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The Lasting Effects of Working while in School: A Long-Term Follow-Up

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Abstract

This paper provides the first experimental evidence on the long-term effects of work-study programs, leveraging a randomized lottery design from a national program in Uruguay. Participation leads to a persistent 11 percent increase in formal labor earnings seven years after the program, driven by a 4 percent increase in the monthly probability of being employed and a 6 percent increase in monthly wages. Effects are significantly larger for men, while remaining positive for women. The program is highly cost-effective, outperforming most job training programs and reaching levels comparable to early childhood investments.

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Keywords: Work-study Program, Youth Employment, School-to-Work Transition, Long-term Effects.

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Youth unemployment represents a critical issue across various global contexts, with rates consistently surpassing those of adult unemployment by a factor of three. This underscores the unique challenges youth face in transitioning from education to employment. The problem is worsened by the prevalence of NEET (Not in Education, Employment, or Training) youth. In 2023, nearly 20 percent of the youth population in Latin America and the Caribbean (LAC) were identified as NEET. One approach to increasing youth employment and facilitating the school-to-work transition is through work-study programs. Working while studying is a relatively common practice among youth in some countries, though overall levels remain low. In 2023, 17 percent of students aged 15–19 in LAC were employed.[1] Evidence on the long-term effects of in-school work remains limited to non-experimental designs, and the experimental literature on active labor market policies offers limited guidance, as most evaluations do not extend beyond three years post-program completion (McKenzie, 2017; Card et al., 2018; Carranza and McKenzie, 2024).[2]

This paper fills this gap in the literature by providing causal evidence on the long-term effects of a work-study program. Leveraging a randomized, lottery-based design, we examine a program in Uruguay that offers youth aged 16 to 20 their first formal work experience within state-owned companies for up to one year, with participation conditional on enrollment in high school or university. We assess its impacts up to seven years after completion. Le Barbanchon et al. (2023), hereafter referred to as LBUA, evaluate the program using data covering up to two years post-completion, documenting significant short-term effects on formal earnings. However, at that point, nearly half of the participants were still enrolled in school, meaning that the short-run findings may not fully capture impacts on post-graduation labor market opportunities. In contrast, seven years after completion, almost all participants have left education, which allows us to study how the short-run benefits of work-study programs evolve over time.

The program may operate through two main channels: early work experience and education. Regarding early work experience, while the program initially provides participants with an advantage, its impact may diminish over time as non-participants accumulate their own work experience, potentially narrowing labor

[1] In comparison, 14 percent of students in Europe and 22.5 percent of high school students in the United States (US) were employed in 2023, with higher employment rates observed among college students.

[2] Notable exceptions include Ibarrarán et al. (2019), Attanasio et al. (2017), and Bandiera et al. (2025), who provide experimental evidence on outcomes measured six, eight to fourteen, and six years after program completion, respectively.

market differences. Conversely, if labor market trajectories depend heavily on first jobs, program effects may persist.[3] In the Uruguayan case, well-regarded positions in state-owned companies could serve as crucial stepping stones to future employment. With respect to education, the program effects are theoretically ambiguous: time demands may crowd out study, while the enrollment conditionality of the program and additional resources may crowd in. A crowding-in effect would provide an additional mechanism through which effects may persist in the long run.

Understanding the long-term effects of combining work and study is critical for several reasons. First, such evidence is essential for cost-benefit analyses, as benefits realized over time are more likely to outweigh program costs. Second, identifying long-term effects provides valuable insights into state dependence, informing policy and program design.

The Uruguayan work-study program offers a unique opportunity to examine the long-term causal effects of combining work and study, as assignment is determined by randomized lotteries. Using administrative data from social security records, we analyze a sample of 90,423 teenagers who applied to the first three program editions, tracking their earnings for up to seven years after completion.

We find that participation in the work-study program increases yearly earnings by approximately 10 to 13 percent from the third year after program completion through the seventh year. This effect is comparable to, or larger than, the 8 percent increase observed two years post-program, as documented in LBUA (2023). The long-term effect on yearly earnings, observed in Year 7, is driven by a 4 percent increase in the monthly probability of being employed (extensive margin) and a 6 percent increase in monthly wages (intensive margin). The earnings effects are not limited to the industries that provided the initial program jobs, and the work experience accumulated during the program appears to be valued by employers across the economy.

By Year 7, treated youth accumulate 0.7 additional years of work experience and 0.27 more years of schooling, consistent with a crowding-in effect in education.[4] A back-of-the-envelope calculation suggests that both channels contribute to the long-term increase in earnings. The results on work experience indicate that its returns do not decline quickly, supporting state dependence, whereby early labor

[3]See Schwandt and von Wachter (2019) and Alves and Varvasino (2025), among others, for evidence on the long-term effects of entering the labor market during periods of high unemployment.

[4]The absence of crowding-out effects on academic performance aligns with findings from a recent related study (Aucejo et al., 2024).

market advantages persist. Moreover, gaining work experience during adolescence may have a stronger impact on long-term career paths than entering the labor force at a later stage.[5]

Our heterogeneity analysis suggests that the persistent effects of the program are stronger for men than for women, while remaining statistically significant for both. Additionally, we find positive earnings effects for both vulnerable and non-vulnerable individuals.

The work-study program is highly cost-effective. Despite relatively high initial costs, the long-term earnings gains are projected to result in full fiscal recovery by age 46 (i.e., 27 years after program participation). The program's long-run cost-effectiveness, as measured by the Marginal Value of Public Funds (MVPF; Hendren and Sprung-Keyser, 2020), exceeds that of youth job training programs and is comparable to that of early childhood investments.

Overall, our findings suggest that work-study programs can facilitate the transition from school to work, with benefits lasting at least seven years beyond program completion. Policies that integrate education with early labor market exposure may therefore serve as an effective strategy for improving long-term labor market outcomes.

This study contributes to four strands of literature. First, it contributes to the literature on the effects of working while in school. Previous research, primarily based on non-experimental data from the US, have found mixed results on the long-term labor market returns of combining work and study (Ruhm, 1997; Hotz et al., 2002; Ashworth et al., 2021). We provide the first causal evidence on the long-term impact of a work-study program, demonstrating that early work experience has lasting earnings gains.

Second, this study contributes to the literature assessing the long-term effects of youth employment programs. Our results align with non-experimental evaluations of federal employment programs in the US, which find positive effects on long-term employment (Scott-Clayton and Minaya, 2016) and lifetime earnings (Aizer et al., 2024). We build on this research by providing experimental evidence from a middle-income country on a work-study program that explicitly requires participants not to drop out of school. This latter distinctive feature is important, as our

[5]Neuroscience research highlights adolescence as a pivotal period for neural development, marked by significant changes in brain regions associated with various cognitive functions (Sebastian et al., 2010; Blakemore and Robbins, 2012).

findings suggest that combining work and study can yield lasting labor market benefits without crowding out educational attainment.

Third, we contribute to the broader literature on active labor market policies (ALMPs). Meta-analyses reveal that most experimental evaluations of ALMPs focus on short-term outcomes, and impacts are typically assessed within three years of program completion (McKenzie, 2017; Card et al., 2018; Agarwal and Mani, 2025; Carranza and McKenzie, 2024), aside from a few notable exceptions (see footnote 2). Moreover, many training programs show only modest or temporary impacts (Blattman and Ralston, 2015; McKenzie, 2017; Card et al., 2018). In contrast, our study extends the time horizon by examining earnings effects seven years after program participation.

Finally, our work relates to the literature on conditional cash transfers (CCTs). The work-study program can be seen as an "in-kind" variant: eligibility and participation require school enrollment, but the transfer takes the form of a paid job rather than cash. Traditional CCTs typically raise school attendance (Baird et al., 2011; de Brauw and Hoddinott, 2011), whereas in our setting, the effects on education are theoretically ambiguous. In addition, paid jobs affect earnings directly through labor market channels. Consistent with the presence of crowding-in effects in education, our results suggest that long-run earnings gains arise from both CCT-style schooling incentives and accumulated work experience.

1 Institutions, Data, and Empirical Design

In this section, we describe the Uruguayan work-study program, the data, and our empirical design. We follow closely the related sections in LBUA (2023).

1.1 YET Program

Since 2012, the work-study program "*Yo Estudio y Trabajo*" (YET) provides youth aged 16 to 20 in Uruguay with a first formal work experience in state-owned companies for up to one year (see Online Appendix Table A1 and Section D for more institutional details).

Youth aged 16 to 20 are eligible to apply for YET if they meet two conditions: 1) they are enrolled in an educational institution, and 2) they have not worked formally for more than 90 consecutive days at the time of application. Using census microdata, LBUA (2023) estimate that 34.6 percent of eligible youth in Uruguay applied to the 2012 edition of the program.

Assignment to the program is determined by lottery at the locality level. The number of participants in each locality depends on the number of jobs offered by state-owned firms partnering with the program. Lottery candidates are randomly ranked within locality, and program offers are made sequentially until local slots are filled. Starting with the third edition in 2014, quotas were introduced in the largest localities to guarantee participation of minority youth: 8 percent of African origin, 4 percent with disabilities, and 2 percent transgender youth.

Importantly, firms cannot choose the youth they hire, nor can candidates select the firm where they work. Program administrators match participants to available positions, considering commuting distance and school schedules but not skills. Since most high schools in Uruguay operate on half-day schedules (morning or afternoon), youths are placed with firms for the opposite half-day. Consequently, placements are logistics-driven, with minimal scope for skill-based matching. The program streamlines hiring for state-owned enterprises, encouraging firms to accept assigned youths, with dismissal only for poor performance—a rare occurrence given high completion rates. Less physically demanding tasks and the prestige of these employers may attract motivated applicants, ensuring high participation.

The program offers part-time jobs of 20 to 30 hours per week, with no overtime allowed. Participants work during normal operating hours, ensuring that school attendance is not hindered. Contracts are temporary, lasting 9 to 12 months, and are non-renewable. In 2016, the fixed remuneration was $446 per month for a 30-hour-per-week job (around $3.7 per hour).[6] The program wage is higher than the national minimum wage of $372 per month for full-time work.

