complier means in brackets.

Table 3: Bounds for the ITT effects on monthly wages (post-program years)

	(1) ITT effect on wages	(2) Lee bounds on wage effects	(3)	(4) Imbens and Manski 95% Confidence Interval	
		Lower	Upper	Lower	Upper
Year 1	5.49	-22.13	23.99	-30.06	32.81
	(5.49)	(4.82)	(5.36)		
	[410.69]				
Year 2	22.05	19.65	31.65	8.87	42.48
	(6.65)	(6.55)	(6.58)		
	[501.88]				

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% higher wages. Robust standard errors shown in parenthesis and control means in brackets. We follow Imbens and Manski (2004) to construct confidence intervals for the bounds.

Table 4: Effect of YET on enrollment in education

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Program year					
Year 0	0.126	0.102	0.018	0.007	0.002
	(0.009)	(0.010)	(0.007)	(0.004)	(0.004)
	[0.731]	[0.500]	[0.203]	[0.017]	[0.023]
Post-Program years					
Year 1	0.037	0.030	0.011	0.003	-0.005
	(0.012)	(0.011)	(0.009)	(0.004)	(0.002)
	[0.608]	[0.321]	[0.265]	[0.022]	[0.015]
Year 2	0.041	0.024	0.009	0.008	0.000
	(0.012)	(0.010)	(0.009)	(0.004)	(0.002)
	[0.452]	[0.225]	[0.205]	[0.025]	[0.007]
Observations	90423	90423	90423	90423	90423

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 18 or less 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." Robust standard errors shown in parenthesis and control complier means in brackets.

Table 5: 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	0.11	0.042	-1.64	-2.32	-0.20
	(0.033)	(0.041)	(0.75)	(1.07)	(0.15)
CCM	0.44	0.24	26.8	6.86	7.75
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. Robust standard errors shown in parentheses.

Table 6: 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	0.57	-0.01	-0.45	-0.12
	(0.01)	(0.01)	(0.01)	(0.00)
	[0.28]	[0.15]	[0.45]	[0.12]
Post-Program years				
Year 1	0.06	-0.01	-0.02	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.33]	[0.25]	[0.27]	[0.14]
Year 2	0.04	-0.02	0.00	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.29]	[0.36]	[0.16]	[0.18]
Observations	90423	90423	90423	90423

Source: Administrative data.
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 18 or less 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. Robust standard errors shown in parenthesis and control complier means in brackets.

Table 7: 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	-0.018	0.063	0.013	-0.028	0.029	-0.043
	(0.033)	(0.037)	(0.052)	(0.038)	(0.059)	(0.039)
CCM	4.03	3.81	3.60	3.69	3.41	3.73
Control sd	0.49	0.57	0.73	0.53	0.83	0.58
	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	0.090	0.11	0.068	0.025	0.073	-0.009
	(0.046)	(0.047)	(0.047)	(0.056)	(0.033)	(0.010)
CCM	4.07	3.99	4.22	4.16	4.11	0.03
Control sd	0.68	0.65	0.68	0.82	0.49	0.15
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). Robust standard errors shown in parentheses.