All program firms are in the public sector and pay wages from their own budgets. Most are large state-owned firms, with only a few positions offered in the public administration. For example, the top five employers in the first three editions are: the electricity company (hiring 22 percent of participants), the water company (21 percent), the oil and gas company (16 percent), the commercial bank of Uruguay (10 percent), and the telephone company (9 percent). The program stipulates that work activities must be in administration or operations, focusing on support tasks.

[6] All amounts are deflated using the Consumer Price Index (CPI) to January 2016 pesos and converted to US dollars at the January 2016 exchange rate (0.033 per peso).

1.2 Data

We use two main data sources: YET program administrative records and social security records. All data are matched at the youth level.

First, data from the online application form completed by all applicants provide basic demographics (age, gender, locality) and educational level. YET administrative records also include information on lottery draws, subsequent offers, and participation. Second, social security records include monthly labor earnings from formal jobs for all applicants from 2011 to 2022.

To balance sample size and long-term analysis, we focus on the first three editions of YET (2012, 2013, and 2014), the same sample as in LBUA (2023). This ensures reasonable statistical power and allows us to observe earnings for seven years post-program. We describe our sample and verify balance between groups that received offers and those that did not in Online Appendix Table A2.

1.3 Empirical Design

In our primary analysis, we focus on the Treatment-on-the-Treated (ToT) effect of the program. We define treatment as working at least one month in a program job and the variable *Offered* as ever receiving a program job offer. To obtain the causal effect, we leverage the lottery design and use the *Offered* variable as an instrument for the treatment indicator.

Under this definition of treatment, the local average treatment effect is equal to the ToT, as no youth can work in a program job without an offer (i.e., there are no always-takers). This effect is identified under the exclusion restriction that the outcomes of youth offered a program job change only through program participation.

Among all applicants, 4 percent apply to more than one locality within a given edition year, and 27 percent to more than one edition. We analyze data at the applicant level and handle multiple applications as follows: for youth in the control group (never offered a program job), we randomly select one application; for those receiving at least one offer, we select the application that generated an offer to maximize statistical power. In Online Appendix B, we show that our main results are robust to using all applications or restricting the sample to applicants who applied in only one edition year.

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

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

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

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, while $Offered_i$ indicates whether individual i received a program job offer. To control for the lottery design, we include *Locality* $\times$ *Edition* fixed effects and quota fixed effects. This accounts for variation in the probability of receiving a job offer across lotteries, depending on the local number of program jobs offered and 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 different localities during edition e ($\#App_i$).[7] To increase precision, we include a vector of covariates $X_{i,0}$ measured at the time of application. It comprises gender, age, household vulnerability status, earnings, and education level in the year before applying to the program. Covariates are balanced between offered and control youth (see Online Appendix Table A2), supporting the exogeneity of the *Offered* variable after accounting for lottery design. Our parameter of interest is γ_t, which we estimate using two-stage least squares, as explained above, and which captures the ToT effect t periods after application. ToT effects are 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).

The first stage is strong: 77 percent of youth receiving an offer work in a program job, and it is homogeneous across program editions (see Online Appendix Table A3). The F-statistic from the first stage is well above the threshold value of 105 suggested by Lee et al. (2022) for a strong instrument with a 5 percent critical value in the second stage.

In Online Appendix B, we show that our results are robust to alternative specifications: omitting controls, clustering standard errors at the locality level, not winsorizing the earning variables, computing intention-to-treat (ITT) estimates, and defining treatment alternatively as working in any firm while being enrolled in school during the program year.

[7] Online Appendix Table B6 shows that using the number of applications fixed effects, instead of controlling for it linearly, barely changes the results.

2 Long-Term Effects on Labor Market Outcomes

In this section, we present the long-term effects of the program on labor market outcomes, measured up to seven years after participation.

2.1 Earnings Effects

Figure 1 presents a graphical visualization of the evolution of average quarterly labor earnings for treated and control compliers, as well as treatment effects on quarterly labor earnings. The dashed line represents the average quarterly earnings of treated youth, while the solid line represents the average for control compliers. As required by program eligibility, earnings are close to zero in the year before applying. Average earnings of control compliers grow from the program year onward, reaching approximately $1,470 per quarter by the end of Year 7 (see values on the right y-axis). In contrast, the average earnings of treated youth peak at around $1,000 per quarter in the second quarter of the program year. Immediately after the program ends, both trends converge, as participants cannot remain employed by program firms. From then onward, the average earnings of treated youth grow at a faster rate than those of control compliers, reaching approximately $1,650 per quarter seven years after the program.

Figure 1 also plots treatment effects on earnings and their 95 percent confidence intervals. During the program year, quarterly treatment effects reach approximately $700 from the second quarter. Immediately after the program, treatment effects naturally decline but they steadily increase thereafter, becoming statistically significant in Year 2. They reach approximately $180 toward the end of the period (see left y-axis).

Table 1 presents our main treatment effects for the program year and each year after participation. We report treatment effects on yearly earnings (Column 1), employment (Columns 2 and 3), wages (Column 4), and contract type (Column 5).

During the program year, treated youth earn almost three times as much as control compliers. After the program, treatment effects on yearly earnings remain positive in all years and are statistically significant from Year 2 onward, strengthening over time. These effects correspond to an 8 percent increase in Year 2, and 10–13 percent in Years 3 to 7 (see Column 1). The short-term effects identified in LBUA (2023) persist and grow over time, reaching 11 percent seven years post-program.

Our administrative data capture earnings exclusively in the formal sector. To assess the potential role of sectoral shifts in total earnings, we use data on informality from the 2022 Continuous Household Survey in Uruguay (ECH, Instituto Nacional de Estadística Uruguay, 2022), restricting the sample to individuals aged 24–28, the age group that treated youth reach seven years post-program. Fourteen percent of workers in this age bracket are employed in the informal sector, earning an average of approximately \$3,500 annually. Explaining the observed \$650 increase in formal earnings in Year 7 solely through reallocation (assuming no change in total earnings) would require a shift of 19 percent of treated individuals from the informal to the formal sector. However, given the 14 percent informality rate, the increase in formal earnings cannot be attributed solely to sectoral reallocation, suggesting that the rise reflects higher total earnings.

2.2 Employment Effects

The positive earnings effects are partly explained by positive employment effects at the extensive margin. Column 2 presents treatment effects on the number of months with positive formal earnings within a year. During the program year, treated youth work in a formal job for almost seven additional months, compared to fewer than three months for control compliers. Employment effects at the extensive margin grow steadily after the program, becoming statistically significant and stabilizing by Year 4. From Year 4 onward, treated youth work approximately one-third of a month more in formal jobs than control compliers, roughly a 5 percent increase.

Treatment effects are weaker when employment is measured at the extensive margin using an indicator for positive earnings in any month over 12 months (Column 3). Still, the long-run employment effects observed in Year 7 are positive and statistically significant, reflecting a 2 percentage point increase in the probability of being employed (3 percent of the control complier mean).

2.3 Wage Effects

Column 4 shows treatment effects on monthly wages, conditional on employment, indicating that the intensive margin also contributes to the long-term earnings gains. Monthly wages are defined as total earnings divided by the number of months with positive earnings, restricting the sample to individuals with at least one month of positive earnings within a year. During the program year, treatment effects are negative, consistent with the program offering part-time jobs. After the program, effects turn positive and statistically significant from Year 2 onward, persisting in the long term. By Year 7, treated youth earn $44 more than control compliers, whose average monthly wages are $709, a 6 percent increase.

To address treatment-induced differences in employment likelihood, we adopt the Lee (2009) approach to estimate bounds on monthly wages effects. Online Appendix Table B9 shows that wage effects are generally robust to selective sample inclusion. Bounds are positive in all post-program years, although the Imbens–Manski confidence intervals for the lower bound include zero in Years 4 and 7. Related studies use less conservative bounding assumptions and treat the ITT effect as a lower bound (see Attanasio et al., 2011; Blanco et al., 2013), which would yield statistically significant lower bounds in our case. Overall, the results indicate a sustained positive wage effect, with limited precision in later years.

2.4 Effects on Labor Contract Types

Next, we explore whether participation in the program has long-term effects on the type of contracts that workers hold. Different contractual arrangements can influence job stability and earnings potential, which may help explain the observed treatment effects on earnings. In Uruguay, employees may be hired on a regular contract with a fixed monthly salary or as daily or hourly workers paid per day or hour. Column 5 reports treatment effects on the number of months working under a regular contract within a year. As expected, given that the program jobs offer regular contracts, treatment effects during the program year are large: treated individuals hold a regular job for over seven additional months, compared to fewer than two months on average among control compliers. After the program, treatment effects on the number of months with a regular job are positive and statistically significant at all horizons. By Year 7, treated youth work in a regular job for 0.6 additional months, relative to an average of six months among control compliers—a 10 percent increase.

Because regular jobs typically yield higher earnings, longer tenure in them may help explain the observed earnings effects. While we cannot isolate the causal effect of regular jobs, Online Appendix Table A4 provides suggestive evidence from a regression among control-group workers, controlling for covariates. By Year 7, each additional month under a regular contract is associated with a $700 increase in yearly earnings—a 35 percent rise compared to workers with non-regular contracts. This correlation suggests that part of the earnings gains for treated individuals stems from greater access to regular, better-paying jobs.

2.5 Comparison with Existing Evidence

Our findings are next compared with non-experimental studies examining the long-term effects of in-school work and youth employment programs. First, we focus on several non-experimental studies that specifically investigate the long-term effects of working while studying in the US. Ruhm (1997) examines returns to working while in high school up to nine years after graduation, controlling for observable differences between employed and non-employed students, while Hotz et al. (2002) study returns up to ten years after graduation. Ashworth et al. (2021) examine wage returns at age 29 from early work experience in high school and college. The latter two studies control for dynamic selection into employment. Our estimates are smaller than those of Ruhm (1997), who finds a 22 percent increase in earnings and a nine percent increase in monthly wages following a 20-hour student job, but larger than the non-statistically significant returns reported by Hotz et al. (2002). Our wage effects are close to those reported by Ashworth et al. (2021) for work experience during college. Hence, our estimates fall within the range of US findings.

Second, we compare our results to non-experimental evaluations of youth employment programs in the US. Consistent with our findings on long-term employment effects, Scott-Clayton and Minaya (2016) find that the US Federal Work-Study Program increases the youth employment rate by 2.4 percentage points six years after college entry. Similarly, Aizer et al. (2024) find that participation in the largest US youth employment and training program leads to a 5.2 percent increase in lifetime earnings.

3 Potential Mechanisms

We study two channels driving the persistent earnings effects of the work-study program: education and work experience.

Formal education raises human capital and has lasting effects on labor market earnings. Although the program's effects on education are a priori ambiguous, we find evidence of crowding-in in the long run: the program increases years of education by 0.27.[8] Most gains are due to increased high school enrollment, statistically significant up to three years post-program. The program did not significantly affect tertiary or university enrollment, even in the long run. In Online Appendix E, we review recent estimates of the returns to schooling in Uruguay and how they vary between OLS and IV estimation (Gethin, 2025). Using these estimates in a back-of-the-envelope computation, we find that the YET effect on schooling would correspond to an earnings increase of between 2.7 and 3.8 percent.

In turn, work experience can raise earnings through various channels, including on-the-job training, learning from coworkers, and improved job matching.[9] The extent to which these effects contribute to long-term earnings depends on how quickly returns to experience decline. If returns diminish rapidly, earnings should converge between participants and controls. Using recent estimates of returns to work experience (Lagakos et al., 2018; Jedwab et al., 2023), we find that the YET effect on cumulative experience (0.7 years by Year 7)[10] would predict a 1.4 to 3.5 percent increase in earnings (see Online Appendix E).

These back-of-the-envelope computations suggest that both channels contribute to the observed earnings effects. However, they should be interpreted with caution, as estimates of returns to education and work experience may suffer from selection bias, birth–time–age collinearity, and measurement error in work experience.

Importantly, the extent to which skills learned through education or work experience are transferable across sectors is key to explaining the earnings effects. If long-term effects were primarily concentrated in the sectors where participants worked during the program, this would suggest that the program experience provides sector-specific skills. Conversely, if significant effects appeared in other sectors, it would indicate that the skills acquired are broadly applicable and valued across different labor market contexts.

The employment register classifies employer industries according to the International Standard Industrial Classification (ISIC Rev. 4). To analyze earnings effects across sectors, we assign each individual's employer to one of four broad categories: Agriculture, Manufacturing and Energy Production, Market Services, and

[8]We calculate additional years of education by summing the treatment effects from Years 0 to 7 in Column 1 of Online Appendix Table A5.

[9]See Adhvaryu et al. (2023) for the role of on-the-job training, Demir et al. (2024) for learning from coworkers, and Cahuc et al. (2021) for signalling.

[10]Computed analogously to education, by summing the treatment effects on months with positive earnings from Years 0 to 7 (Column 2 of Table 1).

Non-Market Services. Individuals not employed in a given category are coded with zero earnings in that category. In Table 2, we show that during the program year, treatment effects are largest in the Manufacturing and Energy Production category, which includes major program employers such as the state-owned electricity and water companies. The second-largest effects are found in the Market Services category, which includes the national commercial bank, representing 10 percent of program jobs. This category also employs the majority of control-group youth during the program year, as it includes retail trade. In contrast, treatment effects on earnings in the Non-Market Services category are smaller and primarily driven by the limited number of public administration positions offered through the program.

Seven years post-program, treatment effects on earnings remain statistically significant and large in relative terms in the Manufacturing and Energy Production category, while no significant effects persist in Non-Market Services. In absolute terms, however, the long-term effects are larger in the Market Services category than those in Manufacturing and Energy Production. This shift suggests that skills acquired in Manufacturing and Energy Production, as well as in Non-Market Services, are transferable and increasingly valued in Market Services.

4 Heterogeneous Effects

Next, we examine heterogeneous effects across youth characteristics: gender, economic vulnerability status, age, and education (all measured at application). Since treatment effects on earnings stabilize around Year 3 (see Table 1), we pool data from Years 3 to 7 to increase statistical power.

We first investigate whether treatment effects differ by gender. Panel (a) of Figure 2 shows that, while both men and women experience statistically significant long-term treatment effects on earnings, the effects are more than double for men. Online Appendix Table A6 confirms this heterogeneity in relative terms: the average treatment effect on earnings over Years 3–7 amounts to 8 percent for women but reaches 17 percent for men, a statistically significant difference (see Column 1). While most of the literature on working while studying focuses on men (e.g., Hotz et al., 2002; Ashworth et al., 2021), this finding contrasts with the limited existing evidence (e.g., Ruhm, 1997) and the broader literature on ALMPs, which generally finds stronger effects for women (Card et al., 2018). To shed light on the drivers of this suggestive gender heterogeneity, Columns 2–6 of Table A6 report impacts on the extensive and intensive margins. On the extensive margin, employment effects over Years 3–7 are not statistically different across genders. In contrast, the intensive margin shows clear divergence: monthly wages rise by 11 percent for men versus 4 percent for women, a difference that is statistically significant at the 5 percent level. Therefore, the heterogeneity in long-run earnings effects is driven primarily by larger wage gains for men.

Second, we explore heterogeneous treatment effects on earnings by vulnerability status (see Panel (b) in Figure 2). Households receiving benefits from the conditional cash transfer program in Uruguay (AFAM-PE) are categorized as vulnerable, as eligibility is determined by a poverty score. We find statistically significant positive effects for both vulnerable and non-vulnerable treated individuals but no statistically significant differences across groups. While many ALMPs target disadvantaged or low-skilled workers, our findings suggest that broadening access beyond these groups could yield meaningful labor market gains.

Finally, Panels (c)–(e) of Figure 2 present treatment effects on earnings by age and education at baseline. The strongest evidence of positive effects is found among 19-year-olds in academic and technical high schools, where the estimates are statistically significant. This is consistent with effects being stronger for individuals at the margin of transitioning between educational levels.

5 Cost-Benefit Analysis

We evaluate the cost-effectiveness of the work-study program using the Marginal Value of Public Funds (MVPF), following the approach of Hendren and Sprung-Keyser (2020), hereafter HSK. The MVPF is defined as the ratio of the benefits to recipients, measured by their willingness to pay (WTP) for the program, to the net cost to the government. We report both a long-run MVPF based on observed outcomes up to seven years after the program and a projected life-cycle MVPF.

For comparison with other program analyses, our baseline computation adopts assumptions similar to those in HSK (2020), particularly regarding the definition of the tax rate and the choice of discount rate (3 percent). MVPF estimates under alternative assumptions are reported in Online Appendix C. Administrative records provide gross earnings, from which we derive net earnings by applying annual tax schedules to deduct income taxes.[11]

Net costs reflect both direct program costs and changes in tax revenues resulting from shifts in participants' earnings. In our baseline scenario, we assume that direct costs are equal to 50 percent of the average net salary paid to participants ($1,650), reflecting the lack of data on the value generated by participants' work, which is expected to be positive. Online Appendix C presents results under alternative assumptions regarding the inclusion of salaries. Administrative costs—which are minor relative to salary expenses—are excluded due to insufficient reliable data, resulting in a slight understatement of total costs.

During the program year, the government may experience an additional revenue loss, as some treated youth might have worked even without the program. However, given the prevailing tax rate and earnings levels, this loss is effectively zero. After program completion, higher participant earnings increase tax revenues by $115 in present value over seven years. Combining these factors, the total net cost of the program amounts to $1,535.

WTP is measured by changes in net earnings, assuming gains are not due to higher effort. If higher effort were a factor, accounting for it would lower the estimated WTP. Conversely, if participants derived non-monetary benefits from their increased work, including them would raise the WTP. After applying income tax rates, the discounted net earnings gains over seven post-program years correspond to a WTP of $4,726. The ratio of WTP to net costs yields an MVPF of 3.1 over the seven-year horizon.

For the life-cycle analysis, we project lifetime earnings effects using the 2022 Continuous Household Survey (ECH). This analysis relies on two assumptions: (i) the ratio of average earnings in the control group at age 26—seven years post-program—to those of the corresponding ECH cohort remains constant over the life cycle; and (ii) the percentage earnings gain for treated youth remains constant over the life cycle. The ratio specified in (i) is 1.13, indicating that average earnings in the control group are 13 percent higher than those of the ECH cohort.

Under these assumptions, the present value of additional earnings from ages 27 to 65 is $19,805, generating an estimated $2,653 in additional tax revenues. After

[11] Following HSK's (2020) conservative approach, we do not deduct payroll taxes, since at least some part of these contributions return to workers. For the life-cycle projection, we apply the tax schedules of 2022, the year the projection begins. Earnings are CPI-deflated to January 2016 and expressed in US dollars.

accounting for program costs, the total net fiscal impact is -$1,118, indicating full recovery of the initial investment. This implies an infinite MVPF, as the WTP is strictly positive. The program is projected to fully repay its initial costs (i.e., to have zero net cost to the government) 27 years after participation, at age 46.[12]

We compare our results with those reported in HSK (2020). Online Appendix Figure A2 shows that YET performs similarly, or better than, most US-based job training programs targeting beneficiaries of comparable age (18 to 21), when evaluated 8 and 21 years after completion. Furthermore, the projected infinite MVPF of YET is comparable to those of US policies targeting children, such as investments in early childhood education, child health insurance, and college access.

6 Conclusions

This study presents the first experimental evaluation of the long-term effects of a work-study program offering formal work experience to young students in state-owned companies. Leveraging a randomized lottery design, we find sustained earnings gains of 10–13 percent in Years 3–7, driven by increases in both employment and monthly wages.

Our findings suggest that well-designed work-study programs can be a cost-effective tool for facilitating school-to-work transitions, particularly in contexts with high youth unemployment. By integrating education with early labor market exposure, such programs can shape career trajectories and generate long-term benefits. Future research could explore whether similar effects hold in private sector arrangements.

A distinctive feature of the studied program is the provision of well-paid, prestigious clerical positions in state-owned firms, involving tasks that foster learning and human capital accumulation while allowing continued schooling. This combination may be central to the program's effectiveness. Although connections are unlikely to be a main channel, since most state-owned enterprises recruit through public contests and very few treated youths are employed in these firms seven years later, the prestige of these jobs may have enhanced motivation, further reinforcing the impact.

Our estimates identify partial-equilibrium treatment effects. If the youth labor market is close to zero-sum, gains for participants could partly reflect displacement of

[12] See the MVPF by projected age in Online Appendix Figure A1.

non-participants rather than aggregate productivity increases. Lacking data to test this hypothesis, aggregate effects remain an open question. Nonetheless, our findings highlight the potential of work-study programs to improve youth outcomes and inform future policy design.

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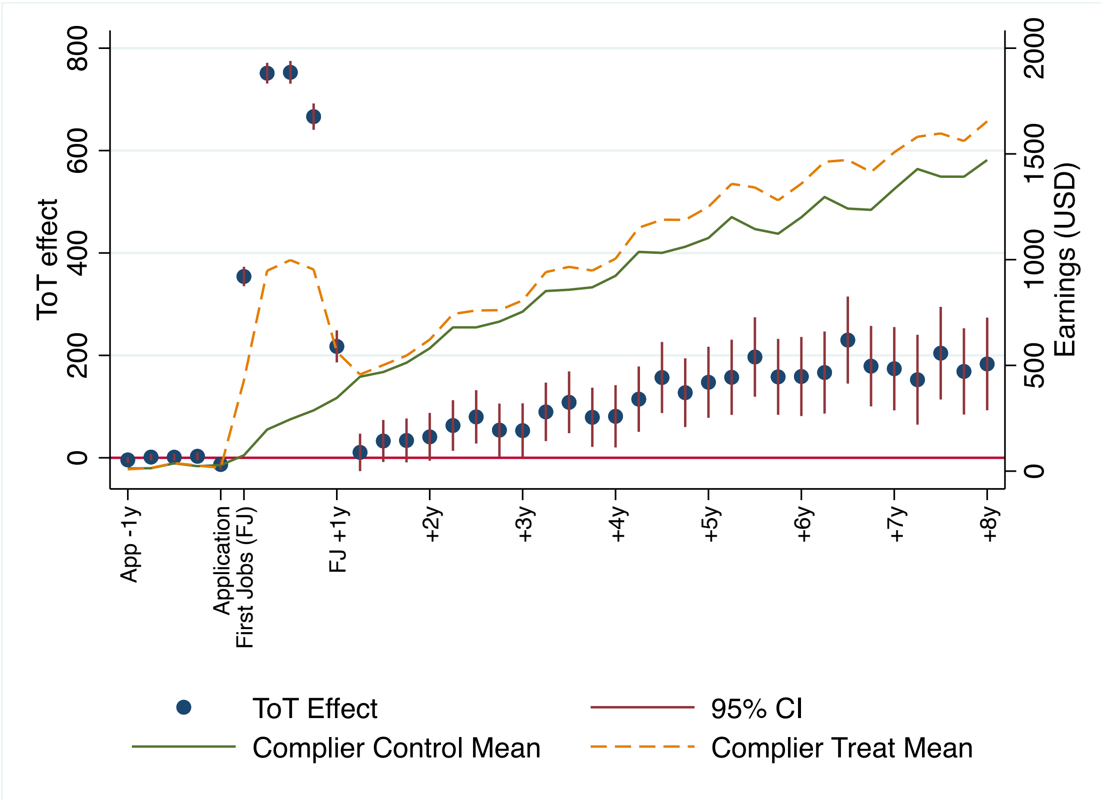
Figures

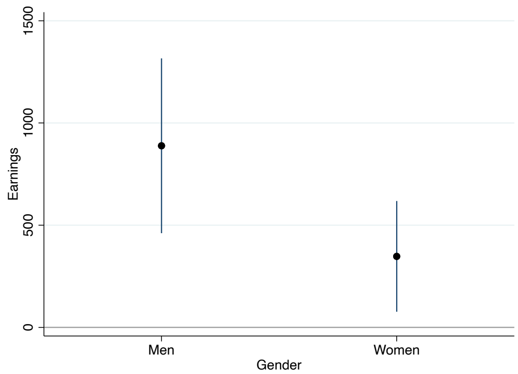
Figure 1: Quarterly Earnings



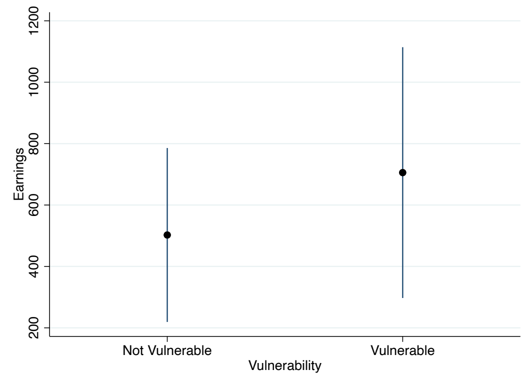
Source: Administrative data and YET Application Form.
Notes: This figure plots the evolution of quarterly treatment effects (left Y-axis), and of average quarterly earnings by treatment group (right axis). We use blue dots to report treatment effects, and red vertical lines for their 95 percent confidence intervals. The dashed orange (resp. solid green) line reports quarterly earnings for the treated individuals (resp. compliers in the control group).

Figure 2: Treatment Effect Heterogeneity by Baseline Characteristics. Average Effect Years 3–7

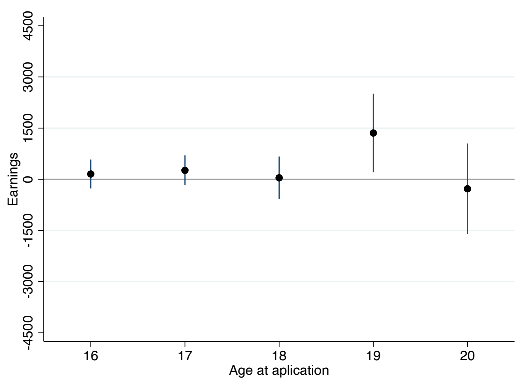
(a) By Gender



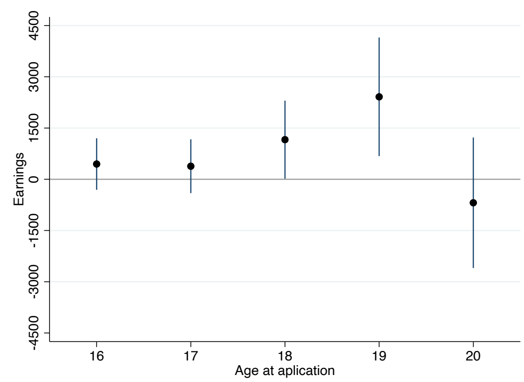
(b) By Financial Vulnerability



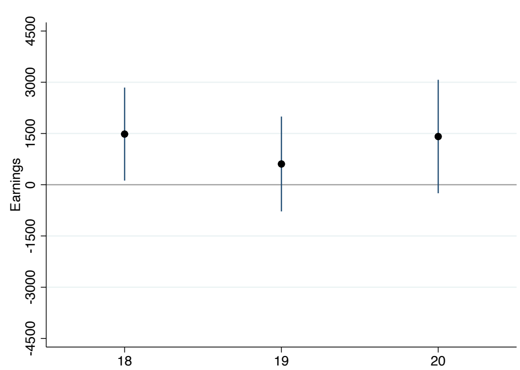
(c) By Age for Academic High Schoolers



(d) By Age for Technical High Schoolers



(e) By Age for University Students



Source: Administrative data and YET Application Form.
Notes: This figure shows treatment effects by gender, household vulnerability, and age and education at application date. Vulnerable households include households receiving a cash transfer. Treatment effects are obtained by two stage least squares regressions of Equation (1), where we further interact the treatment dummy with, respectively, a gender, vulnerability, and age dummy. Vertical lines represent 95 percent confidence intervals.

Tables

Table 1: Effect of YET on Labor Outcomes

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2158.36	6.86	0.56	-20.14	7.64
	(41.72)	(0.08)	(0.01)	(3.15)	(0.07)
	[1141.28]	[2.73]	[0.44]	[360.58]	[1.80]
Post-Program years					
Year 1	92.87	0.01	0.05	7.64	0.42
	(75.17)	(0.12)	(0.01)	(7.48)	(0.11)
	[2075.98]	[4.35]	[0.58]	[421.70]	[3.01]
Year 2	218.96	0.05	0.02	25.65	0.35
	(93.75)	(0.13)	(0.01)	(8.49)	(0.12)
	[2873.93]	[5.35]	[0.65]	[481.45]	[3.85]
Year 3	341.43	0.16	0.01	34.59	0.36
	(108.27)	(0.13)	(0.01)	(9.16)	(0.13)
	[3547.53]	[6.00]	[0.69]	[534.54]	[4.42]
Year 4	512.31	0.33	0.03	38.31	0.55
	(123.41)	(0.13)	(0.01)	(10.41)	(0.13)
	[4295.68]	[6.56]	[0.71]	[596.57]	[4.99]
Year 5	610.84	0.32	0.02	45.90	0.61
	(136.61)	(0.13)	(0.01)	(11.41)	(0.13)
	[4699.80]	[6.74]	[0.73]	[636.71]	[5.25]
Year 6	669.94	0.36	0.01	57.77	0.55
	(148.39)	(0.13)	(0.01)	(12.59)	(0.14)
	[5183.92]	[7.06]	[0.75]	[669.72]	[5.61]
Year 7	652.00	0.31	0.02	43.71	0.58
	(159.64)	(0.13)	(0.01)	(13.42)	(0.14)
	[5761.85]	[7.46]	[0.76]	[708.89]	[5.93]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
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, adjusted for inflation using the CPI and converted into US 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 parentheses, and control complier means in brackets. *Months with regular job:* number of months over 12 months with regular contract.

Table 2: Effect of YET on Earnings by Employer Industry

	(1) Agriculture	(2) Manufacturing & Energy	(3) Market Services	(4) Non-Market Services
Program year				
Year 0	-28.53	1493.12	518.11	177.35
	(7.84)	(38.76)	(46.87)	(29.05)
	[35.67]	[188.57]	[755.37]	[160.05]
Post-Program years				
Year 1	-20.78	56.90	121.87	-65.10
	(13.50)	(40.81)	(65.15)	(36.50)
	[64.42]	[321.15]	[1341.07]	[348.30]
Year 2	-15.65	120.96	170.82	-55.07
	(19.48)	(54.07)	(81.00)	(50.09)
	[74.57]	[410.21]	[1835.93]	[550.33]
Year 3	-0.93	149.99	220.46	-19.20
	(22.35)	(63.13)	(93.70)	(64.46)
	[90.49]	[493.52]	[2177.49]	[781.42]
Year 4	-7.31	215.50	326.64	-9.80
	(26.81)	(73.73)	(106.72)	(80.40)
	[104.53]	[547.17]	[2502.96]	[1139.18]
Year 5	-30.50	250.68	353.63	44.20
	(26.27)	(81.27)	(115.99)	(94.50)
	[128.69]	[574.78]	[2541.96]	[1451.32]
Year 6	-29.47	272.57	407.91	28.59
	(30.88)	(90.37)	(125.15)	(104.73)
	[130.46]	[619.40]	[2669.59]	[1761.55]
Year 7	-30.70	223.33	416.81	44.79
	(31.99)	(95.37)	(135.23)	(115.30)
	[143.25]	[695.89]	[2869.25]	[2048.84]
Observations	90,423	90,423	90,423	90,423

Source: Administrative data and YET Application Form.
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 Agriculture sector. Columns (2) to (4) correspond to the Manufacturing & Energy Sector, Market Services, and Non-Market Services, respectively. Market Services are sectors where services are typically provided in exchange for payment under competitive market conditions. They include Whole-sale and retail trade, Transportation and storage, Accommodation and food service activities, Information and communication, Financial and insurance activities. Non-Market Services are sectors typically funded or provided by the government, non-profit organizations, or institutions where users do not pay directly. They include Public Administration and Defense, Education, Human Health and Social Work Activities. Controls for lottery design (lottery and quota dummies) 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. Earnings are winsorized at the top 1 percent of positive values, adjusted for inflation using the CPI and converted into US dollars. Robust standard errors shown in parenthesis and control complier means in brackets.

Online Appendix

The Lasting Effects of Working while in School: A Long-Term Follow-Up

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Noemí Katzkowicz
Thomas Le Barbanchon
Diego Ubfal*

October 30, 2025

The Online Appendix includes five sections. Section A contains additional tables and figures. Section B assesses the robustness of our main results. Section C presents robustness checks on the cost-benefit analysis. Section D provides further details about the work-study program. Section E presents our back-of-the-envelope computation of the contribution of the education and experience channels.

*Ferrando: Tilburg University. Katzkowicz: Universidad de la República. Le Barbanchon: Bocconi University. Ubfal: World Bank.

A Additional Tables and Figures

Table A1: Main Features of the Program by Edition

Edition	1	2	3
Application Date	May 2012	May 2013	May 2014
Applications	46,544	43,661	31,990
Applicants	46,008	42,643	30,969
Job Offers Made	754	981	955
Jobs Started	592	754	718
Jobs Completed	549	686	660
Sector: Civil	0.82	0.73	0.70
Sector: Industry/Trade	0.02	0.04	0.04
Sector: Banking	0.16	0.23	0.26
Localities	51	64	67

Source: Administrative data and YET Application Form.
Notes: There is a downward trend in applications over time, probably due to the program spending more resources on 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.

Table A2: Balance Between Treatment and Control Groups

	(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)	228.13	800.69	200.21	757.38	0.20
Positive Earnings	0.15	0.36	0.15	0.35	0.84
Months with Positive Earnings	0.71	2.13	0.62	1.97	0.12
Months with Regular Job	0.41	1.63	0.36	1.49	0.09
Panel D. Aggregate orthogonality test for panels A-C p-value (joint F-test)					0.54
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. Vulnerable households include households receiving a cash transfer and/or a food card (labelled as *Highly Vulnerable*). 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 as 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 A3: 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	0.77	0.79	0.77	0.77
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	90,423	36,181	30,410	23,832

Source: Administrative data and YET Application Form.
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 18 or less at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Robust standard errors shown in parenthesis. Results for the first edition are obtained with the same method used to select unique applications as in the other editions. Results are almost identical if we keep the first application.

Table A4: Returns to Having a Regular Job. Control Group

	(1) Total earnings
Program year	
Year 0	428.25
	(1.79)
	[462.39]
Post-Program years	
Year 1	459.73
	(1.82)
	[892.37]
Year 2	494.76
	(1.97)
	[1238.76]
Year 3	533.06
	(2.14)
	[1511.34]
Year 4	579.65
	(2.36)
	[1760.67]
Year 5	626.53
	(2.57)
	[1852.16]
Year 6	666.95
	(2.77)
	[1893.23]
Year 7	697.54
	(2.98)
	[1993.21]
Observations	87,737

Source: Administrative data and YET Application Form.
Notes: The independent variable is defined as number of months with a regular contract. 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. Robust standard errors shown in parenthesis and control means in brackets.

Table A5: Effect of YET on Enrollment in Education

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.
Program year				
Year 0	0.126	0.102	0.018	0.007
	(0.009)	(0.010)	(0.007)	(0.004)
	[0.731]	[0.500]	[0.203]	[0.017]
Post-Program years				
Year 1	0.037	0.030	0.011	0.003
	(0.012)	(0.011)	(0.009)	(0.004)
	[0.608]	[0.321]	[0.265]	[0.022]
Year 2	0.041	0.024	0.009	0.008
	(0.012)	(0.010)	(0.009)	(0.004)
	[0.452]	[0.225]	[0.205]	[0.025]
Year 3	0.033	0.025	0.004	0.003
	(0.011)	(0.009)	(0.008)	(0.004)
	[0.277]	[0.141]	[0.122]	[0.026]
Year 4	0.015	0.011	0.004	0.004
	(0.009)	(0.007)	(0.006)	(0.004)
	[0.145]	[0.085]	[0.052]	[0.025]
Year 5	0.005	0.008	-0.002	0.004
	(0.008)	(0.006)	(0.005)	(0.004)
	[0.104]	[0.055]	[0.049]	[0.021]
Year 6	0.003	-0.003	0.007	0.001
	(0.007)	(0.005)	(0.005)	(0.003)
	[0.085]	[0.044]	[0.042]	[0.016]
Year 7	0.010	0.005	0.005	0.005
	(0.007)	(0.005)	(0.005)	(0.004)
	[0.073]	[0.037]	[0.036]	[0.015]
Observations	90,423	90,423	90,423	90,423

Source: Administrative data and YET Application Form.
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”. For 2017, we do not have data on students taking two exams, and therefore the mean of university registration is underestimated (this applies to Year 4 for edition 1, Year 3 for edition 2, and Year 2 for edition 3). In Column (4), ”Tertiary Non-Univ.” is enrollment in tertiary technical schools. Standard errors robust to heteroskedasticity shown in parentheses, and control complier means in brackets.

Table A6: Treatment Effect Heterogeneity by Gender

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Post-Program years					
Years 3–7					
Treated (T)	889.80	0.35	0.02	70.88	0.71
	(218.28)	(0.17)	(0.01)	(16.63)	(0.19)
T * Female	-542.84	-0.09	-0.01	-49.53	-0.29
	(258.31)	(0.22)	(0.02)	(19.73)	(0.23)
Female	-903.60	-0.38	-0.03	-83.45	0.17
	(30.94)	(0.03)	(0.00)	(2.41)	(0.03)
CCM Male	[5140.69]	[6.96]	[0.74]	[630.79]	[5.03]
CCM Female	[4418.18]	[6.64]	[0.72]	[565.95]	[5.38]
p-value T+T*Female=0	0.01	0.05	0.17	0.04	0.00
Observations	90,423	90,423	90,423	81,217	90,423

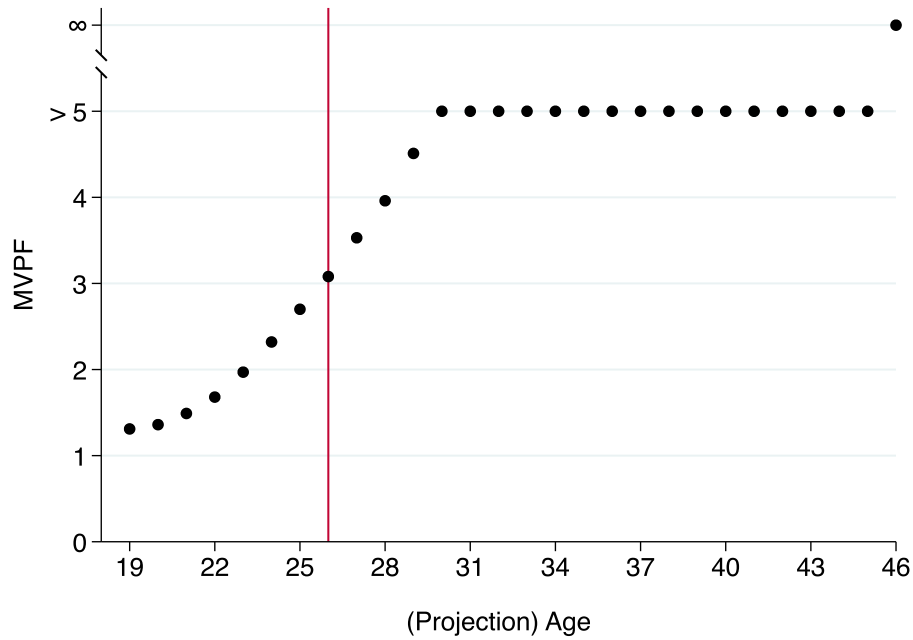
Source: Administrative data and YET Application Form.
Notes: Two stage least squares regressions where we instrument the YET participation dummy, and its interaction with a female dummy with a job offer dummy and the corresponding interaction. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age 18 or less at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Robust standard errors 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).

Table A7: Treatment Effect Heterogeneity by Household Vulnerability

	(1)	(2)	(3)	(4)
	Program Year	**Post-Program years**		
	Year 0	Year 1	Year 2	Years 3-7
Treated (T)	2074.53	56.81	166.59	502.74
	(49.46)	(90.12)	(111.69)	(144.58)
T * Vulnerable	311.54	134.02	194.62	202.78
	(91.08)	(160.79)	(203.41)	(253.40)
Vulnerable	-120.81	-124.06	-271.02	-820.49
	(14.60)	(20.76)	(25.21)	(31.03)
CCM Non-Vulnerable	[1223.60]	[2190.00]	[3057.78]	[5050.76]
CCM Vulnerable	[917.65]	[1766.47]	[2374.90]	[3739.71]
p-value T+T*Vulnerable=0	0.00	0.15	0.03	0.00
Observations	90,423	90,423	90,423	90,423

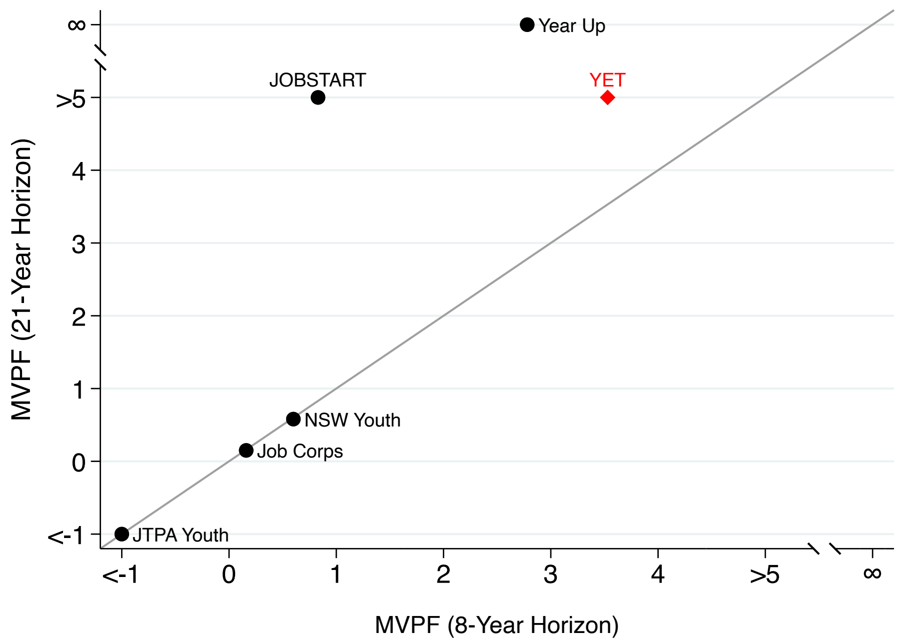
Source: Administrative data and YET Application Form.
Notes: Two stage least squares regressions where we instrument the YET participation dummy, and its interaction with a vulnerability dummy with a job offer dummy and the corresponding interaction. The dependent variable is total earnings. Vulnerable households include households receiving a cash transfer. Controls for lottery design (lottery and quota dummies) 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. Robust standard errors shown in parenthesis. p-value: p-value of the test that the treatment effect for individuals in vulnerable households is zero (sum of the treated and interaction coefficients).

Figure A1: MVPF Over the Life Cycle



Source: Administrative and household survey data.
Notes: This figure plots the MVPF by age. The vertical line indicates the last age for which the MVPF is based on observed data.

Figure A2: MVPF for YET and Job Training Programs. 8– and 21–year Horizon



Source: Authors' calculations based on administrative and household survey data, and estimates from Hendren and Sprung-Keyser (2020).
Notes: This figure plots the MVPF at 21 years after the programs against the MVPF 8 years after the programs. The black circles represent job training programs, based on Table C.I of the Online Appendix in Hendren and Sprung-Keyser (2020) (the 8-year estimate for Job Corps is our own calculation based on the study's replication file). The red diamond represents estimates for YET, the program we study.

B Robustness Checks on Labor Market Results

In this section, we test the robustness of our main results to alternative specifications. Specifically, we show that our main results in Table 1 are robust to omitting controls (see Table B1), clustering the standard errors at the locality level (see Table B2), and not winsorizing the earnings variables (see Table B3).

Additionally, we provide intention-to-treat estimates (ITT) that do not rely on the exclusion restriction, and we obtain consistent results (see Table B4). 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 Table B5). 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.

We assess the robustness of our handling of youth with multiple applications in Tables B6, B7, and B8. In Table B6, we control for the number of applications within an edition-year using fixed effects instead of a linear form. In Table B7, we restrict the estimation sample to applicants who apply to only one edition-year. In Table B8, we run the specification at the application level, clustering standard errors at the applicant level.

Finally, we provide bounds for the ITT effects on monthly wages to account for selection into employment (see Table B9).

Table B1: Effect of YET on Labor Outcomes. No controls

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2137.31	6.83	0.56	-32.26	7.62
	(45.12)	(0.08)	(0.01)	(3.18)	(0.07)
	[1162.33]	[2.76]	[0.44]	[372.70]	[1.81]
Post-Program years					
Year 1	70.89	-0.02	0.05	3.26	0.41
	(78.11)	(0.12)	(0.01)	(7.77)	(0.11)
	[2097.95]	[4.38]	[0.58]	[426.09]	[3.02]
Year 2	200.06	0.03	0.02	22.63	0.35
	(96.60)	(0.13)	(0.01)	(8.80)	(0.13)
	[2892.83]	[5.36]	[0.65]	[484.47]	[3.85]
Year 3	327.46	0.15	0.01	32.76	0.37
	(111.77)	(0.13)	(0.01)	(9.56)	(0.13)
	[3561.50]	[6.00]	[0.69]	[536.37]	[4.42]
Year 4	503.80	0.33	0.03	37.31	0.57
	(128.16)	(0.13)	(0.01)	(10.86)	(0.14)
	[4304.19]	[6.56]	[0.71]	[597.58]	[4.98]
Year 5	607.17	0.32	0.02	45.17	0.63
	(141.89)	(0.13)	(0.01)	(11.92)	(0.14)
	[4703.47]	[6.74]	[0.73]	[637.44]	[5.24]
Year 6	672.52	0.36	0.01	59.44	0.57
	(154.72)	(0.13)	(0.01)	(13.20)	(0.14)
	[5181.34]	[7.06]	[0.75]	[668.05]	[5.59]
Year 7	660.41	0.31	0.02	44.56	0.61
	(167.31)	(0.13)	(0.01)	(14.16)	(0.14)
	[5753.44]	[7.45]	[0.76]	[708.05]	[5.91]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1 without including individual control variables. Controls for lottery design (lottery and quota dummies) are included.

Table B2: Effect of YET on Labor Outcomes. Clustering at Locality Level

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2158.36	6.86	0.56	-20.14	7.64
	(186.61)	(0.36)	(0.04)	(8.84)	(0.26)
	[1141.28]	[2.73]	[0.44]	[360.58]	[1.80]
Post-Program years					
Year 1	92.87	0.01	0.05	7.64	0.42
	(79.49)	(0.13)	(0.01)	(5.80)	(0.09)
	[2075.98]	[4.35]	[0.58]	[421.70]	[3.01]
Year 2	218.96	0.05	0.02	25.65	0.35
	(65.79)	(0.09)	(0.01)	(7.07)	(0.08)
	[2873.93]	[5.35]	[0.65]	[481.45]	[3.85]
Year 3	341.43	0.16	0.01	34.59	0.36
	(78.69)	(0.12)	(0.01)	(5.21)	(0.12)
	[3547.53]	[6.00]	[0.69]	[534.54]	[4.42]
Year 4	512.31	0.33	0.03	38.31	0.55
	(120.05)	(0.13)	(0.01)	(8.80)	(0.17)
	[4295.68]	[6.56]	[0.71]	[596.57]	[4.99]
Year 5	610.84	0.32	0.02	45.90	0.61
	(88.21)	(0.08)	(0.01)	(7.35)	(0.09)
	[4699.80]	[6.74]	[0.73]	[636.71]	[5.25]
Year 6	669.94	0.36	0.01	57.77	0.55
	(111.67)	(0.12)	(0.01)	(8.08)	(0.10)
	[5183.92]	[7.06]	[0.75]	[669.72]	[5.61]
Year 7	652.00	0.31	0.02	43.71	0.58
	(116.75)	(0.14)	(0.01)	(7.96)	(0.09)
	[5761.85]	[7.46]	[0.76]	[708.89]	[5.93]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, but clustering the standard errors at the locality level.

Table B3: Effect of YET on Labor Outcomes. No Winsoring

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2157.97	6.86	0.56	-20.68	7.63
	(43.53)	(0.08)	(0.01)	(3.34)	(0.07)
	[1153.62]	[2.74]	[0.44]	[362.12]	[1.80]
Post-Program years					
Year 1	116.16	0.01	0.05	10.69	0.42
	(79.18)	(0.12)	(0.01)	(8.04)	(0.11)
	[2087.92]	[4.35]	[0.58]	[423.34]	[3.01]
Year 2	252.09	0.05	0.02	29.63	0.35
	(99.32)	(0.13)	(0.01)	(9.24)	(0.12)
	[2890.35]	[5.35]	[0.65]	[483.65]	[3.85]
Year 3	377.10	0.16	0.01	38.87	0.36
	(114.09)	(0.13)	(0.01)	(9.96)	(0.13)
	[3556.65]	[6.00]	[0.69]	[535.61]	[4.43]
Year 4	555.88	0.33	0.03	43.53	0.55
	(130.75)	(0.13)	(0.01)	(11.48)	(0.13)
	[4294.38]	[6.56]	[0.71]	[596.20]	[4.99]
Year 5	679.03	0.32	0.02	53.38	0.61
	(147.25)	(0.13)	(0.01)	(12.80)	(0.13)
	[4693.68]	[6.74]	[0.73]	[636.16]	[5.25]
Year 6	735.40	0.36	0.01	64.94	0.55
	(159.02)	(0.13)	(0.01)	(13.97)	(0.14)
	[5177.61]	[7.06]	[0.75]	[669.12]	[5.61]
Year 7	720.48	0.31	0.02	50.82	0.58
	(173.79)	(0.13)	(0.01)	(15.14)	(0.14)
	[5755.39]	[7.46]	[0.76]	[708.37]	[5.93]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, without winsorizing the dependent variables used in Column (1) and Column (4).

Table B4: Effect of YET on Labor Outcomes. ITT Effects

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	1669.87	5.31	0.44	-17.32	5.91
	(36.11)	(0.08)	(0.01)	(2.72)	(0.08)
	[1321.06]	[3.07]	[0.46]	[380.18]	[2.06]
Post-Program years					
Year 1	71.85	0.01	0.04	5.88	0.33
	(58.22)	(0.09)	(0.01)	(5.77)	(0.09)
	[2260.56]	[4.57]	[0.60]	[438.53]	[3.20]
Year 2	169.41	0.04	0.02	19.74	0.27
	(72.66)	(0.10)	(0.01)	(6.54)	(0.10)
	[3005.40]	[5.42]	[0.66]	[495.25]	[3.93]
Year 3	264.16	0.13	0.01	26.92	0.28
	(83.92)	(0.10)	(0.01)	(7.14)	(0.10)
	[3684.42]	[6.03]	[0.69]	[550.99]	[4.50]
Year 4	396.36	0.26	0.02	29.88	0.43
	(95.76)	(0.10)	(0.01)	(8.13)	(0.10)
	[4406.61]	[6.53]	[0.71]	[611.72]	[4.99]
Year 5	472.60	0.25	0.01	36.12	0.47
	(106.00)	(0.10)	(0.01)	(8.99)	(0.10)
	[4868.15]	[6.73]	[0.72]	[657.42]	[5.24]
Year 6	518.32	0.28	0.01	45.45	0.43
	(115.16)	(0.10)	(0.01)	(9.92)	(0.11)
	[5312.69]	[6.97]	[0.74]	[692.67]	[5.51]
Year 7	504.44	0.24	0.02	34.25	0.45
	(123.86)	(0.10)	(0.01)	(10.53)	(0.11)
	[5766.69]	[7.24]	[0.75]	[725.81]	[5.77]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, but presents ITT effects rather than ToT effects. Control means are presented in brackets.

Table B5: Effect of Working and Studying During Program Year

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	3755.89	11.94	0.98	-93.10	13.29
	(84.89)	(0.20)	(0.02)	(14.98)	(0.22)
	[-53.72]	[-0.56]	[0.02]	[360.43]	[-1.06]
Post-Program years					
Year 1	161.60	0.01	0.09	17.34	0.73
	(130.49)	(0.20)	(0.02)	(16.97)	(0.19)
	[1310.17]	[2.90]	[0.43]	[393.53]	[1.87]
Year 2	381.03	0.09	0.04	50.80	0.61
	(162.63)	(0.22)	(0.02)	(16.81)	(0.22)
	[2205.16]	[4.43]	[0.57]	[443.16]	[3.10]
Year 3	594.14	0.28	0.02	65.96	0.64
	(188.03)	(0.23)	(0.02)	(17.53)	(0.23)
	[2816.54]	[5.18]	[0.62]	[485.90]	[3.69]
Year 4	891.50	0.58	0.05	71.61	0.97
	(214.57)	(0.22)	(0.02)	(19.50)	(0.23)
	[3550.28]	[5.88]	[0.66]	[549.77]	[4.28]
Year 5	1062.97	0.57	0.03	84.23	1.06
	(237.74)	(0.22)	(0.02)	(21.02)	(0.23)
	[3911.72]	[6.15]	[0.68]	[581.59]	[4.62]
Year 6	1165.81	0.62	0.01	105.11	0.97
	(258.65)	(0.23)	(0.02)	(23.05)	(0.24)
	[4342.15]	[6.56]	[0.71]	[604.72]	[5.03]
Year 7	1134.59	0.54	0.04	78.58	1.02
	(277.91)	(0.22)	(0.02)	(24.17)	(0.24)
	[5151.49]	[7.16]	[0.74]	[659.37]	[5.48]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: 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 18 or less at application, a dummy for receiving cash transfers, baseline earnings and dummies for baseline education type. Robust standard errors 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 are the same among those offered and not offered a YET job.

Table B6: Effect of YET on Labor Outcomes (with Number of Applications Fixed Effects)

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2160.26	6.86	0.56	-19.94	7.64
	(41.72)	(0.08)	(0.01)	(3.16)	(0.07)
	[1139.38]	[2.73]	[0.44]	[360.38]	[1.79]
Post-Program years					
Year 1	95.14	0.01	0.05	8.05	0.42
	(75.19)	(0.12)	(0.01)	(7.48)	(0.11)
	[2073.71]	[4.35]	[0.58]	[421.29]	[3.01]
Year 2	220.83	0.05	0.02	26.08	0.35
	(93.80)	(0.13)	(0.01)	(8.49)	(0.12)
	[2872.06]	[5.35]	[0.65]	[481.01]	[3.85]
Year 3	344.16	0.16	0.01	34.94	0.37
	(108.30)	(0.13)	(0.01)	(9.16)	(0.13)
	[3544.80]	[5.99]	[0.69]	[534.19]	[4.42]
Year 4	514.27	0.33	0.03	38.40	0.56
	(123.43)	(0.13)	(0.01)	(10.41)	(0.13)
	[4293.72]	[6.56]	[0.71]	[596.48]	[4.99]
Year 5	612.70	0.32	0.02	46.09	0.61
	(136.62)	(0.13)	(0.01)	(11.42)	(0.13)
	[4697.95]	[6.74]	[0.73]	[636.52]	[5.25]
Year 6	671.91	0.36	0.01	57.97	0.56
	(148.40)	(0.13)	(0.01)	(12.59)	(0.14)
	[5181.95]	[7.06]	[0.75]	[669.52]	[5.60]
Year 7	654.40	0.31	0.02	44.12	0.59
	(159.65)	(0.13)	(0.01)	(13.42)	(0.14)
	[5759.46]	[7.46]	[0.76]	[708.48]	[5.93]
Observations	90,423	90,423	90,423	67,793	90,423

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, using fixed effects for the number of applications across localities instead of controlling for it linearly.

Table B7: Effect of YET on Labor Outcomes (Restricted to Youth Applying to Only One Program Edition)

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2024.36	6.63	0.56	-24.58	7.49
	(45.25)	(0.08)	(0.01)	(3.50)	(0.08)
	[1215.46]	[2.89]	[0.44]	[361.75]	[1.89]
Post-Program years					
Year 1	-66.16	-0.19	0.04	-5.78	0.27
	(83.20)	(0.13)	(0.01)	(8.40)	(0.13)
	[2111.54]	[4.40]	[0.59]	[421.41]	[3.01]
Year 2	116.25	-0.04	0.01	17.65	0.29
	(105.26)	(0.14)	(0.01)	(9.69)	(0.14)
	[2812.66]	[5.26]	[0.64]	[478.75]	[3.71]
Year 3	226.37	0.07	-0.00	26.45	0.29
	(121.49)	(0.15)	(0.01)	(10.41)	(0.15)
	[3425.89]	[5.86]	[0.68]	[527.62]	[4.24]
Year 4	476.15	0.41	0.03	28.66	0.60
	(138.67)	(0.15)	(0.01)	(11.88)	(0.15)
	[4133.36]	[6.37]	[0.70]	[591.54]	[4.72]
Year 5	625.63	0.43	0.03	39.12	0.70
	(154.53)	(0.15)	(0.01)	(13.12)	(0.15)
	[4509.95]	[6.52]	[0.71]	[631.45]	[4.97]
Year 6	736.59	0.44	0.02	55.66	0.63
	(170.31)	(0.15)	(0.01)	(14.65)	(0.16)
	[4944.62]	[6.82]	[0.73]	[660.62]	[5.32]
Year 7	723.99	0.39	0.03	43.53	0.66
	(182.59)	(0.15)	(0.01)	(15.57)	(0.16)
	[5407.99]	[7.11]	[0.74]	[696.39]	[5.54]
Observations	65,239	65,239	65,239	48,089	65,239

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, but restricts the sample to youth who applied to only one edition of the program.

Table B8: Effect of YET on Labor Outcomes (Application-level Regression)

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages	(5) Months with regular job
Program year					
Year 0	2141.51	6.83	0.55	-18.90	7.63
	(44.04)	(0.08)	(0.01)	(3.20)	(0.07)
	[1159.17]	[2.77]	[0.45]	[359.18]	[1.81]
Post-Program years					
Year 1	62.71	-0.05	0.04	5.23	0.33
	(76.66)	(0.12)	(0.01)	(7.68)	(0.12)
	[2099.42]	[4.45]	[0.60]	[419.90]	[3.09]
Year 2	178.47	-0.03	0.02	22.51	0.26
	(98.13)	(0.13)	(0.01)	(9.07)	(0.13)
	[2932.30]	[5.45]	[0.66]	[484.73]	[3.97]
Year 3	270.06	0.06	0.01	31.38	0.22
	(115.57)	(0.14)	(0.01)	(9.71)	(0.14)
	[3645.17]	[6.12]	[0.70]	[538.61]	[4.57]
Year 4	444.78	0.23	0.02	36.92	0.44
	(131.77)	(0.14)	(0.01)	(11.05)	(0.14)
	[4430.35]	[6.72]	[0.72]	[602.18]	[5.17]
Year 5	505.55	0.23	0.01	39.77	0.50
	(144.02)	(0.13)	(0.01)	(11.97)	(0.14)
	[4854.66]	[6.88]	[0.74]	[644.54]	[5.40]
Year 6	554.35	0.27	0.00	48.62	0.46
	(154.72)	(0.14)	(0.01)	(13.12)	(0.14)
	[5349.12]	[7.21]	[0.75]	[679.96]	[5.75]
Year 7	516.32	0.22	0.02	33.07	0.50
	(164.53)	(0.13)	(0.01)	(13.88)	(0.14)
	[5948.84]	[7.61]	[0.77]	[721.26]	[6.08]
Observations	122,195	122,195	122,195	92,753	122,195

Source: Administrative data and YET Application Form.
Notes: Replicates Table 1, but keeps all applications submitted by each individual and clustering standard errors at the applicant level. All applications within the year when the youth participated in the program are considered treated units. We use as instrument whether the youth received an offer in the edition year of that application.

Table B9: 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	5.88	-27.66	28.93	-35.86	38.18
	(5.77)	(4.99)	(5.62)		
	[438.53]				
Year 2	19.74	17.90	29.07	7.27	39.73
	(6.54)	(6.46)	(6.48)		
	[495.25]				
Year 3	26.92	25.78	31.83	14.10	43.51
	(7.14)	(7.10)	(7.10)		
	[550.99]				
Year 4	29.88	6.98	41.00	-5.19	54.30
	(8.13)	(7.40)	(8.08)		
	[611.72]				
Year 5	36.12	33.78	41.72	19.13	56.49
	(8.99)	(8.91)	(8.98)		
	[657.42]				
Year 6	45.45	43.59	52.30	27.40	68.58
	(9.92)	(9.84)	(9.89)		
	[692.67]				
Year 7	34.25	2.25	47.28	-13.30	64.59
	(10.53)	(9.45)	(10.53)		
	[725.81]				
Observations	90,423				

Source: Administrative data and YET Application Form.
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 percent 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 percent 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.

C Robustness Checks on Cost-Benefit Analysis

As noted in the main text, the baseline MVPF for YET is 3.1 over the first seven years and infinite over the life cycle, with full fiscal recovery by age 46. Our baseline specification follows that of Hendren and Sprung-Keyser (2020), hereafter HSK.[1]

In this appendix, we assess the robustness of our conclusions by exploring alternative scenarios that vary the tax rate, direct costs, and discount rate. Specifically, we examine four variants of the baseline scenario. The main features and findings for each variant are summarized in Table C1.

Scenario 2 mirrors Scenario 1, with the difference that it includes payroll taxes in the tax rate. In our baseline specification, payroll taxes are excluded to be conservative and because they are partially returned to workers as benefits. In this variant, however, workers' payroll contributions are counted as government revenue. Including payroll taxes increases the government's fiscal gain from higher post-program earnings, thereby reducing net costs and causing them to turn negative earlier. As a result, the MVPF becomes infinite by age 32, compared to age 46 in the baseline scenario. For comparison, under a 30 percent tax rate—which approximates our age-specific tax rates from the early 30s onward when payroll taxes are included—HSK (2020) show that job training programs yield, on average, MVPFs below 1, while childhood interventions maintain infinite MVPF by the end of the life cycle (see Appendix Figure V, Panel D, in HSK, 2020). Thus, even under the higher tax rate assumption, YET continues to perform comparably to these high-return childhood interventions.

Scenario 3 is identical to Scenario 1, except that it excludes participants' salaries from direct costs. In our baseline scenario, 50 percent of salaries are counted as program costs. Excluding salaries from costs sharply reduces net costs, allowing the program to repay itself by age 20. This scenario reinforces our main conclusions, but it is the least conservative.

Scenario 4 is identical to Scenario 1, except that it includes 100 percent of participants' salaries as direct costs. This is a highly conservative approach, effectively assuming that participants generated no value through their work. Hence, it represents an upper-bound estimate, as firms volunteered to participate and participants likely generated added value through their work. Under this scenario, the program does not repay itself by age 65, resulting in lower performance compared to childhood interventions.

[1] We depart from HSK (2020) in one way. We use yearly tax rates instead of fixed ones, as this information is available in our setting and provides greater precision.

However, the performance of YET relative to job training programs targeting similar age groups remains strong: the MVPF 8 and 21 years after the program are 1.7 and 6, respectively, which compare favorably to many job training programs (see Figure A2).

Scenario 5 applies a higher discount rate of 7 percent to the baseline specification, following a robustness test in HSK (2020). The discount rate is used to discount changes in real earnings back to the program year. This scenario is the most conservative, and under these conditions, the MVPF does not become infinite over the life cycle. Importantly, adopting a higher discount rate does not change our main conclusions. For comparison, HSK (2020) also find that, with a 7 percent discount rate, even childhood interventions no longer yield infinite MVPFs, and our program continues to perform comparably to these interventions under this conservative assumption (see Appendix Figure IV, Panel D, in HSK, 2020).

Overall, these robustness checks show that varying key assumptions does not substantially affect the relative performance of YET.

Table C1: MVPF Under Different Assumptions

Scenario	Discount rate	Payroll taxes	Salaries in costs (%)	MVPF 26	MVPF 65	Age MVPF = ∞
1 (Baseline)	0.03	No	50%	3.08	∞	46
2	0.03	Yes	50%	4.15	∞	32
3	0.03	No	0%	∞	∞	20
4	0.03	No	100%	1.48	41.17	–
5	0.07	No	50%	2.76	24.94	–

Source: Administrative and household survey data.
Notes: Scenario 1 reports the baseline specification, which applies a 3 percent discount rate, excludes payroll taxes from the tax rate, and includes 50 percent of salaries as direct costs. Scenario 2 incorporates payroll taxes into the tax rate. Scenario 3 excludes salaries from direct costs. Scenario 4 includes 100 percent of salaries as direct costs. Scenario 5 is identical to the baseline except that it applies a higher discount rate of 7 percent.

D Institutional Details: The Work-Study Program

The work-study program "Yo Estudio y Trabajo" (YET) operates in 77 localities across Uruguay, covering nearly all major cities.

Applicants can complete the application online or at an employment center. To participate, selected applicants must present proof of enrollment from an educational institution showing at least 240 hours of attendance, a valid national ID, and, if over age 18, an electoral card. Eligibility is verified by cross-checking social security data to confirm that the applicant is not formally employed. Students must also provide updated enrollment proof every three months. Those aged 16–17 receive guidance on how to obtain a work permit.

As of January 2016, participants earn a fixed monthly salary of 13,360 pesos (equivalent to four times the minimum tax unit) for 30 hours of work per week. Pregnant women and mothers with children under age 4—who make up about 4 percent of lottery applicants—receive 50 percent higher wages.

Students can reapply in future rounds under specific rules. Those who begin a job through the program cannot apply again, while those who are selected but do not take up a position may reapply, but do not receive any priority.

Firms agree in advance to participate in the program. Once youths are randomly selected, participating firms are required to hire the individuals assigned by the program. From the firms' perspective, opening temporary one-year positions in state-owned enterprises is not straightforward. The program simplifies this process by enabling them to fill additional vacancies quickly without burdensome procedures. Firms also retain the option to dismiss youths who fail to meet performance expectations; however, high completion rates suggest that this rarely occurred.

Importantly, the list of participating firms is announced publicly before the application period begins. As a result, applicants know which firms are involved at the time of application and can anticipate their potential placements. In some small regions, only a single firm participates, meaning that applicants' information about their likely placement is nearly complete. State-owned enterprises involved in the program are considered prestigious employers, and their positions attract strong demand. In addition, the tasks assigned to program participants are typically less physically demanding than those undertaken by comparable workers outside the program. A plausible explanation for the high completion rates is that applicants were positively selected: those who applied to the program demonstrated a genuine interest in working and were motivated to complete their assignments. Furthermore, participants who complete the full contract period are granted a work certificate.

E Returns to Education and Returns to Experience

In this section, we present our back-of-the-envelope computation of the contribution of the education and experience channels to the long-term effects of the YET work-study program. First, we review existing estimates of the returns to schooling and to work experience from recent literature and obtain a range of relevant estimates for Uruguay. Second, we combine these estimated returns with the work-study program effects on schooling and experience to quantify their respective contributions to Year–7 effects on earnings.

Gethin (2025) reports estimates of the returns to education for 154 countries, including Uruguay. He runs an OLS estimation of a modified Mincerian regression, controlling for gender, an age quartic, and their interaction. For Uruguay, he uses data from the Continuous Household Survey (ECH 2019). The estimated returns to schooling for Uruguay are 9.96 percent. A well-recognized issue with OLS estimates is selection bias. In a meta-analysis of 33 studies, Gethin (2025) finds that IV estimates of the returns to schooling (correcting for selection) are on average 40 percent higher than OLS estimates. Uruguay is not included in the meta-analysis, but applying the average IV-OLS gap, we obtain an upper bound of 14 percent for the returns to schooling.

Jedwab et al. (2023) report estimates of the returns to potential work experience for 145 countries. They run OLS estimations of a Mincerian regression controlling for years of education. Their Figure 3 reports returns to experience of 2.8 percent for Uruguay, slightly higher than the world average. In their online appendix, Figure A.3 reports returns to schooling of 11 percent, close to Gethin (2025).

Lagakos et al. (2018) study the sensitivity of Mincerian returns to potential experience to the well-known cohort–time–age collinearity issue and to measurement error in work experience. They find that, in rich countries, the average wage of workers with 20–24 years of experience is between 91 and 96 percent higher than that of workers with 0-–4 years of experience in Table 3, between 47 and 90 percent higher in Table 4, and between 70 and 100 percent higher in Table 5, depending on the estimation methodology. The lower and upper bounds of these ranges correspond to 59 percent (47/79) and 127 percent (100/79) of their baseline estimate of 79 percent in Table 5. In Figure 3, they show that the percentage wage increase at 20–24 years of experience in Uruguay is 75 percent, which corresponds to a yearly return of around 3.8 percent (Extended National Survey of Households, 2006, available from IPUMS). To account for estimation sensitivity around the Uruguayan estimate, we consider a lower bound of 2.2 percent (= 0.59×3.8) and an upper bound of 4.8 percent (= 1.27×3.8). The returns to experience in Jedwab et al. (2023) lie within that range.

To sum up, relevant estimates of the returns to schooling range from 10 to 14 percent, and those for work experience range from 2 to 5 percent. We find that the work-study program increases education by 0.27 years and actual work experience by 0.7 years. The education channel generates an earnings increase between 2.7 percent (= 0.27×10 percent) and 3.8 percent (= 0.27×14 percent), while the experience channel generates an increase between 1.4 percent (= 0.7×2 percent) and 3.5 percent (= 0.7×5 percent).

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